NBER WORKING PAPER SERIES

SOCIAL INSURANCE SPILLOVERS:
EVIDENCE FROM PAID SICK LEAVE MANDATES
AND WORKERS' COMPENSATION

Xiuming Dong
Johanna Catherine Maclean
David Powell

Working Paper 32751
http://www.nber.org/papers/w32751

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
July 2024

Authors contributed equally to this research and are listed in alphabetical order. Research reported in this publication was supported by the National Institute on Mental Health of the National Institutes of Health under Award Number 1R01MH132552 (PI: Johanna Catherine Maclean). The views expressed herein are those of the authors and do not necessarily reflect the views of the National Institutes of Health. This research was conducted with restricted access to Bureau of Labor Statistics (BLS) data. The views expressed here do not necessarily reflect the views of the BLS. We thank session participants at the American Society of Health Economists Annual Research Conference and Douglas Webber for comments. We thank Maury Gittleman and Michael Letteau for assistance with the National Compensation Survey. All errors are our own. The views expressed here do not necessarily reflect the views of the BLS. We thank session participants at the American Society of Health Economists Annual Research Conference and Douglas Webber for comments. We thank Maury Gittleman and Michael Letteau for assistance with the National Compensation Survey. All errors are our own. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

Social Insurance Spillovers: Evidence From Paid Sick Leave Mandates and Workers' Compensation
Xiuming Dong, Johanna Catherine Maclean, and David Powell
NBER Working Paper No. 32751
July 2024
JEL No. I18,J28,J32

ABSTRACT

Social insurance programs shield individuals from specific risks, but these programs are not necessarily independent of each other. The existence and scope of one program can potentially influence the use of others, especially when the risks covered by the programs are related. This study investigates the relationship between two mandated benefit programs in the United States: state paid sick leave (PSL) mandates and workers' compensation. Unlike most developed countries, the U.S. lacks a federal PSL mandate; however, 15 states have implemented such policies. PSL mandates require firms to provide compensated time off for employee health-related needs, while workers' compensation offers benefits to help workers recover from workplace injuries or illnesses. Using a difference-in-differences analysis, the study explores the impact of state PSL mandates on the usage of workers' compensation benefits. The findings reveal meaningful spillover effects: when states adopt PSL requirements, there is a decrease in workers' compensation benefit receipt. While some evidence suggests possible improvements in health, there are no observed reductions in workplace injury rates specifically, indicating that workers may substitute PSL benefits directly for workers' compensation.

Xiuming Dong
Johns Hopkins University
xdong29@jhu.edu

Johanna Catherine Maclean
Schar School of Policy and Government
George Mason University
Mason Square
Van Metre Hall
3351 Fairfax Drive
Arlington, VA 22201
and NBER
jmaclea@gmu.edu

David Powell
RAND Corporation
1776 Main Street
P.O. Box 2138
Santa Monica, CA 90407
dpowell@rand.org

1 Introduction

Social insurance programs are designed to protect individuals from risks, with different programs offering coverage targeted to address specific uncertainties. While each program is crafted to cover distinct risks, they are not isolated entities; rather, the presence and generosity of one program can impact the utilization of another. These interactions can be complements or substitutes (Inderbitzin et al., 2016), depending on whether a program encourages the uptake of additional social insurance or substitutes for similar benefits. The extent and direction of these spillover effects are influenced by factors such as the alignment of risks targeted by the programs, the efficiency and costs of screening processes to determine eligibility, and the comparative benefits of each program, among other considerations. Recognizing potential spillovers is crucial for a comprehensive assessment of the benefits and costs associated with any program (Baily, 1978; Chetty, 2006) and for evaluating the fiscal externalities linked to the program (Hendren, 2016; Finkelstein and Hendren, 2020), which are pivotal in determining optimal benefit levels (Chetty and Finkelstein, 2013).

Such fiscal externalities and program spillovers have been studied in a variety of contexts across different countries. Borghans et al. (2014) estimate that reductions in disability insurance benefits in the Netherlands increase the use of benefits from other social assistance programs. Petrongolo (2009) finds that stricter unemployment insurance requirements in the United Kingdom lead to increases in use of disability benefits. Karlström et al. (2008) observe increases in the use of paid sick leave and unemployment insurance in Sweden when disability screening criteria become more restrictive. Staubli (2011) finds that stricter criteria for disability insurance in Austria increase the use of sickness insurance; related work estimates that extended unemployment benefits in Austria reduce short-term use of disability benefits with evidence of downstream program complementarities (Inderbitzin et al., 2016). Research also considers whether unemployment insurance and workers' compensation in Canada are substitutes (Fortin and Lanoie, 1992; Fortin et al., 1999).

In the U.S. context, there is research examining the relationship between retirement benefits and disability insurance enrollment (Duggan et al., 2007) and substitution between the Aid to Dependent Families with Children (ADFC) and Supplemental Security Income (SSI) programs (Schmidt and Sevak, 2004; Garrett and Glied, 2000). McInerney and Simon (2012) find that less generous workers' compensation does not increase the use of disability insurance in the U.S. while Schmidt et al. (2020) conclude that Medicaid expansions do not meaningfully alter disability insurance applications.[1]

This literature generally explores the interplay between two government-operated social insurance programs. Social insurance benefits can be administered either directly by the government or mandated to be offered by employers. In contrast to much of the literature, this study investigates potential substitution between two employer-mandated benefits. Moreover, prior research typically uses variation in program generosity to examine the impacts on alternative forms of social insurance. Here, we analyze the implementation of a new social insurance program to assess its impact on an established program that has been in place since the early-1900s.

This paper studies the impact of state paid sick leave (PSL) mandates on the use of workers' compensation (WC) in the United States. PSL benefits typically target short-term (personal or family) health issues and entitle eligible employees to full salary compensation. While common in most developed countries, PSL mandates are relatively new to the United States. WC is the oldest social insurance program in the U.S. and has been a critical component of the social safety net, providing benefits to workers who become injured or ill while working. The program includes a range of possible benefits including cash benefits (two-thirds of a worker's wages up to a statutory maximum for temporary disabilities), medical benefits, and vocational rehabilitation.

Understanding the impacts and spillovers of PSL mandates is especially important given the active policy landscape surrounding this benefit. Research on PSL in the U.S. is in its infancy since PSL mandates are relatively uncommon. As of October 2023, only 14 states and DC, 17 cities, and four counties have adopted or announced a PSL mandate (National Partnership of Women & Families, 2023). In addition, examining the policy implications for WC is particularly crucial, considering the program's significant role in the U.S. social safety net. Workplace injuries represent substantial health and financial shocks to individuals and their families, and WC is designed to alleviate some of these risks. In 2020, overall direct program costs for WC totaled \$97.2 billion (Murphy and Wolf, 2022), larger than the costs of the SSI program – \$55.4 billion (Congressional Research Service, 2024) – and the Earned Income Tax Credit – \$48.9 billion (IRS, 2023).[2] These costs are borne by both firms and workers (Gruber and Krueger, 1991).[3]

We study the fiscal externalities related to the introduction of state PSL mandates

[1]While not directly studying two separate social insurance programs, Lawson (2017) quantifies the costs of unemployment insurance benefits on tax revenue in the U.S.

[2]We deflate the reported estimates \$64.6 (Supplemental Security Income) and \$57.0 billion (Earned Income Tax Credit) in 2023 dollars to 2020 dollars using the Consumer Price Index.

[3]In a Summers (1989) framework, employers can shift the costs of WC, or PSL, benefits to workers in the form of lower wage or non-wage compensation. We will test for such behaviors in our analyses.

on WC cash benefits. PSL mandates provide workers with up to seven days of PSL each year with 100% wage replacement, and some mandates require the provision of unpaid sick leave (UPSL) as well (Pichler and Ziebarth, 2020b). There is little evidence of interactions between employer mandates, and one motivation for studying interactions between PSL mandates and WC is the possible scope for these programs to have meaningful substitution effects between them.

Workers could substitute PSL benefits, which tend to be more generous (per day), for WC benefits.[4] Application and screening costs are arguably higher for WC than PSL, suggesting an additional motivation for substitution among marginally ill or injured workers towards PSL. Most WC claims do not involve any time away from work, but WC is designed to cover long-term absences from work when necessary. Workers who require 'medical only' or longer-term WC are not likely to view PSL as a feasible substitute as PSL does not provide financial support for healthcare nor longer-term separations. While distinct from each other in many ways, these programs address related concerns (for some workers) by partially protecting workers from financial risks associated with health issues, suggesting scope for possible substitution between the two.[5]

Alternatively, PSL mandates provide workers with the financial protection to take short-term separations from work to rest/recover or receive healthcare, thereby preventing conditions from worsening and reducing the need for WC. PSL might also provide additional time for workers to apply for WC benefits, which would suggest that PSL mandates will increase WC income receipt. PSL mandates could further influence WC claiming behavior more broadly through a host of mechanisms, including shifting labor supply or labor demand, influencing workplace injury rates, or affecting claiming behavior conditional on an injury. In this paper, we consider many of these mechanisms by studying broader measures of labor supply and demand, health outcomes, time use including time spent applying for benefits, injury rates independent of WC claims, WC benefit receipt, and other related variables. By examining a comprehensive range of outcomes, we narrow down the possible mechanisms driving our main findings.

We combine several national databases over the period 2005 to 2019 to study the relationship between state PSL mandates and WC outcomes. First, we use restricted data from the National Compensation Survey (NCS) to study the impact of state PSL mandates on worker access to and use of PSL. Second, we use the Current Population

[4] However, PSL benefits are subject to taxation (i.e., workers are taxed on the income they earn while on PSL) while WC benefits are generally not subject to income taxes.

[5] Another important distinction is that PSL mandates allow workers to take paid time off from work to attend to the health needs of dependents while WC benefits accrue solely to the worker, or in the case of a fatality, the worker's surviving family.

Survey (CPS) to investigate changes in absence from work for a health-related reason with pay. Third, we test the effect of PSL mandates on WC income receipt in the CPS, and we complement this analysis using administrative data on state-level WC expenditures from the Bureau of Economic Analysis (BEA). Fourth, we examine broader changes in labor supply, labor demand, health, and time use in the CPS, U.S. Census County Business Patterns database (CBP), and American Time Use Survey (ATUS). This analysis allows us to explore whether changes in WC claiming are driven by variation in labor demand or supply, time spent applying for benefits, or overall health status. Fifth, we evaluate the effects of PSL mandates on fatal and non-fatal injury rates using worksite data from the Bureau of Labor Statistics (BLS). These metrics are collected independently of WC claiming and should provide evidence about the extent to which workplace injury rates are impacted versus WC claiming behavior changing.

Our analysis leverages the staggered adoption of state PSL mandates. We implement a two-step difference-in-differences procedure which imputes counterfactual outcomes using information from untreated observations. This approach is robust to bias associated with a staggered treatment regime and treatment effect heterogeneity and dynamics (Gardner, 2022; Borusyak et al., 2024).

Our findings demonstrate a causal link between state PSL mandates and WC income receipt. Following a state PSL mandate, worker access to PSL, use of PSL, use of UPSL, and the probability of being absent from work with pay for a health-related reason increase. Evidence based on time-use data further suggests that workers use PSL to take time away from work to rest. We subsequently find a positive spillover of state PSL mandates on WC-related outcomes. We estimate that the probability of receiving any WC income declines by 13.5%, and the level of WC income falls by 15.3% post-PSL mandate. The estimates imply an elasticity of WC costs with respect to PSL costs of -0.92. Evidence based on BEA expenditure data also shows similar declines in WC costs.

There is little evidence that our WC findings are driven by shifts in labor supply or demand, suggesting that the decline in WC benefit receipt is not due mechanically to a reduction in hours or weeks worked (i.e., exposure to possible workplace injuries or illnesses). We also observe no evidence that workplace injury rates themselves are directly impacted. Instead, the evidence is most consistent with workers sometimes using PSL as a substitute source of insurance in lieu of claiming WC benefits.

This substitution itself defrays a meaningful share of the costs of PSL mandates. In the conclusion, we calculate the paid sick leave costs associated with PSL mandates using evidence from the literature. Given this estimate, our findings imply that the cash benefits savings to WC alone offset 14% of the costs associated with PSL mandates.

In the next section, we provide more background about the two social insurance programs that we study. Section 3 discusses the data sources and the empirical strategy. We present the results in Section 4 and conclude in Section 5.

2 Background

2.1 Paid sick leave in the United States

The U.S. is one of just four industrialized countries that does not have a federal PSL mandate (Pichler and Ziebarth, 2020b). While the Family and Medical Leave Act (FMLA) provides eligible employees with up to 12 weeks of unpaid leave in a 12-month period for an employee who has a serious health condition that makes the employee unable to work, FMLA benefits are not available for shorter duration health issues. For example, unpaid FMLA leave cannot be used for healthcare professional visits to treat short-term acute injuries caused by cuts, falls, and/or muscle sprains unless the conditions incapacitate the employee or their family member for more than three consecutive days and involve ongoing medical treatment. In addition, many workers are ineligible for FMLA coverage because they work for small employers who are exempt or do not meet the Act's work history requirements.[6]

More generally, large portions of the U.S. workforce are unable to take time off for health, healthcare, or family responsibility needs without losing earnings. In 2021, nearly 30% of employees reported no access to PSL (Rosa and Asfaw, 2023), although government data suggest that coverage rates are higher with employers stating that 78% of civilian jobs provide PSL (U.S. Bureau of Labor Statistics, 2020).[7] The inability to take time off without losing earnings may prevent some individuals from seeking treatment for themselves or their family members. Despite the lack of a federal provision, PSL is popular, with 84% of Americans supporting policies that would mandate PSL benefits (Global Strategy Group, 2021). Recently, in 2023, U.S. Senators Rosa DeLauro and Bernie Sanders announced the Healthy Families Act, which would provide nearly all employees with seven days of PSL per year (Bernie Sanders and Rosa DeLauro, 2023).[8]

[6] FMLA covers workers who worked 1,250 hours in the last 12 months in businesses with more than 50 employees. The Department of Labor states that 44% of workers are ineligible for FMLA benefits (Heymann et al., 2021).

[7] The reasons behind the discordance between benefits reported by employees in survey settings and employers in establishment surveys are unclear. Possible reasons include employees not being aware of their benefits and employers reporting overly generous benefit packages.

[8] The Healthy Families Act was first introduced by U.S. Senator Ted Kennedy in 2005. The Act has been re-introduced multiple times since 2005 but has not been implemented by the time of writing.

Despite federal inaction, several states have adopted PSL policies. Figure 1 shows the geographic distribution of these mandates (effective or announced) across states as of October 2023, using legal data prepared by the National Partnership of Women & Families (2023). Figure 2 depicts the temporal variation in these mandates. DC (which we treat as a state in our analysis) adopted the first state-level mandate in the U.S. in 2008. By October 2023, 15 states had adopted or announced a PSL mandate. Additionally, 17 cities and four counties have adopted PSL policies.[9] In our main analyses, for reasons described in Section 3.1, we focus on state PSL mandates. However, our results are not appreciably different if we incorporate sub-state policies (Section 4.5).

PSL mandates require employers to allow employees to accumulate and then use PSL days, with unused benefits generally available to be rolled over to the following year. For each 30 to 40 hours of work (depending on the state), employees earn one hour of paid sick leave (up to seven days per year), which they can use when they or a family member becomes sick. Unlike state paid family and medical leave policies, PSL benefits are financed by employers, who are required to post information about these benefits at the worksite to inform employees of them. There are some differences across states (e.g., accrual rates and exemptions – smaller firms or student workers). See National Partnership of Women & Families (2023) for full details on state PSL mandates. In our work, given the recency of PSL mandates in the U.S., we study whether a state has a mandate in place and not the impact of specific provisions.

During the COVID-19 pandemic, the U.S. federal government implemented a temporary PSL policy under the Families First Coronavirus Response Act (FFCRA) which, from April 1, 2020 through December 31, 2020, offered a sub-set of employees ('non-essential workers' employed in firms with 50-500 workers) up to two weeks of PSL for COVID-19 related illness, exposure, or family responsibilities (Andersen et al., 2023). While we provide some results covering this period, the presence of a federal policy potentially mitigates the impact of state policies so we focus on the period prior to the federal policy in our main analysis.

