# Time for mental healthcare: Evidence from paid sick leave mandates

Matthew Eisenberg, Yimin Ge,
Ezra Golberstein, Johanna Catherine Maclean*

Version: February 9, 2026

**Abstract**

We study the effects of state paid sick leave (PSL) mandates on mental healthcare use. To do so, we use all-payer health insurance claims data from IQVIA 2015-2022 combined with difference-in-differences methods. Findings suggest that overall mental healthcare use does not change post-PSL mandate. The null finding overall masks clinically relevant changes along the intensive margin: patients substitute away from lower levels of care to higher levels of care as time constraints are relaxed. Further, analysis of survey data hints that this change in treatment use improves mental health.
Keywords: Paid sick leave; social insurance; social insurance.
JEL codes: I1; I11.

*Eisenberg: Center for Mental Health and Addiction Policy, Department of Health Policy and Management, Johns Hopkins University. Ge: Center for Mental Health and Addiction Policy, Department of Health Policy and Management, Johns Hopkins University. Golberstein: Division of Health Policy and Management, University of Minnesota School of Public Health. Maclean (corresponding author): Schar School of Policy and Government, George Mason University, & NBER & IZA, 3351 Fairfax Drive, Arlington VA, USA, 22201, email: jmaclea@gmu.edu. All authors contributed equally to this study. Authors 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). Dr. Eisenberg received grants unrelated to this work from the National Institute on Drug Abuse, the National Institute on Mental Health, the National Institute on Nursing Research, and the Commonwealth Fund. Dr. Golberstein received a grant from the Commonwealth Fund. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Institutes of Health or the Commonwealth Fund. 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 participants at the NIH HEAL May 2025 Workshop, 2025 American Society of Health Economists Meeting, and the University of Southern California Health Economics Seminar Series, and Monica Deza and Thomas Hegland for excellent comments, and Maury Gittleman and Michael Lettau for assistance with the National Compensation Survey. All errors are our own.

# 1 Introduction

The notion that consuming healthcare requires both financial resources and time has long been understood in economics (Grossman, 1972). A large literature investigates the price-elasticity of healthcare demand (Newhouse, 1993; Duarte, 2012; Kowalski, 2016; Ellis et al., 2017). However, the relationship between time and healthcare demand has received less attention (Coffey, 1983), in spite of evidence that consumers face binding tradeoffs between time spent at work and time using healthcare (Aguiar et al., 2013). Recent work in health and labor economics emphasizes that mental health treatment decisions involve substantial non-monetary costs such as time and psychological efforts, and underscores the importance of time availability and job flexibility in the production of mental health. For example, one study shows that even though psychotherapy is effective, treatment uptake remains low because individuals face a significant amount of time and dis-utility costs to receive this care (Cronin et al., 2025). Similarly, another study shows that mental health and job characteristics are dynamically intertwined, that stressful jobs worsen mental health and limit individuals' ability to invest time in care, while improved mental health enhances labor market performances (Jolivet and Postel-Vinay, 2025). Moreover, workers place, arguably, high value on jobs that support their mental health: workers are willing to forgo up to 12% of their hourly wage to work in a low-stress (vs. high-stress) job (Jolivet and Postel-Vinay, 2025), which is similar to the willingness to pay displayed by workers for job attributes such as the ability to set one's schedule and job autonomy (Maestas et al., 2023).

To the extent that time matters for using healthcare, policies that reduce the cost of time required for such care may affect patterns of healthcare use. In this study, we examine the impact of recent state paid sick leave (PSL) mandates on mental healthcare use. While the U.S. – unlike the majority of developed countries – lacks a federal paid sick leave policy, 18 states and the District of Columbia have adopted PSL mandates. These laws require most employers to let workers earn about one hour of paid sick time for every 30-40 hours worked (National Partnership of Women & Families, 2023). PSL mandates provide an ideal setting to study the issue of time costs affecting healthcare use due to their staggered implementation across states and over the past 20 years.

Mental healthcare is a useful setting in which to study time costs because treatment often requires repeated visits that may be difficult to schedule around work. In addition, mental health conditions are under-diagnosed and under-treated (Merikangas et al., 2011; Green et al., 2019; Whitney and Peterson, 2019), and mental healthcare is often viewed as more discretionary than treatment for physical health conditions (Frank and McGuire, 2000). These features suggest that demand for mental healthcare could be especially sensitive to

non-monetary costs such as time. At the same time, access to care is limited in many parts of the U.S. with 57% of counties deemed mental healthcare shortage areas (Kaiser Family Foundation, 2023). Limited provider availability can restrict appointment supply, which may mute the extent to which additional paid time off work translates into higher utilization.

Time costs may nevertheless shape the type of treatment used among patients who do obtain care. Mental health treatment ranges from relatively low-time medication to high-time psychotherapy. Medication treatment often requires an initial clinician visit and periodic follow up medication management appointments. By contrast, regular psychotherapy (e.g., cognitive behavioral therapy) is a central component of effective care for many mental health conditions (Kaczkurkin and Foa, 2015; Moorey and Hollon, 2021), and is time-intensive both in terms of overall treatment duration with many patients receiving weekly care for extended periods of time and per episode as a single therapy session often includes 45 minutes of direct clinician time. Because medications and psychotherapy can be complements or substitutes for each other depending on a patient's clinical situation, relaxing time constraints could change both the overall level of care and the composition of care. These facts motivate two key predictions. First, if work-time constraints are an important barrier, PSL mandates should increase outpatient mental healthcare use, particularly time-intensive care, and may reduce reliance on medications. Second, overall mental healthcare use may change little and responses may show up as shifts across treatment types. For example, if there are provider shortages, patients might move from regular therapy visits to shorter and less frequent medication management visits. The bidirectional influences of mental health status and employment motivates analyses of how features of employment arrangements affect mental healthcare use (Jolivet and Postel-Vinay, 2025).

To study the impact of state PSL mandates on mental healthcare use, we combine all-payer IQVIA claims data from 2015 to 2022 with difference-in-differences and event-study methods that are robust to bias associated with a staggered treatment roll out. We estimate the impact of state PSL mandates on overall mental healthcare use and by service type: dispensed medications, outpatient services, and inpatient services. We explore heterogeneity in state PSL mandate effects across patient demographics (age and sex) and payers (commercial coverage, Medicare, Medicaid, and other).

Our results suggest that overall mental healthcare use does not vary following adoption of a state PSL mandate, though there are clinically meaningful changes in the type of care received.[1] In particular, post-mandate dispensed prescription medications decline while both

[1]Though, as we discuss in Section 3.1, we will focus on a sample of people who have had a mental health or substance use disorder diagnosis. Given the manner in which diagnoses are used by healthcare professionals to convey 'medical necessity' to insurance companies for reimbursement purposes, we do not believe that our focus on this sample leads to substantial bias in our results.

outpatient and inpatient care increases, and we find further evidence that consumers may substitute pharmaceutical treatment for (more time-intensive) outpatient or inpatient care when time constraints are relaxed. We also offer suggestive evidence that PSL mandates ultimately lead to improvements in self-reported mental health.

We make several contributions to the economics literature. First, our findings shed new light on the labor-leisure trade-off in health production (Coffey, 1983), and adds to recent research on the time costs of mental healthcare (Cronin et al., 2025). Second, we explore whether different treatment modalities (dispensed prescription medications, outpatient care, and inpatient care) are complements or substitutes in the production of mental health, there is limited evidence on the extent to which different levels of care are substitutes or complements among mental healthcare patients. Third, we test for spillovers from labor market policies (PSL mandates) to health insurance markets, and thus contribute to the large literature that explores linkages between labor and insurance markets in the U.S. (Summers, 1989). Finally, we add to the growing literature (described in Section 2.2) that explores the impacts of PSL mandates on American society.

This paper proceeds as follows. Section 2 provides an overview of mental healthcare and U.S. state PSL mandates. Data, variables, and methods are outlined in Section 3. Results are reported in Section 4. Finally, Section 5 offers a discussion and conclusion.

# 2 Background

## 2.1 Mental healthcare

Mental health treatment spans a wide continuum, from brief advice in a primary care setting to extended inpatient care. We group the main settings in the U.S. by the typical time commitment required from patients.

Family physicians, internists, and primary care physicians deliver a large share of initial mental healthcare. In particular, these clinicians can provide routine screening, short counseling visits, specialist referrals, and initiation and renewal of psychotropic medications (Siu et al., 2016; Olfson et al., 2016). For most patients, time costs are modest, consisting of a 15-30 minute visit added on to existing primary-care appointments. More complex cases may require more specialized treatment (Mee-Lee et al., 2013; Ramanuj et al., 2019).

Psychiatrists, clinical psychologists, psychiatric nurse practitioners, and licensed clinical social workers provide diagnosis, medication management, and psychotherapy in office or tele-medicine-based formats (Olfson et al., 2016). While a standard medication management check requires roughly 15 minutes per quarter, evidence-based psychotherapies such

as cognitive behavioral therapy generally involve 45-minute sessions once a week for eight to 20 weeks. Supply is limited; more than half of U.S. counties have no practicing psychiatrists (Kaiser Family Foundation, 2023). These gaps in provider supply raise the opportunity cost of time by lengthening wait times and travel distance.

Community centers offer a mix of group counseling, medication management, and skills training. Partial-hospitalization programs deliver four to six hours of structured therapy each weekday, typically for two to four weeks, allowing patients to return home at night and on weekends. Patients therefore trade-off several hours per day for intensive symptom management without the requirement of an inpatient admission.

Emergency departments, psychiatric emergency services, and inpatient psychiatric units manage crises such as suicidal ideation, psychosis, or severe withdrawal. Stays range from 24 hours in an observation unit to several weeks in an inpatient ward. Although these services are time-intensive, they are often unavoidable once risk reaches a critical threshold.

Across this continuum, the direct time required from patients rises with treatment intensity: minutes for a primary-care screen, hours per week for psychotherapy, and complete time removal from work for residential or inpatient care. Because PSL mandates reduce the financial penalty of taking time off, their effects are likely to vary by setting.

A recent paper of particular relevance to our work implements a dynamic structural model of mental health treatment choice that explicitly considers time costs in the decision between psychotherapy and medication-only treatment modalities (Cronin et al., 2025). A key finding in Cronin et al. (2025) is that eliminating time and employment costs of psychotherapy would substantially increase psychotherapy use. Our paper complements this analysis by leveraging policy variation in access to PSL benefits which we expect to reduce time costs of care in a way that would disproportionately affect the overall costs of psychotherapy.

## 2.2 Paid sick leave in the U.S.

Most developed countries have federal policies in place that provide PSL to employees (Pichler and Ziebarth, 2024). The U.S. is one of the few developed countries that does not have such a policy. Federal PSL policy has been advanced several times since 2005 when the *Healthy Families Act* was proposed in the U.S Senate by Senator Theodore Kennedy and most recently in 2023 (Sanders and DeLauro, 2023), but no efforts have been successful.

The COVID-19 era highlighted gaps in PSL access: the *Families First Coronavirus Response Act* (FFCRA) temporarily required certain employers to provide short-term paid sick time in 2020 (Andersen et al., 2023). Outside of that emergency measure, many states and localities have enacted permanent PSL mandates that require employers to provide fully

paid, short duration leave usable for one's own health and for dependents. These mandates are structured for brief, repeatable absences (e.g., evaluations, weekly therapy). Our study evaluates these state PSL mandates.[2]

18 states, the District of Columbia, and several cities and counties adopted or announced a PSL mandate by November, 2025 (National Partnership of Women & Families, 2023; A Better Balance, 2025). State PSL mandates require employers to provide, on average, seven days of PSL per year. Leave can be used for the eligible employee's health or the health of dependents, all states include children and spouses as dependents and some states include additional dependents (e.g., domestic partners and grandparents). Of particular relevance for our study, mandated PSL can be used for mental healthcare (National Partnership of Women & Families, 2023). Mandates vary to some extent in terms of accrual rates, exemptions (e.g., small firms), provision of unpaid sick leave, and waiting periods, but all state mandates are broadly similar to the *Healthy Families Act* (2005) and provide 100% wage replacement.

A reasonable question to ask is the extent to which leave provided by state PSL mandates is sufficient to cover mental healthcare. State mandates provide, on average, seven days of leave per year (National Partnership of Women & Families, 2023). A counseling session typically lasts 45-55 minutes – not including wait or travel times. Seven days of PSL per year could include roughly 30.5 therapy visits.[3] The average duration of an inpatient hospital stay is three to seven days while residential care is typically 30- to 90-days (Schiffer, 2023). Thus, state PSL mandates likely provide sufficient time for outpatient and inpatient care, but not residential care, for employees. However, mandated PSL may allow eligible employees to support their dependents' receipt of all types of mental healthcare (e.g., transporting patients to treatment, providing childcare), suggesting that PSL mandates could indirectly facilitate both outpatient and inpatient care for dependents. More frequent outpatient mental healthcare treatment, or interaction with healthcare providers generally, for the eligible employee (or their dependents) may increase the likelihood that – if a crisis situation arises – the patient may be more likely to be referred to inpatient care. Moreover, PSL could serve as a 'bridge' to other, longer-term, types of employment leave such as disability, Workers Compensation, and so forth. That is, as workers apply for longer term leave or during mandatory waiting periods for such leave, workers may use mandated PSL benefits.

There is a small but growing literature on the impacts of PSL mandates. Maclean et al.

[2]The federal *Family and Medical Leave Act* (FMLA) of 1993 provides unpaid job protected leave with eligibility requirements and high administrative burden, and thus is conceptually distinct from the state PSL mandates we will study.

[3]For this calculation, we estimate that seven days of PSL = 3,360 minutes (= seven days × eight hours per day × 60 minutes per hour). We also assume 110 minutes per therapy session which we calculate as 55 minutes × two (assuming travel and wait time are equivalent to clinician time).

(2025) show that state PSL mandates increase the probability that an employee has access to PSL by 32% and increase the use of PSL by 22% using detailed data on establishments from 2009 to 2022. Analyses of survey data find complementary evidence that self-reported employee access to PSL increases following a state PSL mandate (Ahn and Yelowitz, 2016; Callison and Pesko, 2022). Several studies show that healthcare use – as measured by prescriptions, vaccinations, contraception use, and expenditures (Wen et al., 2017; Maclean et al., 2023, 2024; Callison et al., 2023; Hebert et al., 2024) – increases post-mandate while measures of potentially unnecessary care, as captured by emergency department visits, decrease (Ma et al., 2022). A complete consensus has not yet been achieved as Guo and Peng (2024) finds no change in self-reported preventive care post-mandate using survey data. There is also some evidence that health may improve following the enactment of a state PSL mandate (Wolf et al., 2022; Slopen, 2023). Finally, state PSL mandates increase caregiving to both children and older adults (Arora and Wolf, 2024; Byker et al., 2023; Guo and Peng, 2024; Maclean and Pabilonia, 2025), suggesting that mandated PSL may be used to support dependents' healthcare, potentially including mental healthcare.

Arguably the most similar study to ours is Maclean et al. (2024). The authors show that Medicaid-financed prescription medications for mental health disorders increase by 7% following a state PSL mandate over the period 2011-2024. The two studies have complementary objectives and make different contributions to the literature. In particular, Maclean et al. (2024) uses aggregate counts of dispensed prescriptions while we focus on a sample of patients with a diagnosis for a behavioral health condition (i.e., mental health or substance use disorder). Thus, Maclean et al. (2024) cannot differentiate between patients who may use a mental health medication for a non-mental healthcare purpose (e.g., bupropion may be used to treat depression and for smoking cessation) while we study a sample of patients who have a behavioral health diagnosis and thus are more likely to capture patients receiving mental healthcare. Related to this point, Maclean et al. (2024) examines aggregate counts and cannot discriminate between more patients receiving care or current patients filling more prescriptions, our granulated claims data allows us to separately consider these two margins. Further, we consider all payers (commercial, Medicare, Medicaid, and other), as time may be more or less binding for different groups of patients. Finally, we examine mental healthcare beyond simply dispensed medications, instead we consider the full suite of services available to patients. Of particular relevance given our interest in financially protected time,

non-pharmacological services are relatively more time-intensive than prescriptions.[4,5]

### 2.3 Conceptual framework

Mental health treatment spans brief primary care encounters to weekly psychotherapy and, for some, inpatient or residential care. The time burden associated with these care types rises with intensity. When time is scarce, patients may rely on less time-intensive options (e.g., prescriptions) and delay or forgo services that require recurring visits. If a policy lowers the time price of care, the total use of care could rise, and the mix of services received could shift toward more time intensive care. State PSL mandates provide about seven fully paid days per year – with employees able to carry forward unused leave, can be used for an employee's own health and for dependents, and explicitly cover mental healthcare. Seven days is ample time for starting and maintaining outpatient therapy and can cover short inpatient stays, though this leave is insufficient for residential care (e.g., 30 or 90 day stays in specialized treatment centers), though mandated PSL could serve as 'bridge' to longer-term care (e.g., workers may take mandated PSL following a health shock, and use this time to apply for longer-term benefits such as disability). Mandated PSL may also enable caregivers to provide transport or child care, indirectly supporting dependents' treatment.

This short conceptual framework guides us to three testable hypotheses that we examine empirically below. (i) We expect little change in 'any use' overall, but a shift away from prescriptions toward outpatient/inpatient care. (ii) We expect increases in 'any use' for children if caregivers use PSL to initiate treatment. (iii) We expect heterogeneity by payer to be consistent with differences in out-of-pocket costs.

## 3 Data and methods

### 3.1 IQVIA claims data

We use two all-payer claims databases from IQVIA to measure both prescriptions and medical claims (IQVIA Institute, 2025; McGinty et al., 2025; IQVIA Institute for Human Data Science, 2024), and we describe these sources next.

[4]While we emphasize changes in mental healthcare use driven by demand-side factors, we note the possibility that PSL mandates could also impact supply-side factors. For example, if healthcare professionals recommend more intensive care following adoption of a PSL mandate (perhaps believing that patients overall now face lower time costs). We cannot separate demand- and supply-side drivers in our analysis, but we expect demand-side factors to be more responsive to state PSL mandate adoptions.

[5]However, we note that our study, based on the manner in which our claims data set is formed, cannot study the impact of state PSL mandates on the probability that a person receives a mental health or substance use diagnosis. Future research, using alternative data sources, could consider this question.

Longitudinal Prescription (LRx) Database: The LRx data track patient prescriptions over time. IQVIA receives approximately 3.7B prescription claims for 250M patients each year. These data capture approximately 94% of all raw prescription transactions from retail pharmacies across the U.S., 74% of traditional and specialty mail order pharmacies, and 75% for long-term care pharmacies. LRx data are received electronically by IQVIA from pharmacies, payers, software providers, and transactional clearinghouses. LRx data contain prescription-level information on the pharmaceutical product dispensed, prescription specifications (e.g., dose, duration), prescriber, payer, and geographical location of the patient.

Medical Claims (Dx) Database: Dx data represent pre-adjudicated professional claims generated by office-based physicians and specialists and collected by IQVIA through practice management software and claims clearinghouses, or 'switches.' These data are sourced from the Centers for Medicaid and Medicare Services-1500 form-based or electronic data interchange 837p claim transactions, the standard reimbursement form for all non-cash claims. Medical claims contain patient-level diagnoses, procedures performed, tests ordered, and drugs prescribed during visits to U.S. office-based healthcare professionals, ambulatory, and general healthcare sites, as well as hospitals and skilled nursing facilities. Approximately 96% of licensed physicians, documented by the American Medical Association, are captured.

Our sample includes patients with at least one mental health- or substance use-related diagnosis from 2015 to 2022. We focus on this sample as the IQVIA data are very costly to use and including a sub-set of claims most relevant for our study made accessing these data financially feasible. A limitation of this sample is that we are not able to study the effect of a PSL mandate on the probability that a patient receives a mental health- or substance use-related diagnoses, which is an interesting avenue for future research. See Table A1 for example diagnoses, a full list is available on request from the corresponding author. A concern with this sample is that PSL mandates, through greater healthcare use overall, could increase the probability that a person receives a mental health- or substance use-related diagnosis, and thus appear in our data. We do not believe that this concern is likely to lead to bias in our analysis because our outcome measures do not use the size of the treated population as a denominator and furthermore, patients receiving mental health or substance use treatment without any mental health or substance use diagnosis over an extended period of time is very uncommon (Toth et al., 2025). For example, major insurers (e.g., Medicare, Medicaid, and many commercial insurers) require a mental health or substance use diagnosis for reimbursement of the associated services. Diagnoses codes are used to show 'medical necessity' of the services, failure to include such diagnoses codes can lead to a denial of the claim, meaning that the provider will not be reimbursed.

We consider several measures of mental healthcare use in our analysis, measured at the

monthly level of each state unless otherwise noted. First, we examine overall mental healthcare use measures, including both the number of patients and the number of claims for any mental healthcare service delivered through dispensed prescription medications, outpatient visits, or inpatient visits. Second, we assess utilization by treatment setting. For medications, we calculate the number of prescription claims, the number of patients with a prescription, and the total number of prescription supply days. We also examine outpatient and inpatient services separately, reporting both the number of patients and the number of claims for each. Across our main analyses, we consistently consider both patient-level and claim-level measures to capture service utilization comprehensively. Tables A2 and A3 (column [1]) provide categories of treatment modalities. To isolate care received in outpatient and inpatient settings, we use information on the 'place of service' field provided in the IQVIA database that indicates the location in which the service is received. Table A3 (column [2]) lists the places of service we use to categorize outpatient and inpatient care.

The IQVIA data include information on patients which we will use to study potential heterogeneity in the effect of a state PSL mandate on mental healthcare utilization. We study three dimensions of heterogeneity: age (less than 18 years, 18 to 64 years, and 65 years and older), sex (male and female), and payer (commercial, Medicare, Medicaid, and other). Our claims data do not include any additional patient demographics.

Our outcomes display skewness, Figure A1 reports the raw count (divided by 1,000) of any mental healthcare claims (Panel A) and patients (Panel B) while Figure A2 reports logged versions of these outcomes, each figure has a normal distribution super-imposed. The logged variables are closer to normally distributed than the raw counts. Thus, we take the logarithm of our outcomes to account for this skewness, we will convert coefficient estimates to the percent change as follows: $(e^{\hat{\beta}} - 1) \times 100$. We will report results in Section 4.7 where we do not log the outcomes and results are not appreciably different. Further, there are no zero values in any of our (unlogged) variables. Therefore, taking the logarithm of these variables is reasonable (Chen and Roth, 2024).

## 3.2 Paid sick leave mandates

We use data on state PSL mandates from the National Partnership of Women & Families (2023) and A Better Balance (2025). For all state mandates, we use the date (month-year) on which the policy became effective (i.e., employers had to provide PSL benefits to eligible employees) to define whether or not a state has a policy in place in our analyses. More specifically, we do not use the enactment date – which typically proceeds the effective date by approximately three months in most states – of the mandate to define treatment status.

Although we do not consider city or county mandates in our main analyses, San Francisco California adopted the first PSL mandate in February 2007. The District of Columbia was the first state[6] to adopt a PSL mandate in May 2008 and Connecticut became the second state to implement such a policy in January 2012, and California and Massachusetts adopted PSL mandates in July 2015. By November 2025, 18 states and the District of Columbia had such a mandate in place or had announced a mandate to be adopted in the future. Figure 1 reports the geographic distribution of PSL mandates across the U.S. Figure A3 shows the temporal variation. Notably, state PSL mandates display staggering in terms of rollout, this feature of our identifying variation shapes our empirical approach, which we outline in Section 3. Table 1 lists the effective month and year for each state and the District of Columbia that adopts or announces a PSL mandate by November 2025, and the number of employees made newly eligible for PSL as a result of these mandates by the end of 2023 (National Partnership of Women & Families, 2023).

Notably, states implemented PSL mandates before, during, and after our study period. This policy rollout has implications for the states that offer variation in our analysis. The District of Columbia and Connecticut are excluded from our analysis sample as these states adopt a PSL mandate prior to the first year of our study – 2008 and 2012, respectively – and are thus 'always treated,' and are inappropriate controls in our difference-in-differences approach (described in Section 3.4). California (2015), Massachusetts (2015), Oregon (2016), Arizona (2017), Vermont (2017), Maryland (2018), New Jersey (2018), Rhode Island (2018), Washington (2018), Colorado (2021), New York (2021), and New Mexico (2022) implemented mandates during the study period while Minnesota (2024), Alaska (2025), Michigan (2025), Missouri (2025), and Nebraska (2025) implemented mandates after 2022, the last year of our study period. States that adopted policies during our study period offer variation used to identify coefficient estimates in both our difference-in-differences regression and event-studies (described in Section 3.4). We will code states that implement PSL mandates after our study period (i.e., 2023-2025) as in their pre-treatment period in the event-study – for example, Minnesota, which adopts a policy in 2024, will be coded as -2 in 2022 and -3 in 2021. As we will discuss in Section 4.2, our data are somewhat imbalanced in event-time, however, we will explore the implications of this data feature for our results.