There is a small but growing literature investigating the effects of state and local PSL mandates in the U.S. There is a much larger literature based on the experiences of European countries, where PSL policies have been in place for a longer period and are typically more generous. We focus our attention on the U.S. experience here. Maclean et al. (2024) demonstrate that following a state PSL mandate, the probability that

[9] When a state and a sub-state jurisdiction both adopt a PSL, the most generous policy is binding. We note that state preemption laws can limit the ability of sub-state jurisdictions to implement policies more generous than the state mandate.

an employer offers PSL to an employee increases by 31.6%. Studies based on survey data show that employee-reported access to PSL also increases following a mandate (Ahn and Yelowitz, 2016; Callison and Pesko, 2022; Maclean et al., 2023). Maclean et al. (2024) document that employees increase the use of PSL and unpaid sick leave by 20.2% and 134.4% per year. In turn, healthcare use (check-ups, contraception, fertility treatment, screenings, prescriptions, and vaccinations) increases (Pichler and Ziebarth, 2017; Maclean et al., 2024; Pichler et al., 2021; Callison et al., 2023; Maclean et al., 2023, 2024; Guo and Peng, 2024), health improves (Slopen, 2023), and employee productivity increases (Callison and Pesko, 2022; Chunyu et al., 2022). Slopen (2024) shows that PSL mandates improve employment and earnings outcomes among women; in particular, women with children and with lower levels of education. Correspondingly, emergency department episodes decline post-mandate (Ma et al., 2022). Arora and Wolf (2024), Guo and Peng (2024), and Maclean and Pabilonia (2024) document increases in care-giving, in particular care for older adults and in families with minor children in the household.[10] One study shows that PSL mandates may reduce consumer bankruptcies, suggesting that these benefits provide financial protection to employees (Miller, 2022).[11] Pichler and Ziebarth (2020b) provide a comprehensive review of the U.S. PSL literature.

Of particular interest to our study, Dong (2022) investigates the effect of the 2007 San Francisco, California PSL mandate over the period 2001 to 2012 and shows that WC claim rates decrease by 6% post-mandate. This study varies from the current one in several respects. First, we examine state mandates which, due to their broader scope, may pose greater challenges for employers to circumvent, and employees are likely to be more cognizant of state-level policies. Second, the San Francisco law only included up to three days of PSL, which might suggest that impacts should be smaller than the effects of state laws (which cover up to seven days). Further, employers in San Francisco were more likely to (voluntarily) offer PSL pre-mandate than comparable firms across the country. Colla et al. (2014) report that in 2006 (the year before the San Francisco mandate) 73% of private firms offered PSL while just 61% of private workers across the country had access (Kramer and Ziberman, 2008).[12] Second, we study income receipt and do not measure claims that do not involve a separation from work ('medical only' as described in Section 2.2). There may be differential elasticities across the alternative

[10] Guo and Peng (2024) find limited evidence on the use of self-reported preventive care or risky behaviors, as measured by excessive drinking, in survey data.

[11] The author documents no evidence that business bankruptcies increase post-mandate, which is in line with the findings of Maclean et al. (2024) that state PSL mandates are not costly to employers, with costs per employee-hour increasing by 5.8 cents post-mandate.

[12] Between 2007 and 2008, the National Compensation Survey changed the measurement of access to PSL benefits, thus data from 2008 are more comparable to that used in Colla et al. (2014).

types of claims; in particular, medical only claims are potentially less substitutable for PSL time as PSL mandates do not confer financial support for healthcare. Third, we study a more recent time period when PSL coverage has become more common. Workers may now be more aware of these benefits than in the Dong (2022) time period. We build on the Dong (2022) study by examining the impact of PSL mandates at the national level and considering a range of proxies for work capacity and exploring potential mechanisms.

2.2 Workers' compensation

WC is the oldest social insurance program in the U.S. The program requires employers to provide predefined benefits to employees when they are injured at work or have acquired an occupational disease. Currently, WC coverage is mandatory for employers in all but three states (Texas, South Dakota, and Wyoming), where employers can opt out of the WC system (Cabral et al., 2022; Murphy and Wolf, 2022; Jinks et al., 2020). In 2020, state WC programs covered 135.6 million workers at a cost to employers of \$97.2 billion (Murphy and Wolf, 2022). WC provides no-fault coverage for almost all workplace injuries and illnesses. Workers report the injury/illness to the employer, the employer's WC insurance party adjudicates the worker's claim, and denials can be appealed to the state WC commission (Murphy and Wolf, 2022). Notably, both physical and mental health conditions can be approved for illnesses/injuries for a WC claim.

Employers typically purchase WC coverage from a private insurer or a state fund, or they self-insure. WC benefits vary across states in magnitude and duration and generally depend on the claim type. There are three broad types of WC claims: i) medical only – these benefits cover 100% of all medical costs associated with the workplace injury/illness; ii) medical and cash benefit claims – these benefits cover medical care costs and compensated time away from work to recover; and iii) funeral, burial expenses, and cash benefits to families of workers who die while working.

Temporary disability claims pay benefits covering approximately two-thirds of the worker's gross pre-injury weekly wage (up to a maximum) until the worker is able to return to work or a maximum duration is met. Permanent disability claims involve workers with injuries or illnesses resulting in permanent impairment.[13] Though most claims are medical-only, in 2018, only 10% of the total WC benefits paid out were for medical-only claims, 3% were related to fatalities, and 87% of total benefits were for cash

[13] A literature has evaluated how benefit generosity impacts the number of claims and benefit duration (Neuhauser and Raphael, 2004; Krueger, 1990a,b; Bronchetti and McInerney, 2012; Meyer et al., 1995). Cabral and Dillender (2024) examine how benefit generosity affects both income benefit duration and medical spending.

benefit claims (Murphy and Wolf, 2022; Abouk et al., 2023). In this paper, we study WC cash benefit receipt, which reflects the majority of WC benefits received in the U.S. and is likely a closer substitute for PSL as medical or vocational rehabilitation services are not covered by PSL mandates.

Estimates suggest that only half of eligible workers place a WC claim (Lakdawalla et al., 2007; Baidwan et al., 2020). Workplace environment, employer incentives, and whether the employer offers alternative benefits – such as paid sick time – may have a significant impact on WC claim filing (Biddle and Roberts, 2003). Fear of retaliation from the employer, lack of knowledge regarding reporting, and distrust of reporting consequences are other reasons for WC under-claiming (Fan et al., 2006; Lipscomb et al., 2013, 2015; Green et al., 2019). The reporting burden on workers is typically substantially less for a PSL claim than for a WC claim. Workers generally do not need to provide documentation to use PSL (National Partnership of Women & Families, 2023); instead, they must notify the employer (notification can be waived for emergency leave). This evidence suggests that marginally ill or injured workers may choose to claim PSL over WC when both benefits are available (Scherzer et al., 2005; Kyung et al., 2023), though other factors are also important.

3 Data and methods

We combine several data sets to study WC benefit receipt as well as a number of complementary outcomes which help us speak to the underlying mechanisms driving the main relationships identified in the paper. In this section, we describe these data sources in detail followed by a discussion of our difference-in-differences strategy.

3.1 Paid sick leave mandates

We use legal data prepared by the National Partnership for Women & Families (NPWF) to measure state PSL mandates. The NPWF is a non-partisan research group that collates legal data on policies that support working families. We use effective dates provided by NPWF on all state-level PSL mandates adopted or announced by October 2023 (National Partnership of Women & Families, 2023). We will primarily leverage changes that occurred between 2005 and 2019, but we will also show results using state policy changes implemented or announced through October 2023.

Table A1 provides a list of the states that have adopted or announced a PSL mandate by October 2023 with the effective dates of the policies. Figure 1 shows the geographic distribution of these mandates, while Figure 2 displays the staggered rollout of policies over time. There is some geographic clustering of mandates across states; there are very few states in the Midwest and South regions that have adopted PSL mandates. Table A1 also reports estimates, prepared by the NPWF (2023), of the number of employees gaining PSL coverage for the first time due to these mandates. The total number across all state mandates is over 21 million. In addition, many employees likely gain more generous coverage post-mandate since employees may work in jobs that offer some PSL, but the coverage is not as generous as the levels codified in the mandate.

Four states have adopted or announced paid time off (PTO) mandates (Illinois [2024], Michigan [2019], Maine [2021], and Nevada [2020]), but not PSL mandates (National Partnership of Women & Families, 2023).[14] These PTO policies, only one of which occurs during our main study period (Michigan), provide employees with paid time off regardless of purpose. We follow legal coding proposed by the NPWF and treat PSL and PTO mandates as distinct policies since PTO mandates provide less protection for employees than PSL mandates. For example, PTO mandates offer limited or no protection against employer retaliation for employees who request or use PTO; do not limit the employer's ability to require the employee to locate a replacement employee during the period when the employee is on leave; do not offer protected ability to take leave without advance notice; and impose no limitations on documentation or requirements needed to be granted paid leave. We control for PTO mandates in our primary specification, but in a robustness check (see Section 4.5) we report results where we construct an indicator for either a PSL or a PTO mandate, and the results are not meaningfully different.

We focus on state mandates in this paper since the location of employment, not the location of residence, is the salient unit of geography for our purposes.[15] We do not have information on the location of employment in the Annual Social and Economic Supplement (ASEC), only the location of residence. In the 2019 American Community Survey (Ruggles et al., 2023), we find that 97% of employed adults 25 to 62 years of age live and work in the same state, but just 77% live and work in the same county. Further, in the public use ASEC (discussed below), sub-state geography information is not reported for all respondents due to privacy concerns. There are several variables that allow researchers to link sub-state information to the data, but these variables do not completely characterize the location of residence for all respondents (Van Riper et al., 2021) and errors are likely, both in coding untreated areas as treated and vice versa. Moreover, the ASEC is not designed to be representative at the sub-state level, which

[14] No state has adopted both a PSL and a PTO mandate.

[15] In the NCS analysis, sample sizes are too small to study sub-state mandates (Maclean et al., 2024).

raises concerns about the quality of estimates based on such analyses; see Maclean et al. (2023) for a general discussion of this issue. However, despite these caveats in Section 4.5, we report results that use variation from the sub-state policies (to the extent possible), and our findings are not appreciably different.

3.2 Data on workers' compensation income

Our primary data source on WC benefit receipt is the (public use) CPS ASEC. The ASEC is fielded between February and April each year by the U.S. Census Bureau on behalf of the BLS. Approximately 150,000 adult (15 years and older) respondents are asked a range of questions related to income sources, benefit receipt, health insurance, labor market participation, and demographics.

The ASEC asks respondents for the amount of pre-tax income that they receive from WC payments or from other payments that are the result of a job-related injury or illness.[16] WC income pertains to income received in the past calendar year – for example, a respondent to the 2019 survey reports income in 2018. We use outcomes from calendar years 2005 to 2019 in our analyses. We truncate the analyses in 2019 to avoid confounding shocks from the COVID-19 pandemic and the adoption of a federal PSL mandate in 2020, though we also report results including calendar years 2020 to 2022. We study respondents ages 25 to 62 years. In our main analyses, we do not exclude the non-employed, but the findings are robust to their exclusion (see Section 4.5).

We use the ASEC data to construct two measures of annual WC income receipt: i) any WC income (zero if no income, one otherwise) and ii) the (unconditional) income from WC (zero if no income). We convert the level of income to 2019 terms using the Consumer Price Index-Urban Consumers. Both WC measures capture important information. The first measure speaks to any claiming within the year. The second measure also includes intensive margin effects such as longer duration claims.[17] These measures only capture respondent-reported cash benefits from WC, not other WC benefits such as medical and rehabilitation benefits. Cash benefits are received when the worker requires a separation from work to recover from the injury or illness. As PSL does not provide re-

[16] We use the IPUMS harmonized ASEC variable *INCWKCOM*. This variable does not include income from sick pay, disability, or retirement. The question wording in the 2019 ASEC is as follows: *How much did (name/you) receive (weekly/every other week/twice a month/monthly) in Worker's Compensation during 2018?*

[17] Total annual benefits – conditional on any receipt – can vary for reasons other than variation in injury duration, including who is receiving benefits since payments are based on pre-injury weekly wages. We do control for education, which will – to some extent – capture differences in wages and, hence, differences in WC income benefits. We will not be able to distinguish whether PSL alters injury duration, multiple claims within a year, or changes in the composition of those claiming WC benefits.

sources to purchase healthcare or rehabilitation services, medical and rehabilitation WC benefits do not offer the same degree of substitution with PSL as do cash WC benefits.

WC information is self-reported by respondents and, thus, subject to non-response and misreporting (Meyer et al., 2015). We do not expect non-response or misreporting to be correlated with state PSL mandate adoption; thus, we anticipate that our results will be attenuated as we do not observe all WC income potentially impacted by the policy.[18]

While the ASEC data are imperfect, there is no national database of WC claims available for our purposes. There are federal WC programs for specific worker types (e.g., railroad workers) and states have their own programs. Our research design leverages variation in state policies for identification of PSL mandate effects so we require observing data on both adopting and non-adopting states over a number of years. Researchers often rely on the ASEC to analyze WC outcomes when studying state-level policies (Gruber and Krueger, 1991; Krueger, 1990a; Hirsch et al., 1997; Bronchetti and McInerney, 2012; Ghimire and Maclean, 2020; Bronchetti and McInerney, 2021; Abouk et al., 2023).[19]

We will evaluate several other outcomes using the ASEC. To explore 'first-stage' effects, we will construct an indicator for being away from work due to a health-related reason with pay in the week prior to the survey.[20] This variable proxies access to and use of PSL among employees. We also use the information on employment status, weeks worked per year, usual hours worked per week, full-time work, hourly wage, self-reported health, and self-reported work-limiting disability to study whether state PSL mandates lead to changes in broader measures of health and labor supply.[21] Understanding these outcomes provides evidence about the possible general equilibrium effects of PSL mandates on the labor market and helps isolate whether declines in WC receipt are driven mechanically by declines in employment or hours worked. We also explore whether employers reduce offerings of other benefits (see for example, Summers [1989]) which might independently alter the demand for WC benefits. To this end, we consider employer-sponsored health insurance offers and annual premiums.[22]

[18]In a robustness check, we exclude imputed observations as misreporting is more substantial for such observations (Meyer et al., 2015). Results are not appreciably different.

[19]The American Community Survey does not include WC income.

[20]To create this variable, we combine three IPUMS harmonized variables: i) *ABSENT* (absent from work last week), ii) *WHYABSNT* (reason for absence from work), and iii) *PAYIFABS* (paid if absent from work last week). Non-employed respondents and those in the military are coded as missing.

[21]We use the following IPUMS variables: employment status (*EMPSTAT*), ususal hours worked per week (*UHRSWORKLY*), full-time work (*WORKLY* and *UHRSWORKLY*), hourly wage (*HOURWAGE*), self-reported health (*HEALTH*), and self-reported work-limiting disability (*DISABWRK*).

[22]To measure employer-sponsored health insurance, we use the IPUMS variable *INCLUGH*. The question wording is '*(Do/Does/Did) (name/you) get it through a former employer, a union, a group or association, the Indian Health Service, a school, or some other way?*.' This variable is only available through 2018. To measure premiums, we use the IPUMS variable *HIPVAL* which measures total family

Finally, to test for one potential threat to identification, we will study the rate of past-year across-state moves (Moffitt, 1992) in the ASEC.[23] We construct an indicator variable that is equal to one if the respondent reports residing in a different state at the time of the survey than as of one year ago, and zero otherwise.

3.3 Other data sources

We complement our ASEC analyses to further explore mechanisms explaining the main findings of the paper. We describe these data here.

National Compensation Survey: We study PSL offers, and PSL and USPL utilization in the restricted use National Compensation Survey (NCS) 2009-2019. The NCS is a nationally representative survey of establishments. These data are administered by the BLS and are used to produce official U.S. government statistics on compensation and labor costs. The NCS collects data on jobs in the surveyed establishments; thus the data include information on job characteristics but no information on the workers who hold these jobs. The BLS samples jobs probabilistically within establishments for selection into the survey. Establishments remain in the survey for three to five years. We include private and government establishments in our analysis of the NCS.

We will use the NCS to study the effects of state PSL mandates on sick leave outcomes. To study PSL outcomes, we construct three variables to capture the impact of PSL mandates on employee access to these benefits: i) an indicator variable coded one if the establishment offers PSL benefits to an employee in the sampled job (the indicator is as of March in each year), ii) average PSL use among employees in a sampled job (measured in hours per year), and iii) average UPSL use among employees in a sampled job (measured in hours per year).[24] We exclude 52,527 observations that have imputed PSL information (as recommended by NCS administrators). For the PSL and UPSL use variables, we aggregate quarterly data to construct year t PSL use measures by aggregating Q2, Q3, and Q4 for years t-1 and Q1 in year t following Maclean et al. (2024). Using all four quarters provides a complete picture of annual leave-taking.[25]

American Time Use Survey: We use data from the American Time Use Survey

payments for health insurance premiums. Only families that pay health insurance premiums have a valid response to this item from 2011-2018, and all respondents are asked the question in 2019.

[23] We use the IPUMS variable *MIGRATE1*.

[24] These outcomes are similar to those studied in Maclean et al. (2024). Given differences in time periods and methods, we include our own analysis of these outcomes in this paper.

[25] The NCS includes a variable measuring WC costs to employers (costs per hour worked). Unfortunately, the BLS has elected to discontinue the collection of WC information in the NCS due to low response rates and other data quality concerns (Gittleman et al., 2023). For this reason, we do not include an analysis of WC costs from the NCS in the manuscript.

(ATUS) to test whether reported daily time use on work (which we view as an alternative proxy for PSL use), rest (which we proxy with leisure activities), purchasing self-healthcare, purchasing healthcare, and applying for or using government benefits (including WC benefits) change following the adoption of a state PSL mandate. We use harmonized ATUS-X data maintained by IPUMS (Flood et al., 2023).[26]

The ATUS, conducted by the U.S. Census Bureau on behalf of the BLS, is an annual survey that collects data on the amount of time people spend on particular activities. Respondents to the ATUS have completed all eight months of the CPS and are surveyed two to five months after completing the CPS. Respondents complete a diary of all time spent over a 24-hour period. We retain respondents 25 to 62 years of age who report data for a weekday. We apply ATUS-provided weights.

Bureau of Economic Analysis: The BEA reports information on compensation received by individuals with employment-related injuries and illnesses paid by state and federal government administered funds but excluding privately administered workers' compensation funds (Bureau of Economic Analysis, 2024).[27,28] While incomplete, this variable represents an administrative (not self-reported) measure of WC costs which is uniformly reported across states, providing a useful complementary metric to our ASEC measure.[29] Using these data, we construct WC expenditures (in 2019 dollars) per 100,000 state i) residents and, as a separate measure, ii) employed persons.

County Business Patterns: We use data on the total number of establishments and the total number of employees working at these establishments per state from the U.S. Census Bureau's CBP database (U.S. Census Bureau, 2022) as a measure of labor demand. The Census Bureau defines an establishment as a '*single physical location at which business is conducted or services or industrial operations are performed.*' The CBP captures the universe of known establishments in the U.S. as of March 12th of each year. The U.S. Census Bureau receives data from the Internal Revenue Service constructed from business tax returns to create the CBP.[30]

BLS injury data: We draw administrative data from the BLS on non-fatal and fatal injury rates at U.S. worksites. To measure non-fatal injuries, we use the Survey

[26] We use the following IPUMS variables or activity codes: working (variable: *BLSWORK*), rest (variable: *BLSLEIS*), purchasing self-healthcare (variable: *BLSPCAREHEALTH*), purchasing healthcare (variable: *BLSPURCHEALTH*), and apply for/using government benefits (activity code: 100102).

[27] The specific name of the BEA data series is the *SAINC35 Personal Current Transfer Receipts*.

[28] We use the series reported in University of Kentucky Center for Poverty Research (2023).

[29] Our assumption is that state PSL mandates do not systematically shift the use of WC from people paid through state funds to people paid through private funds. To the extent that such a shift does occur, the ASEC measure does not suffer from this same bias.

[30] For a full discussion of CBP quality, see Deza et al. (2022).

of Occupational Injuries and Illnesses (SOII) which collects workplace injury data from approximately 200,000 employers each year.[31] We study nonfatal injuries involving days away from work per 10,000 full-time equivalent workers. Injury data are missing for some states in some years, leaving us with 370 observations representing 45 states 2011-2019 (we exclude DC as this state adopts a PSL mandate prior to 2011). To measure fatal worksite injuries, we use the BLS Census of Fatal Occupation Injuries (CFOI) (Bureau of Labor Statistics, 2023) 2005-2019; there are no missing data in the CFOI. The outcome is fatal workplace injuries per 100,000 full-time equivalent workers. Studying these outcomes is useful for understanding mechanisms through which PSL mandates potentially impact WC benefit receipt. By promoting overall health, PSL mandates may prevent workers from experiencing injuries. If better overall health is an important pathway, then we would expect workplace injuries to decline post-PSL mandate. On the other hand, if substitution across programs (i.e., PSL for WC) is the dominant channel, then we may not expect to observe declines in these outcomes.

3.4 Summary statistics and trends

Figures 3 report trends in our WC income receipt variables from the ASEC. Both the probability that a respondent has any WC income in the past year and the amount of WC income are declining over our study period. Table 1 reports summary statistics in the ASEC, column (1) reports the full sample, and columns (2) and (3) report summary statistics for states that adopt a PSL mandate and states that do not. We use years prior to any state adopting a PSL mandate (2005-2007). Less than 1% of the overall sample reports WC income and the average amount of past-year WC income is $64.80.

States that adopt PSL mandates have higher rates of WC income receipt and levels of income from WC than states that do not adopt prior to any PSL adoption. In particular, the prevalence of any WC income (level of WC income) is 0.9% ($110.9) in states that later adopt a PSL while the rate (income level) is 0.7% ($67.6) in states that do not adopt a PSL mandate. Paid family and medical leave mandates are more common among states that will later adopt a PSL mandate. Most demographic variables are similar across the two groups, though states that later adopt a PSL mandate have higher shares of Hispanic residents and educational attainment than states that do not. We will control for these characteristics in our regressions.

[31] We do not use pre-2011 data as there are concerns about quality (Bureau of Labor Statistics, 2022).

3.5 Methods

We use difference-in-differences methods to study the impact of state PSL mandates on WC outcomes. Our primary regression specification takes the following form:

$$WC_{ist}(0) = \alpha_s + \gamma_t + \boldsymbol{X}'_{ist}\boldsymbol{\beta} + \epsilon_{ist}, \tag{1}$$

$$WC_{ist} - \widehat{WC}_{ist}(0) = \delta PSL_{s,t-1} + \mu_{ist}, \tag{2}$$

where $WC_{ist}(0)$ represents an untreated WC outcome (any income or the level of income) for respondent i in state s in year t. Equation (1) is estimated using only observations in which $PSL_{s,t-1} = 0$. $PSL_{s,t-1}$ is an indicator coded one if a state has a PSL mandate in place at any point in year $t-1$ and zero otherwise. We lag the mandate one year to avoid including partially treated years. In addition, lagging the PSL mandate in this manner allows workers time to learn about and accrue benefits.

Equation (1) includes state (α_s) and year (γ_t) fixed-effects to account for pre-existing state differences in the outcome and common secular shocks. We also control for time-varying state and individual characteristics ($\boldsymbol{X}_{ist}$). State characteristics include paid medical and family leave mandates (National Partnership for Women & Families, 2022), paid time off mandates (National Partnership of Women & Families, 2023), poverty rates (University of Kentucky Center for Poverty Research, 2023), and population size (University of Kentucky Center for Poverty Research, 2023). The individual characteristics include age (in years), sex (male and female, with male as the omitted category), race (White, Black, and other race, with White race as the omitted category), Hispanic ethnicity, and highest level of educational attainment (less than high school, high school, some college, and a college degree or higher, with less than high school as the omitted category). The predictions from equation (1) are used as counterfactual estimates ($\widehat{WC}_{ist}(0)$) for the treated observations (i.e., observations with a PSL mandate in place). δ is our parameter of interest, estimated in equation (2).

The PSL mandates are adopted at different times across states. Using two-way fixed-effects (TWFE) regressions in such a setting can lead to biased parameter estimates attributable to the interaction of two sources: i) dynamics in treatment effects can lead to 'forbidden comparisons' (Borusyak et al., 2024) in which early treated units are used as the comparison group for later treated units, and ii) heterogeneity in treatment effects (e.g., PSL mandates could have heterogeneous effects in different adopting states).

Given these concerns, we employ a two-step imputation procedure proposed by Gardner (2022) that is not vulnerable to these sources of bias ('2SDID'). Borusyak et al. (2024) introduces a similar approach. Both estimators will produce the same results in this context. In the first stage of this procedure, we estimate the relationships between the time-varying covariates and fixed-effects included in equation (1) using only the untreated observations (i.e., never-treated and not-yet-treated observations). The estimated parameters are then used to residualize the outcomes for treated and untreated observations. These parameter estimates are not subject to bias from the above-noted concerns with staggered treatment adoption as no treated observations are used in estimation. In the second stage of the 2SDID procedure – equation (2), the residualized outcomes are regressed on the treatment variable (using all observations).[32] 2SDID is developed within a generalized method of moments framework and the standard error estimates account for both the two-step estimation process (Hansen, 1982) and state-level clustering (Butts and Gardner, 2021). We show in sensitivity analyses that the TWFE results are not appreciably different from the 2SDID estimates.

We make some modifications to equations (1) and (2) when we use alternative data sources (see Section 3.3). For example, when the data are available at the state-year level, we estimate a state-level version of these specifications and use population weights.

4 Results

4.1 Do PSL mandates increase sick leave use?

Before testing the impact of state PSL mandates on WC outcomes, we examine the 'first-stage' effects of these policies. Using the NCS, we explore the impact of state PSL mandates on the probability that an establishment offers PSL, and PSL and USPL use by employees (Table 2). After the adoption of a mandate, PSL offers increase by 13.2 percentage points ('ppts') while annual hours of PSL and UPSL use increase by 1.57 and 0.25 per worker, respectively. Comparing these coefficient estimates to the mean in PSL-mandate adopting states pre-mandate (this is the comparison we use henceforth) implies 18.5%, 7.3%, and 51.7% increases in PSL access, PSL use, and UPSL use post-mandate, respectively.[33] Previous studies have also examined these relationships (Maclean et al., 2024, 2023), but we establish that they hold for our sample and specification.

A limitation of the NCS is that the dataset includes information on jobs, but not the people who hold the jobs. In our main analysis, we study adults ages 25-62 years, but we cannot study PSL outcomes for this specific age group in the NCS. As a complementary

[32]This approach also guards against bias due to treatment heterogeneity which is correlated with covariates (Caetano et al., 2022; Powell, 2021).

[33]Some of the mandates adopted by states require employers to offer UPSL to employees (National Partnership of Women & Families, 2023).

analysis, we use CPS data to study whether PSL mandates impact the probability that a respondent is away from work for a health-related reason with pay at any point in the week prior to the survey.[34] We present this result in column (4) of Table 2. We find that adopting a state PSL mandate leads to a 0.04 ppt (16.7%) increase in the probability of being absent from work with pay for a health-related reason.

This first-stage analysis suggests that the adoption of a state PSL mandate leads to statistically and economically significant increases in employee access to PSL and PSL and UPSL use, which is consistent with earlier studies. Next, we turn to our main analysis of the impact of PSL mandates on WC outcomes.

4.2 Workers' compensation cash benefits

4.2.1 Overall effects

Table 3 reports our estimates of the impact of state PSL mandates on WC cash benefits. Panel A presents results for any WC income and Panel B reports results for the amount of WC income. In columns (1) through (3), we 'build-up' the regression specification by sequentially adding control variables. This approach permits us to explore the sensitivity of our coefficient estimates to the inclusion of specific control variables. Column (1) reports results that include just state and year fixed-effects while columns (2) and (3) report results that add time-varying state-level and respondent-level characteristics, respectively. Column (3) is our preferred specification. In column (4), we use our preferred specification, but we include the calendar years 2020-2022 (i.e., the COVID-19 years and the time in which a federal PSL mandate was in place).

In the most parsimonious specification (column [1]) we observe that a state PSL mandate leads to a 0.09 ppt (12.2%) decline in the probability of any WC income and a $12.40 (14.6%) decline in income from WC. In our primary specification, reported in column (3), we see a 0.10 ppt (13.5%) decline in any WC income (statistically significant at the 10% level) and a $13.00 (15.3%) reduction in WC income post-PSL mandate (statistically significant at the 1% level). For WC income, we cannot reject a 6.6% reduction in any WC income at a 5% significance level.[35]

We see little evidence that these coefficient estimates are sensitive to the inclusion of respondent- or state-specific controls. Findings are similar, though smaller in magnitude when we include the pandemic period (column [4]). In particular, the estimate for any

[34] Since this measure refers to the previous week (and not the previous year like the income measures), we link PSL mandates based on the survey year.

[35] To calculate this number, we use the smallest value in the 95% confidence interval in column (3) of Table 3. The calculation is as follows: $= (100 \times [3.7847 \times 1.96 - 12.9993]/85.0297)\% = -6.6\%$.

WC income is not statistically distinguishable from zero when these years are included in the sample.[36] The attenuation in coefficient estimates could be due to the pandemic or (temporary) federal PSL mandate.[37]

Our analysis relies on a 'parallel trends assumption' – in the absence of a state PSL mandate, adopting and non-adopting states would have followed the same trends in WC outcomes in the post-period. This assumption is untestable as counterfactual outcomes are not observed for the adopting states post-policy. We estimate an event-study to provide suggestive evidence on the appropriateness of the parallel trends assumption in our data. We decompose the binary interaction – in equation (2) – between the post-period and treated states with a series of event-time indicators from four or more years pre-mandate to four or more years post-mandate.[38,39] The event-study results are shown in Figure 4. The coefficient estimates on the policy lead variables are generally small in magnitude and not statistically different from zero. This pattern of results offers suggestive evidence that the two groups of states followed similar trends in WC outcomes pre-mandate. Post-mandate, we observe declines in both the probability of any WC income and the level of WC income received. The declines begin to become observable roughly one year post-mandate – the modest time delay in observing effects is not surprising given that we code the first partial year the mandate is in place as the effective year and employees must accrue benefits prior to using them (National Partnership of Women & Families, 2023).

In Figure A1, we re-estimate the event-study using different specifications and samples. In particular, we i) remove time-varying covariates, ii) trim the end-points – we exclude observations that are more than four periods prior to or after the event for treated states (we include all years of data for comparison states), iii) code states that adopt a PSL mandate post-2019 as controls – i.e., we code these states as zero for all policy leads and lags), iv) drop states that adopt PSL mandates post-2019, and v) use two-way fixed-effects regression rather than the Gardner (2022) 2SDID method.[40] Results are not appreciably different across the alternative specifications and samples.

[36] Results are similar to the main estimates if we simply exclude 2020, the year that was arguably most impacted by the COVID-19 pandemic and the federal PSL mandate.

[37] FFCRA was mandatory in 2020 but became voluntary in 2021.

[38] There is no omitted period in the Gardner (2022) event-study, thus we report a coefficient estimate for all time-to-event periods. The method implicitly normalizes the average of the pre-period to zero.

[39] We code states that adopt a PSL mandate after 2019 in their pre-treatment period (e.g., New York adopts a PSL mandate in 2021, thus we code that state as -2 in 2019).

[40] In TWFE, we normalize the coefficient estimates to zero in the period prior to mandate adoption.

4.2.2 Heterogeneous responses

Different groups of workers may respond heterogeneously to PSL mandates. We estimate the impact of state PSL mandates on WC outcomes in different samples based on respondent characteristics (Table A2) and job characteristics (Table A3). In particular, we estimate separate regressions by age (25-39 and 40-62 years), sex (male and female), race (White and non-White), ethnicity (Hispanic and non-Hispanic), physicality of the job (physically demanding and non-physically demanding), and firm size (less than 100 workers and 100 or more workers).[41] Overall, while the results are noisier when we stratify the sample, the coefficient estimates suggest that PSL mandate adoption reduces WC income receipt among all groups. However, men and Hispanic individuals appear to experience particularly large reductions in WC income post-mandate (see Table A2).

We also estimate larger WC income reductions among those employed in physically demanding jobs relative to those employed in non-physically demanding jobs, and in larger versus smaller firms (see Table A3). The finding that effects are larger for workers in physically demanding jobs hints that PSL is used by injured workers to recover from worksite injuries or illnesses (potentially in substitution for claiming WC). The more substantial estimated magnitudes for large firms are consistent with workers in smaller firms not necessarily being eligible for PSL benefits according to the adopted mandates in some states (National Partnership of Women & Families, 2023).

4.3 Discussion of magnitudes

In this sub-section, we consider the implications of the estimated magnitudes. We first compare the estimated effects for both paid absences and WC benefits. For these metrics, comparing levels is challenging since PSL use is measured in hours per worker-year or paid absences within a week, and our WC outcomes are measured in terms of any annual use and total annual income. We estimate that PSL mandates increase paid absences for health-related reasons within the past week by 16.7%. In response, annual rates of WC benefit receipt decrease by 13.5%. Since the rate of <u>annual</u> WC benefit receipt is higher than the <u>weekly</u> paid absence rate, the WC level effects are larger, but the implied percentage changes are similar.