We focus our analysis on state and not sub-state PSL mandates. The rationale for this decision is that what is important for PSL mandates is the location of employment while our data record the location in which treatment is received or a prescription is dispensed. Roughly 97% of employed adults live and work in the same state, but only 75% live and work in the same county (Ruggles et al., 2023). Thus, relying on the state is less likely

[6]The District of Columbia is technically a district and not a state.

to induce measurement error into the data. Further, 51%, 37%, and 67% of counties have no psychiatrists, psychologists, and psychiatric nurse practitioners (Andrilla et al., 2018), suggesting that many patients will receive care outside their county of employment, further leading to concerns about using sub-state mandates in our analysis. However, we will show that our results are not different if we incorporate sub-state mandates (Section 4.7).

States have adopted other paid leave policies. For example, 13 states and the District of Columbia have paid medical and family leave policies (National Partnership for Women & Families, 2024). These state-run programs offer eligible employees longer-term leave for illness or injury recovery, and bounding with a new child. Such policies provide substantially more leave than PSL mandates – for example, the California policy provides up to six weeks of leave, and likely impacts mental healthcare use through differential channels than PSL mandates. Nonetheless, we will control for state medical and family leave policies in our regressions. Five states – California, Hawaii, New Jersey, New York, and Rhode Island – adopted Temporary Disability Insurance (TDI) in the 1940s and 1960s, these policies are time-invariant over our study period (Pichler and Ziebarth, 2024).

Four states (Illinois, Maine, Michigan, and Nevada) have adopted paid time off mandates, though only three of these mandates are adopted during our study period (National Partnership of Women & Families, 2023).[7] These mandates provide financially protected time away from work regardless of purpose, and thus could be a substitute for PSL. However, legal scholars differentiate paid time off mandates from PSL mandates as the former do not provide explicit worker protection in the state statutes. For example, PSL mandates prohibit employers from retaliating against workers for using leave, do not require workers to locate a 'replacement' worker, and wave the requirement that employees request permission to use leave in advance for emergencies, among other factors.[8] Given these differences, we treat PSL mandates and paid time off mandates as different policies, though we will show in Section 4.7 that our findings are not different if we instead expand our definition of PSL mandates to include paid time off mandates (i.e., code a state as having a PSL mandate if that state has a PSL mandate or a paid time off mandate) or exclude states that adopt paid time off mandates from the analysis sample. Notably, Michigan's paid time off mandate transitioned to a PSL mandate in February 2025.[9]

[7]One county and one city have also adopted paid time off mandates: Bernalillo, New Mexico and West Hollywood, California.

[8]We note that U.S states, with Montana being an exception, are 'employment at will' states, that is employers can generally fire employees as long as the reason is not illegal. Such an employment landscape could open the door to firings based on PSL use in mandating states, but that would be illegal. Furthermore, this legal issue is present for all labor market policies, not simply state PSL mandates.

[9]The state originally adopted a PSL mandate, to be effective in March 2019, but the state legislature removed several components of the law, thus leading to the National Partnership of Women & Families

## 3.3 Other data sources

We examine 'first-stage' effects, that is the effect of a state PSL mandate on PSL access and use. To do so, we turn to the National Compensation Survey (NCS) 2009-2022. The NCS is a nationally representative dataset of establishments that is coordinated by the Bureau of Labor Statistics (BLS). The NCS is used by the federal government to provide statistics on employer benefits and labor costs, and is used to adjust the wages of federal employees. The unit of observation in the NCS is a job in an establishment in a state in a year. Establishments and jobs are sampled probabilistically within regions and establishments. A human resources manager at each establishment works with a BLS economist to complete the survey. We construct two measures: i) the probability that a job has PSL as a benefit and ii) the average quarterly hours of PSL used following Maclean et al. (2025), in particular we use quarter one of the focal year and quarters two, three, and four of the preceding year, using a four-quarter average minimizes concerns about seasonability in need for PSL (e.g., influenza season). We weight the data by BLS-provided weights.

We examine changes in mental health following state PSL mandate implementation using the 2015-2022 Behavioral Risk Factor Surveillance Survey (BRFSS) which is maintained by the Centers for Disease Control and Prevention (CDC). Each year over 400,000 respondents 18 years and over are asked a range of questions regarding health status, health behaviors, and preventive healthcare use (the BRFSS does not include information on mental healthcare use). We focus on all adults 18 years and older. The BRFSS asks respondents to indicate how many days in the last 30 they were in 'bad mental health.' We use this information to construct the count of the number of such days. We note that this measure is subjective and there could be unobservable heterogeneity across respondents in interpretation of the question, but we view this metric as offering a summary of general mental well-being, at least as perceived by individuals. Data are weighted by BRFSS-provided weights.

## 3.4 Methods

We rely on difference-in-differences and event-study methods to test the impact of state PSL mandates on mental healthcare outcomes. As outlined in Section 2, PSL mandates have rolled out across U.S. states at different points in time, which can lead to bias in standard two-way fixed effects (TWFE) difference-in-differences methods. More specifically, TWFE uses all possible two-by-two difference-in-differences contrasts available in the data. This approach will lead to some reasonable contrasts (i.e., comparing a state that adopts a PSL

(2023) to classify this policy as a paid time off mandate. After a series of law suits, in November 2024 the state's Governor re-instated the 2019 law – a PSL mandate – with an effective date of February 2025. See Ballotpedia (2025) for a summary.

mandate to a state that never adopts a PSL mandate, and comparing a state treated earier in the panel to a state that is treated later in the sample) and some unreasonable contrasts (i.e., comparing a later treated unit to a unit treated earlier). Further, for efficiency, TWFE variance weights the data such that states treated in the middle of the panel (i.e., 2018 in our analysis) will receive disproportionate weight in the estimation of the average treatment effect on the treated (ATT). For these reasons, we chose to use an imputation estimator that is not vulnerable to these sources of bias. In particular, we use the two-stage estimator proposed by Gardner (2022). In the first stage (using not-yet-treated and never-treated units), the procedure estimates the relationships between the included covariates and the treatment variable. This information is then used to impute the 'missing' counterfactual for the treated group post-treatment. In the second stage, the estimator takes the difference between the average (realized) outcomes in the treated state and the (imputed) outcomes in the untreated state for the treated units to construct an estimate of the ATT. Standard errors account for both within-state clustering and the counterfactual imputation (Butts and Gardner, 2021; Gardner, 2022). Equations 1 and 2 outline this procedure:

$$MH_{st}(0) = \alpha_s + \gamma_t + \boldsymbol{X}_{st}\boldsymbol{\beta} + \epsilon_{st}, \tag{1}$$

$$MH_{st}(1) - \widehat{MH}_{st}(0) = \delta PSL_{s,t} + \mu_{st}, \tag{2}$$

$MH_{st}(1)$ and $MH_{st}(0)$ are mental healthcare outcomes in the (realized) treated and (imputed) untreated state in state $s$ and time-period (month-year) $t$. As discussed in Section 3.1, we log our outcomes to account for skewness. $X_{st}$ includes state-level time-varying covariates: paid medical and family leave policies, paid time off mandates, Temporary Aid to Needy Families monthly benefit for a family of four (\$),[10] demographics (age, sex, race, ethnicity, and education), and population (National Partnership of Women & Families, 2023; National Partnership for Women & Families, 2024; University of Kentucky Center for Poverty Research, 2025; Flood et al., 2024).[11] Alpha is a vector of state fixed effects and gamma is a vector of time (month-year) fixed effects. We weight the data by the state population though we will report unweighted results in Section 4.7. Standard errors account for both the counterfactual imputation and correlation within state and over time.

While we rely on the Gardner (2022) approach in our main analyses, we note that – to the best of our knowledge – the literature has not reached consensus on the most appropriate estimator, and the most appropriate estimator likely varies across research settings. There

[10] We inflate monthly benefits to 2022 dollars using the Consumer Price Index – Urban Consumers.

[11] Paid medical and family leave, Temporary Aid to Needy Families, and population information varies at the year-state level while all other state-level controls vary at the state-year-month level.

is, broadly, consensus that difference-in-differences estimators which prohibit 'forbidden' comparisons – that is comparing units treated later in the sample to those treated earlier in the sample – are more suitable than procedures that allow such comparisons (e.g., TWFE). The Gardner (2022) procedure falls into the class of estimators that prohibits such contrasts. We select the Gardner (2022) difference-in-differences estimator as this procedure has several attractive features for our study. First, as described above, coefficient estimates obtained using the Gardner (2022) procedure are not vulnerable to bias from a staggered treatment rollout when there are dynamics and heterogeneity in treatment effects (Goodman-Bacon, 2021). The Gardner (2022) procedure also avoids bias when treatment effect heterogeneity is correlated with the included time-varying covariates (Caetano et al., 2022; Powell, 2021). For example, previous research documents spillovers across social insurance programs – e.g., Duggan et al. (2007) and Dong et al. (2024) – and thus we might expect that the effect of PSL mandates could vary with features of the broader policy landscape or potentially state-level demographics – e.g., age of the population. The approach performs well in terms of inference relative to other 'new' difference-in-differences estimators (Gardner et al., 2024; Mizushima and Powell, 2025), suggesting that we minimize concerns around both over- and under-rejecting the null hypothesis by using this estimator. Finally, the Gardner (2022) procedure relies on regression, which is a concept familiar to many micro-economists, and is computationally efficient (i.e., low run-time). On the other hand, there are some limitations to using the Gardner (2022). For example, because the procedure relies on linear regression, we must assume that the linear regression we use to establish the counterfactuals in the first stage is correctly specified — other estimators such as the Callaway and Sant'Anna (2021) procedure do not impose this assumption, if the regression is mis-specified, then our resulting coefficient estimates could be biased. However, we will show in Section 4.3 that our results are not appreciably different if we use alternative difference-in-differences estimators – including Callaway and Sant'Anna (2021), and or change the covariates we include in the Gardner (2022) procedure which allows us to assess stability of the counterfactual imputation with different (albeit linear) specifications.

## 3.5 Summary statistics and trends

Table 2 reports summary statistics of our mental healthcare outcomes (scaled by 1,000 for readability) and state-level variables for three groups of states: i) all states (measured in all years), ii) states that adopt or announce a PSL mandate by November, 2025 (measured prior to mandate adoption), and iii) states that do not adopt or announce a PSL mandate by November 2025 (measured in all years). Table A4 reports the same statistics, but excludes

the COVID-19 period (i.e., we drop data from March, 2020 to December, 2022 from the sample). All measures of mental healthcare use are higher in states that do not adopt a PSL mandate than in states that do. For example, the average overall (across all modalities) number of mental healthcare claims is 1,592 in states that later adopt a PSL mandate and 1,715 in states that do not. Baseline inpatient use is low relative to outpatient. In mandate enacting states prior to implementation, the average state-month has 35 inpatient claims and eight inpatient patients, compared with 402 outpatient claims and 175 outpatient patients. Paid medical and family leave policies are only adopted by states that also implement a PSL mandate while paid time off mandates are implemented by non-PSL mandating adopting states only during our study period. Monthly Temporary Aid to Needy Families for a family of four is $787 in PSL mandate adopting states and $485 in other states. Demographics are fairly similar across the two groups of states.

In Figure 2, we report trends in overall mental healthcare, dispensed prescriptions, outpatient care, and inpatient care, but we stratify the sample based on adoption/announcement of a state PSL mandate by November, 2025 (Figure A4 reports trends in these outcomes over our study period, we log outcomes as we do in our regression analyses).We center the data around the year in which the mandate became effective (i.e., employers were required to provide these benefits to employees) for states that implement this policy. For states that do not adopt a PSL mandate, we assign the median period (month-year) of implementation among states that do adopt the data (October, 2018) and center the data around this 'placebo' effective period. At the national level, all outcomes are trending upward over the study period, though increases are more modest for prescriptions than for other metrics. We observe limited evidence that trends depart for our measures of any mental healthcare use at in the effective period. However, trends in prescriptions appear to decline modestly for states that adopt a PSL mandate once the policy is implemented relative to states that do not adopt this policy. Contrawise, trends in outpatient and inpatient episodes rise in mandate-adopting states relative to other states.

# 4 Results

## 4.1 First-stage effects

Table 3 reports first-stage results using the NCS data, event-studies are reported in Figure A5. Here we find that, following implementation of a state PSL mandate, there is a 12.2 percentage point (ppt) and 2.3 hour increase in the probability that a job has PSL and average quarterly PSL use, respectively. Comparing the coefficient estimates to the

pre-mandate mean in PSL mandate adopting states implies a 16.9% increase in access to PSL and a 9.8% increase in average quarterly PSL use.

We note that these findings are in line with, but smaller in magnitude than, Maclean et al. (2025). We suspect that the main difference leading to differing findings is that Maclean et al. (2025) include only private jobs in their analysis while we include both private and government jobs as we do not isolate private employees in the main analysis of the IQVIA data. Government jobs are more likely than private jobs – absent mandates – to offer PSL to employees and including government jobs likely leads to more modest estimated effect sizes. With these findings in hand, we turn to our main research question, the effect of state PSL mandates on mental healthcare utilization.

## 4.2 Mental healthcare utilization: Extensive margin

Figure 3 reports event-study results for our mental healthcare measures – any mental health treatments, prescriptions, outpatient episodes, and inpatient episodes. Prior to mandate adoption, there is no observable difference in trends for states that will and will not adopt a mandate. This pattern of null findings supports the hypothesis that our data can satisfy the parallel trends assumption. Examination of the policy lags shows some evidence that overall claims (but not patients) increase roughly two years-post mandate. Prescriptions decline following PSL mandate adoption, while our metrics for both outpatient and inpatient episodes increase. The modest time delay in PSL mandate effects is reasonable as employees must first learn about and earn PSL prior to using these benefits, and PSL may be used for mental healthcare – if more discretionary than other forms of healthcare (Frank and McGuire, 2000) – after other physical healthcare.[12]

A concern with the results reported in Figure 3 is that – due to our study period (2015-2022) and when states adopted PSL mandates – different states contribute to identification of each event-time indicator. Put differently, the sample is imbalanced in event-time. Table A5 reports the states that contribute identifying variation to each event-time indicator. We next estimate the event-study on a balanced sample of states. That is, we include only treatment states that are observed in all event-time indicators, we make no changes to the untreated (non-PSL mandate adopting) states. In this exercise, we attempt to maximize

[12]Under standard difference-in-differences assumptions (i.e., no anticipation and parallel trends in outcomes), the pre-treatment causal parameters are zero, so lead coefficients estimates should be small in magnitude and hover close to zero, straying from zero due only to sampling error, and associated confidence intervals should be tight as the only variance should be due to sampling error (de Chaisemartin and d'Haultfoeuille, 2024). However, in the post-treatment period, the coefficient estimates and standard errors capture variance in the data (i.e., the sampling error flagged above) and treatment effects, which may be heterogeneous across treated units or over time since the event (Callaway and Sant'Anna, 2021). Thus, the standard errors can be larger in the post-period than in the pre-period, which is what we observe.

both the number of leads and lags we include, and the number of treatment states, with the result that we use three lead variables (-3 through -1) and the year of the event. We exclude all event-time indicators more than three years prior to the event and those more than one year post-event. Results are reported in Figure A8. Given the length of this event-study and our expectation that mandate effects will increase over time as employees learn about and earn benefits, form relationships with mental healthcare professionals, and so forth, we focus our attention on the pre-period. We observe no evidence of differential pre-trends and some evidence that inpatient and outpatient episodes rise following mandate adoption. Next, we estimate a 'leave-one-out' analysis in which we sequentially exclude each treatment state to examine whether any specific state(s) drive our findings. Results are reported in Figures A9, A10, A11, and A12. We do not find evidence that any one state(s) is driving the effects.

Table 4 reports difference-in-differences results for any mental healthcare use, prescriptions, outpatient episodes, and inpatient episodes, respectively. We observe no statistically significant evidence that our measures of any mental healthcare use (claims and patients) change following a state PSL mandate adoption. However, this overall null finding masks heterogeneity across treatment setting, in particular, we observe a shift away from less time intensive and towards more time intensive care as time-based barriers are relaxed following mandate adoption. As discussed in Section 3, we convert coefficient estimates to the percent change as follows: $(e^{\hat{\beta}}-1)\times 100$. We see a 3.5% to 3.9% reduction in prescriptions – arguably the least time-intensive setting we consider – post-mandate. Alternatively, outpatient and inpatient treatment metrics (claims and patients) increase by 22.4% to 24.7% and 15.5% to 25.6%, respectively, after adoption of a state PSL mandate. The results are situated in the context that the majority of patients receiving outpatient mental health services also use medication treatment, consistent with prior evidence from the Medical Expenditure Panel Survey (MEPS) reported by Cronin et al. (2025). In our data, 72.3% of patients with an outpatient mental health visit have a medication prescription during the study period, while 27.7% do not receive any such prescriptions.

The finding that inpatient care increases post-PSL mandate potentially warrants some attention. Thinking through the extent to which mandated PSL benefits (which confer, on average, seven days per year for employees) should increase inpatient care is important. While some inpatient mental healthcare can be of quite long duration (e.g., 60 or 90 day stays in residential treatment centers), some care is of much shorter duration, for example, the average duration of inpatient hospitalization is three to seven days (Schiffer, 2023), suggesting that PSL mandates could reasonably increase inpatient care. Moreover, unused days can be rolled over to the following year (see Section 2.2) and employees could use mandated PSL to support their dependents' (who may not be employed) receipt of mental

healthcare in inpatient settings. PSL may also serve as a 'bridge' to longer-term leave such as disability. For example, a workers could use mandated PSL while they apply for additional leave or during mandatory waiting periods for longer-term leave. Finally, more frequent interactions with healthcare professionals generally may lead to patients being made aware of the seriousness of their condition and a recommendation that inpatient care is needed.

We note that relative effects for each treatment modality (any, prescriptions, outpatient, and inpatient) are larger for patient vs. claim counts. We have bootstrapped the difference between the claim and patient outcomes using a non-parametric bootstrap with 500 repetitions. The differences in the coefficient estimates are all statistically distinguishable from zero at the 1% level for any, prescriptions, and inpatient treatment, while the difference is imprecise for outpatient. The $p$-values are as follows: <0.000 for any treatment, <0.000 for prescriptions, 0.194 for outpatient, and <0.000 for inpatient.[13]

To dig deeper into the possibility that state PSL mandates reduce time-barriers which leads patients to shift from less time-intensive to more time-intensive treatment, we construct measures of mental healthcare that reflect a range of time requirements. We create seven patient-level mutually exclusive care patterns over six-month periods of time: i) prescriptions, outpatient care, and inpatient care; ii) no prescriptions, outpatient care, and inpatient care; iii) prescriptions, outpatient care, and no inpatient care; iv) no prescriptions, outpatient care, and no inpatient care; v) prescriptions, no outpatient, and inpatient care; vi) prescriptions, no outpatient care, and no inpatient care; and vii) no prescriptions, no outpatient care, and inpatient care. We choose a six-month window (rather than the monthly level in our main analysis) as patients may space out treatment, suggesting that one month may not be sufficient time to capture the salient treatment patterns that we seek to study. Results, reported in Table 5, support the hypothesis that patients shift toward more time-intensive care post-PSL mandate: the number of patients with medications and no outpatient and no inpatient care declines by 7.3% post-mandate while the number of patients with no prescriptions and outpatient but no inpatient care, no medications and inpatient but no outpatient care, and no medications and outpatient and inpatient care increase by 21.9%, 13.9%, and 22.0%, respectively; the number of patients with medications and outpatient but no inpatient care, medications and inpatient but no outpatient care, and medications, outpatient, and inpatient care increase by 19.1%, 1.2%, and 19.7%, respectively. More specifically, these results suggest that when time is contained, patients rely on less time-intensive care, but when time constraints are relaxed, patients are able to consume care that requires more time. Event-study results for these outcomes are reported in Figure 4, there is no evidence of differential pre-trends between states that will and will not adopt a PSL

[13]For prescriptions, we compare prescription and patient count outcomes.

mandate, which again supports the hypothesis that the data can satisfy parallel trends.

In Figure 5, we report heterogeneity in PSL mandate effects across age, sex, and payer. We might expect heterogeneity in mandate effects across these groups. For example, women are more likely to use mental healthcare then men and are less likely to work in jobs that offer PSL when employers are not mandated to do so. Moreover, patients with different insurance statuses may have heterogeneous cost-sharing for mental healthcare services, and (full) cost reduction of using mental healthcare post-PSL mandate may be more or less salient to these groups of patients.[14] For brevity, we use patient counts for each of our treatment setting variables (any, prescriptions, outpatient, and inpatient). While there are differences in magnitude, mental healthcare use appears to respond similarly to PSL mandate adoption across the groups we consider. However, some notable differences in response to the policy change emerge. First, the number of children with any mental healthcare increases post-mandate while the number of Medicare patients declines. The finding that children's use of mental healthcare increases on the extensive margin suggests that parents allocate newly available PSL benefits to their children's mental health. Additionally, children may be more likely to receive a diagnosis for a mental health disorder in any given healthcare encounter than adults, which might suggest that PSL-induced general healthcare could increase diagnoses and, in turn, elevated mental healthcare use. Earlier work shows that elder care increases following adoption of a state PSL mandate (Arora and Wolf, 2024; Guo and Peng, 2024), therefore, the decline in Medicare mental healthcare use may be partially driven by more engagement with adult children among Medicare beneficiaries. Second, the number of patients with mental health prescriptions covered by Medicaid or other sources does not appear to decline post-state PSL mandate.

The null finding for Medicaid stands in contrast to Maclean et al. (2024) who show that aggregate counts of dispensed prescription medications increase post-PSL mandate, and deserves some attention. While we lack the data to fully reconcile the two findings, we can raise some hypotheses. The increase documented in Maclean et al. (2024) could be driven by patients without a specific mental health or substance use diagnosis, our sample includes patients who have a claim with such a diagnosis at some point 2015-2022. Moreover, the 95% confidence interval ranges from -11.9 to +7.4% for our Medicaid findings, thus this range includes increases in dispensed prescription medications similar to those reported in Maclean et al. (2024) – 7.0%, thus the two sets of results are potentially not in conflict and

[14]Notably, some patients – either through their insurance plan or via an employee assistance program – may be eligible for some mental healthcare (e.g., a pre-specified number of counseling visits each year) with no cost-sharing. See, for example, https://www.fickewirth.com/the-loop-detail.php?Employee-Assistance-Program-vs.-Behavioral-Health-Coverage-197=&utm_source=chatgpt.com, website last accessed 7/2/2025.

may instead reflect noise in the IQVIA data. Finally, while IQVIA offers more comprehensive coverage of dispensed prescription medications than other insurance claims databases, these data likely miss a non-trivial amount of Medicaid prescriptions. In particular, IQVIA has somewhat limited coverage of state-specific systems, pharmacies that do not participate in Medicaid programs, and manufacturer-sponsored patient assistance or rebate programs. Further, because Medicaid programs are state-run and some states have limited data-sharing agreements with national data aggregators such as IQVIA, some Medicaid-financed medications are not reported to IQVIA. The State Drug Utilization Database employed by Maclean et al. (2024) is not subject to these concerns and potentially provides a more complete picture of Medicaid prescriptions than does the IQVIA database.

Over our study period, several states adopted paid medical and family leave policies. These policies provide workers with longer-term leave that can be used for own disability, disability of a dependent, and bonding with a new child. As noted above, mandated PSL could serve as a 'bridge' for workers as they apply for paid medical and family leave through state agencies. That is, workers may use mandated PSL as they wait for paid medical and family leave or wait for benefits to be awarded as some state programs have waiting periods (National Partnership for Women & Families, 2024). To explore this possibility, we construct a measure that takes on a value of one if state has both a PSL mandate and a paid medical and family leave policy in place, and zero otherwise. We do not control for paid medical and family leave policies in this specification (due to collinearity).

We also consider possible interactions between PSL mandates and tele-medicine. In particular, tele-medicine may be a complement to or substitute for PSL mandate for patients seeking mental healthcare. For example, mandated PSL may allow workers to take an hour or two away from work for an online mental healthcare session with a psychologist, suggesting that PSL mandates and tele-medicine are complements. Alternatively, the ability to receive tele-medicine might substitute for PSL, that is a worker can, perhaps, use a lunch break for an online counseling session rather than taking time off work. To explore this possibility, we have constructed measures of mental healthcare use that involve tele-medicine. More specifically, we focus on outpatient visits, where tele-medicine care is feasible, and separately examine outpatient visits where tele-medicine is recorded on the claim and outpatient visits that do not appear to involve tele-medicine (Table A6). We define tele-medicine visits using procedure and modifier codes (Table A7 and Table A8). While we observe increases for both types of care, the relative effect size is substantially larger for tele-medicine visits, the coefficient estimate reflects a doubling of the number of tele-medicine visits. While not conclusive evidence, our findings suggest that mandated PSL and tele-medicine are complements for mental healthcare patients.