[41] We follow Ghimire and Maclean (2020) in our definition of job physicality. The firm size information is based on self-reported information by CPS respondents. We use the IPUMS variable *FIRMSIZE*. The question wording is as follows: '*Counting all locations where (this employer/(name/you)) (operates/operate), what is the total number of persons who work for ((name's/your) employer)/name/you))?*' We only include employed respondents in the analysis in which we stratify by firm size as non-employed respondents are not asked this question.

To generate a cost-elasticity, we consider the change in WC costs as a function of PSL costs, which we assume are proportional to time away from work with pay. This calculation implicitly assumes that the rise in UPSL does not generate additional costs for the employer. Under these assumptions, our estimates imply an elasticity of -0.92.

There is limited empirical evidence on how WC claiming behavior responds to non-WC policy changes. We consider the existing evidence and a related literature studying health and labor supply interactions. Ghimire and Maclean (2020) find that state laws legalizing medical use of cannabis, by providing an additional source of pain treatment, decrease the probability of WC income receipt by 13% among adults. Abouk et al. (2023) show that, following the adoption of a state law that legalizes recreational cannabis for adult use, WC income receipt and the amount of WC income received decline by 21.1% and 18.8% among adults ages 40 to 62 years of age. These studies suggest that WC claims are highly responsive to spillovers from other policies, though these policies are different in nature than the PSL mandates we examine.

A handful of studies examine the extent to which labor supply and absences respond to changes in access to pain management prescription medication and to the availability of medical care. To the extent that PSL is used to treat personal health issues, these studies may also provide some relevant information about labor and social insurance responsiveness. Garthwaite (2012) exploits the 2004 removal of Vioxx from the U.S. market after the drug was associated with adverse health events including heart attacks. Following Vioxx removal, Garthwaite (2012) demonstrates a 10% decline in labor supply (i.e., any work at all) among adults 55-75 years. Using administrative data from Norway, Bütikofer and Skira (2018) estimate that the 2004 removal of Vioxx increased absences from work for sickness by 7-12% among adults 40-60 years of age, while the earlier introduction of Vioxx to the Norwegian market decreased these outcomes by 12-16%. Powell and Seabury (2018) estimate that a decline in WC medical care generosity led to an 8% decline in post-injury earnings.

There is little evidence in the literature about how the availability of PSL affects WC claims so benchmarking the estimates of this paper to related work is difficult. However, the literature has found that policies not specifically targeting WC programs and shocks to pain management access or medical care can have comparable effects.

We can also compare our findings with other studies that investigate spillovers across social insurance programs. The comparisons are not straightforward as the 'dose' of the policy shocks differ – studies often exploit changes in the generosity of government-run programs while we investigate the introduction of a new social program that mandates employers to offer a benefit to employees. With that caveat in mind, U.S. studiesexploiting changes in the generosity of WC or expansions on eligibility for Medicaid find limited evidence of spillovers to disability benefit receipt (see Section 1). There is more evidence of substitution in other countries. Using Canadian data, Fortin et al. (1999) document that a reduction in unemployment insurance benefit generosity increases WC claims, estimating an elasticity of 0.5 to 0.7. Staubli (2011) finds that reductions in disability insurance receipt increase use of sickness insurance. Using results from Tables 1 and 4 of the paper,[42] the estimates imply a 0.47 elasticity. Borghans et al. (2014) demonstrate that workers offset 30% of lost DI benefits with other social insurance programs. The implied elasticity of substitution is over 3.2.[43] In general, we find that our elasticity estimate of 0.92 is comparable to those found in the literature on substitution across programs, which includes a wide range of substitution responses.

4.4 Time use, broader labor measures, and health metrics

4.4.1 Time use

We report findings from our analysis of the time use data in Table A4. We find that post-mandate, time spent working declines by 9.5 minutes (per weekday), equivalent to a 2.6% reduction. This finding confirms results reported earlier in the manuscript that PSL mandates lead to more time away from work (see Table 2, column 4). Below, we find little evidence of changes in labor supply when studying metrics such as employment propensities and usual hours worked, consistent with the reductions in minutes spent on work per weekday reflecting paid time off and not broader changes in employment or labor demand. These findings imply that, post-mandate, workers take time away from work to recover from an injury or illness.

Next, we use the ATUS to see how people spend financially-protected time away from work. We use data on time spent in leisure activities (which we use as proxies for resting, potentially to recover from a work-related injury or illness), in self-healthcare, and in purchasing healthcare. We find that our proxy for time spent resting increases by 8.3 minutes per day (a 3.6% increase) which is nearly identical in magnitude (and opposite in sign) to our findings for time spent working. We observe no statistically significant change in time spent on self-healthcare or purchasing healthcare post-mandates; the coefficient estimates are negative but imprecise. Finally, we observe no statistically significant effects of state PSL mandates on time spent applying for or using government benefits generally (including WC), the coefficient estimate in the benefit application time

[42] We use the 'Before' information from the last columns of Table 1 to establish baselines and the last column of Table 4 for the estimated changes.

[43] Authors' calculation.

use regressions carries a negative sign. This result suggests that PSL mandates and WC benefits are not complements by providing time for injured workers to apply for WC benefits. Overall, our findings based on time use data are consistent with workers using PSL to take time to rest and recover from work-related injuries or illnesses.

4.4.2 Broader labor measures

We next examine the impact of PSL mandate adoption on other labor metrics. These measures offer complementary evidence about the relationship between PSL and work-related health while also providing insights into the underlying mechanisms for any observed relationship between PSL mandates and WC income. Studying complementary metrics can alleviate concerns about the role of measurement errors in self-reported WC benefit receipt specifically.

We consider several measures of labor supply and productivity available in the ASEC (Table 4): employed (zero/one), weeks worked in the past year, usual hours worked per week in the past year (zero for those who do not work), full-time employment (zero/one) in the past year,[44] and hourly wage.[45] Examining the hourly wage variable allows us to partially explore whether state PSL mandates lead employers to reduce other benefits as labor costs rise or whether workers gain productivity post-mandate.

We observe that post-PSL mandate adoption, there is no statistically significant change in the probability of any employment and usual hours worked. However, the number of weeks worked increases by 0.17 (0.4%) and full-time work probability increases 0.005 (0.6%). Finally, we find that employers do not reduce wages post-PSL mandates; instead, we observe that wages may rise, similar to earlier studies (Pichler and Ziebarth, 2020a; Maclean et al., 2023, 2024; Slopen, 2024).

As a complementary metric to consider the possible role of confounding labor demand shocks, we turn to the CBP data and explore the effect of PSL mandates on the number of establishments per state and the number of employees working in these establishments (Table A5). We convert these variables to rates per 100,000 state residents. Coefficient estimates imply a 3.0% increase in the number of establishments and a 3.4% increase in the number of employees, though the employee finding is not statistically different from zero at conventional levels. Overall, our analysis of labor market outcomes suggests that the decline in WC income receipt is not driven by a reduction in workplace

[44] We define full-time employment as usually working 30 hours or more per week.

[45] Hourly wage data is derived from the out-going rotation group sample. We use the IPUMS variable *HOURWAGE* to construct this variable. Only wage/salary workers and those who are paid hourly, and are in outgoing rotation groups, are provided this information.

injuries due to a concurrent labor demand contraction, though we study injury rates at U.S. worksites more directly below.

4.4.3 Alternative measures of health and injuries

The ASEC includes self-reported measures of health and work-limiting disability. We examine these outcomes in Table A6 as complementary work capacity metrics. Post-mandate, there is no observable change in the probability of reporting that one's health is fair or poor, or excellent or very good. However, the likelihood of reporting a work-limiting disability declines by 0.004, equivalent to a 5.3% reduction. Earlier work suggests that the CPS work-limiting disability measure can be used to track disability in the population (Burkhauser et al., 2002). However, the prospect of PSL mandates – which allow the worker financially protected time to recover from an injury or illness related to a work-limiting disability – may lead a worker with a disability to perceive that disability no longer limits their ability to work. PSL may also allow workers to recover from overexertions, or invest in health, which prevents future work-limiting disabilities.

Next, we use data from the BLS SOII and CFOI to test whether state PSL mandates impact worksite injuries, both non-fatal and fatal (Table 5). We estimate small and statistically insignificant positive effects. Thus, we do not observe evidence that PSL mandates reduce the rate of workplace injuries that result in time off from work.[46] This (null) finding would be consistent with PSL mandates affecting WC claiming behavior, suggesting that people substitute PSL benefits for WC benefits, despite no change in injury rates. While we observe some evidence of general health improvements above, these changes are not impacting time away from work due to injuries/illnesses. This change would be necessary to reduce the propensity of WC cash benefit receipt. Instead, the evidence suggests that claiming behavior has changed, and PSL benefits are being used instead of WC benefits.

4.4.4 Other employer-based benefits

Finally, we test whether employers reduce insurance offerings to offset the cost of newly mandated PSL and time use. Lower insurance rates could also influence WC claiming behavior (Bronchetti and McInerney, 2021). To this end, we use data available in the ASEC on the probability that a respondent has employer-sponsored health insur-

[46]The WC analysis above studied costs per person while this analysis studies injuries per full-time equivalent worker. However, given the Table 4 results, we do not believe that changes to the denominator are driving the results.

ance (ESI) and the level of health insurance premiums.[47] Employers could reduce the offering of ESI or increase premiums to employees. Results, reported in Table A7, reveal no evidence that employers respond in this manner. Post-PSL mandate, the probability that a respondent has ESI increases by 0.4% and there is no statistically significant change in health insurance premiums. This pattern is in line with evidence provided by Maclean et al. (2024) suggesting that, post-PSL policy, employers increase the generosity of non-mandated benefits, potentially to compete for employees.

4.4.5 Summary of findings

Our analysis of the impact of state PSL mandates on WC income and related metrics suggests that these policies improve workers' ability to take time off from work to rest following an injury/illness incurred while working, and PSL may have some downstream benefits in terms of improved health. However, we do not observe changes in workplace injury rates that require time off from work, a necessity for receiving WC indemnity benefits. These results suggest that injured or ill workers may see the available benefits as substitutes.

4.5 Additional sensitivity analyses

In this section, we assess the extent to which the main findings for WC income receipt in the ASEC are potentially attributable to our specific sample and specification. One concern with our analysis is the reliance on self-reported WC information. To partially address this issue, we examine the impact of state PSL mandates on state-level WC expenditures. We convert expenditures to a rate per 1,000. We report 2SDID results in Table A8. Post-PSL mandate, as shown in Panel A, state-level expenditures decrease by 14.1% (comparing the coefficient estimate, -\$1.31, with the baseline mean in PSL adopting states, \$9.21). Scaling costs by employment suggests similar effect sizes: a 16.0% decline. These relative effect sizes are very similar to our main findings for WC income in the ASEC (Table 3). Overall, our BEA results are largely consistent with the ASEC findings. While both data sets have their limitations, the similarity of the results across data sources suggests that the limitations do not lead to bias.

We further consider the sensitivity of the ASEC estimates in Figure A2. We report findings from our main specification and sample followed by results from a series of sensitivity analyses. First, we include additional covariates: we control for region-by-year fixed-effects and, separately, occupation and industry fixed-effects. The results are

[47] The latter outcome is only available from 2011 onward, leading to a reduced sample size.

generally robust to these controls. While state PSL mandates have not been adopted uniformly across the U.S., our results do not appear to be driven by region-, occupation-, or industry-specific trends in WC outcomes.

Second, we use alternative measures to code PSL and other related mandates. We incorporate sub-state mandates into our PSL coding by constructing a measure that is coded as one if the respondent resides in either a state with a state-level mandate or in a state with a sub-state-level mandate for a locality with 500,000 or more residents (National Partnership of Women & Families, 2023).[48] As an alternative, we provide results that code the mandate variable as one for everyone in a locality who has a sub-state-level mandate, and zero for everyone in the rest of the state.[49,50] The results are generally similar across these tests.

We also consider sensitivity to including an indicator variable that takes on a value of one if the state borders another state with a PSL mandate.[51] In addition, we condition a variable that is coded as one (zero otherwise) if the state has a PSL or PTO mandate. The coefficient estimates are generally stable as we make these changes.

Third, we aggregate the analysis data to the state-year level. Fourth, we drop sets of states in which there is a large number of workers who live in one state and work in another (specifically, the DC, MD, and VA area, and the NY, NJ, and CT area), and states that do not mandate employers to provide WC (TX, SD, and WY). Fifth, we include only private workers as we might expect such workers to be the most directly impacted by state PSL mandates (Maclean et al., 2024)[52] and those respondents with

[48]Specifically, we 'turn-on' the treatment variable for the following states (years): New York (2015-2019) to account for the New York City mandate, Illinois (2018-2019) to account for the Cook County mandate, Pennsylvania (2016-2019) to account for the city of Philadelphia mandate, Maryland (2017-2019) to account for the Montgomery County mandate, California (2007-2019) to account for the San Francisco mandate, Washington (2013-2019) to account for the Seattle mandate, Oregon (2015-2019) to account for the Portland mandate, and Minnesota (2018-2019) to account for the Minneapolis and Saint Paul mandates. We note that some states will have multiple large cities adopting a PSL mandate, here we only focus on the first such city's adoption.

[49]To this end, we use information on metro area, MSA, and county to link the sub-state mandates to the CPS. There is measurement error in the matching as the public-use CPS data suppress geographic information. We use the following IPUMS-created variables: *METFIPS*, *METAREA*, and *COUNTY*. We code two sub-state mandates that are noted in Maclean et al. (2024) – Jersey City, NJ and Portland, OR – but not in National Partnership of Women & Families (2023). details on our matching procedure are available on request. See Van Riper et al. (2021) for more details on geographic information available in the public use CPS.

[50]In unreported results available on request, we also exclude localities with a sub-state mandate (e.g., San Francisco, California).

[51]Here we report results using current border state PSL mandate status to match other time-varying covariates which are matched to the data on the current year and state. In unreported analyses available on request, we use the lagged border variable and the results are not different.

[52]In unreported analyses, we test whether the passage of a state PSL mandate impacts the probability of working in a private job. The coefficient estimate on the PSL mandate in this regression is small in

ESI as they may be likely to work for employers impacted by PSL mandates. We also exclude observations with imputed WC income information (n=1,805). As expected, the magnitudes increase. Sixth, we use two-way fixed-effects regression and estimate similar effects as with 2SDID.

Figure A2 reports 95% confidence intervals. Not all coefficient estimates in the sensitivity analyses are statistically different from zero at that level of confidence. However, the point estimates are generally insensitive to these changes.[53]

Seventh, we conduct a 'leave-one-out' analysis in which we sequentially exclude each state that adopts or announces a PSL mandate by October 2023, as shown in Figure A3. We see some evidence of sensitivity for the 'any WC' outcome but much less sensitivity when studying WC income levels.

Next, we test for balance across treated and untreated states. We aggregate the data to the state-year level and sequentially regress each of the time-varying control variables on the lagged state PSL, state fixed-effects, and year fixed-effects. Figure A4 provides evidence that state PSL mandates do not predict our time-varying covariates.

We also explore the extent to which workers migrate to states with PSL mandates in place. We use data in the ASEC on past-year residence to construct the share of the population that moved across state lines in the past year. As reported in Table A9, we do not observe any evidence of such behaviors.

5 Discussion and conclusion

This study explores the spillover effects of state-mandated paid sick leave (PSL) policies on workers' compensation (WC) income receipt in the United States. Unlike much of the literature on program substitution, we study two mandated benefits and we exploit the adoption of a policy instead of changes in program generosity. Employing a difference-in-differences approach, our analysis provides evidence that the introduction of state PSL mandates is associated with a significant reduction in the receipt of WC cash benefits. Our findings indicate that the likelihood of receiving WC income decreases by 13.5%, and the amount of WC cash benefits declines by 15.3% following the implementation of PSL mandates. These reductions in WC claims become evident approximately one year after the mandates take effect. Notably, the proportional declines in WC bene-

magnitude and not statistically distinguishable from zero. Results are available on request.

[53] All but two coefficient estimates are statistically different from zero at the 10% level. For any WC income, the coefficient estimate in the specification in which we control for border states is not precise at the 10% level. For the level of WC income, the coefficient estimate generated in the sample of only private workers is not statistically different from zero at the 10% level.

fits correspond closely with the estimated increases in the usage of PSL resulting from the adoption of these mandates. We observe equivalent declines when studying self-reported or administrative WC measures. The results suggest economically meaningful spillovers between the two mandated benefits and imply a cost elasticity of -0.92.

Studying time use data, we observe statistically significant increases in time spent resting, consistent with PSL providing opportunities to address health issues. We do not find any evidence that PSL provides time to apply for WC benefits. By investigating a broader set of outcomes, we observe some complementary evidence of increases in labor supply. We also find evidence of reductions in the probability of reporting a work-limiting disability, though the availability of PSL may affect self-assessments of work-limiting disabilities if this benefit impacts workers' perceptions of their ability to manage their disabilities in the future.

However, we do not observe evidence of any decline in workplace injuries resulting in time off from work. This finding casts doubts on the role of work capacity improvements – due to health investments and resting in response to injuries – in driving our main results. While we observe suggestive evidence that PSL mandates improve health, these improvements do not themselves appear to influence time away from work due to injuries and, thus, outcomes related to receipt of WC indemnity benefits. Instead, the results generally suggest a primary role for the use of PSL benefits as substitutes for WC claims.

We use our coefficient estimates to assess what impact we might expect PSL mandates to have on WC costs in the U.S. In 2020, the total costs of WC cash benefits in the U.S. were $31.2 billion (Murphy and Wolf, 2022). Using our estimates, a federal PSL mandate would reduce WC costs by $3.5 billion above and beyond the existing reductions induced by existing state mandates.[54] There may be reductions in other (non-cash) WC costs as well, suggesting that total impacts on WC could be even larger.

Maclean et al. (2024) estimate that PSL mandates increase firm sick leave costs by 5.8 cents per hour worked.[55] Using CPS ASEC data on hours worked for our time period, we estimate that the per-person[56] (for ages 25-61) cost of a PSL mandate is $91.95. Thus, our estimates imply that the reduction in WC costs alone defrays 14% of total PSL costs.[57] This is a meaningful offset and reflects that both programs can be used to

[54] We assume that WC benefits are proportional to population size. In 2020, 74% of the population resided in states without a state PSL mandate in place so we assume that an equivalent share of WC 2020 income costs ($23.1 billion) occurred in states that would gain coverage due to the hypothetical federal mandate. This implies that costs would be reduced by $3.5 billion (= $23.1 billion ∗ -15.3%).

[55] See Table 2, Panel B of Maclean et al. (2024). This finding is specific to private sector workers but, for the purposes of this calculation, we assume that it applies to all workers.

[56] This estimate includes non-workers to remain consistent with the WC results.

[57] We use the Table 3, Column 3 estimate for this calculation.

address worker health issues in related ways. The programs are also independent in many ways. PSL mandates may also induce more care-taking of family members (Arora and Wolf, 2024; Guo and Peng, 2024; Maclean and Pabilonia, 2024) and incentivize workers to take time off who would not consider filing a WC claim (e.g., a worker who has the flu may use PSL benefits, when available, but would not likely claim WC as a result of the illness). Such responses to PSL mandates would not impact WC costs, reducing the scope for offsets. Only a subset of PSL use is related to workplace injuries and illnesses; the estimated extent of the substitution between the two programs suggests demand for alternatives to WC due to differences in generosity or administrative burdens.

Our paper has limitations. First, we rely mainly on survey data to measure WC income receipt, though we confirm our findings using administrative state-level data on WC expenditures from the Bureau of Economic Analysis. Second, state PSL mandates are relatively new policies, and thus our findings reflect the experiences of early adopting states and arguably capture short- to medium-run effects. Third, due to the infancy of PSL mandates in the U.S., we lack sufficient variation in state PSL policy provisions to study what components of these mandates have the most impact on WC.

PSL offers immediate and short-term time off for recovery from a workplace injury. We observe some evidence of longer-term health improvements which may potentially drive changes in WC costs. However, there is more evidence that workers treat PSL and WC benefits as substitutes given the partial overlap between the purposes of the programs. We find that the reduction in WC costs alone provides a meaningful partial offset to the costs associated with PSL mandates. The positive spillovers of PSL mandates are important factors when considering their effectiveness and optimal policy.

References

Abouk, R., K. M. Ghimire, J. C. Maclean, and D. Powell (2023). Pain management and work capacity: Evidence from Workers' Compensation and marijuana legalization. *Journal of Policy Analysis and Management*.

Ahn, T. and A. Yelowitz (2016). Paid sick leave and absenteeism: The first evidence from the US. *Available at SSRN 2740366*.

Andersen, M., J. C. Maclean, M. F. Pesko, and K. Simon (2023). Does paid sick leave encourage staying at home? Evidence from the United States during a pandemic. *Health Economics 32*(6), 1256–1283.

Arora, K. and D. A. Wolf (2024). Paid leave mandates and care for older parents. *The Milbank Quarterly*.

Baidwan, N. K., N. W. Carroll, B. Ozaydin, and N. Puro (2020). Analyzing Workers' Compensation claims and payments made using data from a large insurance provider. *International Journal of Environmental Research and Public Health 17*(19), 7157.

Baily, M. N. (1978). Some aspects of optimal unemployment insurance. *Journal of Public Economics 10*(3), 379–402.

Bernie Sanders and Rosa DeLauro (2023). Healthy Families Act of 2023.

Biddle, J. and K. Roberts (2003). Claiming behavior in workers' compensation. *Journal of Risk and Insurance 70*(4), 759–780.

Borghans, L., A. C. Gielen, and E. F. Luttmer (2014). Social support substitution and the earnings rebound: Evidence from a regression discontinuity in disability insurance reform. *American Economic Journal: Economic Policy 6*(4), 34–70.

Borusyak, K., X. Jaravel, and J. Spiess (2024). Revisiting event study designs: Robust and efficient estimation. *Review of Economic Studies*, rdae007.

Bronchetti, E. T. and M. McInerney (2012). Revisiting incentive effects in workers' compensation: Do higher benefits really induce more claims? *ILR Review 65*(2), 286–315.

Bronchetti, E. T. and M. P. McInerney (2021). Health care reform and workers' compensation: Evidence from Massachusetts. *ILR Review 74*(2), 388–418.

Bureau of Economic Analysis (2024). State personal income and employment: Concepts and method. Technical report, Bureau of Economic Analysis.

Bureau of Labor Statistics (2022). Survey of Occupational Injuries and Illnesses (SOII) [dataset]. Data retrieved from https://www.bls.gov/respondents/iif/.

Bureau of Labor Statistics (2023). Census of Fatal Occupational Injuries (CFOI) [dataset]. Data retrieved from https://www.bls.gov/iif/fatal-injuries-tables.htm.

Burkhauser, R. V., M. C. Daly, A. J. Houtenville, and N. Nargis (2002). Self-reported work-limitation data: What they can and cannot tell us. *Demography 39*(3), 541–555.

Bütikofer, A. and M. M. Skira (2018). Missing work is a pain: The effect of Cox-2 inhibitors on sickness absence and disability pension receipt. *Journal of Human Resources 53*(1), 71–122.

Butts, K. and J. Gardner (2021). {did2s}: Two-stage difference-in-differences. *arXiv preprint arXiv:2109.05913*.

Cabral, M., C. Cui, and M. Dworsky (2022). The demand for insurance and rationale for a mandate: Evidence from workers' compensation insurance. *American Economic Review 112*(5), 1621–1668.

Cabral, M. and M. Dillender (2024). The impact of benefit generosity on workers' compensation claims: Evidence and implications. *American Economic Journal: Applied Economics 16*(3), 436–481.

Caetano, C., B. Callaway, S. Payne, and H. S. Rodrigues (2022). Difference in differences with time-varying covariates. *arXiv preprint arXiv:2202.02903*.

Callison, K. and M. F. Pesko (2022). The effect of paid sick leave mandates on coverage, work absences, and presenteeism. *Journal of Human Resources 57*(4), 1178–1208.

Callison, K., M. F. Pesko, S. Phillips, and J. A. Sosa (2023). Cancer screening after the adoption of paid-sick-leave mandates. *New England Journal of Medicine 388*(9), 824–832.

Chetty, R. (2006). A general formula for the optimal level of social insurance. *Journal of Public Economics 90*(10-11), 1879–1901.

Chetty, R. and A. Finkelstein (2013). Social insurance: Connecting theory to data. In *Handbook of Public Economics*, Volume 5, pp. 111–193. Elsevier.

Chunyu, L., P. F. Volpin, and X. Zhu (2022). Do paid sick leave mandates increase productivity? *Available at SSRN 4096707*.

Colla, C. H., W. H. Dow, A. Dube, and V. Lovell (2014). Early effects of the San Francisco paid sick leave policy. *American Journal of Public Health 104*(12), 2453–2460.

Congressional Research Service (2024, April). Supplemental Security Income (SSI). In Focus.

Deza, M., J. C. Maclean, and K. Solomon (2022). Local access to mental healthcare and crime. *Journal of Urban Economics 129*, 103410.

Dong, X. (2022). Sick leave mandates reduces workers' compensation claims in California: Data from 2001 to 2012. Technical report, 11th Annual Conference of the American Society of Health Economics.

Duggan, M., P. Singleton, and J. Song (2007). Aching to retire? the rise in the full retirement age and its impact on the Social Security Disability rolls. *Journal of Public Economics 91*(7-8), 1327–1350.

Fan, Z. J., D. K. Bonauto, M. P. Foley, and B. A. Silverstein (2006). Underreporting of work-related injury or illness to workers' compensation: Individual and industry factors. *Journal of Occupational and Environmental Medicine*, 914–922.

Finkelstein, A. and N. Hendren (2020). Welfare analysis meets causal inference. *Journal of Economic Perspectives 34*(4), 146–167.

Flood, S., L. Sayer, D. Backman, and A. Chen (2023). American Time Use Survey Data Extract Builder: Version 3.2 [dataset]. College Park, MD: University of Maryland and Minneapolis, MN: IPUMS.

Fortin, B. and P. Lanoie (1992). Substitution between unemployment insurance and workers' compensation: An analysis applied to the risk of workplace accidents. *Journal of Public Economics 49*(3), 287–312.

Fortin, B., P. Lanoie, and C. Laporte (1999). Is workers' compensation a substitute for unemployment insurance? *Journal of Risk and Uncertainty 18*, 165–188.

Gardner, J. (2022). Two-stage differences in differences. *arXiv preprint arXiv:2207.05943*.

Garrett, B. and S. Glied (2000). Does state AFDC generosity affect child SSI participation? *Journal of Policy Analysis and Management 19*(2), 275–295.

Garthwaite, C. L. (2012). The economic benefits of pharmaceutical innovations: The case of cox-2 inhibitors. *American Economic Journal: Applied Economics 4*(3), 116–137.

Ghimire, K. M. and J. C. Maclean (2020). Medical marijuana and Workers' Compensation claiming. *Health Economics 29*(4), 419–434.

Gittleman, M. B., M. K. Lettau, and G. Phipps (2023). Removing workers' compensation costs from the National Compensation Survey. *Monthly Labor Review*.

Global Strategy Group (2021). New survey shows voters in senate battleground states want paid leave urgently, as part of infrastructure package action. Paid Leave For All.

Green, D. R., S. G. Gerberich, H. Kim, A. D. Ryan, P. M. McGovern, T. R. Church, A. Schwartz, and R. F. Arauz (2019). Knowledge of work-related injury reporting and perceived barriers among janitors. *Journal of Safety Research 69*, 1–10.

Gruber, J. and A. B. Krueger (1991). The incidence of mandated employer-provided insurance: Lessons from workers' compensation insurance. *Tax Policy and the Economy 5*, 111–143.

Guo, X. R. and L. Peng (2024). Paid-sick-leave mandates: Care provision and health behavior effects. *Available at SSRN 4815899*.

Hansen, L. P. (1982). Large sample properties of generalized method of moments estimators. *Econometrica: Journal of the Econometric Society*, 1029–1054.

Hendren, N. (2016). The policy elasticity. *Tax Policy and the Economy 30*(1), 51–89.

Heymann, J., A. Sprague, A. Earle, M. McCormack, W. Waisath, and A. Raub (2021). US sick leave in global context: US eligibility rules widen inequalities despite readily available solutions. *Health Affairs 40*(9), 1501–1509.

Hirsch, B. T., D. A. Macpherson, and J. M. DuMond (1997). Workers compensation recipiency in union and nonunion workplaces. *ILR Review 50*(2), 213–236.

Inderbitzin, L., S. Staubli, and J. Zweimüller (2016). Extended unemployment benefits and early retirement: Program complementarity and program substitution. *American Economic Journal: Economic Policy 8*(1), 253–288.

IRS (2023, 3). Statistics for tax returns with the Earned Income Tax Credit (EITC).

Jinks, L., T. J. Kniesner, J. Leeth, and A. T. Lo Sasso (2020). Opting out of workers' compensation: Non-subscription in Texas and its effects. *Journal of Risk and Uncertainty 60*, 53–76.

Karlström, A., M. Palme, and I. Svensson (2008). The employment effect of stricter rules for eligibility for DI: Evidence from a natural experiment in Sweden. *Journal of Public Economics 92*(10-11), 2071–2082.

Kramer, N. and A. Ziberman (2008). New definitions of employee access to paid sick leave and retirement benefitsin the National Compensation Survey. Technical report, Bureau of Labor Statistics.

Krueger, A. B. (1990a). Incentive effects of workers' compensation insurance. *Journal of Public Economics 41*(1), 73–99.

Krueger, A. B. (1990b). Workers' compensation insurance and the duration of workplace injuries.

Kyung, M., S.-J. Lee, C. Dancu, and O. Hong (2023). Underreporting of workers' injuries or illnesses and contributing factors: A systematic review. *BMC Public Health 23*(1), 558.

Lakdawalla, D. N., R. T. Reville, and S. A. Seabury (2007). How does health insurance affect Worker's Compensation filing? *Economic Inquiry 45*(2), 286–303.

Lawson, N. (2017). Fiscal externalities and optimal unemployment insurance. *American Economic Journal: Economic Policy 9*(4), 281–312.

Lipscomb, H. J., J. Nolan, D. Patterson, V. Sticca, and D. J. Myers (2013). Safety, incentives, and the reporting of work-related injuries among union carpenters: "You're pretty much screwed if you get hurt at work". *American Journal of Industrial Medicine 56*(4), 389–399.

Lipscomb, H. J., A. L. Schoenfisch, and W. Cameron (2015). Non-reporting of work injuries and aspects of jobsite safety climate and behavioral-based safety elements among carpenters in Washington state. *American Journal of Industrial Medicine 58*(4), 411–421.

Ma, Y., K. J. Johnston, H. Yu, J. F. Wharam, and H. Wen (2022). State mandatory paid sick leave associated with a decline in emergency department use in the US, 2011–19. *Health Affairs 41*(8), 1169–1175.

Maclean, J. C., E. Golberstein, and B. Stein (2024). State paid sick leave mandates associated with increased mental health disorder prescriptions among Medicaid enrollees. *Health Affairs Scholar*, qxae045.

Maclean, J. C. and S. W. Pabilonia (2024). Paid sick leave and childcare. Technical report, National Bureau of Economic Research.

Maclean, J. C., S. Pichler, and N. R. Ziebarth (2024). Mandated sick pay: Coverage, utilization, and welfare effects. Technical report, National Bureau of Economic Research.

Maclean, J. C., I. Popovici, and C. J. Ruhm (2023). Does paid sick leave facilitate reproductive choice? Technical report, National Bureau of Economic Research.

Maclean, J. C., S. Tello-Trillo, and D. Webber (2023). Losing insurance and psychiatric hospitalizations. *Journal of Economic Behavior & Organization 205*, 508–527.

McInerney, M. and K. Simon (2012). The effect of state workers' compensation program changes on the use of federal Social Security Disability Insurance. *Industrial Relations: A Journal of Economy and Society 51*(1), 57–88.

Meyer, B. D., W. K. Mok, and J. X. Sullivan (2015). Household surveys in crisis. *Journal of Economic Perspectives 29*(4), 199–226.

Meyer, B. D., W. K. Viscusi, and D. L. Durbin (1995). Workers' compensation and injury duration: Evidence from a natural experiment. *The American Economic Review 85*(3), 322.

Miller, M. M. (2022). The impact of paid sick leave laws on consumer and business bankruptcies. *Journal of Empirical Legal Studies 19*(4), 844–896.

Moffitt, R. (1992). Incentive effects of the US welfare system: A review. *Journal of Economic Literature 30*(1), 1–61.

Murphy, G. T. and J. Wolf (2022). Workers' compensation: Benefits, coverage, and costs, 2022. *National Academy of Social Insurance*.

National Partnership for Women & Families (2022, 10). State paid family & medical leave insurance laws.

National Partnership of Women & Families (2023, October). Paid sick days statutes.

Neuhauser, F. and S. Raphael (2004). The effect of an increase in worker's compensation benefits on the duration and frequency of benefit receipt. *Review of Economics and Statistics 86*(1), 288–302.