## 4.3 Alternative difference-in-differences estimators

In our main analyses, we rely on the Gardner (2022) estimator, Section 3.4 lists our rationale. We next show robustness to using alternative difference-in-differences estimators that are not vulnerable to bias associated with a staggered policy rollout and traditional TWFE. We consider four estimators used in the economics literature: Callaway and Sant'Anna (2021), Wooldridge (2023), Borusyak et al. (2024), and TWFE.[15] Results are reported in Tables A9 (any mental healthcare), A10 (dispensed prescription medications), A11 (outpatient care), A12 (inpatient care), and A13 (substitution measures).

While not identical, the results are quite similar across the alternative approaches. We do find evidence when using the Borusyak et al. (2024) estimator and TWFE that the extensive margin of mental healthcare may increase post-PSL mandate, at least as measured by the number of claims (the coefficient estimate for patients is imprecise). Notably, coefficient estimates generated in TWFE specifications are generally smaller in size and less likely to rise to the level of statistical significance relative to other estimators.

## 4.4 Patient demographics

An important idea to consider when evaluating the effect of PSL mandates on mental healthcare use is the 'marginal' worker (or dependent) seeking treatment. The estimated treatment effects reported thus far combine impacts for patients who previously had a diagnosis and for those patients who obtained a diagnoses and treatment after adoption of the PSL mandate. We next examine whether and in what ways the demographics of patients with diagnosis records have shifted after the mandate is adopted.

Our claims data include limited demographic information on patients. Given the available information we have at hand, we examine changes in i) average patient age, ii) share of patients who are female, and iii) share of patients who reside in rural (vs. urban) areas (i.e., non-metropolitan vs. metropolitan counties) – here we use the U.S. Department of Agriculture (2024) definition. Results from this analysis are reported in Table A14. We observe that the average age of patients declines by roughly six months (1.0%) and patients are more likely to be male (0.3 ppts or 0.5%) post-mandate, but there is no change in the share of patients who reside in rural areas. These findings are useful for interpreting our main findings on mental healthcare utilization.

[15] We use *csdid* written by Rios-Avila et al. (2021), *wooldid* written by Hegland (2023), and *didimputation* written by Borusyak (2023). To estimate TWFE, we use a procedure (*reghdfe*) developed by Correia (2016).

## 4.5 Mental health outcomes

We have documented that state PSL mandates do not appear to impact the extensive margin of mental healthcare use, but — for patients in treatment — relaxing a time constraint allows these patients to transition from a lower level of care (mainly relying on prescription medications) to a higher level of care (using both prescriptions and outpatient/inpatient care). Our findings hint that, for these 'incumbent' patients, management of mental health outcomes may improve as patients require fewer prescriptions when they are better able to access (more time intensive) outpatient (e.g., talk therapy) and inpatient care. We next more directly study that possibility by examining mental health outcomes in the BRFSS. We report difference-in-differences results in Table 6 and event-study results in Figure 6.

Our findings suggest that the number of days reporting bad mental health in the last 30 days declines by 0.28 days or 7.3% relative to the baseline, though our findings are driven by non-elderly adults (18-64 years of age). These results are in line with those reported by Slopen (2023) which shows that women experience better mental health post-mandate. We note that any improvements in mental health may be driven by the changes in mental healthcare service use we document and other factors. For example, state PSL mandates may increase employment outcomes and economic standing (Slopen, 2024; Maclean et al., 2025). Additionally, PSL mandates reduce the spread of infectious diseases (Pichler et al., 2021), which could have positive spillovers to mental health.

## 4.6 Substance use disorder treatment

While our paper focuses primarily on mental healthcare use, given the substantial co-morbidities across mental health and substance use disorders – approximately half of all individuals diagnosed with a mental health disorder will experience a substance use disorder in their lifetime and vice-versa (Ross and Peselow, 2012; Kelly and Daley, 2013), we also study substance use disorder treatment receipt. Similar to our primary mental healthcare outcomes, we examine several metrics: i) any use (claims and patients), ii) dispensed prescription medications (prescriptions, patients, and prescription days), iii) outpatient services (claims and patients), and iv) inpatient services (claims and patients). Table A1 lists the codes that we use to create these metrics.

Tables A15, A16, A17, A18, and A19 report results for substance use disorder treatment metrics. Event-studies are reported in Figures A7 and A13. We observe no evidence of differential pre-trends between states that do and do not adopt a PSL mandate.

Interestingly, we observe a slightly different pattern of results for substance use disorder treatment. In particular, there appears to be an increase in the extensive margin of care:

post-PSL mandate there is a 18.8% increase in patients receiving substance use disorder treatment and no change in dispensed prescription medications. Patient counts for both outpatient and inpatient both increase post-PSL – by 25.7% and 22.1% respectively, but only inpatient claim counts increase (22.5%). Similar to mental healthcare, we see declines in the least intensive treatment pattern (no outpatient/inpatient and medications) and increases in more time-intensive patterns (i.e., outpatient/inpatient without medications).

## 4.7 Robustness checking

We conduct a number of robustness checks on our main mental healthcare outcomes (any use, prescriptions, outpatient, and inpatient) using different specifications, samples, and estimators. For brevity, we focus on the number of patients with any mental healthcare use, prescriptions, outpatient treatment, and inpatient treatment.

In Figure A14 we report results from different specifications and samples. First, we change the set of included controls: i) exclude time-varying covariates, ii) replace time fixed effects with region-by-time fixed effects (using the four U.S. regions), and iii) include time-varying economic controls (unemployment rate, poverty rate, and effective minimum wage (University of Kentucky Center for Poverty Research, 2025)) and proxies for tele-medicine availability (state-level indicators for tele-medicine coverage and reimbursement in fully insured commercial and Medicaid plans).[16] Controlling for tele-medicine availability is important as use of these services increased sharply during the COVID-19 pandemic and remained common in the post-pandemic period (Friedman et al., 2022).

Second, we use unweighted regression. Third, we vary how we control for PSL mandates: i) lag the mandate one year, ii) include an indicator variable taking on a value of one if there is a sub-state PSL mandate in the state-time period (National Partnership of Women & Families, 2023), and iii) control for bordering a state with a PSL mandate. We drop the South region as no states in this region have adopted or announced a PSL mandate by the end of our study period. We also drop three states that adopt paid time off mandates during our study period (i.e., Maine, Michigan, and Nevada) and we expand our definition of PSL mandates to also include paid time off mandates.

In the final panel of Figure A14 we report results when we exclude the pandemic period (i.e., March, 2020 onward). Here, we see somewhat stronger evidence that mental healthcare use overall increases, but less evidence of a decline in dispensed medications. While we lack the data to fully explore this issue, the somewhat differential findings could suggest that the pandemic altered the relationship between PSL mandates and mental healthcare use or

[16]The effective minimum wage is the higher value of the state or federal minimum wage. We inflate the effective minimum wage to 2022 dollars using the Consumer Price Index-Urban Consumers.

heterogeneity across states that adopt PSL mandates before and after the pandemic period. Better understanding this relationship is an interesting avenue for future research.

In Figure A15 panel A we report results in which we do not log the outcome. Results are similar, though we lose precision for the dispensed prescription medication coefficient estimate. In Figure A15 panel B, we report results using a Poisson difference-in-differences specification that is robust to bias associated with a staggered treatment rollout when treatment effects are dynamic or heterogeneous (Wooldridge, 2023; Hegland, 2023), and in panel C we take the inverse hyperbolic sign of our outcomes, again using the Gardner (2022) procedure. In the Poisson specification, estimates of treatment effects are reported as differences in linear predictions. Findings are very similar to our main results, which suggests that accounting for the skewed nature of our outcomes – either through taking the logarithm/inverse hyperbolic sign or using a Poisson – offers a gain in terms of statistical precision.

Figures A16 and A17 report regression results in which we sequentially exclude each treated state and each sample year (i.e., 'leave-one-out' analyses), respectively. The objective of this exercise is to explore the extent to which our findings may be driven by specific PSL mandate-adopting states or years. Results are qualitatively similar across the leave-one-out samples, though when we drop New York, while the coefficient estimates are generally similar to our findings based on the full sample, the standard errors increase. We suspect that this pattern of results is likely attributable to the way in which the Gardner (2022) estimates the average treatment effect on the treated (ATT). That is, in the first stage, the estimator uses only untreated observations (not-yet-treated and never treated) to estimate the imputed counterfactual that will be used in the second stage (see Equations 1 and 2). New York is treated late in study period (January, 2021, see Table 1), thus this state contributes a relatively large share of the not-yet-treated data. Removing this state appears to reduce the precision of our first stage prediction. For example, in Figure A18, we use the Callaway and Sant'Anna (2021) to estimate the state leave-one-out analysis and we do not observe the drop in precision when we exclude New York. The Callaway and Sant'Anna (2021) procedure estimates the ATT for each 'group' of states that adopt a PSL mandate in the same time period ('GATTs'), and then constructs and overall estimate of the ATT from the GATTs, thus this approach differs from the Gardner (2022) approach in terms of using pre-treatment data to impute untreated counterfactuals.

Finally, we test for spillover effects, that is, we explore whether states that border PSL mandate-adopting states also gain in terms of better ability to access mental healthcare treatment. A non-trivial number of employees work outside their state of residence, suggesting that focusing only on treated states may miss some of the mandate impacts. Additionally, this analysis allows us to assess whether our data violate the Stable Unit Treatment Value

Assumption (SUTVA) assumption required for difference-in-differences methods to recover causal estimates. To explore possible spillovers and SUTVA violations, we first drop all states that adopt a PSL mandate by 2022. We then re-define our treatment variable as bordering a state with a PSL mandate and estimate Equations 1 and 2, similar to our exclusion of Connecticut and DC, we exclude 'always treated' PSL-mandate border states in this analysis. We report results in Figure A19. We observe no evidence of spillovers or SUTVA violations: no coefficient estimate rises to the level of statistical significance.

# 5 Discussion and conclusion

Healthcare requires both time and financial resources (Grossman, 1972). Mental healthcare, in particular, is time-intensive. We study the effect of recent state policies that plausibly relax time constraints for mental healthcare patients: mandates that compel employers to provide employees with roughly seven days of PSL per year, and this leave can be used for a wide range of health-related activities, including receiving mental healthcare.

We have several findings. First, we confirm earlier work and show that employees experience meaningful gains in both access to and use of PSL using detailed data on U.S. establishments. Second, our findings suggest that state PSL mandates may increase mental healthcare along the extensive margin for most groups of patients, we observe no change in the number of overall claims or patients. An exception to this pattern is children, which suggests that parents face time barriers in ensuring that their children receive healthcare, but PSL mandates relax this constraint. Furthermore, these findings are in line with earlier work documenting an increase in childcare provision among families post-PSL mandate (Maclean and Pabilonia, 2025) and correlational studies documenting that children whose parents have PSL benefits through their employer receive more mental healthcare than other children (Seixas and Macinko, 2020). Third, state PSL mandates lead to decreases in relatively low time-intensive treatment (prescriptions) and increases in more time-intensive treatment (outpatient and inpatient care). These findings suggest that, at least some, patients view more and less time-intensive mental healthcare as substitutes. That is, when the 'price' of time-intensive care declines following a state PSL mandates, some patients substitute towards more 'expensive' time-intensive care. To the best of our knowledge, there is scant literature exploring such substitution patterns. Our results are notably congruent with recent research which finds that individuals increase their use of psychotherapy when faced with lower time costs of using care (Cronin et al., 2025). Finally, using survey data, we show that self-reported mental health improves following adoption of a state PSL mandate.

The increase we observe for children in the extensive margin of any mental health care

(Figure 5) is consistent with a mechanism in which parents use state mandated PSL to start or sustain treatment for a child, which might be particularly important given the high rates of mental health disorders among children (Haidt, 2024; Currie, 2025). The ability of parents, following a PSL mandate, to support their children's use of mental healthcare can potentially allow treatment for such conditions. State PSL mandates allow leave to be used for dependents and typically provide about seven fully paid days per year, a format that fits the short, recurring time required for evaluations and weekly psychotherapy sessions. In contrast, state paid medical and family leave programs are designed for longer blocks of time with partial wage replacement, which may be a poor fit when a child needs one to two hours per week rather than weeks of dedicated care time as is offered by paid medical and family leave policies. Prior work documents that state PSL mandates increase caregiving to children and older adults and that lack of PSL among parents is a barrier to children's nonemergency care (Byker et al., 2023; Arora and Wolf, 2024; Maclean and Pabilonia, 2025; Seixas and Macinko, 2020). Our findings also hint that some parents may prioritize their children's receipt of mental healthcare over their own as we do not observe a corresponding increase in the extensive margin of mental healthcare use for other groups post-mandate.

Our study is subject to limitations. First, we use a sample of patients with any mental health or substance use disorder diagnosis over the entire 2015-2022 sample period. This focus allows us to examine patients 'at risk' for using mental healthcare services and to exclude some treatments that are not likely specifically mental healthcare (e.g., using an antidepressant to treat chronic pain or in smoking cessation efforts). However, our sample restriction prevents us from identifying some patients on the extensive margin of using care who may have used treatment but never have a recorded mental health or substance use disorder diagnosis. As such, our estimates of extensive margin responses should be interpreted with that limitation in mind. Second, while our prescription claims data cover roughly 94% of all retail pharmacies in the U.S., our medical claims data have lower coverage. Third, due to the timing of our data (2015-2022), we are not able to exploit variation from all state PSL mandates. Finally, our data are imbalanced in event-time, more specifically, in event-studies, coefficient estimates for time-to-event indicators are identified off different states.

Our coefficient estimates show a shift from prescription only management toward more time intensive services post-state PSL mandate. This mix likely increases medical spending in the near term. We do not observe clinical outcomes, symptom severity, school attendance, crises, hospitalizations, or caregiver productivity, so we do not conduct a formal cost/benefit analysis. Net budget and welfare effects would depend on whether added visits improve symptoms and reduce later acute events. A full accounting of those benefits and any downstream savings is an important topic for future work.

Inpatient psychiatric care typically addresses acute safety risks and severe symptom exacerbations (e.g., suicidality or self-harm risk, severe major depressive episodes, psychotic mania, severe eating disorders) and, in some cases, medically supervised withdrawal for substance use disorders. Given this case mix, even small changes in inpatient utilization may have large welfare effects if these changes improve timely stabilization and facilitate effective follow-up for patients. Our findings do not measure welfare directly, but the observed shift toward higher-intensity care alongside outpatient care under state PSL mandates is consistent with lower time costs enabling patients and caregivers to access appropriate inpatient services when clinically indicated.

In summary, we add new information on the impacts of state PSL mandates on mental healthcare utilization. Our findings suggest that these mandates allow incumbent patients to substitute to more intensive mental healthcare as time constraints decline, and these changes in treatment patterns may lead to improvements in preconceptions of mental health.

# References


A Better Balance (2025). Interactive overview of paid sick time laws in the United States.

Aguiar, M., Hurst, E., and Karabarbounis, L. (2013). Time use during the great recession. *American Economic Review*, 103(5):1664–1696.

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

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

Andrilla, C. H. A., Patterson, D. G., Garberson, L. A., Coulthard, C., and Larson, E. H. (2018). Geographic variation in the supply of selected behavioral health providers. *American Journal of Preventive Medicine*, 54(6):S199–S207.

Arora, K. and Wolf, D. A. (2024). Paid leave mandates and care for older parents. *The Milbank Quarterly*, 102(3):732–764.

Ballotpedia (2025). Michigan paid sick leave initiative (2018).

Borusyak, K. (2023). DID_IMPUTATION: Stata module to perform treatment effect estimation and pre-trend testing in event studies.

Borusyak, K., Jaravel, X., and Spiess, J. (2024). Revisiting event-study designs: Robust and efficient estimation. *Review of Economic Studies*, 91(6):3253–3285.

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

Byker, T., Patel, E., and Ramnath, S. (2023). Who cares? Paid sick leave mandates, caregiving, and gender. *National Tax Journal*, 76(3):649–677.

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

Callaway, B. and Sant'Anna, P. H. (2021). Difference-in-differences with multiple time periods. *Journal of Econometrics*, 225(2):200–230.

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

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

Chen, J. and Roth, J. (2024). Logs with zeros? Some problems and solutions. *The Quarterly Journal of Economics*, 139(2):891–936.

Coffey, R. M. (1983). The effect of time price on the demand for medical-care services. *Journal of Human Resources*, pages 407–424.

Correia, S. (2016). reghdfe: Estimating linear models with multi-way fixed effects. In *2016 Stata Conference*, number 24. Stata Users Group.

Cronin, C. J., Forsstrom, M. P., and Papageorge, N. W. (2025). What good are treatment effects without treatment? Mental health and the reluctance to use talk therapy. *The Review of Economic Studies*, 92(3):1699–1737.

Currie, J. (2025). Presidential address: Investing in children to address the child mental health crisis. *American Economic Review*, 115(5):1369–1399.

de Chaisemartin, C. and d'Haultfoeuille, X. (2024). Difference-in-differences estimators of intertemporal treatment effects. *Review of Economics and Statistics*, pages 1–45.

Dong, X., Maclean, J. C., and Powell, D. (2024). Social insurance spillovers: Evidence from paid sick leave mandates and Workers' Compensation. Technical report, National Bureau of Economic Research.

Duarte, F. (2012). Price elasticity of expenditure across health care services. *Journal of Health Economics*, 31(6):824–841.

Duggan, M., Singleton, P., and Song, J. (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.

Ellis, R. P., Martins, B., and Zhu, W. (2017). Health care demand elasticities by type of service. *Journal of Health Economics*, 55:232–243.

Flood, S., King, M., Rodgers, R., Ruggles, S., Warren, J. R., Backman, D., Chen, A., Cooper, G., Richards, S., Schouweiler, M., and Westberry, M. (2024). Integrated Public Use Microdata Series, Current Population Survey: Version 12.0 [dataset]. Minneapolis, MN.

Frank, R. G. and McGuire, T. G. (2000). Economics and mental health. In *Handbook of Health Economics*. Elsevier.

Friedman, A. B., Gervasi, S., Song, H., Bond, A. M., Chen, A. T., Bergman, A., David, G., Bailey, J. M., Brooks, R., and Smith-McLallen, A. (2022). Telemedicine catches on: Changes in the utilization of telemedicine services during the COVID-19 pandemic. *American Journal of Managed Care*, 28(1).

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

Gardner, J., Thakral, N., Tô, L. T., and Yap, L. (2024). Two-stage differences in differences.

Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics*, 225(2):254–277.

Green, C., Jung, H.-Y., Wu, X., Abramson, E., Walkup, J. T., Ford, J. S., and Grinspan, Z. M. (2019). Do children with special health care needs with anxiety have unmet health care needs? An analysis of a national survey. *Maternal and Child Health Journal*, 23:1220–1231.

Grossman, M. (1972). On the concept of health capital and the demand for health. *Journal of Political Economy*, 80(2):223–255.

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

Haidt, J. (2024). *The anxious generation: How the great rewiring of childhood is causing an epidemic of mental illness*. Penguin.

Hebert, R., Callison, K., Pesko, M. F., and Sturm, S. (2024). The effect of paid sick leave on healthcare expenditures.

Hegland, T. A. (2023). WOOLDID: Stata module to estimate difference-in-differences treatment effects with staggered treatment onset using heterogeneity-robust two-way fixed effects regressions.

IQVIA Institute (2025). Available IQVIA data. https://www.iqvia.com/insights/the-iqvia-institute/available-iqvia-data. Accessed: 2025-05-16.

IQVIA Institute for Human Data Science (2024). Report on stimulant trends in the United States. Technical report, U.S. Drug Enforcement Administration, Diversion Control Division. Accessed: 2025-05-16.

Jolivet, G. and Postel-Vinay, F. (2025). A structural analysis of mental health and labour market trajectories. *The Review of Economic Studies*, 92(3):1920–1954.

Kaczkurkin, A. N. and Foa, E. B. (2015). Cognitive-behavioral therapy for anxiety disorders: An update on the empirical evidence. *Dialogues in Clinical Neuroscience*, 17(3):337–346.

Kaiser Family Foundation (2023). Mental health care professional shortage area.

Kelly, T. M. and Daley, D. C. (2013). Integrated treatment of substance use and psychiatric disorders. *Social Work in Public Health*, 28(3-4):388–406.

Kowalski, A. (2016). Censored quantile instrumental variable estimates of the price elasticity of expenditure on medical care. *Journal of Business & Economic Statistics*, 34(1):107–117.

Lieb, D. A. (2025). Missouri governor repeals paid sick leave law approved last year by voters.

Ma, Y., Johnston, K. J., Yu, H., Wharam, J. F., and Wen, H. (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., Golberstein, E., and Stein, B. (2024). State paid sick leave mandates associated with increased mental health disorder prescriptions among Medicaid enrollees. *Health Affairs Scholar*, page qxae045.

Maclean, J. C. and Pabilonia, S. W. (2025). The effects of state paid sick leave mandates on parental childcare time. *Journal of Health Economics*, 103:103033.

Maclean, J. C., Pichler, S., and Ziebarth, N. R. (2025). Mandated sick pay: Coverage, utilization, and crowding-in. *Journal of the European Economic Association*, page jvaf008.

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

Maestas, N., Mullen, K. J., Powell, D., Von Wachter, T., and Wenger, J. B. (2023). The value of working conditions in the united states and implications for the structure of wages. *American Economic Review*, 113(7):2007–2047.

McGinty, E. E., Ge, Y., Yu, J., Tormohlen, K. N., and Eisenberg, M. D. (2025). Telebuprenorphine initiations for opioid use disorder without in-person relationships. *JAMA Network Open*, 8(3):e250001–e250001.

Mee-Lee, D., Shulman, G., Fishman, M., et al. (2013). *The ASAM criteria*. 3rd edition.

Merikangas, K. R., He, J.-p., Burstein, M., Swendsen, J., Avenevoli, S., Case, B., Georgiades, K., Heaton, L., Swanson, S., and Olfson, M. (2011). Service utilization for lifetime mental disorders in US adolescents: Results of the National Comorbidity Survey–Adolescent Supplement (NCS-A). *Journal of the American Academy of Child & Adolescent Psychiatry*, 50(1):32–45.

Mizushima, Y. and Powell, D. (2025). Inference with modern difference-in-differences methods. Technical report, RAND.

Moorey, S. and Hollon, S. D. (2021). Cognitive behavioral therapy for depression. In *Oxford Research Encyclopedia of Psychology*.

National Partnership for Women & Families (2024). State paid family & medical leave insurance laws.

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

Newhouse, J. P. (1993). *Free for all?: Lessons from the RAND health insurance experiment*. Harvard University Press.

Olfson, M., Blanco, C., and Marcus, S. C. (2016). Treatment of adult depression in the United States. *JAMA Internal Medicine*, 176(10):1482–1491.

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

Pichler, S. and Ziebarth, N. R. (2024). Sick leave and medical leave in the United States: A categorization and recent trends. Technical report, Institute for Labor Economics. IZA Policy Paper No. 206.

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

Ramanuj, P., Ferenchik, E., Docherty, M., Spaeth-Rublee, B., and Pincus, H. A. (2019). Evolving models of integrated behavioral health and primary care. *Current Psychiatry Reports*, 21(1):1–12.

Rios-Avila, F., Callaway, B., and Sant'Anna, P. (2021). csdid: Difference-in-differences with multiple time periods in Stata. In *Stata Conference*, page 47.

Ross, S. and Peselow, E. (2012). Co-occurring psychotic and addictive disorders: Neurobiology and diagnosis. *Clinical Neuropharmacology*, 35(5):235–243.

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

Sanders, B. and DeLauro, R. (2023). Healthy Families Act of 2023.

Sant'Anna, P. H. and Zhao, J. (2020). Doubly robust difference-in-differences estimators. *Journal of Econometrics*, 219(1):101–122.

Schiffer, M. (2023). Understanding levels of care in mental health treatment. Anxiety and Depression Association of America.

Seixas, B. V. and Macinko, J. (2020). Unavailability of paid sick leave among parents is a barrier for children's utilization of nonemergency health services: Evidence from the National Health Interview Survey. *The International Journal of Health Planning and Management*, 35(5):1083–1097.

Siu, A. L., Bibbins-Domingo, K., Grossman, D. C., Baumann, L. C., Davidson, K. W., Ebell, M., García, F. A., Gillman, M., Herzstein, J., Kemper, A. R., et al. (2016). Screening for depression in adults: US Preventive Services Task Force recommendation statement. *JAMA*, 315(4):380–387.