Petrongolo, B. (2009). The long-term effects of job search requirements: Evidence from the UK JSA reform. *Journal of Public Economics 93*(11-12), 1234–1253.

Pichler, S., K. Wen, and N. R. Ziebarth (2021). Positive health externalities of mandating paid sick leave. *Journal of Policy Analysis and Management 40*(3), 715–743.

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

Pichler, S. and N. R. Ziebarth (2020a). Labor market effects of US sick pay mandates. *Journal of Human Resources 55*(2), 611–659.

Pichler, S. and N. R. Ziebarth (2020b). Sick leave and medical leave in the United States: A categorization and recent trends. *Paid Leave for Illness, Medical leave and Disabilities, AEI-Brookings Paid Leave Project*, 31–59.

Powell, D. (2021). *The labor supply consequences of the opioid crisis*. RAND.

Powell, D. and S. Seabury (2018). Medical care spending and labor market outcomes: Evidence from workers' compensation reforms. *American Economic Review 108*(10), 2995–3027.

Rosa, R. R. and A. Asfaw (2023). Percentage of currently employed adults aged 18 years who have paid sick leave, by education level–National Health Interview Survey, 2021. *Morbidity & Mortality Weekly Report 72*(17).

Ruggles, S., S. Flood, M. Sobek, D. Brockman, G. Cooper, S. Richards, and M. Schouweiler (2023). IPUMS USA: Version 13.0 [dataset]. Minneapolis, MN.

Scherzer, T., R. Rugulies, and N. Krause (2005). Work-related pain and injury and barriers to workers' compensation among las vegas hotel room cleaners. *American Journal of Public Health 95*(3), 483–488.

Schmidt, L. and P. Sevak (2004). AFDC, SSI, and welfare reform aggressiveness: Caseload reductions versus caseload shifting. *Journal of Human Resources 39*(3), 792–812.

Schmidt, L., L. D. Shore-Sheppard, and T. Watson (2020). The impact of the ACA Medicaid expansion on disability program applications. *American Journal of Health Economics 6*(4), 444–476.

Slopen, M. (2023). The impact of paid sick leave mandates on women's health. *Social Science & Medicine 323*, 115839.

Slopen, M. (2024). The impact of paid sick leave mandates on women's employment and economic security. *Journal of Policy Analysis and Management*.

Staubli, S. (2011). The impact of stricter criteria for disability insurance on labor force participation. *Journal of Public Economics 95*(9-10), 1223–1235.

Summers, L. H. (1989). Some simple economics of mandated benefits. *The American Economic Review 79*(2), 177–183.

University of Kentucky Center for Poverty Research (2023, 2). UKCPR national welfare data, 1980-2021. University of Kentucky Center for Poverty Research.

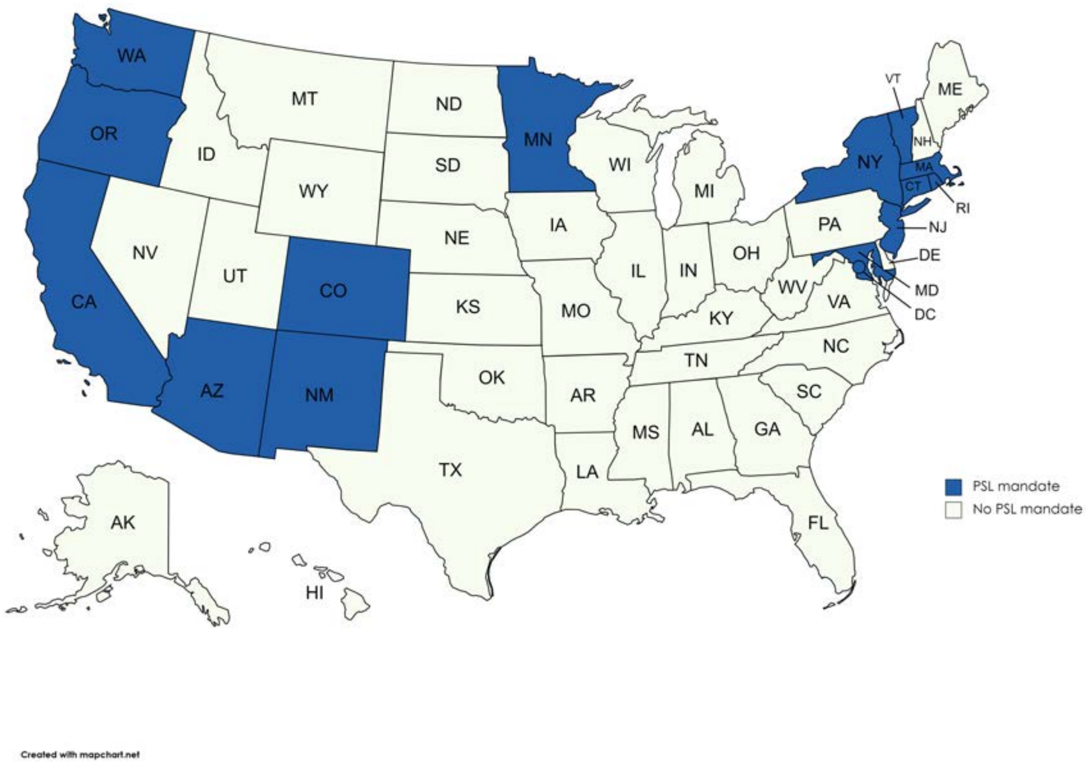
U.S. Bureau of Labor Statistics (2020). Paid Sick Leave Benefits Factsheet. https://www.bls.gov/ebs/factsheets/paid-sick-leave.htm.

U.S. Census Bureau (2022). County Business Patterns (CBP) Dataset [dataset]. Data retrieved from https://www.census.gov/programs-surveys/cbp/data/datasets.html.

Van Riper, D., S. Flood, and F. Roberts (2021, 9). Unraveling geographic complexities in the Current Population Survey.

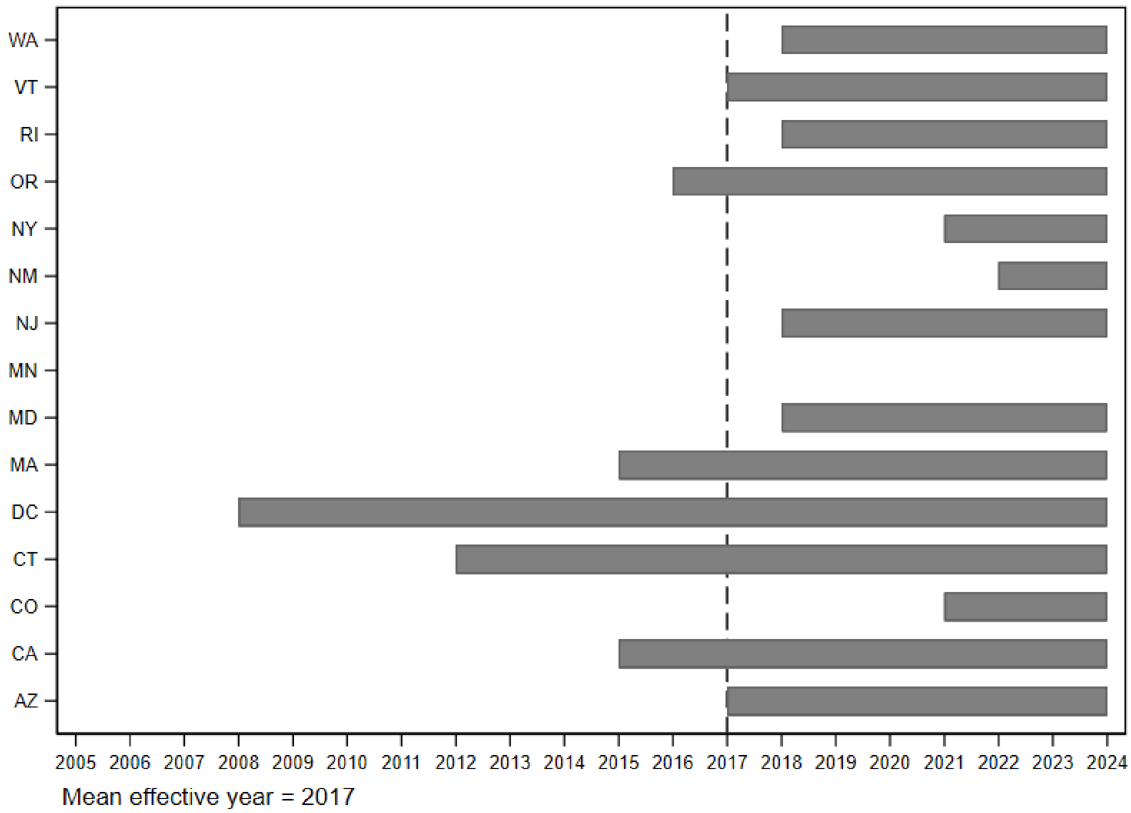
Figures and Tables

Figure 1: Geographic distribution of of all adopted or announced state PSL mandates in the U.S. as of 2023 Q3



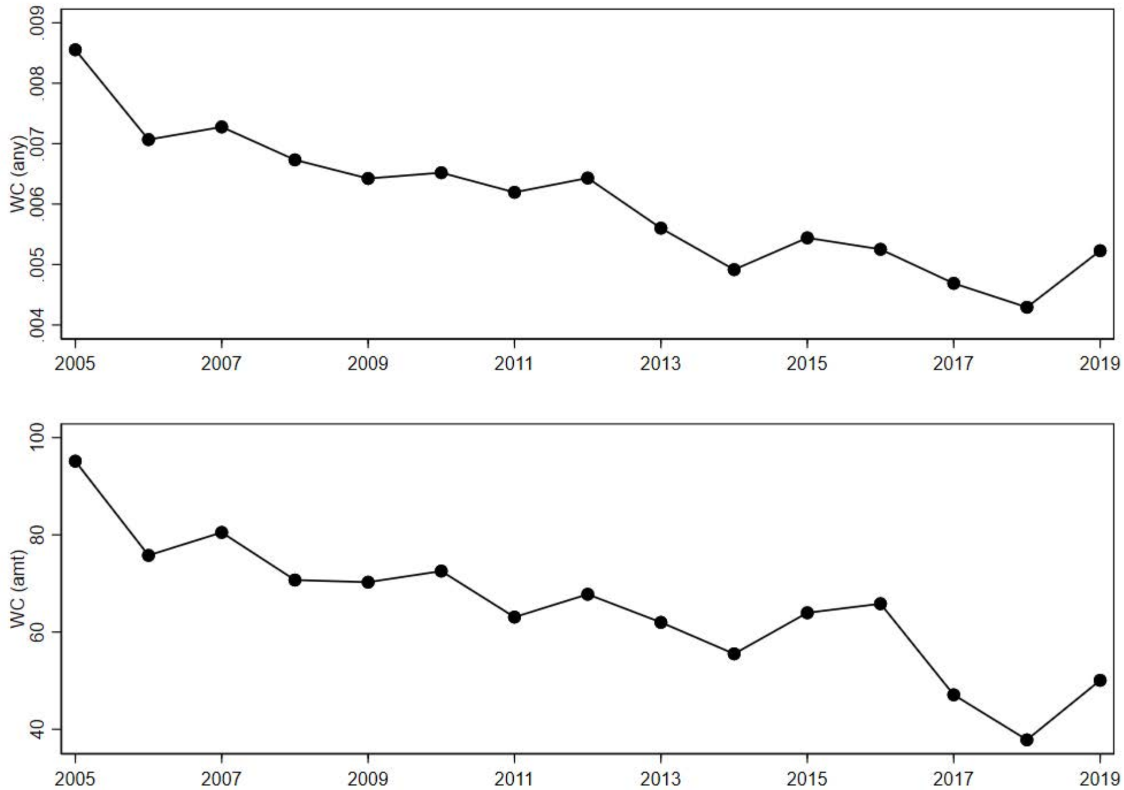
Notes: All state PSL mandates are effective or announced by October 2023. Data source is the National Partnership of Women & Families. Treatment states (effective years) are as follows: AZ (2017), CA (2015), CO (2021), CT (2012), DC (2008), MA (2015), MD (2018), MN (2024), NJ (2018), NM (2022), NY (2021), OR (2016), RI (2016), VT (2017), and WA (2018). We code the effective year as the first (partial or full) year in which the mandate is in place.

Figure 2: Staggered rollout of all adopted or announced state PSL mandates in the U.S. as of 2023 Q3



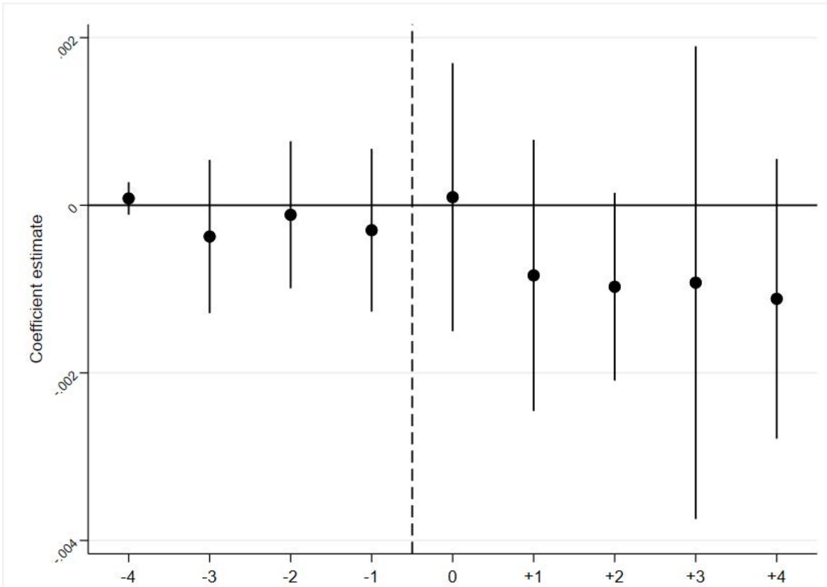
Notes: All state PSL mandates are effective or announced by October 2023. Data source is the National Partnership of Women & Families. Treatment states (effective years) are as follows: AZ (2017), CA (2015), CO (2021), CT (2012), DC (2008), MA (2015), MD (2018), MN (2024), NJ (2018), NM (2022), NY (2021), OR (2016), RI (2016), VT (2017), and WA (2018). We code the effective year as the first (partial or full) year in which the mandate is in place.
The dotted line at 2017 denotes the mean year of adoption.

Figure 3: Trends in WC income receipt outcomes among adults: CPS 2005-2019

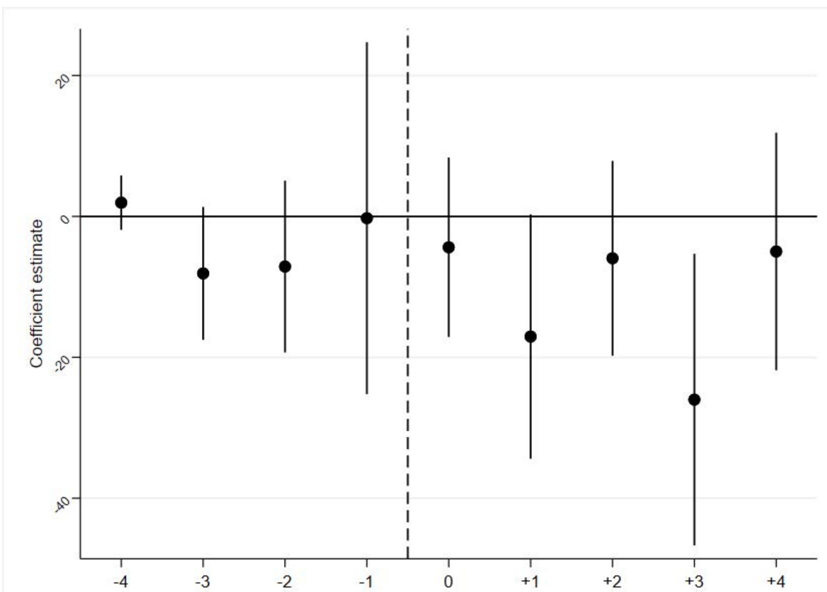


Notes: Data are weighted by CPS-provided weights prior to aggregating.