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*, 43(4):1129–1151.

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

Toth, M., Gibbons, B., Levinson, A., Kar, N., Keyes, V., Nye, E., Jacobus-Kantor, L., and Creedon, T. (2025). Behavioral health diagnosis, service utilization, and spending among older adult Medicare beneficiaries: A chartbook. Technical report, Office of the Assistant Secretary for Planning and Evaluation.

University of Kentucky Center for Poverty Research (2025). UKCPR national welfare data, 1980-2023. University of Kentucky Center for Poverty Research.

U.S. Department of Agriculture, E. R. S. (2024). Rural-urban continuum codes: 2023 update. Dataset, updated January 2024. Available at: https://www.ers.usda.gov/data-products/rural-urban-continuum-codes.

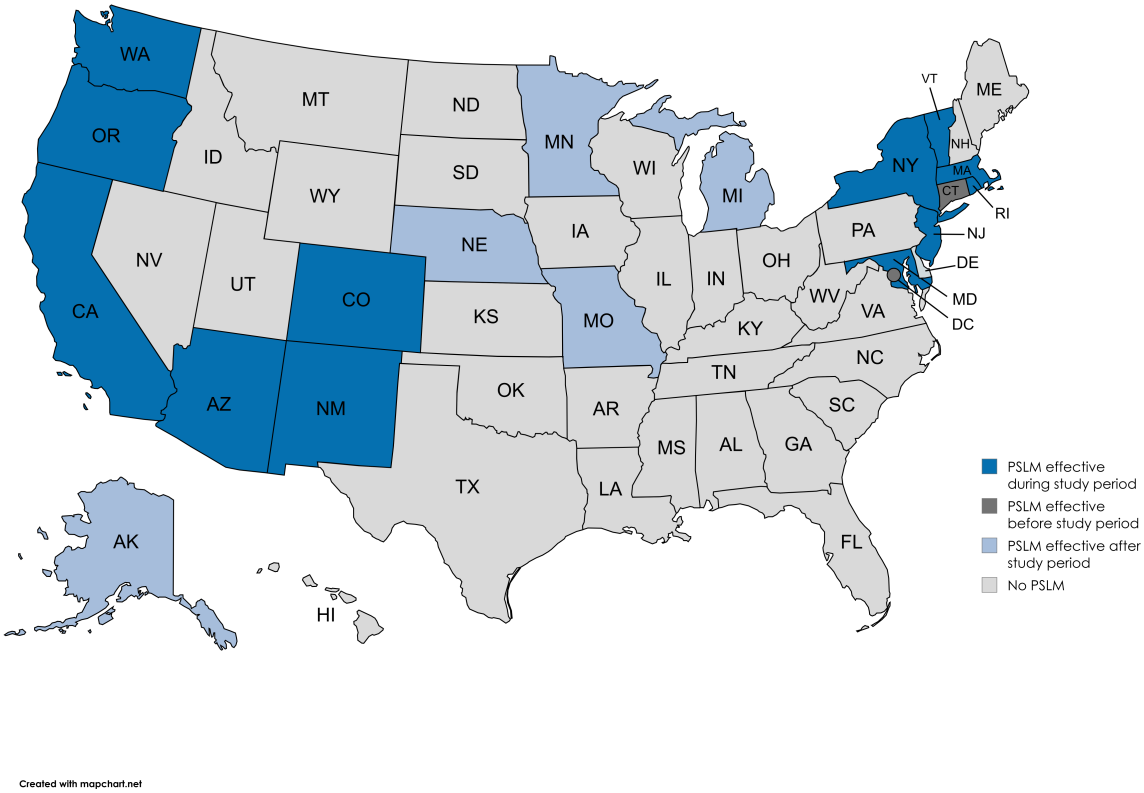
Wen, H., Hockenberry, J. M., Borders, T. F., and Druss, B. G. (2017). Impact of Medicaid expansion on Medicaid-covered utilization of buprenorphine for opioid use disorder treatment. *Medical Care*, 55(4):336–341.

Whitney, D. G. and Peterson, M. D. (2019). US national and state-level prevalence of mental health disorders and disparities of mental health care use in children. *JAMA pediatrics*, 173(4):389–391.

Wolf, D. A., Montez, J. K., and Monnat, S. M. (2022). US state preemption laws and working-age mortality. *American Journal of Preventive Medicine*, 63(5):681–688.

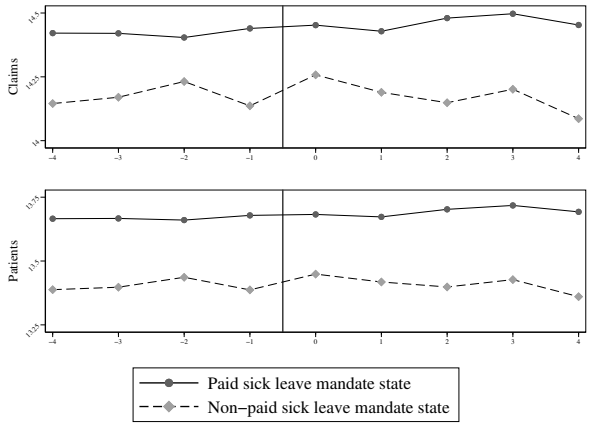
Wooldridge, J. M. (2023). Simple approaches to nonlinear difference-in-differences with panel data. *The Econometrics Journal*, 26(3):C31–C66.

Figure 1: Geographic distribution of of all adopted or announced state paid sick leave mandates in the United States as of November, 2024

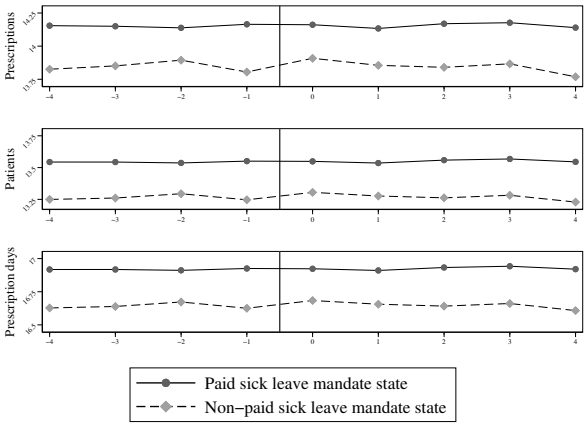



Notes: The data sources are National Partnership of Women & Families (2023) and A Better Balance (2025). All state paid sick leave mandates are effective or announced by November, 2025. See Table 1 for paid sick leave mandate effective dates.

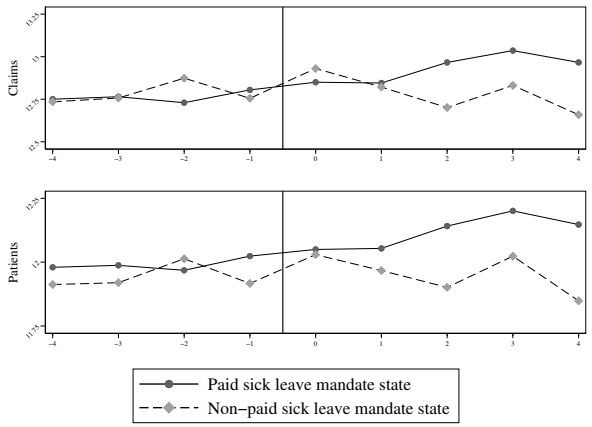
Figure 2: Trends in mental healthcare utilization in states that do and do not adopt/announce a paid sick leave mandate: IQVIA claims database 2015-2022



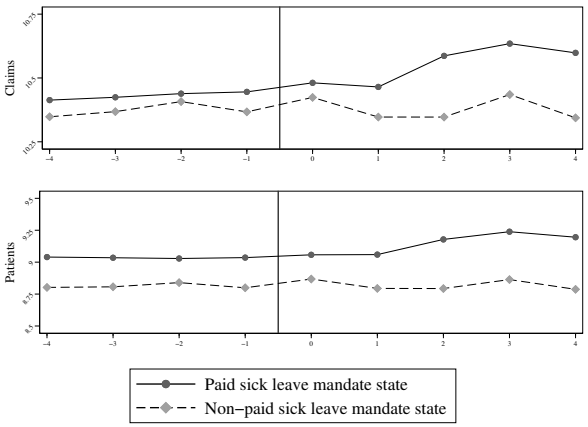

Panel A: Any




Panel B: Prescriptions



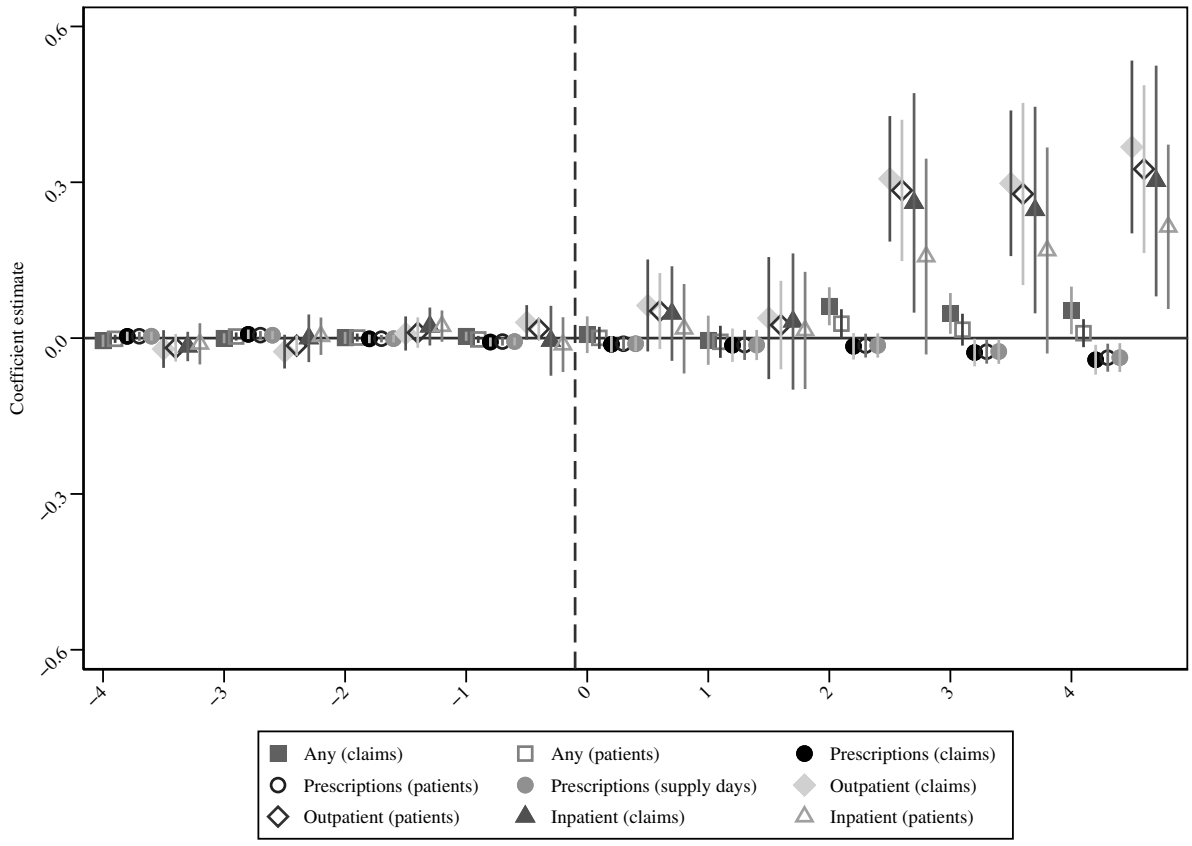

Panel C: Outpatient




Panel D: Inpatient

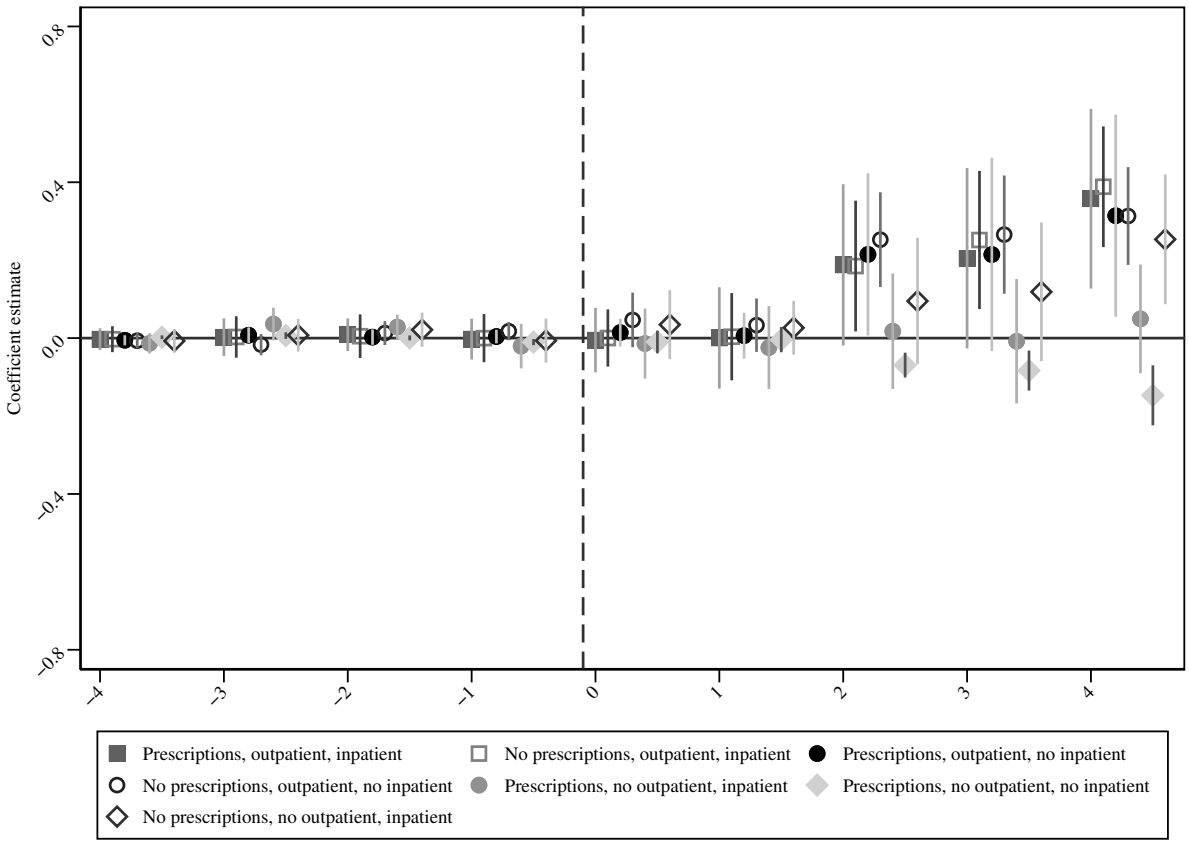
Notes: This figure reports unadjusted trends in mental healthcare use in states that do and do not adopt a paid sick leave mandate by November, 2025. The vertical line divides the period into the pre-mandate and post-mandate period. Data for states that adopt a paid sick leave mandate are centered around the month-year of mandate adoption. Data for states that do not adopt a paid sick leave mandate are centered around the median effective period (month-year) among mandate-adopting states (October, 2018). Data are trimmed in event-time, periods more than four year prior to/following the effective date for mandate adopting states and October, 2018 among other states. Outcomes are log transformed. Connecticut and the District of Columbia are excluded from the sample as these jurisdictions are treated in all years 2015-2022. The unit of observation is a treatment group-year where treatment group is coded as one if the state adopts or announces a paid sick leave mandate by November, 2024 and zero otherwise. Data are weighted by the state population prior to aggregation.

Figure 3: The effect of a state paid sick leave mandate on mental healthcare utilization using an event-study utilizing a two-stage difference-in-differences procedure: IQVIA claims database 2015-2022



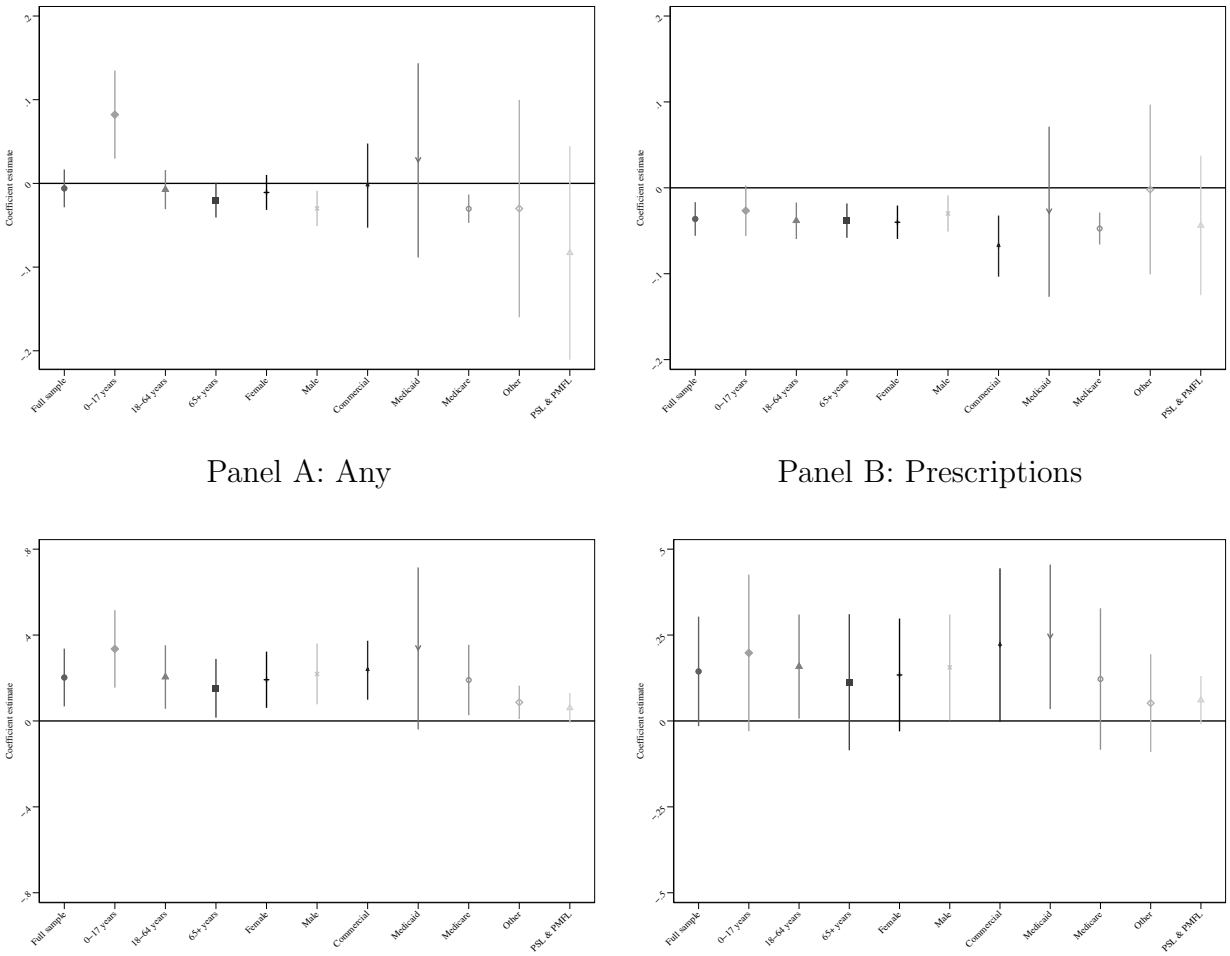

Notes: This figure reports results of a regression of mental healthcare utilization on indicators for time-to-paid sick leave adoption and other controls. Outcomes are log transformed. The unit of observation is a state in a year-month. Connecticut and the District of Columbia are excluded from the sample as these jurisdictions are treated in all years 2015-2022. We utilize the Gardner (2022) two-stage difference-in-differences procedure. All regressions estimated with ordinary least squares and control for time-varying state characteristics, state fixed effects, and period (month-year) fixed effects. There is no omitted category in the Gardner (2022) procedure event-study. For treated states, observations more than four years pre-mandate or four years post-mandate are excluded. Data are weighted by the state population per state-year. Coefficient estimates are reported in circles and 95% confidence intervals that account for within-state clustering are reported with vertical lines.

Figure 4: The effect of a state paid sick leave mandate on mental healthcare utilization substitution measures utilizing a two-stage difference-in-differences event-study: IQVIA claims database 2015-2022




Notes: This figure reports results of a regression of mental healthcare utilization on indicators for time-to-paid sick leave adoption and other controls. Outcomes are log transformed. The unit of observation is a state in a year-six-month bin. Connecticut and the District of Columbia are excluded from the sample as these jurisdictions are treated in all years 2015-2022. We utilize the Gardner (2022) two-stage difference-in-differences procedure. All regressions estimated with ordinary least squares and control for time-varying state characteristics, state fixed effects, and period (six month bin-year) fixed effects. There is no omitted category in the Gardner (2022) procedure event-study. For treated states, observations more than four years pre-mandate or four years post-mandate are excluded. Data are weighted by the state population per state-year. Coefficient estimates are reported in circles and 95% confidence intervals that account for within-state clustering are reported with vertical lines.

Figure 5: Heterogeneity in the effect of a state paid sick leave mandate on mental health-care utilization using a two-stage difference-in-differences procedure: IQVIA claims database 2015-2022




Panel A: Any

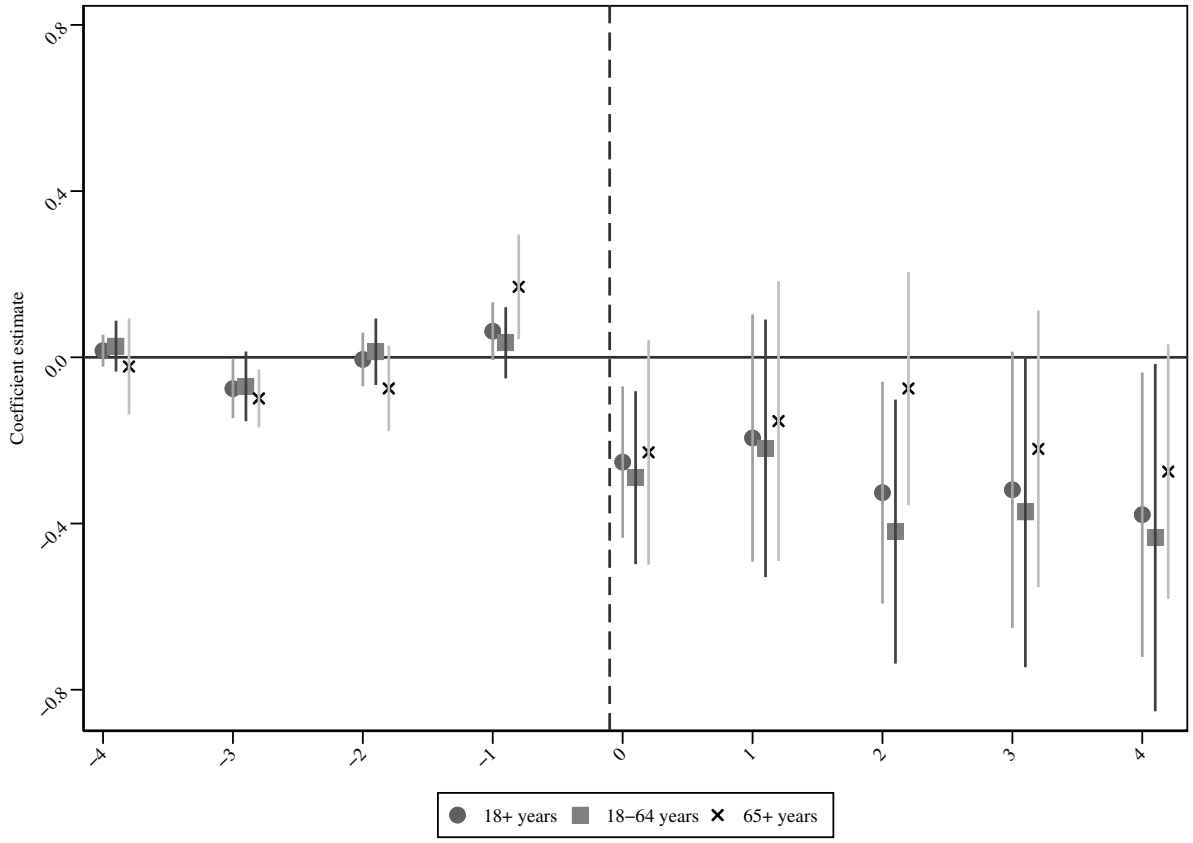
Panel B: Prescriptions

Panel C: Outpatient

Panel D: Inpatient

Notes: This figure reports results of a regression of mental healthcare utilization on a state paid sick leave mandate and other controls across different samples of patients. The sample is reported in the legend. Outcomes are patient counts and are log transformed. The unit of observation is a state in a year-month. Connecticut and the District of Columbia are excluded from the sample as these jurisdictions are treated in all years 2015-2022. We utilize the Gardner (2022) two-stage difference-in-differences procedure. All regressions estimated with ordinary least squares and control for time-varying state characteristics, state fixed effects, and period (month-year) fixed effects. Data are weighted by the state population per state-year. Coefficient estimates are reported in shapes and 95% confidence intervals that account for within-state clustering are reported with vertical lines.