Figure 4: Effect of a PSL mandate on WC outcomes among adults using Gardner two-step event-study: CPS 2005-2019



Panel A: Any WC income



Panel B: WC income (amount)

Notes: We use 2SDID for estimation. The first step uses only untreated observations and models the outcomes as a function of state-level variables, demographics, state fixed-effects, and year fixed-effects. The estimated parameters are used to construct counterfactuals for the treated observations. The treatment effect estimates are weighted averages of the differences between the observed and counterfactual outcomes for the treated observations. The unit of observation is a respondent in state in a year. Data are weighted by CPS-provided weights. −4 includes four or more years prior to the policy change. +4 includes four or more years after the policy change. States that adopt a PSL mandate after 2019 are coded in their pre-treatment period. Circles represent coefficient estimates. 95% confidence intervals that account for within-state clustering are reported with vertical lines.

Table 1: Summary statistics: CPS 2005-2007

Sample:	All states	States that adopt a PSL policy	States that do not adopt a PSL policy
Any WC income	0.0076	0.0091	0.0067
WC income	83.8	110.9	67.6
Paid family and medical leave mandate	0.12	0.33	0
Poverty rate	12.4	11.8	12.8
Population (millions)	16.0	16.3	15.9
Age	43.0	42.8	43.1
Female	0.51	0.50	0.51
Male	0.49	0.50	0.49
White	0.81	0.80	0.81
African American	0.12	0.093	0.14
Other race	0.073	0.11	0.050
Non-Hispanic	0.86	0.80	0.90
Hispanic	0.14	0.20	0.10
Less than high school	0.12	0.13	0.11
High school	0.30	0.26	0.33
Some college education	0.27	0.26	0.28
College degree	0.31	0.35	0.28
Observations	311,315	112,810	198,505

Notes: The unit of observation is a respondent in state in a year. Data are weighted by CPS-provided weights.

Table 2: Effect of a PSL mandate on paid sick leave, unpaid sick leave, and paid absences from work for health-related reasons

Outcome:	PSL offer	PSL use (hours)	UPSL use (hours)	Absent with pay
Paid sick leave mandate	0.132***	1.573**	0.245***	0.0004**
(lagged one year)	(0.037)	(0.643)	(0.056)	(0.0002)
Dataset	NCS	NCS	NCS	CPS
Pre-treatment mean, PSL states	0.710	21.594	0.474	0.0024
Observations	500,862	500,862	500,862	1,488,232

Notes: We use 2SDID for estimation. The first step uses only untreated observations and models the outcomes as a function of state-level variables, demographics, state fixed-effects, and year fixed-effects. The estimated parameters are used to construct counterfactuals for the treated observations. The treatment effect estimates are weighted averages of the differences between the observed and counterfactual outcomes for the treated observations. The unit of observation is a respondent in state in a year. PSL use and UPSL use are measured in hours per year. Data are weighted by NCS- or CPS-provided weights. For the NCS analyses, imputed observations are excluded. Standard errors are adjusted for clustering at the state level and are reported in parentheses.
***, **, and * = statistically different from zero at the 1%, 5%, and 10% level.

Table 3: Effect of a PSL mandate on WC outcomes among adults using Gardner two-step difference-in-differences estimator

Specification:	(1)	(2)	(3)	(4)
		Panel A: Any WC income		
		(Pre-treatment mean in PSL states=0.0074)		
Paid sick leave mandate	-0.0009*	-0.0009*	-0.0010*	-0.0006
(lagged one year)	(0.0005)	(0.0005)	(0.0005)	(0.0006)
		Panel B: WC income (amount)		
		(Pre-treatment mean in PSL states=$85.0297)		
Paid sick leave mandate	-12.4188***	-12.9607***	-12.9993***	-8.4577**
(lagged one year)	(3.6814)	(3.6296)	(3.7847)	(4.1884)
State FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
State polices	N	Y	Y	Y
Respondent demographics	N	N	Y	Y
Include pandemic	N	N	N	Y
Years	2005-2019	2005-2019	2005-2019	2005-2022
Observations	1,459,709	1,459,709	1,459,709	1,682,307

Notes: We use 2SDID for estimation. The first step uses only untreated observations and models the outcomes as a function of state-level variables, demographics, state fixed-effects, and year fixed-effects. The estimated parameters are used to construct counterfactuals for the treated observations. The treatment effect estimates are weighted averages of the differences between the observed and counterfactual outcomes for the treated observations. The unit of observation is a respondent in state in a year. Data are weighted by CPS-provided weights. Standard errors are adjusted for clustering at the state level and are reported in parentheses.
***, **, and * = statistically different from zero at the 1%, 5%, and 10% level.

Table 4: Effect of a PSL mandate on employment outcomes among adults using Gardner two-step difference-in-differences estimator: CPS 2005-2019

Outcome:	Coefficient estimate (standard error)
Employed	0.0041
	(0.0026)
Pre-treatment mean, PSL states	0.9387
Observations	1,168,155
Weeks worked	0.1689**
	(0.0698)
Pre-treatment mean, PSL states	38.3917
Observations	1,459,709
Usual hours worked per week	-0.0259
	(0.0815)
Pre-treatment mean, PSL states	40.1366
Observations	1,176,489
Full time work	0.0052***
	(0.0015)
Pre-treatment mean, PSL states	0.8995
Observations	1,176,489
Hourly wage	0.6074***
	(0.0257)
Pre-treatment mean, PSL states	18.3134
Observations	86,447

Notes: We use 2SDID for estimation. The first step uses only untreated observations and models the outcomes as a function of state-level variables, demographics, state fixed-effects, and year fixed-effects. The estimated parameters are used to construct counterfactuals for the treated observations. The treatment effect estimates are weighted averages of the differences between the observed and counterfactual outcomes for the treated observations. The unit of observation is a respondent in state in a year. Data are weighted by CPS-provided weights. Standard errors are clustered at the state level and are reported in parentheses.
***, **, and * = statistically different from zero at the 1%, 5%, and 10% level.

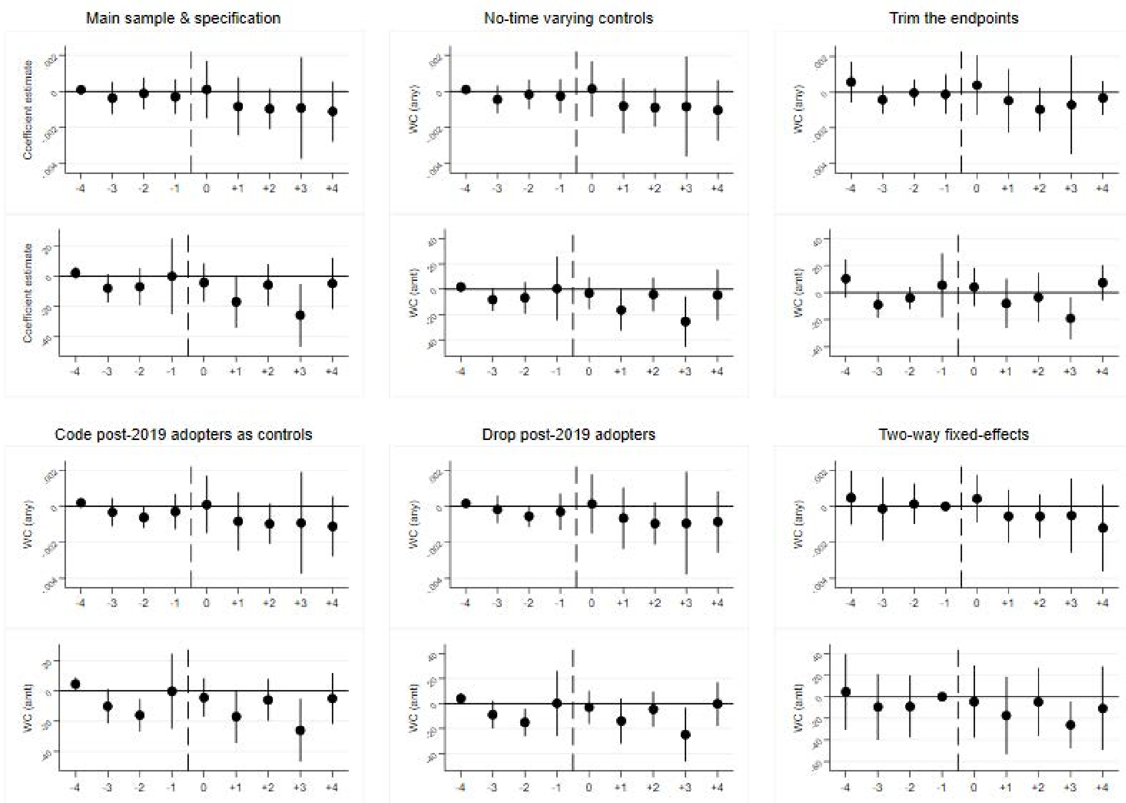
Table 5: Effect of a PSL mandate on state-level non-fatal and fatal injuries using Gardner two-step difference-in-differences estimator: Bureau of Labor Statistics

Outcome :	Non-fatal injuries per 10,000 FTEs	Fatal injuries per 100,000 FTEs
Paid sick leave mandate	1.66	0.03
(lagged one year)	(2.58)	(0.14)
Years	2011-2019	2005-2019
Dataset	SOII	COFI
Pre-treatment mean, PSL states	135.56	2.64
Observations	370	663

Notes: DC is excluded from the non-fatal injury sample as that state adopated a PSL mandate in 2008, prior to the earliest year of data. We use 2SDID for estimation. The first step uses only untreated observations and models the outcomes as a function of state-level variables, state fixed-effects, and year fixed-effects. The estimated parameters are used to construct counterfactuals for the treated observations. The treatment effect estimates are weighted averages of the differences between the observed and counterfactual outcomes for the treated observations. The unit of observation is a state in a year. Data are weighted by the state population. Standard errors are clustered at the state level and are reported in parentheses. Non-fatal injury data are not available for all states.
***, **, and * = statistically different from zero at the 1%, 5%, and 10% level.

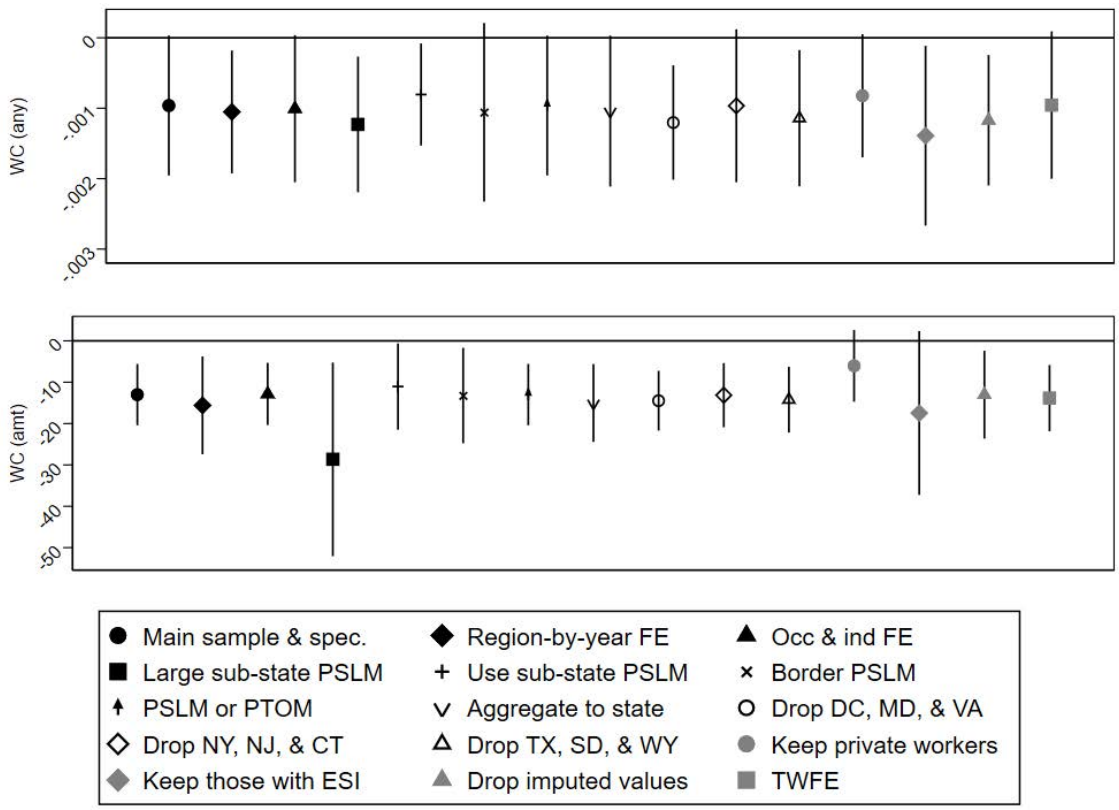
Appendix

Figure A1: Effect of a PSL mandate on WC outcomes among adults using alternative event-study specifications and samples: CPS 2005-2019



Notes: We use 2SDID for estimation unless otherwise noted. The first step uses only untreated observations and models the outcomes as a function of state-level variables, demographics, state fixed-effects, and year fixed-effects. The estimated parameters are used to construct counterfactuals for the treated observations. The treatment effect estimates are weighted averages of the differences between the observed and counterfactual outcomes for the treated observations. The unit of observation is a respondent in state in a year. Data are weighted by CPS-provided weights. −4 includes four or more years prior to the policy change. +4 includes four or more years after the policy change. States that adopt a PSL mandate after 2019 are coded in their pre-treatment period, unless otherwise noted. Circles represent coefficient estimates. 95% confidence intervals that account for within-state clustering are reported with vertical lines. 2SDID does not require normalizing a specific estimate to 0 (all estimates are implicitly normalized to the pre-period average).

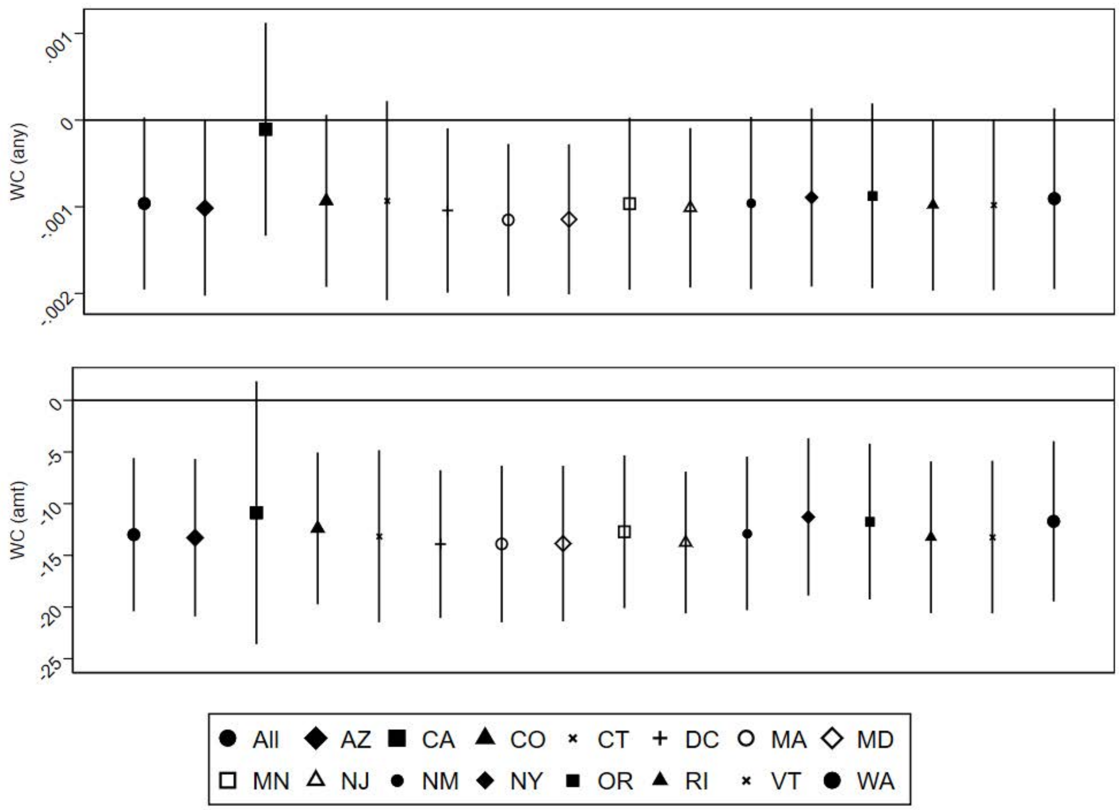
Figure A2: Effect of a PSL mandate on WC outcomes among adults using alternative specifications and samples: CPS 2005-2019



Notes: We use 2SDID for estimation unless otherwise noted. The first step uses only untreated observations and models the outcomes as a function of state and year fixed-effects. Additional state and respondent controls are also included unless otherwise noted. The estimated parameters are used to construct counterfactuals for the treated observations. The treatment effect estimates are weighted averages of the differences between the observed and counterfactual outcomes for the treated observations. The unit of observation is a respondent in state in a year (unless otherwise noted). Data are weighted by CPS-provided weights. 95% confidence intervals that account for within-state clustering are reported with vertical lines.

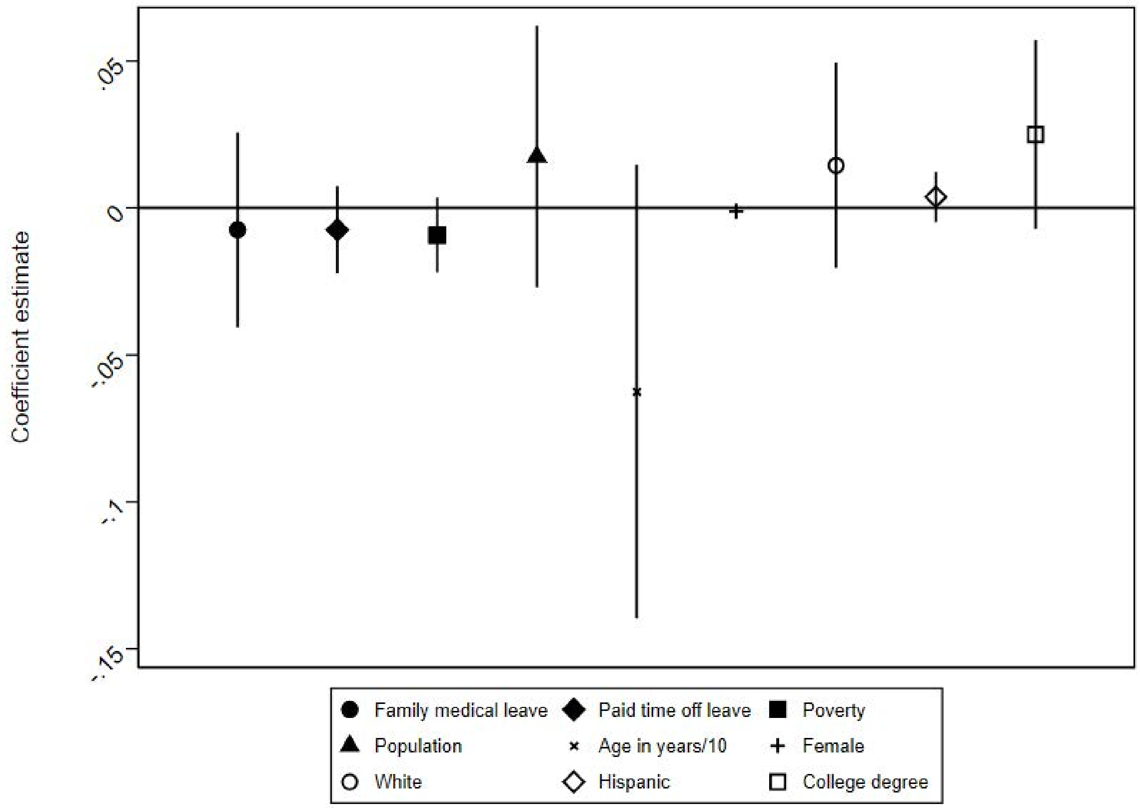
'Region-by-year FE' = region*year interactions were included in the model
'Occupation FE' and 'Industry FE' = occupation and industry fixed-effects included based on respondents' most recent occupation and occupation (those without an occupation or industry have a separate code for no occupation or industry)
'Large sub-state PSLM' = PSL indicator equal to one if a large ($\geq$ 500,000) locality within state adopts PSL
'Use sub-state PSLM' = PSL indicator equal to one based on state or sub-state PSL adoption (to the best extent that we can map people in CPS to sub-state localities)
'Border PSLM' = include indicator if state borders a state with a PSL mandate
'PSLM or PTOM' = PSL indicator also include PTO mandate adoption
'Aggregate to state' = analysis performed at state-level
'Drop DC, MD, & VA' = drop states in which commuting between states is common
'Drop NY, NJ, & CT' = drop states in which commuting between states is common
'Drop TX, SD, & WY' = drop states that do not mandate employers provide WC
'Keep private workers' = non-private workers dropped
'Keep those with ESI' = those without employed-sponsored insurance (ESI) dropped
'Drop imputed values' = drop respondents with imputed WC information
'TWFE' = two-way fixed-effects results

Figure A3: Effect of a PSL mandate on WC outcomes among adults sequentially dropping each state that adopts a PSL mandate by 2023 Q3: CPS 2005-2019



Notes: We use 2SDID for estimation. The first step uses only untreated observations and models the outcomes as a function of state-level variables, demographics, state fixed-effects, and year fixed-effects. The estimated parameters are used to construct counterfactuals for the treated observations. The treatment effect estimates are weighted averages of the differences between the observed and counterfactual outcomes for the treated observations. The unit of observation is a respondent in state in a year. Circles represent coefficient estimates. 95% confidence intervals that account for within-state clustering are reported with vertical lines.

Figure A4: Test of balance: 2005-2019



Notes: The outcome variable listed in the legend, each coefficient estimate is from a separate regression. Each specification regresses the listed variable on the lagged PSL mandate, state fixed-effects, and year fixed-effects. The unit of observation is a state in a year. Data are aggregated to the state-year level. Data are weighted by CPS-provided weights prior to aggregating to the state-year level. Circles represent coefficient estimates. 95% confidence intervals that account for within-state clustering are reported with vertical lines.

Table A1: State PSL mandate effective dates and impact: National Partnership of Women and Families

State	Effective date	Employees gaining coverage for the first time
Arizona	7/2017	934,000
California	7/2015	6,900,000
Colorado	1/2021	813,000
Connecticut	1/2012	200,000
District of Columbia	5/2008	220,000
Massachusetts	7/2015	900,000
Maryland	2/2018	750,000
Minnesota	1/2024	N/A†
New Mexico	7/2022	286,000
New York	1/2021	2,600,000
New Jersey	10/2018	1,200,000
Oregon	1/2016	473,000
Rhode Island	7/2018	100,000
Vermont	1/2017	60,000
Washington	1/2018	1,000,000

Notes: State PSL mandates adopted or announced as of October 2023. Estimates of employees gaining PSL coverage for the first time based on National Partnership of Women & Families (2023) 'Law/Bill Number and Impact.'

†The NPWF has not released data on the number of employees gaining PSL through the MN policy change.

Table A2: Heterogeneity by respondent demographics in the effect of a PSL mandate on WC outcomes among adults Gardner two-step difference-in-differences estimator: CPS 2005-2019

Outcome:	WC (any)	WC (amt)
Sample: 25 to 39 years	-0.0008	-16.0722***
	(0.0007)	(4.1978)
Pre-treatment mean, PSL states	0.0053	$45.7393
Observations	582,690	582,690
Sample: 40 to 62 years	-0.0011**	-11.4260**
	(0.0005)	(5.4859)
Pre-treatment mean, PSL states	0.0088	$111.1598
Observations	877,019	87,7019
Sample: Men	-0.0009*	-15.2703*
	(0.0005)	(8.1350)
Pre-treatment mean, PSL states	0.0086	$109.4715
Observations	699,376	699,376
Sample: Women	-0.0010	-10.2443
	(0.0008)	(8.7444)
Pre-treatment mean, PSL states	0.0062	$61.2388
Observations	760,333	760,333
Sample: White	-0.0011**	-13.0912***
	(0.0005)	(4.6956)
Pre-treatment mean, PSL states	0.0074	$86.5483
Observations	1,146,387	1,146,387
Sample: Non-White	-0.0004	-13.7528*
	(0.0007)	(8.3529)
Pre-treatment mean, PSL states	0.0072	$79.7888
Observations	313,322	313,322
Sample: Hispanic	-0.0023***	-28.6797***
	(0.0005)	(8.2488)
Pre-treatment mean, PSL states	0.0082	$86.9721
Observations	253,983	253,983
Sample: Non-Hispanic	-0.0006	-8.6727**
	(0.0006)	(4.2453)
Pre-treatment mean, PSL states	0.0072	$84.5088
Observations	1,205,726	1,205,726

Notes: We use 2SDID for estimation. The first step uses only untreated observations and models the outcomes as a function of state-level variables, demographics, state fixed-effects, and year fixed-effects. The estimated parameters are used to construct counterfactuals for the treated observations. The treatment effect estimates are weighted averages of the differences between the observed and counterfactual outcomes for the treated observations. The unit of observation is a respondent in state in a year. Data are weighted by CPS-provided weights. Standard errors are clustered at the state level and are reported in parentheses.
***, **, and * = statistically different from zero at the 1%, 5%, and 10% level.

Table A3: Heterogeneity by job physicality and firm size in the effect of a PSL mandate on WC outcomes among adults Gardner two-step difference-in-differences estimator: CPS 2005-2019

Outcome:	WC (any)	WC (amt)
Sample: Physically demanding job	-0.0012	-18.8747**
	(0.0009)	(8.8360)
Pre-treatment mean, PSL states	0.0108	$85.5621
Observations	292,002	292,002
Sample: Not physically demanding job	-0.0009*	-11.3142***
	(0.0006)	(3.8733)
Pre-treatment mean, PSL states	0.0067	$84.9174
Observations	1,167,707	1,167,707
Sample: Firms with $<$ 100 workers†	-0.0006	-1.8992
	(0.0004)	(4.8141)
Pre-treatment mean, PSL states	0.0039	$28.3280
Observations	628,592	628,592
Sample: Firms with $\geq$ 100 workers†	-0.0006	-11.4278***
	(0.0004)	(4.3894)
Pre-treatment mean, PSL states	0.0069	$55.3366
Observations	849,574	849,574

Notes: We use 2SDID for estimation. The first step uses only untreated observations and models the outcomes as a function of state-level variables, demographics, state fixed-effects, and year fixed-effects. The estimated parameters are used to construct counterfactuals for the treated observations. The treatment effect estimates are weighted averages of the differences between the observed and counterfactual outcomes for the treated observations. The unit of observation is a respondent in state in a year. Data are weighted by CPS-provided weights. Standard errors are clustered at the state level and are reported in parentheses. A physically demanding job is defined using the IPUMS harmonized occupation variable *occ90ly* (codes 473 to 905).
† Sample includes only employed respondents.
***, **, and * = statistically different from zero at the 1%, 5%, and 10% level.

Table A4: Effect of a PSL mandate on time use using Gardner two-step difference-in-differences estimator: ATUS 2005-2019

Outcome:	Coefficient estimate (standard error)
Work time	-9.4963**
	(4.5537)
Pre-treatment mean in PSL states	361.5202
Rest time	8.3298**
	(3.4661)
Pre-treatment mean in PSL states	230.4119
Self healthcare time	-0.0718
	(0.6879)
Pre-treatment mean in PSL states	3.9454
Purchase healthcare time	-0.2565
	(0.7523)
Pre-treatment mean in PSL states	3.7927
Apply for/use gov't benefits †	-0.0093
	(0.0411)
Pre-treatment mean in PSL states	0.1827
Observations	58,311

Notes: We use 2SDID for estimation. Outcome is minutes per day spent on listed activity. The first step uses only untreated observations and models the outcomes as a function of state-level variables, demographics, state fixed-effects, and year fixed-effects. The estimated parameters are used to construct counterfactuals for the treated observations. The treatment effect estimates are weighted averages of the differences between the observed and counterfactual outcomes for the treated observations. Outcomes are minutes per average day. The regression includes state-level variables, demographics, state fixed-effects, and time (month-year) fixed-effects. The unit of observation is a respondent in state in a state in a year. Data are weighted by ATUS-provided weights. Standard errors are clustered at the state level and are reported in parentheses.
† = Includes workers compensation benefits.
***, **, and * = statistically different from zero at the 1%, 5%, and 10% level.

Table A5: Effect of a PSL mandate on establishment outcomes per 100,000 state residents using Gardner two-step difference-in-differences estimator: CBP 2005-2019

Outcome:	Establishments	Employees
Paid sick leave mandate	76.10**	1317.20
(lagged one year)	(31.13)	(1065.02)
Pre-treatment mean, PSL states	2,541	38,595
Observations	765	765

Notes: We use 2SDID for estimation. The first step uses only untreated observations and models the outcomes as a function of state-level variables, state fixed-effects, and year fixed-effects. The estimated parameters are used to construct counterfactuals for the treated observations. The treatment effect estimates are weighted averages of the differences between the observed and counterfactual outcomes for the treated observations. The unit of observation is a state in a year. Data are weighted by the state population. Standard errors are clustered at the state level and are reported in parentheses.
***, **, and * = statistically different from zero at the 1%, 5%, and 10% level.

Table A6: Effect of a PSL mandate on health outcomes among adults using Gardner two-step difference-in-differences estimator: CPS 2005-2019

Outcome:	Coefficient estimate (standard error)
Fair or poor health	-0.0000
	(0.0030)
Pre-treatment mean, PSL states	0.1068
Observations	1,488,232
Very good or excellent health	0.0052
	(0.0054)
Pre-treatment mean, PSL states	0.6415
Observations	1,488,232
Health (1-5)	0.0025
	(0.0154)
Pre-treatment mean, PSL states	2.1884
Observations	1488232
Work-limiting disability	-0.0042**
	(0.0018)
Pre-treatment mean, PSL states	0.0786
Observations	1,488,232

Notes: We use 2SDID for estimation. The first step uses only untreated observations and models the outcomes as a function of state-level variables, demographics, state fixed-effects, and year fixed-effects. The estimated parameters are used to construct counterfactuals for the treated observations. The treatment effect estimates are weighted averages of the differences between the observed and counterfactual outcomes for the treated observations. The unit of observation is a respondent in state in a year. Data are weighted by CPS-provided weights. Standard errors are clustered at the state level and are reported in parentheses.
***, **, and * = statistically different from zero at the 1%, 5%, and 10% level.

Table A7: Effect of a PSL mandate on ESI and premiums among adults using Gardner two-step difference-in-differences estimator: CPS 2011-2019

Outcome	ESI	Premiums
Paid sick leave mandate	0.0038***	-47.3425
(lagged one year)	(0.0010)	(113.1532)
Pre-treatment mean, PSL states	0.8980	$2545.4469
Observations	658,673	922,820

Notes: We use 2SDID for estimation. The first step uses only untreated observations and models the outcomes as a function of state-level variables, demographics, state fixed-effects, and year fixed-effects. The estimated parameters are used to construct counterfactuals for the treated observations. The treatment effect estimates are weighted averages of the differences between the observed and counterfactual outcomes for the treated observations. The regression includes state-level variables, demographics, state fixed-effects, and year fixed-effects. The unit of observation is a respondent in state in a year. Data are weighted by CPS-provided weights. Standard errors are clustered at the state level and are reported in parentheses.
***, **, and * = statistically different from zero at the 1%, 5%, and 10% level.

Table A8: Effect of a PSL mandate on state-level WC expenditures using Gardner two-step difference-in-differences estimator: BEA 2005-2019

Outcome:	Per 1,000 state residents	Per 1,000 state employees
Paid sick leave mandate	-1.3020*	-3.1378**
(lagged one year)	(0.6878)	(1.5029)
Pre-treatment mean, PSL states	$9.2051	$19.6241
Observations	765	765

Notes: We use 2SDID for estimation. The first step uses only untreated observations and models the outcomes as a function of state-level variables, state fixed-effects, and year fixed-effects. The estimated parameters are used to construct counterfactuals for the treated observations. The treatment effect estimates are weighted averages of the differences between the observed and counterfactual outcomes for the treated observations. The outcome variable is state-level WC expenditures divided by the number of i) residents and ii) employed people in the state scaled by 1,000. The unit of observation is a state in a year. Data are weighted by population size. Standard errors are clustered at the state level and are reported in parentheses.
***, **, and * = statistically different from zero at the 1%, 5%, and 10% level.

Table A9: Effect of a PSL mandate on migration outcomes among adults using Gardner two-step difference-in-differences estimator: CPS 2005-2019

Outcome	Move across state lines
Paid sick leave mandate	-0.0011
(lagged one year)	(0.0009)
Pre-treatment mean, PSL states	0.0155
Observations	1,482,698

Notes: We use 2SDID for estimation. The first step uses only untreated observations and models the outcomes as a function of state-level variables, demographics, state fixed-effects, and year fixed-effects. The estimated parameters are used to construct counterfactuals for the treated observations. The treatment effect estimates are weighted averages of the differences between the observed and counterfactual outcomes for the treated observations. The unit of observation is a respondent in state in a year. Data are weighted by CPS-provided weights. Standard errors are clustered at the state level and are reported in parentheses.
***, **, and * = statistically different from zero at the 1%, 5%, and 10% level.