Figure 6: The effect of a state paid sick leave mandate on the number of days in bad mental health in the past 30 days using an event-study utilizing a two-stage difference-in-differences procedure: Behavioral Health Risk Factor Surveillance Survey 2015-2022




Notes: This figure reports coefficient estimates from a regression of the number of days in bad mental health in the past 30 days on indicators for time-to-paid sick leave adoption and other controls. The unit of observation is a respondent in a state in a year-month. Connecticut and the District of Columbia are excluded from the sample as these jurisdictions are treated in all years 2015-2022. We utilize the Gardner (2022) two-stage difference-in-differences procedure. All regressions estimated with ordinary least squares and control for time-varying state characteristics, state fixed effects, and period (month-year) fixed effects. There is no omitted category in the Gardner (2022) procedure event-study. For treated states, observations more than four years pre-mandate or four years post-mandate are excluded. Data are weighted by Behavioral Risk Factor Surveillance Survey sample weights. Coefficient estimates are reported in circles and 95% confidence intervals that account for within-state clustering are reported with vertical lines.

Table 1: State paid sick leave mandate effective dates and number of employees gaining access for the first time in the United States: National Partnership of Women & Families (2023) and A Better Balance (2025)

| State | Effective date | Employees gaining coverage for the first time |
|---|---|---|
| Alaska | 7/2025 | N/A† |
| **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** |
| Michigan | 2/2025 | N/A† |
| Minnesota | 1/2024 | N/A† |
| Missouri | 5/2025†† | N/A† |
| Nebraska | 10/2025 | 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: This table reports the effective date for each state paid sick leave mandate and the number employees gaining access to paid sick leave as a result of the mandate adoption. The data sources are National Partnership of Women & Families (2023) and A Better Balance (2025). Italics font indicates that the state PSL mandate implementation date occurred prior to the study period (i.e., before 2015). Bold font indicates that the state PSL mandate implementation date occurred during our study period (2015-2022). Regular font indicates that the PSL mandate implementation occured after the study period (i.e., before 20222). Dates are in the format of month/year. State paid sick leave mandates adopted or announced as of November, 2025. Estimates of employees gaining paid sick leave coverage for the first time based on National Partnership of Women & Families (2023) '*Law/Bill Number and Impact.*'

†The National Partnership of Women & Families (2023) has not released data on the number of employees gaining paid sick leave through these policy changes.

††The Missouri paid sick leave mandate has been repealed and the repeal date is 8/2025 (Lieb, 2025).

Table 2: Summary statistics: IQVIA database 2015-2022

| Sample: | All states | States that adopt a mandate, pre-mandate | States that do not adopt a mandate |
|---|---|---|---|
| **Mental healthcare use (,000s)** | | | |
| Any use (claims) | 1917 | 1592 | 1715 |
| Any use (patients) | 891 | 735 | 814 |
| Prescription (claims) | 1401 | 1154 | 1290 |
| Prescriptions (patients) | 778 | 636 | 724 |
| Prescription (days) | 22794 | 18600 | 21243 |
| Outpatient (claims) | 464 | 402 | 380 |
| Outpatient (patients) | 198 | 175 | 167 |
| Inpatient (claims) | 48 | 35 | 44 |
| Inpatient (patients) | 11 | 8 | 9 |
| **State-level variables** | | | |
| Paid sick leave mandate | 0.2 | 0 | 0 |
| Paid time off mandate | 0.0 | 0.1 | 0.01 |
| Family & medical leave policy | 0.2 | 0.2 | 0 |
| Temporary Aid to Needy Families* | 647 | 788 | 485 |
| 0-20 years | 0.3 | 0.3 | 0.3 |
| 21-39 years | 0.3 | 0.3 | 0.3 |
| 40-64 years | 0.3 | 0.3 | 0.3 |
| 65+ years | 0.2 | 0.2 | 0.2 |
| Male | 0.5 | 0.5 | 0.5 |
| Female | 0.5 | 0.5 | 0.5 |
| White | 0.8 | 0.8 | 0.8 |
| Black | 0.1 | 0.1 | 0.2 |
| Other race | 0.1 | 0.1 | 0.1 |
| Hispanic | 0.2 | 0.2 | 0.2 |
| College degree | 0.3 | 0.3 | 0.3 |
| State population | 14.8 | 11.2 | 12.4 |
| Observations** | 4704 | 952 | 3072 |

Notes: This table reports summary statistics. The unit of observation is a state in a month-year. Connecticut and the District of Columbia are excluded from the sample as these jurisdictions are treated in all years 2015-2022. Data are weighted by the state population.

* Temporary Aid to Needy Families monthly benefit ($) for a family of four.

**Columns (2) and (3) do not sum to column (1) – 4704 vs. 4024 – as column (2) only includes observations in paid sick leave mandate-adopting stats prior to mandate implementation (thus, all 'treated' units are excluded).

Table 3: The effect of a state paid sick leave mandate (lagged one year) on access to and use of paid sick leave: National Compensation Survey 2009 to 2022

| Outcome variable →: | Access | Utilization |
|---|---|---|
| Paid sick leave mandate | 0.122*** | 2.268*** |
| | (0.028) | (0.656) |
| Percent change† | 16.9% | 9.8% |
| Pre-treatment mean, treatment states | 0.721 | 23.127 |
| Observations | 691388 | 691388 |

Notes: The outcome variables are i) an indicator for a job having paid sick leave benefits ('Access') and ii) the quarterly hours of paid sick leave used ('Utilization'). This table reports coefficient estimates from a regression of paid sick leave access and utilization on state paid sick leave mandates and other controls. The unit of observation is an establishment in a job in a state in a year. The District of Columbia is excluded from the sample as this jurisdiction is treated in all years 2009-2022. We utilize a Gardner (2022) two-stage difference-in-differences procedure. All regressions estimated with ordinary least squares and control for time-varying state characteristics, state fixed-effects, and year fixed-effects. Data are weighted by National Compensation Survey provided sample weights. Standard errors that account for within-state clustering are reported in parentheses.
***,**, and * = statistically different from zero at the 1%, 5%, and 10% level.
†Percent change = $\hat{\beta} \div$ per-treatment mean value in states that adopt a paid sick leave mandate $\times$ 100%.

Table 4: The effect of a state paid sick leave mandate on mental health treatment and prescriptions utilizing a two-stage difference-in-differences procedure: IQVIA 2015–2022

| Outcome: | Claims/Prescriptions | Patients | Days |
|---|---|---|---|
| **Panel A. Any mental health treatment use** | | | |
| State paid sick leave mandate | 0.014 | -0.006 | |
| | (0.018) | (0.012) | |
| Pre-treatment mean (,000s), level | 1592 | 735 | |
| Observations | 4704 | 4704 | |
| **Panel B. Dispensed mental health prescriptions** | | | |
| State paid sick leave mandate | -0.043*** | -0.036*** | -0.036*** |
| | (0.011) | (0.010) | (0.010) |
| Pre-treatment mean (,000s), level | 1154 | 636 | 18600 |
| Observations | 4704 | 4704 | 4704 |
| **Panel C. Outpatient mental health service use** | | | |
| State paid sick leave mandate | 0.221*** | 0.202*** | |
| | (0.060) | (0.069) | |
| Pre-treatment mean (,000s), level | 402 | 175 | |
| Observations | 4704 | 4704 | |
| **Panel D. Inpatient mental health service use** | | | |
| State paid sick leave mandate | 0.228** | 0.144* | |
| | (0.108) | (0.081) | |
| Pre-treatment mean (,000s), level | 35 | 8 | |
| Observations | 4704 | 4704 | |

Notes: Panels report coefficient estimates from regressions of mental health treatment utilization on state paid sick leave mandates and other controls. The unit of observation is a state-month. Connecticut and the District of Columbia are excluded from the sample as they are always treated. Outcomes are log transformed. Regressions include state characteristics, state fixed effects, and time (month-year) fixed effects. Data are weighted by the state population. Regressions are estimated with a two-stage difference-in-differences procedure proposed by Gardner (2022). Standard errors that account for within-state clustering are reported in parentheses. Column 'Days' corresponds to prescription days in Panel B. ***,**, and * = statistically different from zero at the 1%, 5%, and 10% level.

Table 5: The effect of a state paid sick leave mandate on substitution mental health service use utilizing a two-stage difference-in-differences procedure: IQVIA 2015-2022

| Outcome variable ↓: | Coefficient estimate (Standard error) |
|---|---|
| Prescriptions, outpatient, inpatient | 0.180* |
| | (0.102) |
| Pre-treatment mean (,000s), level | 12.284 |
| No prescriptions, outpatient, inpatient | 0.199*** |
| | (0.069) |
| Pre-treatment mean (,000s), level | 5.465 |
| Prescriptions, outpatient, no inpatient | 0.175* |
| | (0.094) |
| Pre-treatment mean (,000s), level | 258.451 |
| No prescriptions, outpatient, no inpatient | 0.198*** |
| | (0.050) |
| Pre-treatment mean (,000s), level | 194.942 |
| Prescriptions, no outpatient, inpatient | 0.012 |
| | (0.062) |
| Pre-treatment mean (,000s), level | 11.112 |
| Prescription, no outpatient, no inpatient | -0.076*** |
| | (0.023) |
| Pre-treatment mean (,000s), level | 742.285 |
| No prescriptions, no outpatient, inpatient | 0.130* |
| | (0.075) |
| Pre-treatment mean (,000s), level | 9.606 |
| Observations | 784 |

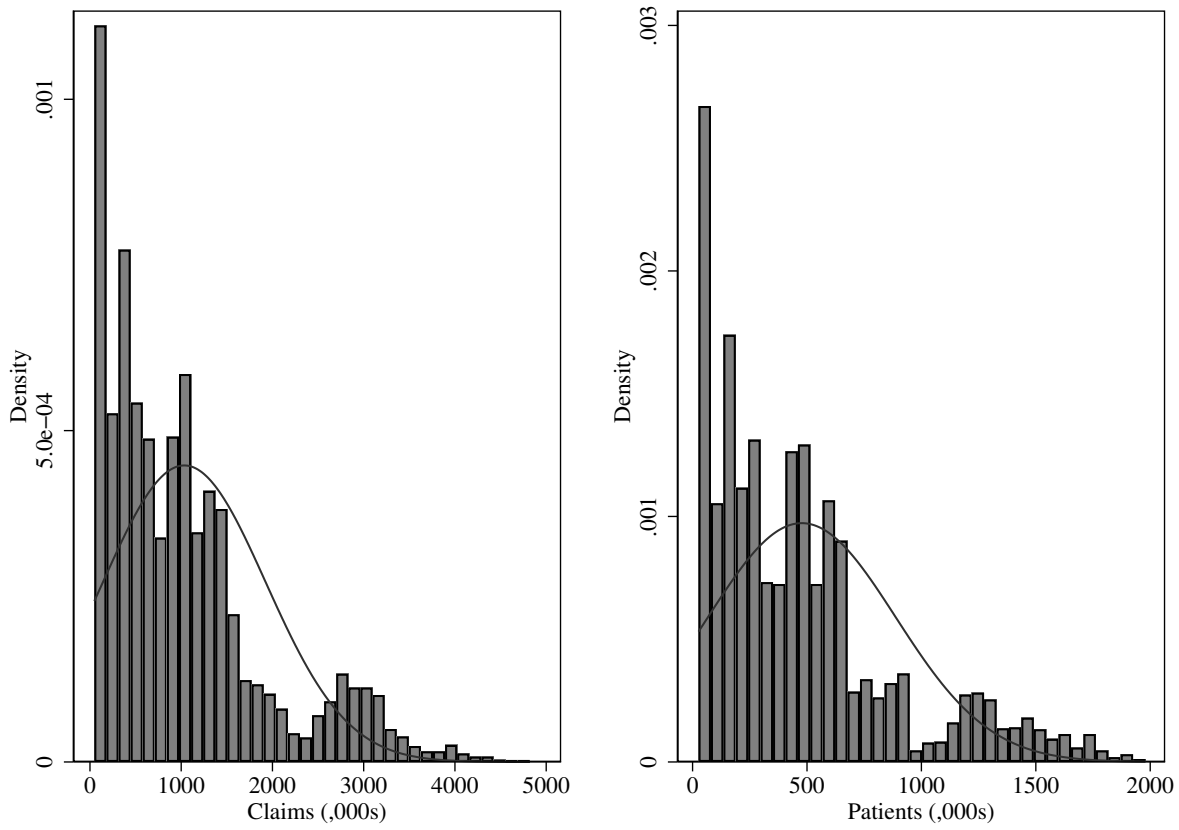
Notes: This table reports coefficient estimates from a regression of service utilization on state paid sick leave mandates and other controls. The unit of observation is a state in a six-month period in a year. Connecticut and the District of Columbia are excluded from the sample as they are always treated. Outcomes are log transformed. The regression includes state characteristics, state fixed effects, and time (six-month-year) fixed effects. Data are weighted by the state population. Regressions are estimated with a two-stage difference-in-differences procedure proposed by Gardner (2022). Standard errors that account for within-state clustering are reported in parentheses.
***,**, and * = statistically different from zero at the 1%, 5%, and 10% level.

Table 6: Effect of a state paid sick leave mandate on the number of days in bad mental health in the past 30 days paid sick leave using difference-in-differences: Behavioral Risk Factor Surveillance Survey 2015-2022

| Outcome variable → | Days in bad mental health |
|---|---|
| 18+ years | -0.281*** |
| | (0.103) |
| Pre-treatment mean, treatment states | 3.983 |
| Percent change† | -7.05% |
| Observations | 3266458 |
| 18-64 years | -0.351*** |
| | (0.125) |
| Pre-treatment mean | 4.378 |
| Percent change† | -8.02% |
| Observations | 2107430 |
| 65+ years | -0.134 |
| | (0.118) |
| Pre-treatment mean | 2.481 |
| Percent change† | -5.40% |
| Observations | 1159028 |

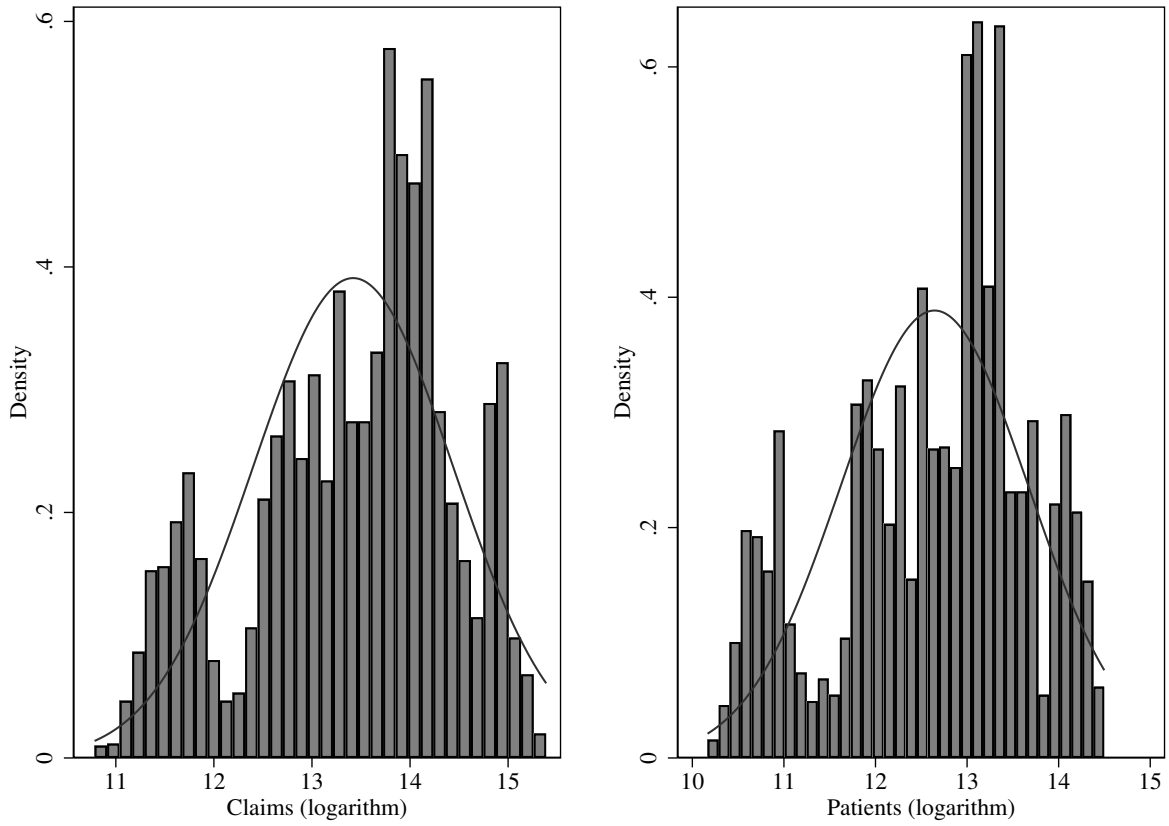
Notes: This table reports coefficient estimates from a regression of the number of days in bad mental health in the past 30 days on state paid sick leave mandates and other controls. The unit of observation is a respondent in state in a six-month period in a year. Connecticut and the District of Columbia are excluded from the sample as they are always treated. Outcomes are log transformed. The regression includes state characteristics, state fixed effects, and time (month-year) fixed effects. Data are weighted by Behavioral Risk Factor Surveillance Survey sample weights. Regressions are estimated with a two-stage difference-in-differences procedure proposed by Gardner (2022). Standard errors that account for within-state clustering are reported in parentheses.
***,**,* = statistically different from zero at the 1%,5%,10% level.
†Percent change = $\hat{\beta} \div$ pre-treatment mean value in states that adopt a paid sick leave mandate $\times$ 100%.

Figure A1: Distribution of any mental healthcare use (,000s): IQVIA 2015-2022



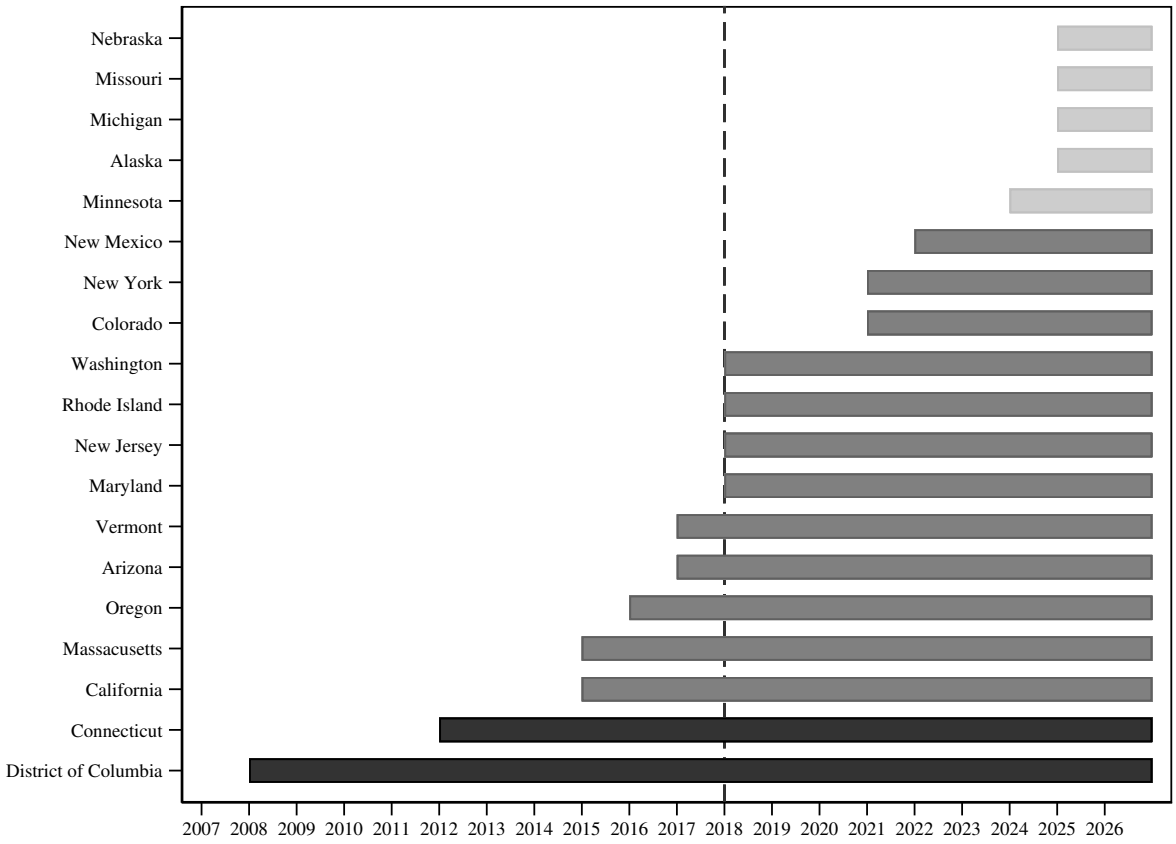

Notes: This figure reports the distribution of mental healthcare use measured by claims for mental health and patients with a mental health condition. The outcomes are divided by 1,000 for readability. The unit of observation is a state in a month-year. Connecticut and the District of Columbia are excluded from the sample as these jurisdictions are treated in all years 2015-2022.

Figure A2: Distribution of any mental healthcare use (logarithm): IQVIA 2015-2022



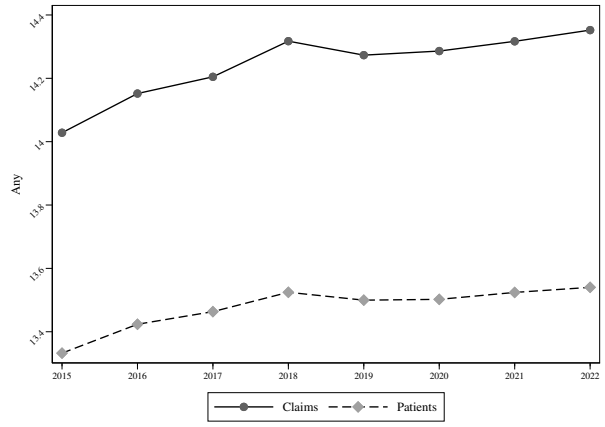

Notes: This figure reports the distribution of mental healthcare use measured by claims for mental health and patients with a mental health condition. The outcomes log-transformed. The unit of observation is a state in a month-year. Connecticut and the District of Columbia are excluded from the sample as these jurisdictions are treated in all years 2015-2022.

Figure A3: Temporal distribution of of all adopted or announced state paid sick leave mandates in the United States as of November, 2025

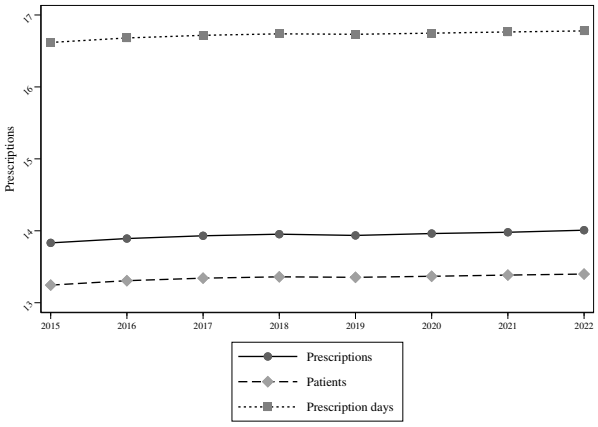



Notes: The data sources are National Partnership of Women & Families (2023) and A Better Balance (2025). All state paid sick leave mandates are effective or announced by November, 2025. Black shade indicates states that adopt a paid sick leave mandate prior to the beginning study period (i.e., prior to 2015). Medium gray shade indicates states that adopt a paid sick leave mandate during the study period (i.e., 2015-2022). Light gray shade indicates states that adopt a paid sick leave mandate after the study period (i.e., after 2022). See Table 1 for paid sick leave mandate effective dates.

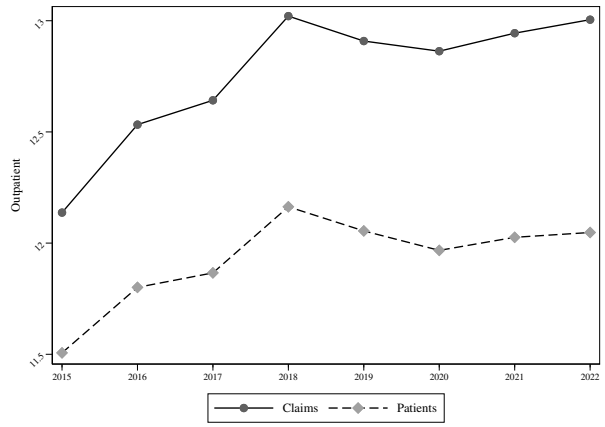
Figure A4: Trends in mental healthcare utilization: IQVIA claims database 2015-2022



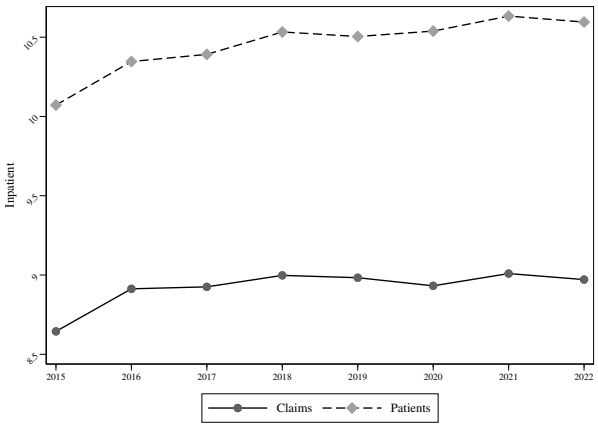

Panel A: Any




Panel B: Prescriptions



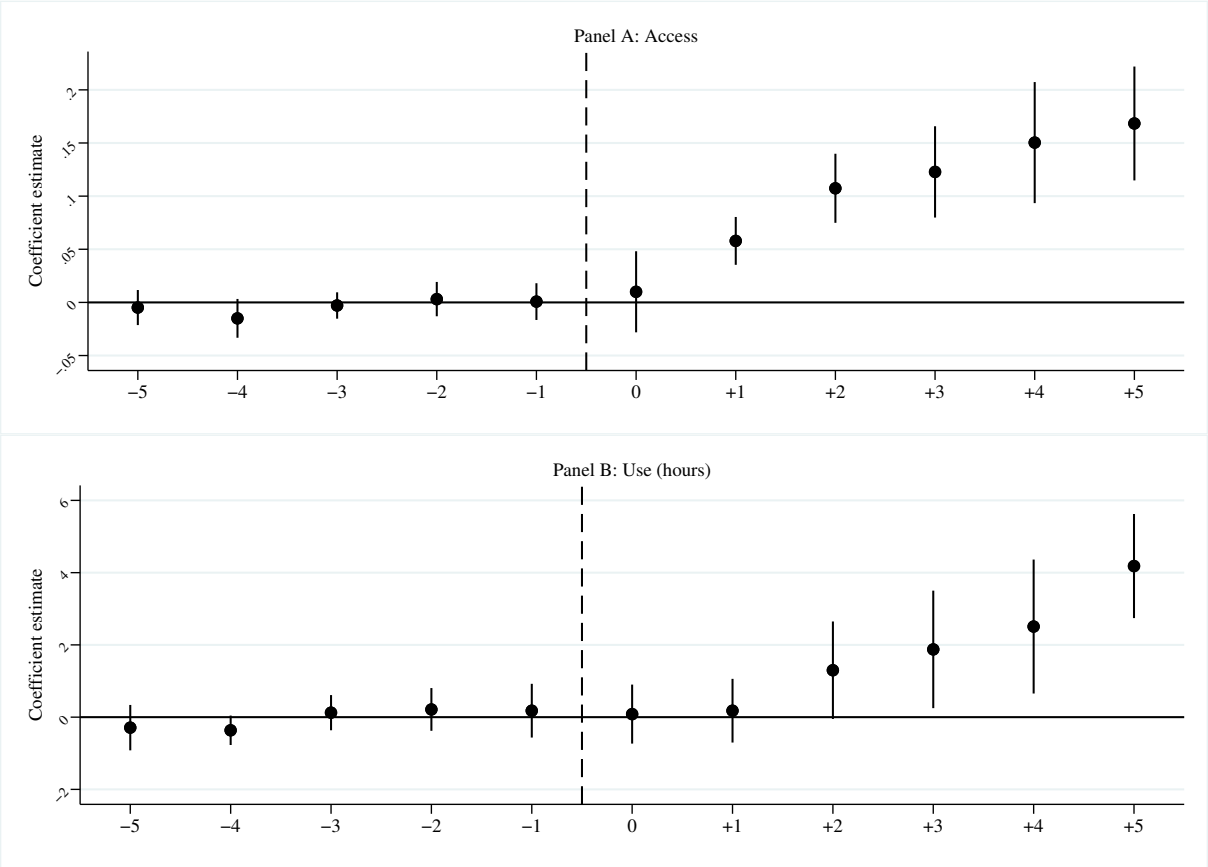

Panel C: Outpatient




Panel D: Inpatient

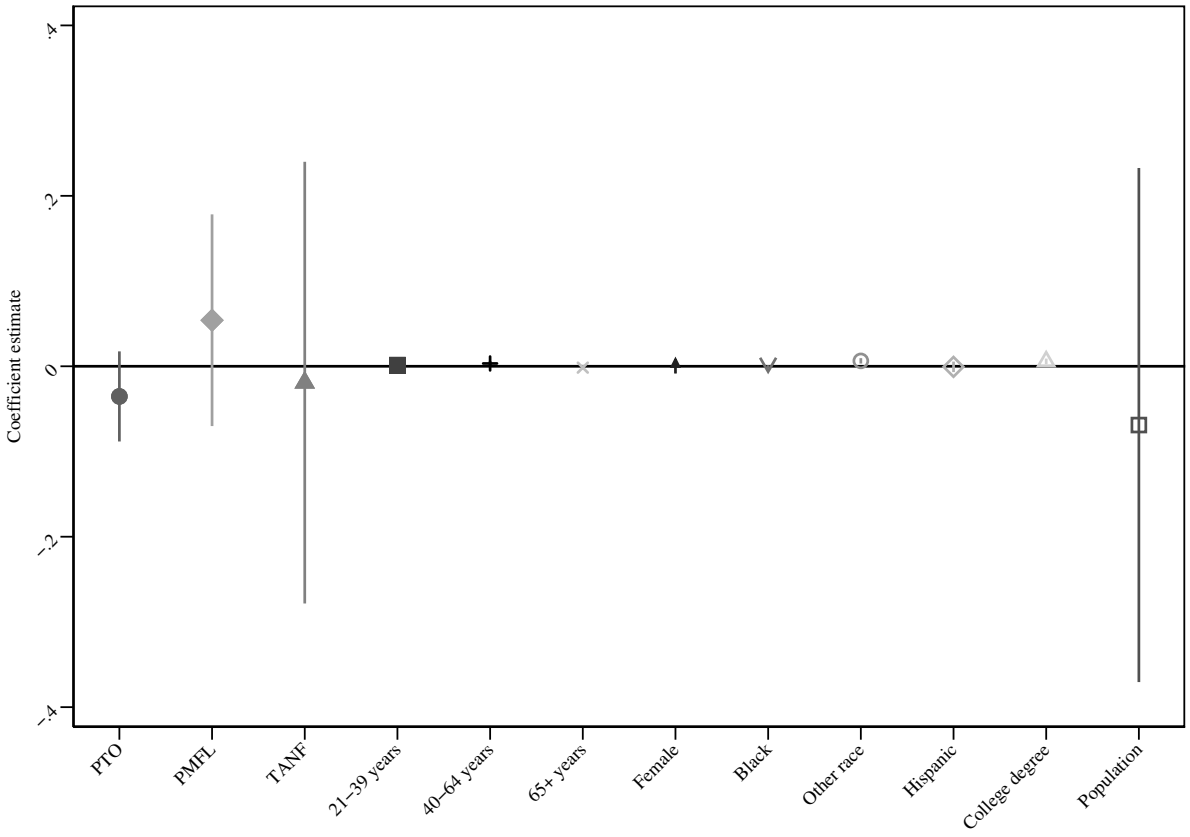
Notes: This figure reports unadjusted trends in mental healthcare use. Outcomes are log transformed. Connecticut and the District of Columbia are excluded from the sample as these jurisdictions are treated in all years 2015-2022. The unit of observation is a year. Data are weighted by the state population prior to aggregation.

Figure A5: The effect of a state paid sick leave mandate on access to and use of paid sick leave utilizing a two-stage difference-in-differences event-study: National Compensation Survey 2009-2022



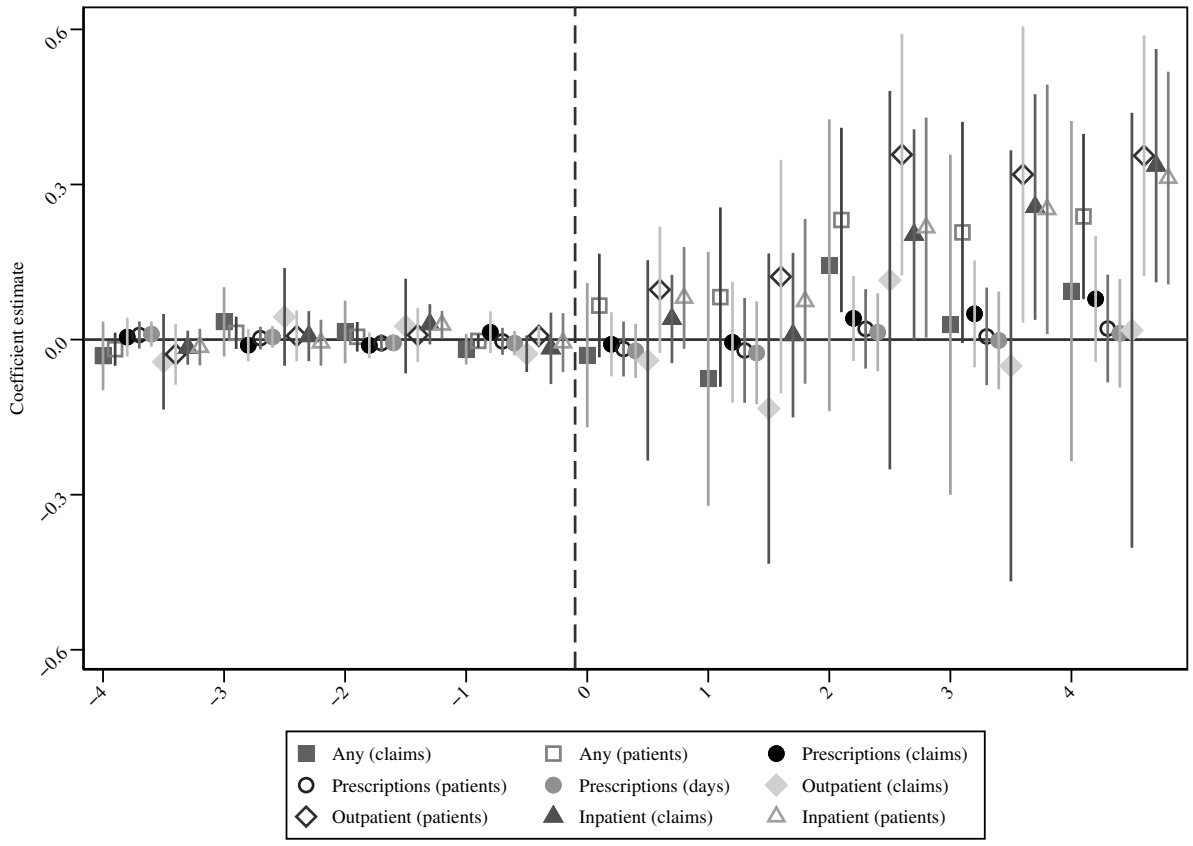

Notes: This figure reports results of a regression of paid sick leave access and utilization on indicators for time-to-paid sick leave adoption and other controls. We have a longer study period in the National Compensation Survey than in the IQVIA data, thus we are able to include additional leads and lags in the event-study. The unit of observation is an establishment in a job in a state in a year. The District of Columbia is excluded from the sample as this jurisdiction is treated in all years 2009-2022. We utilize the Gardner (2022) two-stage difference-in-differences procedure. All regressions estimated with ordinary least squares and control for time-varying state characteristics, state fixed effects, and year fixed effects. There is no omitted category in the Gardner (2022) procedure event-study. For treated states, observations more than five years pre-mandate or five years post-mandate are excluded. Data are weighted by National Compensation Survey provided sample weights. Coefficient estimates are reported in circles and 95% confidence intervals that account for within-state clustering are reported with vertical lines.

Figure A6: Test of covariate balance: Various data sources 2015-2022



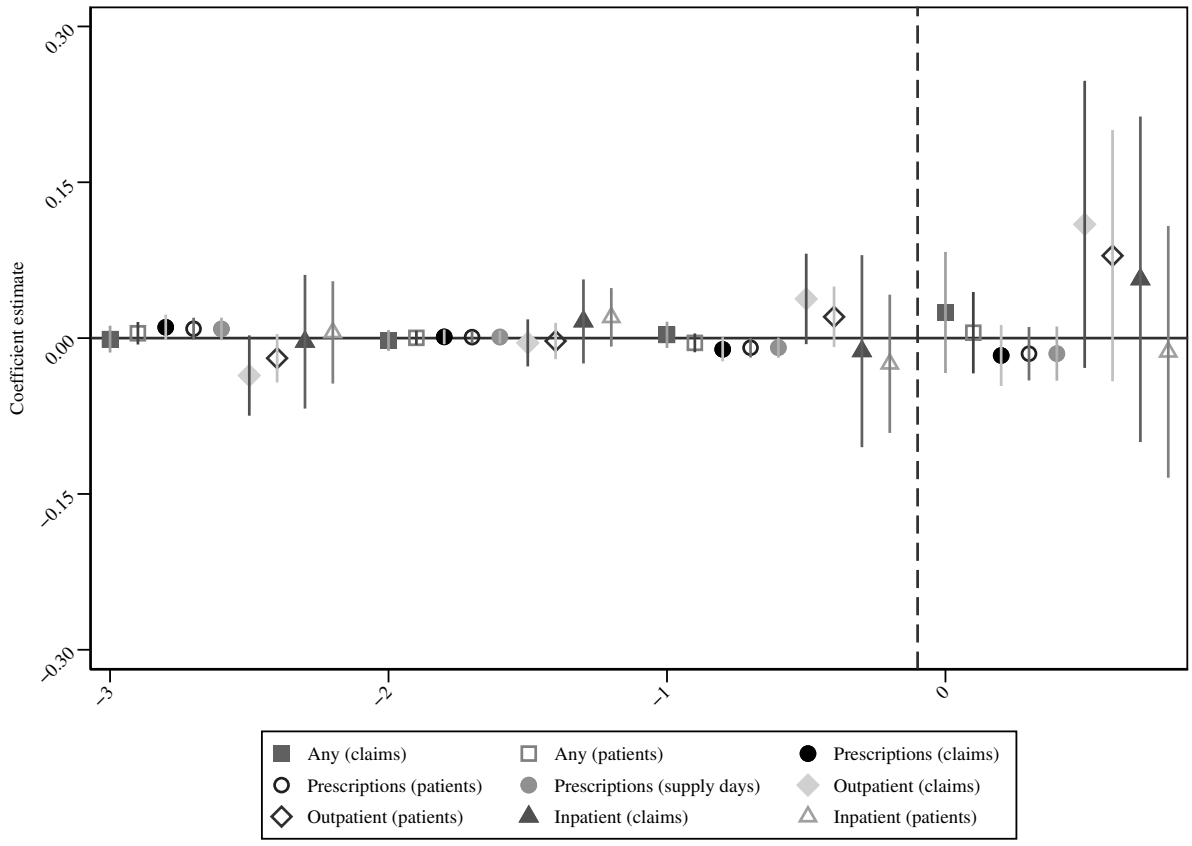

Notes: See Section 3 for information on data sources. This figure reports results from a regression of each covariate included in equation (1) on state and month-year fixed effects. The legend lists the outcome variable in each regression. The unit of observation is a state in a month-year. Connecticut and the District of Columbia are excluded from the sample as these jurisdictions are treated in all years 2015-2022. We utilize a Gardner (2022) two-stage difference-in-differences procedure. Data are weighted by the state population. Coefficient estimates are reported in shapes and 95% confidence intervals that account for within-state clustering are reported with vertical lines.

Figure A7: The effect of a state paid sick leave mandate on substance use disorder utilization using an event-study utilizing a two-stage difference-in-differences procedure: IQVIA claims database 2015-2022



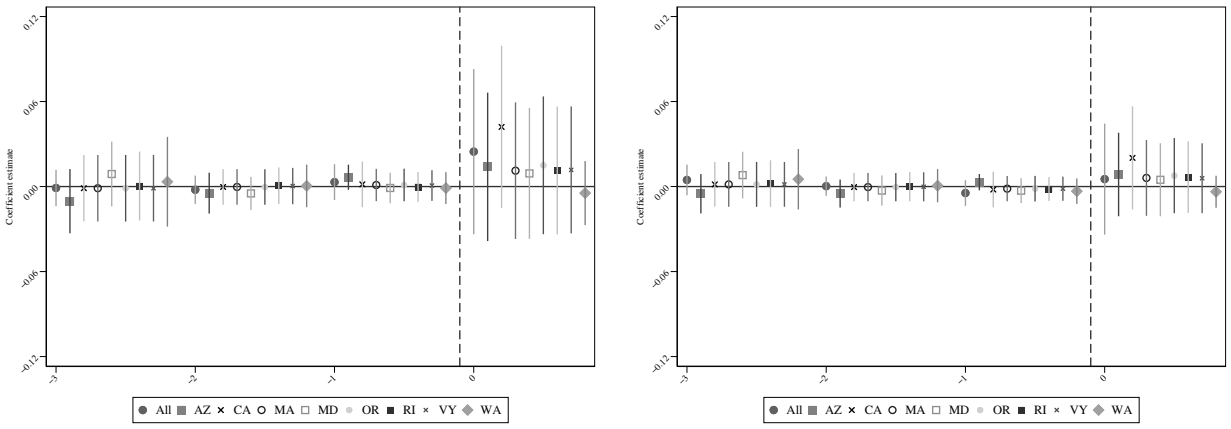

Notes: This figure reports results of a regression of substance use disorder utilization on a state paid sick leave mandate and other controls across different samples of patients. The sample is reported in the legend. Outcomes are patient counts and are log transformed. The unit of observation is a state in a year-month. Connecticut and the District of Columbia are excluded from the sample as these jurisdictions are treated in all years 2015-2022. We utilize the Gardner (2022) two-stage difference-in-differences procedure. All regressions estimated with ordinary least squares and control for time-varying state characteristics, state fixed effects, and period (month-year) fixed effects. Data are weighted by the state population per state-year. Coefficient estimates are reported in shapes and 95% confidence intervals that account for within-state clustering are reported with vertical lines.

Figure A8: The effect of a state paid sick leave mandate on mental healthcare utilization using an event-study utilizing a two-stage difference-in-differences procedure in a balanced sample of treatment states: IQVIA claims database 2015-2022




Notes: This figure reports results of a regression of mental healthcare utilization on indicators for time-to-paid sick leave adoption and other controls including mandate-adopting states that are observed -3, -2, and -1 years prior to mandate adoption and the year of adoption (i.e. Arizona, Colorado, Maryland, New Jersey, New Mexico, New York, Rhode Island, and Washington) and states that do not adopt or announce a paid sick leave mandate by November, 2025. Outcomes are log transformed. The unit of observation is a state in a year-month. Connecticut and the District of Columbia are excluded from the sample as these jurisdictions are treated in all years 2015-2022. We utilize the Gardner (2022) two-stage difference-in-differences procedure. All regressions estimated with ordinary least squares and control for time-varying state characteristics, state fixed effects, and period (month-year) fixed effects. There is no omitted category in the Gardner (2022) procedure event-study. For treated states, observations more than four years pre-mandate or four years post-mandate are excluded. Data are weighted by the state population per state-year. Coefficient estimates are reported in circles and 95% confidence intervals that account for within-state clustering are reported with vertical lines.

Figure A9: The effect of a state paid sick leave mandate on any mental healthcare utilization using an event-study utilizing a two-stage difference-in-differences procedure in a balanced sample of treatment states sequentially excluding each mandate-adopting state: IQVIA claims database 2015-2022

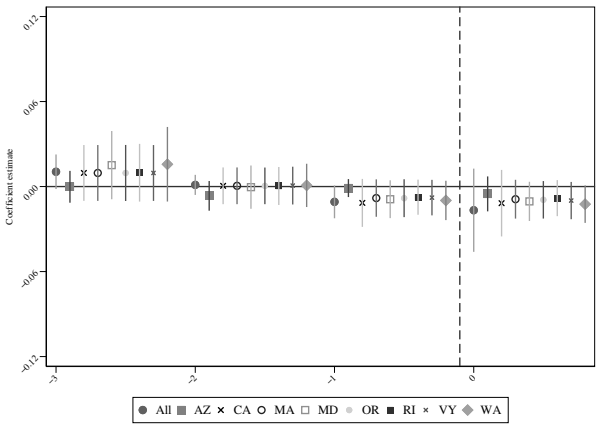



Panel A: Claims

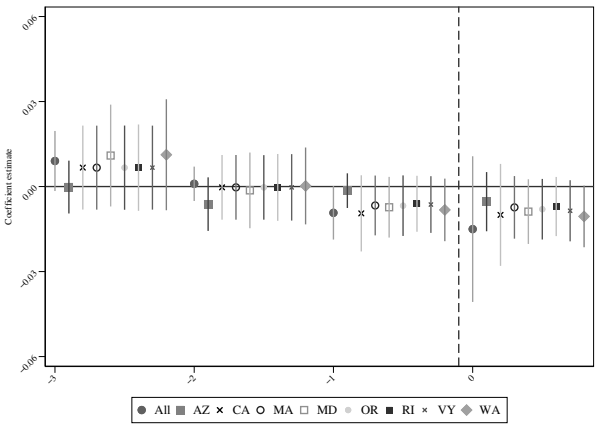
Panel B: Patients

Notes: This figure reports results of a regression of mental healthcare utilization on indicators for time-to-paid sick leave adoption and other controls including mandate-adopting states that are observed -3, -2, and -1 years prior to mandate adoption and the year of adoption (i.e. Arizona, Colorado, Maryland, New Jersey, New Mexico, New York, Rhode Island, and Washington) and states that do not adopt or announce a paid sick leave mandate by November, 2025. Outcomes are log transformed. The unit of observation is a state in a year-month. Connecticut and the District of Columbia are excluded from the sample as these jurisdictions are treated in all years 2015-2022. We utilize the Gardner (2022) two-stage difference-in-differences procedure. All regressions estimated with ordinary least squares and control for time-varying state characteristics, state fixed effects, and period (month-year) fixed effects. There is no omitted category in the Gardner (2022) procedure event-study. For treated states, observations more than four years pre-mandate or four years post-mandate are excluded. Data are weighted by the state population per state-year. Coefficient estimates are reported in circles and 95% confidence intervals that account for within-state clustering are reported with vertical lines.

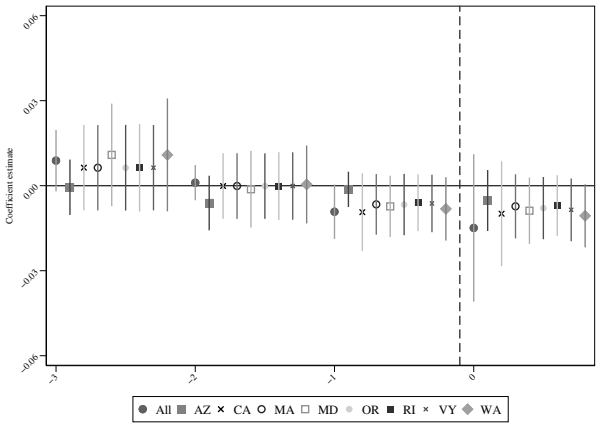
Figure A10: The effect of a state paid sick leave mandate on mental healthcare prescriptions using an event-study utilizing a two-stage difference-in-differences procedure in a balanced sample of treatment states sequentially excluding each mandate-adopting state: IQVIA claims database 2015-2022




Panel A: Claims



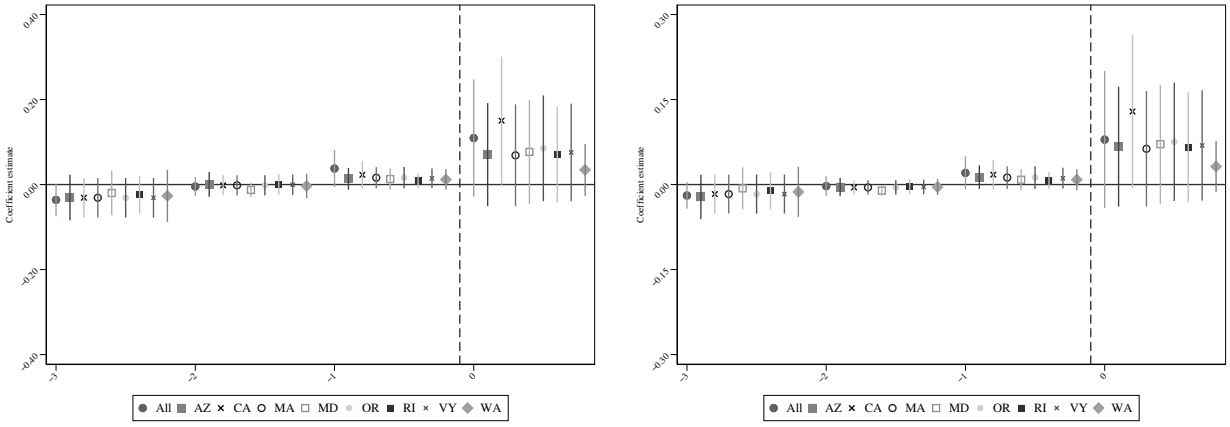

Panel B: Patients




Panel C: Prescription days

Notes: This figure reports results of a regression of mental healthcare utilization on indicators for time-to-paid sick leave adoption and other controls including mandate-adopting states that are observed -3, -2, and -1 years prior to mandate adoption and the year of adoption (i.e. Arizona, Colorado, Maryland, New Jersey, New Mexico, New York, Rhode Island, and Washington) and states that do not adopt or announce a paid sick leave mandate by November, 2025. Outcomes are log transformed. The unit of observation is a state in a year-month. Connecticut and the District of Columbia are excluded from the sample as these jurisdictions are treated in all years 2015-2022. We utilize the Gardner (2022) two-stage difference-in-differences procedure. All regressions estimated with ordinary least squares and control for time-varying state characteristics, state fixed effects, and period (month-year) fixed effects. There is no omitted category in the Gardner (2022) procedure event-study. For treated states, observations more than four years pre-mandate or four years post-mandate are excluded. Data are weighted by the state population per state-year. Coefficient estimates are reported in circles and 95% confidence intervals that account for within-state clustering are reported with vertical lines.

Figure A11: The effect of a state paid sick leave mandate on outpatient mental healthcare utilization using an event-study utilizing a two-stage difference-in-differences procedure in a balanced sample of treatment states sequentially excluding each mandate-adopting state: IQVIA claims database 2015-2022

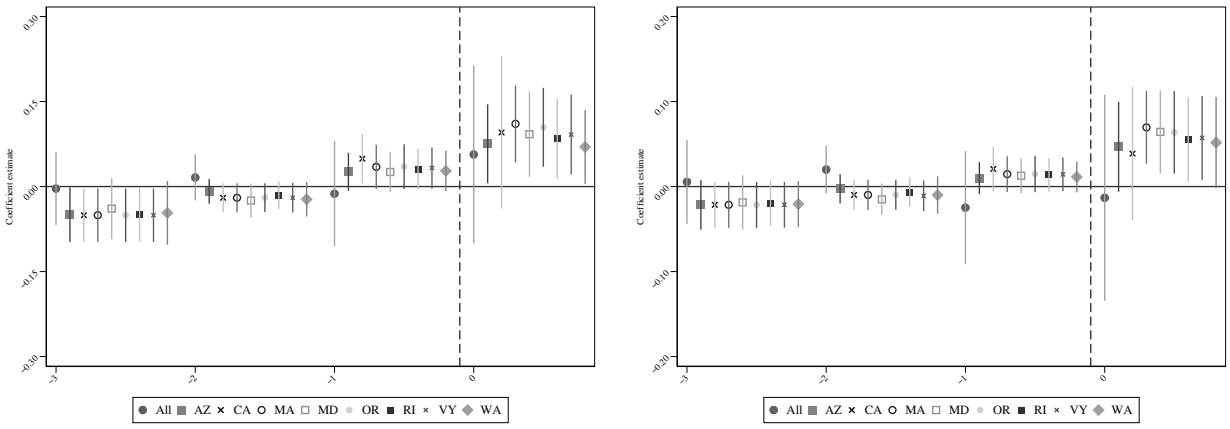



Panel A: Claims

Panel B: Patients

Notes: This figure reports results of a regression of mental healthcare utilization on indicators for time-to-paid sick leave adoption and other controls including mandate-adopting states that are observed -3, -2, and -1 years prior to mandate adoption and the year of adoption (i.e. Arizona, Colorado, Maryland, New Jersey, New Mexico, New York, Rhode Island, and Washington) and states that do not adopt or announce a paid sick leave mandate by November, 2025. Outcomes are log transformed. The unit of observation is a state in a year-month. Connecticut and the District of Columbia are excluded from the sample as these jurisdictions are treated in all years 2015-2022. We utilize the Gardner (2022) two-stage difference-in-differences procedure. All regressions estimated with ordinary least squares and control for time-varying state characteristics, state fixed effects, and period (month-year) fixed effects. There is no omitted category in the Gardner (2022) procedure event-study. For treated states, observations more than four years pre-mandate or four years post-mandate are excluded. Data are weighted by the state population per state-year. Coefficient estimates are reported in circles and 95% confidence intervals that account for within-state clustering are reported with vertical lines.

Figure A12: The effect of a state paid sick leave mandate on intpatient mental healthcare utilization using an event-study utilizing a two-stage difference-in-differences procedure in a balanced sample of treatment states sequentially excluding each mandate-adopting state: IQVIA claims database 2015-2022

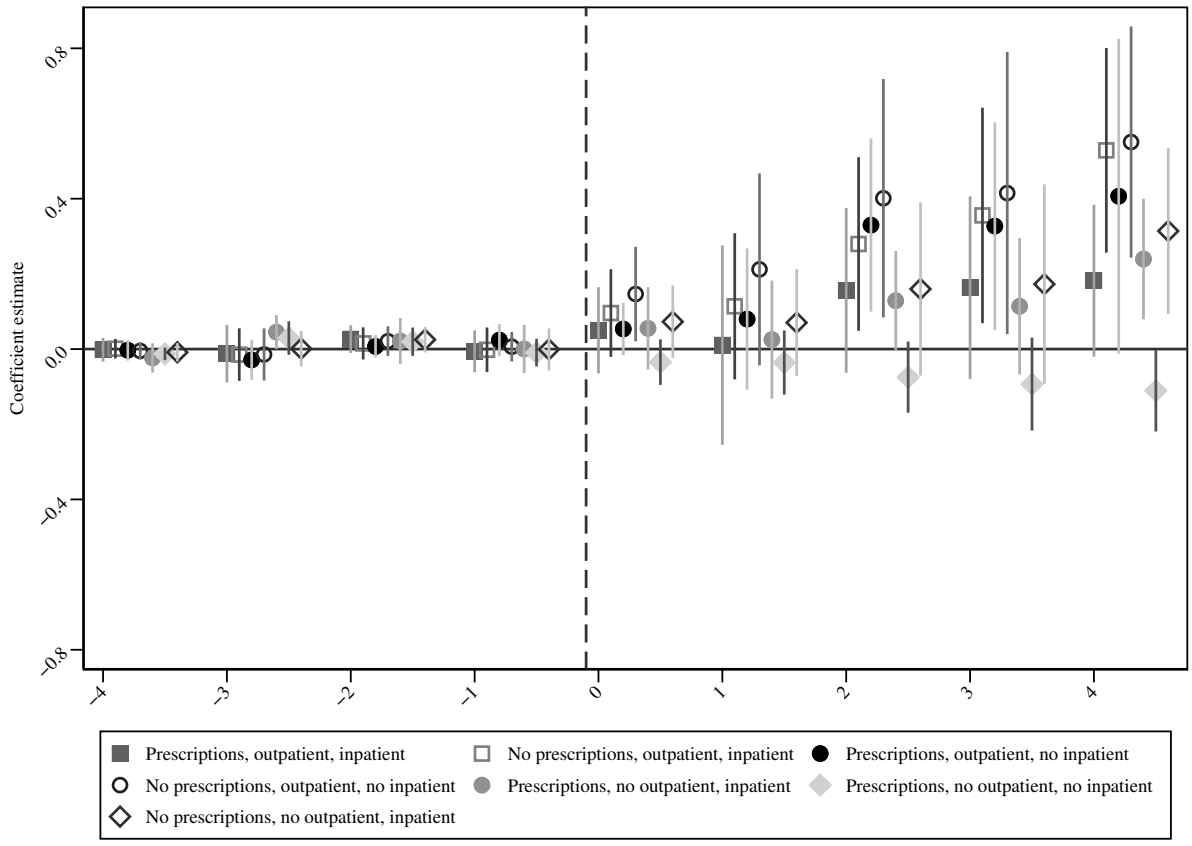



Panel A: Claims

Panel B: Patients

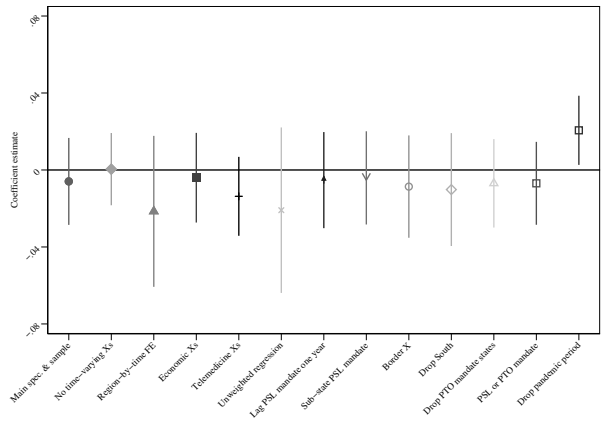
Notes: This figure reports results of a regression of mental healthcare utilization on indicators for time-to-paid sick leave adoption and other controls including mandate-adopting states that are observed -3, -2, and -1 years prior to mandate adoption and the year of adoption (i.e. Arizona, Colorado, Maryland, New Jersey, New Mexico, New York, Rhode Island, and Washington) and states that do not adopt or announce a paid sick leave mandate by November, 2025. Outcomes are log transformed. The unit of observation is a state in a year-month. Connecticut and the District of Columbia are excluded from the sample as these jurisdictions are treated in all years 2015-2022. We utilize the Gardner (2022) two-stage difference-in-differences procedure. All regressions estimated with ordinary least squares and control for time-varying state characteristics, state fixed effects, and period (month-year) fixed effects. There is no omitted category in the Gardner (2022) procedure event-study. For treated states, observations more than four years pre-mandate or four years post-mandate are excluded. Data are weighted by the state population per state-year. Coefficient estimates are reported in circles and 95% confidence intervals that account for within-state clustering are reported with vertical lines.

Figure A13: The effect of a state paid sick leave mandate on substance use disorder utilization substitution measures utilizing a two-stage difference-in-differences event-study: IQVIA claims database 2015-2022

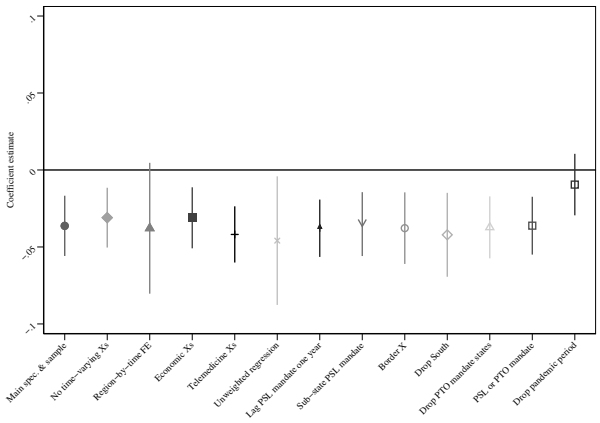



Notes: This figure reports results of a regression of substance use disorder utilization on indicators for time-to-paid sick leave adoption and other controls. Outcomes are log transformed. The unit of observations is a state in a year-month. Connecticut and the District of Columbia are excluded from the sample as these jurisdictions are treated in all years 2015-2022. We utilize the Gardner (2022) two-stage difference-in-differences procedure. All regressions estimated with ordinary least squares and control for time-varying state characteristics, state fixed effects, and period (month-year) fixed effects. There is no omitted category in the Gardner (2022) procedure event-study. For treated states, observations more than four years pre-mandate or four years post-mandate are excluded. Data are weighted by the state population per state-year. Coefficient estimates are reported in circles and 95% confidence intervals that account for within-state clustering are reported with vertical lines.

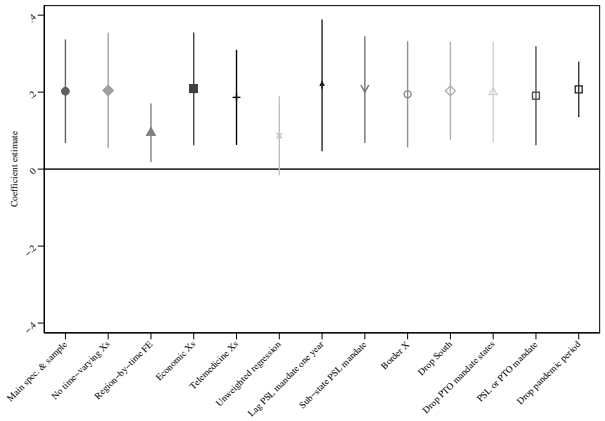
Figure A14: The effect of a state paid sick leave mandate on mental healthcare utilization using different samples & specifications utilizing a two-stage difference-in-differences procedure: IQVIA claims database 2015-2022



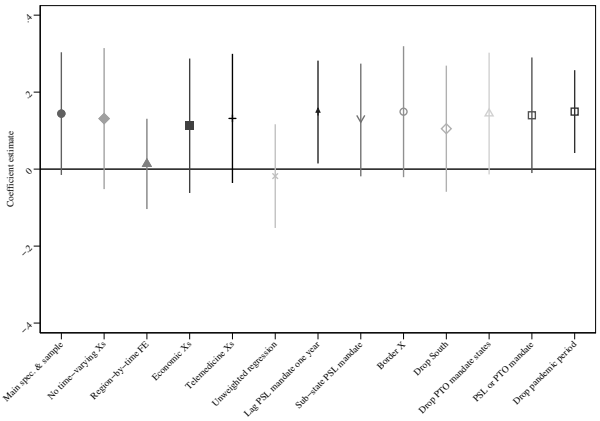

Panel A: Any




Panel B: Prescriptions



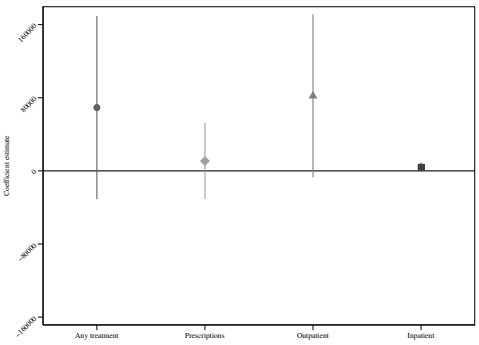

Panel C: Outpatient



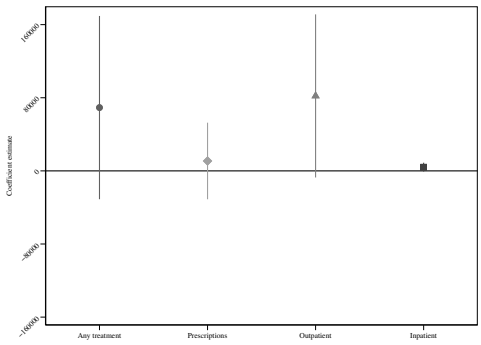

Panel D: Inpatient

Notes: Spec. = specifications. FE = fixed effects. This figure reports results of a regression of mental healthcare utilization on a state paid sick leave mandate and other controls using different samples and specifications to assess the robustness of our findings. Outcomes are patient counts and are log transformed. The unit of observations is a state in a year-month. Connecticut and the District of Columbia are excluded from the sample as these jurisdictions are treated in all years 2015-2022. We utilize the Gardner (2022) two-stage difference-in-differences procedure. All regressions estimated with ordinary least squares and control for time-varying state characteristics, state fixed effects, and period (month-year) fixed effects unless otherwise noted. Data are weighted by the state population per state-year unless otherwise noted. Coefficient estimates are reported in shapes and 95% confidence intervals that account for within-state clustering are reported with vertical lines.

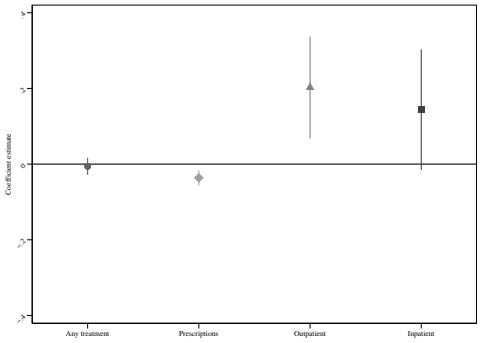
Figure A15: The effect of a state paid sick leave mandate on mental healthcare utilization not logging the outcome utilizing a two-stage difference-in-differences procedure: IQVIA claims database 2015-2022




Panel A: Ordinary least squares



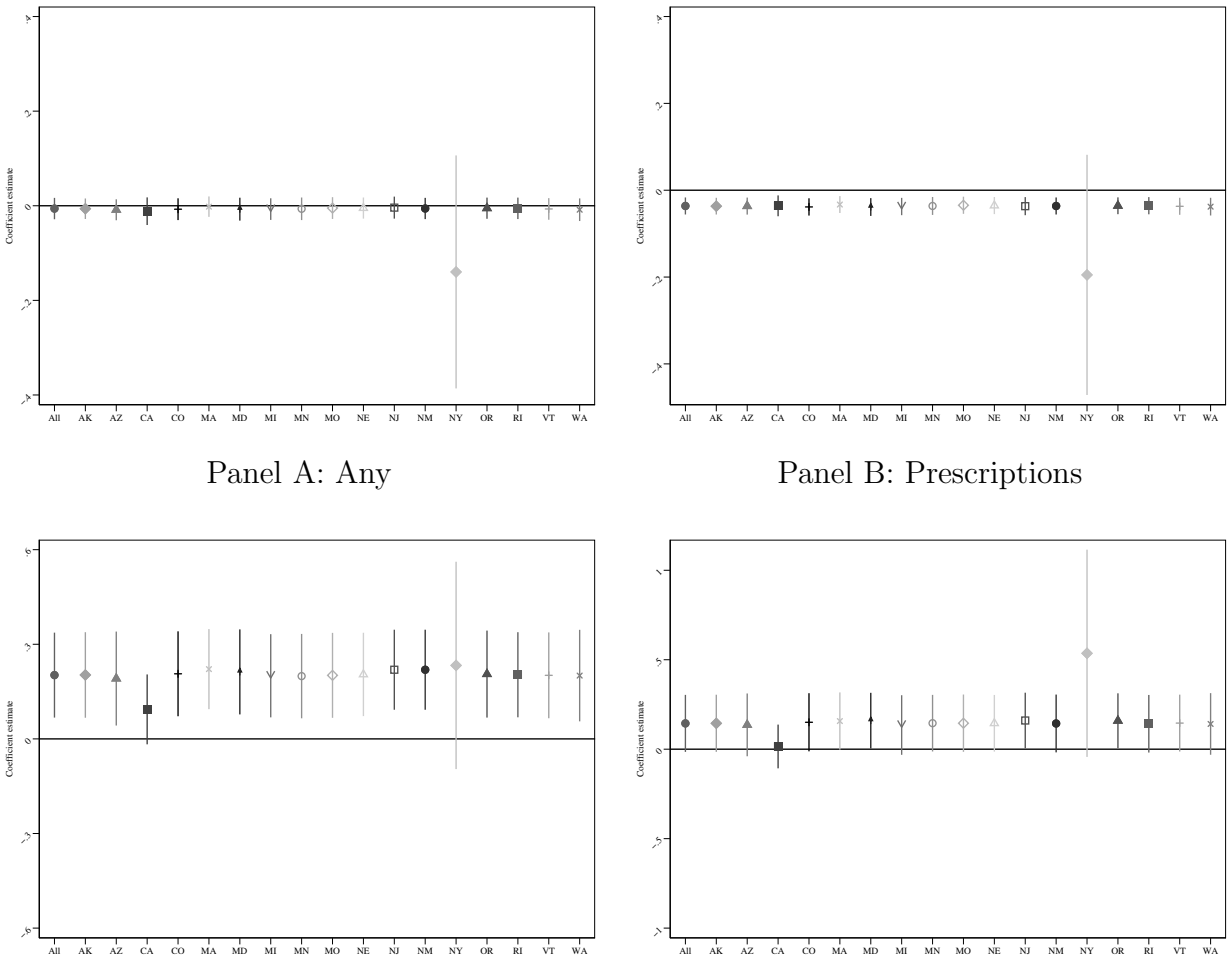

Panel B: Poisson




Panel C: Inverse hyperbolic sign

Notes: This figure reports results of a regression of mental healthcare utilization on a state paid sick leave mandate and other controls. Outcomes are patient counts. In panel A, outcomes are not logged and are scaled by 100,000 for readability. In panel B, a Poisson specification is employed, treatment effects are presented in terms of differences in linear predictions. In panel C, an inverse hyperbolic sign transformation is applied to the patient counts. The unit of observations is a state in a year-month. Connecticut and the District of Columbia are excluded from the sample as these jurisdictions are treated in all years 2015-2022. We utilize the Gardner (2022) two-stage difference-in-differences procedure in panels A and C, and Wooldridge (2023) in panel B. In panel A, we scale patient counts by 100,000 for readability. All regressions control for time-varying state population, state characteristics, state fixed effects, and period (month-year) fixed effects unless otherwise noted. Data are weighted by the state population per state-year unless otherwise noted. Coefficient estimates are reported in shapes and 95% confidence intervals that account for within-state clustering are reported with vertical lines.

Figure A16: The effect of a state paid sick leave mandate on mental healthcare utilization utilizing a two-stage difference-in-differences procedure sequentially excluding each state that adopts or announces a paid sick leave mandate by July, 2025: IQVIA claims database 2015-2022




Panel A: Any

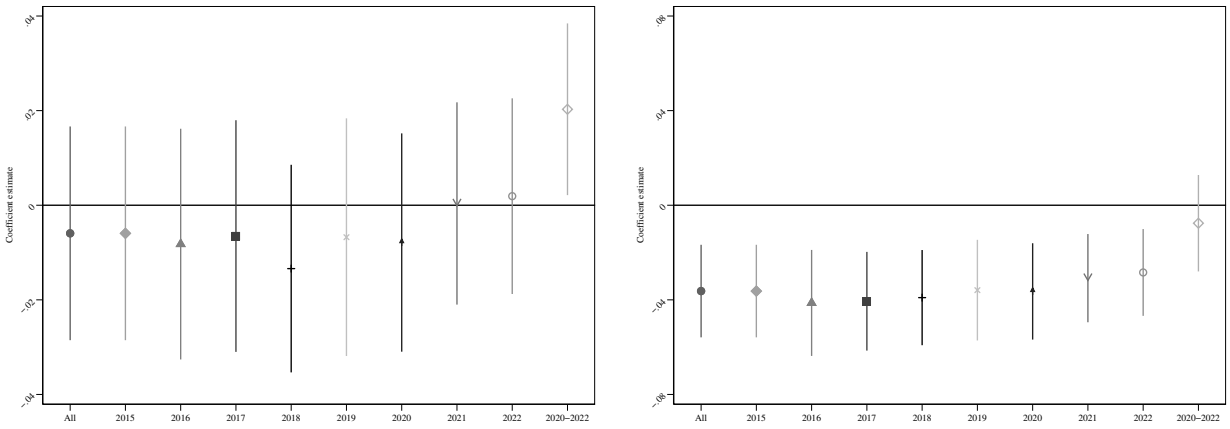
Panel B: Prescriptions

Panel C: Outpatient

Panel D: Inpatient

Notes: This figure reports results of a regression of mental healthcare utilization on a state paid sick leave mandate and other controls sequentially excluding each state that adopts or announces a paid sick leave mandate by November, 2024 from the sample, the excluded state is listed in the legend. Outcomes are patient counts and are log transformed. The unit of observations is a state in a year-month. Connecticut and the District of Columbia are excluded from the sample as these jurisdictions are treated in all years 2015-2022. We utilize the Gardner (2022) two-stage difference-in-differences procedure. All regressions estimated with ordinary least squares and control for time-varying state characteristics, state fixed effects, and period (month-year) fixed effects. Data are weighted by the state population per state-year. Coefficient estimates are reported in shapes and 95% confidence intervals that account for within-state clustering are reported with vertical lines.

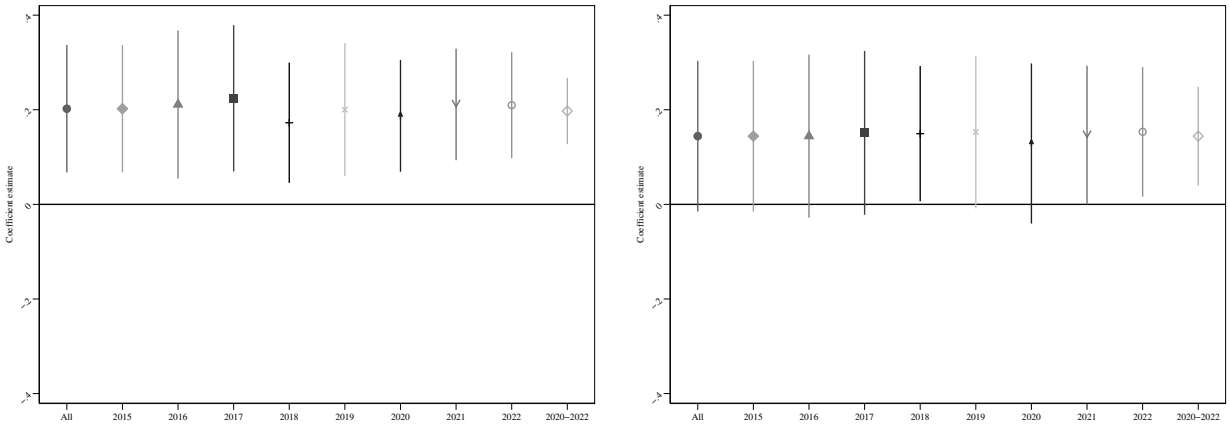
Figure A17: The effect of a state paid sick leave mandate on mental healthcare utilization utilizing a two-stage difference-in-differences procedure sequentially excluding each year: IQVIA claims database 2015-2022




Panel A: Any




Panel B: Prescriptions



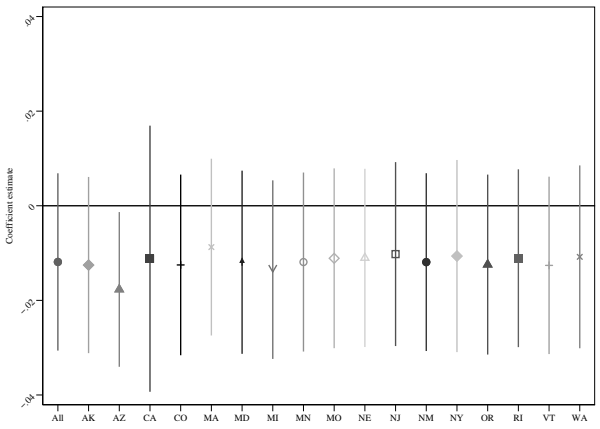

Panel C: Outpatient



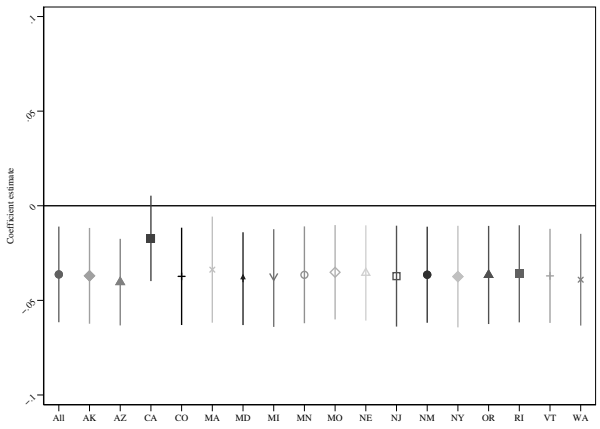

Panel D: Inpatient

Notes: This figure reports results of a regression of mental healthcare utilization on a state paid sick leave mandate and other controls sequentially excluding each year from the sample, the excluded year is listed in the legend. Outcomes are patient counts and are log transformed. The unit of observations is a state in a year-month. Connecticut and the District of Columbia are excluded from the sample as these jurisdictions are treated in all years 2015-2022. We utilize the Gardner (2022) two-stage difference-in-differences procedure. All regressions estimated with ordinary least squares and control for time-varying state characteristics, state fixed effects, and period (month-year) fixed effects. Data are weighted by the state population per state-year. Coefficient estimates are reported in shapes and 95% confidence intervals that account for within-state clustering are reported with vertical lines.

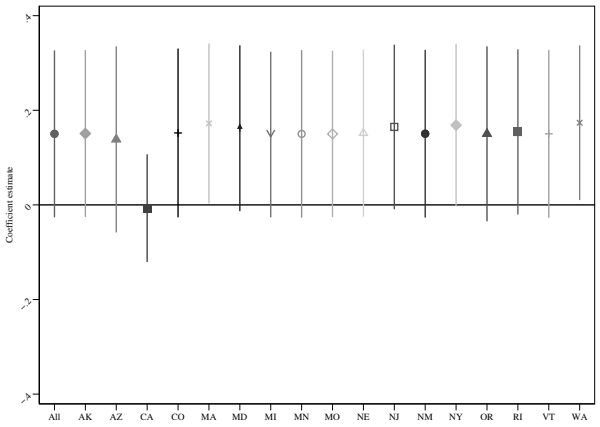
Figure A18: The effect of a state paid sick leave mandate on mental healthcare utilization sequentially excluding each state that adopts or announces a paid sick leave mandate by November, 2024 using the Callaway and Sant'Anna (2021) procedure: IQVIA claims database 2015-2022



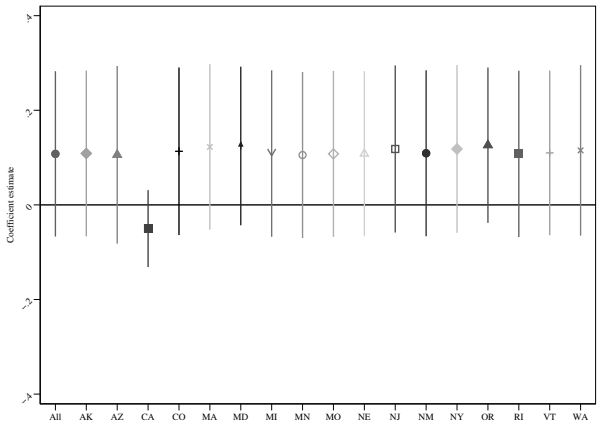

Panel A: Any




Panel B: Prescriptions



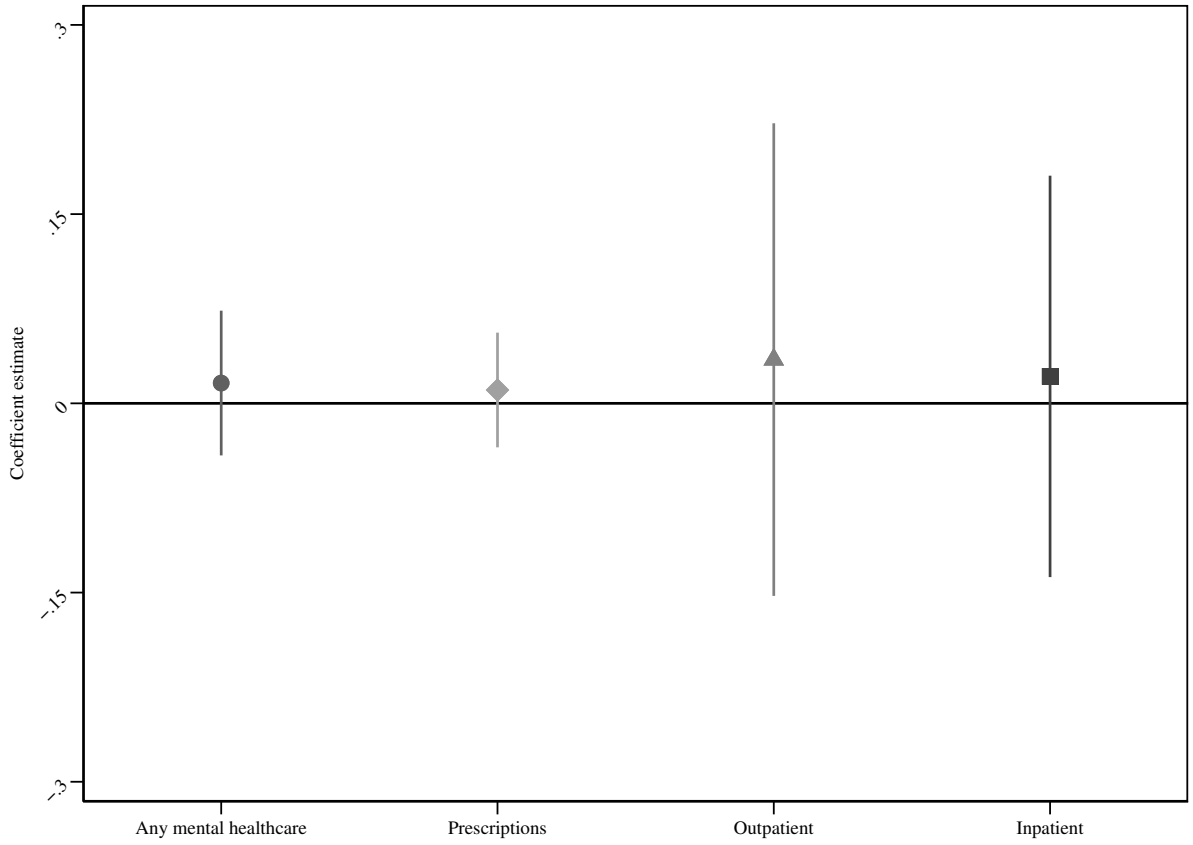

Panel C: Outpatient




Panel D: Inpatient

Notes: This figure reports results of a regression of mental healthcare utilization on a state paid sick leave mandate and other controls sequentially excluding each state that adopts or announces a paid sick leave mandate by November, 2024 from the sample, the excluded state is listed in the legend. Outcomes are patient counts and are log transformed. The unit of observations is a state in a year-month. Connecticut and the District of Columbia are excluded from the sample as these jurisdictions are treated in all years 2015-2022. We utilize the Callaway and Sant'Anna (2021) difference-in-differences procedure. State and time fixed effects are accounted for in the procedure. Data are weighted by the state population per state-year. Coefficient estimates are reported in shapes and 95% confidence intervals that account for within-state clustering are reported with vertical lines.

Figure A19: The spillover effect of a state paid sick leave mandate on mental healthcare utilization in border states utilizing a two-stage difference-in-differences procedure: IQVIA claims database 2015-2022




Notes: This figure reports results of a regression of mental healthcare utilization on an indicator variable for bordering a state with a paid sick leave mandate and other controls. States that adopt a PSL mandate by December 2022 are excluded from the sample. Outcomes are patient counts and are log transformed. The unit of observations is a state in a year-month. Connecticut and the District of Columbia are excluded from the sample as these jurisdictions are treated in all years 2015-2022. We utilize the Gardner (2022) two-stage difference-in-differences procedure. All regressions estimated with ordinary least squares and control for time-varying state characteristics, state fixed effects, and period (month-year) fixed effects. Data are weighted by the state population per state-year. Coefficient estimates are reported in shapes and 95% confidence intervals that account for within-state clustering are reported with vertical lines.

Table A1: ICD codes for identifying mental health and substance use disorder visits

| Categories | Example codes |
|---|---|
| Mental health | |
| **ICD-9 codes** | |
| (general mental health) | 295, 296, 297, 298, 300, 301, 302, 306<br>307, 308, 309, 311, 312, 313, 314, 316 |
| (suicide-related) | E950, E951, E952, E953, E954,<br>E955, E956, E957, E958, V6284 |
| **ICD-10 codes** | |
| (general mental health) | F20, F21, F22, F23, F24, F25, F28, F29<br>F30, F31, F32, F33, F34, F39, F40, F41<br>F42, F43, F44, F45, F48, F50, F53, F54,<br>F59, F60, F63, F68, F69, F84, F90, F91,<br>F93, F94, F95, F98, F99 |
| (suicide-related) | R45851, T1491, X71-X83,<br>T36-T50 with the 6th character of 2†<br>T51–T65 with the 6th character of 2††<br>T71 with the 6th character of 2 |
| Substance use disorder | |
| **ICD-9 codes** | 291, 292, 303, 304, 305 |
| **ICD-10 codes** | F10, F11, F12, F13, F14, F15, F16, F18, F19 |

Notes: Authors' analysis of the IQVIA 2015-2022 database.
†Except for T369, T379, T399, T414, T427, T439, T459, T479, and T499, which are included if the 5th character is 2.
††Except for T519, T529, T539, T549, T569, T579, T580, T581, T589, T599, T609, T610, T611, T619, T629, T639, T640, T648, and T659, which are included if the 5th character is 2.

Table A2: Mental health and substance use disorder medication categories & examples

| **Categories** | **Example medications** |
|---|---|
| Mental health | |
| Attention deficit disorder - other | atomoxetine, clonidine, guanfacine |
| Antidepressant | amitriptyline, bupropion, escitalopram |
| Anti-anxiety - other | ataraxoid, buspirone, hydroxyzine |
| Anticonvulsants | carbamazepine, gabapentin, lamotrigine |
| Antipsychotic-1st gen | chlorpromazine, fluphenazine, haloperidol |
| Antipsychotic-2nd gen | aripiprazole, clozapine, olanzapine |
| Benzodiazepines | alprazolam, clonazepam, diazepam |
| Hypnotic - other | eszopiclone, ramelteon, zaleplon |
| Injectable antipsychotic | fluphenazine decanoate, haloperidol lactate |
| Lithium | lithium, lithium aspartate, lithium carbonate |
| Stimulants | adderall, amphetamine, methamphetamine |
| Substance use disorder | |
| Buprenorphine | buprenorphine |
| Naltrexone | naltrexone |
| Acamprosate | acamprosate |
| Disulfiram | disulfiram |

Notes: Authors' analysis of the IQVIA 2015-2022 database.

Table A3: Mental health and substance use disorder service categories & examples

| Categories | Place of service descriptions |
|---|---|
| Outpatient | Office |
| | hospital outpatient |
| | non-residential substance abuse treatment facility |
| | clinic - freestanding |
| | on campus - outpatient hospital |
| | community mental health center |
| | independent clinic |
| | clinic - community mental health center |
| | special facility - other |
| | federally qualified health center |
| | off campus - outpatient hospital |
| | clinic - other |
| | clinic - federally qualified health center (FQHC) |
| | psychiatric facility - partial hospitalization |
| | public health clinic |
| | clinic - comprehensive outpatient rehabilitation facility (CORF) |
| | clinic - outpatient rehabilitation facility (ORF) |
| | rural health clinic |
| | comprehensive outpatient rehabilitation facility |
| | clinic - rural health |
| | school |
| | mobile unit |
| | tribal 638 free-standing facility |
| | Indian health service free-standing facility |
| Inpatient | Inpatient hospital |
| | hospital inpatient (including Medicare Part A) |
| | inpatient psychiatric facility |
| | comprehensive inpatient rehabilitation facility |
| | hospital inpatient (Medicare Part B only) |

Notes: Authors' analysis of the IQVIA 2015-2022 database.

Table A4: Summary statistics: IQVIA database January, 2015 - February, 2020 (i.e., the pre-COVID-19 pandemic period)

| Sample: | All states | States that adopt a mandate, pre-mandate | States that do not adopt a mandate |
|---|---|---|---|
| **Mental healthcare use (,000s)** | | | |
| Any use (claims) | 1841 | 1616 | 1641 |
| Any use (patients) | 872 | 752 | 790 |
| Prescription (claims) | 1373 | 1178 | 1248 |
| Prescriptions (patients) | 764 | 650 | 705 |
| Prescription (days) | 22333 | 18996 | 20636 |
| Outpatient (claims) | 425 | 402 | 352 |
| Outpatient (patients) | 191 | 179 | 163 |
| Inpatient (claims) | 44 | 36 | 41 |
| Inpatient (patients) | 10 | 9 | 9 |
| **State-level variables** | | | |
| Paid sick leave mandate | 0.2 | 0 | 0 |
| Paid time off mandate | 0.01 | 0.0 | 0.001 |
| Family & medical leave policy | 0.2 | 0.3 | 0 |
| Temporary Aid to Needy Families* | 654 | 810 | 488 |
| 0-20 years | 0.3 | 0.3 | 0.3 |
| 21-39 years | 0.3 | 0.3 | 0.3 |
| 40-64 years | 0.3 | 0.3 | 0.3 |
| 65+ years | 0.2 | 0.2 | 0.2 |
| Male | 0.5 | 0.5 | 0.5 |
| Female | 0.5 | 0.5 | 0.5 |
| White | 0.8 | 0.8 | 0.8 |
| Black | 0.1 | 0.1 | 0.2 |
| Other race | 0.1 | 0.1 | 0.1 |
| Hispanic | 0.2 | 0.2 | 0.2 |
| College degree | 0.3 | 0.3 | 0.3 |
| State population | 14.7 | 11.7 | 12.2 |
| Observations** | 3038 | 734 | 1984 |

Notes: This table reports summary statistics. The unit of observation is a state in a month-year. Connecticut and the District of Columbia are excluded from the sample as these jurisdictions are treated in all years 2015- February, 2022. Data are weighted by the state population.

* Temporary Aid to Needy Families monthly benefit ($) for a family of four.

**Columns (2) and (3) do not sum to column (1) – 3038 vs. 2718 – as column (2) only includes observations in paid sick leave mandate-adopting stats prior to mandate implementation (thus, all 'treated' units are excluded).

Table A5: States that contribute to each lead and lag variable in the event-study: National Partnership of Women & Families (2023) and A Better Balance (2025)

| Lead & lag ↓ | States |
|---|---|
| -4 | AK, CO, MD, MI, MN, MO, NE, NJ, NM, NY, RI |
| -3 | AK, AZ, CO, MD, MI, MN, MO, NE, NJ, NM, NY, RI, WA |
| -2 | AZ, CO, MD, MN, NJ, NM, NY, RI, VT, WA |
| -1 | AZ, CA, CO, MA, MD, NJ, NM, NY, OR, RI, VT, WA |
| 0 | AZ, CA, CO, MA, MD, NJ, NM, NY, OR, RI, VT, WA |
| +1 | AZ, CA, CO, MA, MD, NJ, NY, OR, RI, VT, WA |
| +2 | AZ, CA, MA, MD, NJ, OR, RI, VT, WA |
| +3 | AZ, CA, MA, MD, NJ, OR, RI, VT, WA |
| +4 | AZ, CA, MA, MD, NJ, OR, RI, VT, WA |

Notes: This table reports the states that contribute to identification of each lead and lag variable. The data sources are National Partnership of Women & Families (2023) and A Better Balance (2025). State paid sick leave mandates adopted or announced as of November, 2025.

Table A6: The effect of a state paid sick leave mandate on outpatient mental health treatment use that involves and does not involve tele-medicine utilizing a two-stage difference-in-differences procedure: IQVIA 2015-2022

| Outcome variable →: | No tele-medicine | Tele-medicine |
|---|---|---|
| State paid sick leave mandate | 0.190** | 1.069*** |
| | (0.076) | (0.178) |
| Pre-treatment mean (,000s), level | 171 | 6 |
| Observations | 4704 | 4691 |

Notes: This table reports coefficient estimates from a regression of any service utilization (patient counts) on state paid sick leave mandates and other controls. The unit of observation is a state in a month in a year. Connecticut and the District of Columbia are excluded from the sample as they are always treated. Outcomes are log transformed. The regression includes state characteristics, state fixed effects, and time (month-year) fixed effects. Data are weighted by the state population. Regressions are estimated with a two-stage difference-in-differences procedure proposed by Gardner (2022). Standard errors that account for within-state clustering are reported in parentheses.
***,**, and * = statistically different from zero at the 1%, 5%, and 10% level.
The number of observations in the tele-medicine sample is smaller than the main sample as there are some state-month-year pairs with no patients with tele-medicine claims.

Table A7: Procedure and modifier codes for identifying telehealth visits, part 1

| **Type** | **Procedure codes or modifiers** | **Description** |
|---|---|---|
| Telephone | G2025 | RHC/FQHC distant site telehealth service |
| Telephone | 99441 | Non-Face-to-Face Telephone E&M Services, 5-10 mins |
| Telephone | 99442 | Non-Face-to-Face Telephone E&M Services, 11-20 mins |
| Telephone | 99443 | Non-Face-to-Face Telephone E&M Services, 21-30 mins |
| Telephone | 98966 | A nonphysician provider telephone E&M services, 5-10 mins |
| Telephone | 98967 | A nonphysician provider telephone E&M services, 11-20 mins |
| Telephone | 98968 | A nonphysician provider telephone E&M services, 21-30 mins |
| Online E&M | 99421 | Non-Face-to-Face On-Line Digital E&M Service, 5-10 mins mins |
| Online E&M | 99422 | Non-Face-to-Face On-Line Digital E&M Service, 11-20 mins mins |
| Online E&M | 99423 | Non-Face-to-Face On-Line Digital E&M Service, 21+ mins mins |
| Online E&M | G2061 | Online Assessment of established patient by Qualified Nonphysician Healthcare Professional, 5-10 mins |
| Online E&M | G2062 | Online Assessment of established patient by Qualified Nonphysician Healthcare Professional, 11-20 mins |
| Online E&M | G2063 | Online Assessment of established patient by Qualified Nonphysician Healthcare Professional, 21+ mins |
| Online E&M | 98970 | Replaced code G2061 in 2021 |
| Online E&M | 98971 | Replaced code G2062 in 2021 |
| Online E&M | 98972 | Replaced code G2063 in 2021 |

Notes: E&M = evaluation and management. Authors' analysis of the IQVIA 2015-2022 database.

Table A8: Procedure and modifier codes for identifying telehealth visits, part 2

| Type | Procedure codes or modifiers | Description |
|---|---|---|
| Online check-in | G2012 | Brief communication technology-based service, e.g. virtual check-in, to an established patient, effective 2021 |
| Online check-in | G2251 | Brief communication technology-based service by a qualified health care professional who cannot report evaluation and management services, not originating from a related E/M service provided within the previous 7 days nor leading to a service or procedure within the next 24 hours or soonest available appointment; 5-10 minutes of medical discussion, effective 2021 |
| Online check-in | G2252 | Brief communication technology-based service by a physician or other qualified health care professional who can report evaluation and management services, not originating from a related E/M service provided within the previous 7 days nor leading to an E/M service or procedure within the next 24 hours or soonest available appointment; 11-20 minutes of medical discussion, effective 2021 |
| Telephone, modifier | 93 | Modifier for telephone, effective 2022 |
| Telehealth, modifier | GT | Modifier code for synchronous telemedicine |
| Telehealth, modifier | GQ | Modifier code for asynchronous telemedicine |
| Telehealth, modifier | 95 | Modifier code for synchronous telemedicine |
| Telehealth, modifier | G0 | Modifier code for synchronous telemedicine for acute stroke |

Notes: Authors' analysis of the IQVIA 2015-2022 database.

Table A9: The effect of a state paid sick leave mandate on any mental health treatment use utilizing alternative difference-in-differences estimators: IQVIA 2015-2022

| Outcome variable →: | Claims | Patients |
|---|---|---|
| Gardner (2022) | 0.014 | -0.006 |
| | (0.018) | (0.012) |
| Callaway and Sant'Anna (2021) | 0.005 | -0.012 |
| | (0.014) | (0.010) |
| Wooldridge (2023) | 0.014 | -0.006 |
| | (0.014) | (0.010) |
| Borusyak et al. (2024) | 0.024** | 0.000 |
| | (0.011) | (0.008) |
| Two-way fixed effects | 0.032* | 0.008 |
| | (0.016) | (0.010) |
| Pre-treatment mean (,000s), level | 1592 | 735 |
| Observations | 4704 | 4704 |

Notes: This table reports coefficient estimates from a regression of any service utilization on state paid sick leave mandates and other controls. The unit of observation is a state in a month in a year. Connecticut and the District of Columbia are excluded from the sample as they are always treated. Outcomes are log transformed. The regression includes time-varying state characteristics, state fixed effects, and time (month-year) fixed effects. In application of the Callaway and Sant'Anna (2021) procedure we do not control for time-varying state characteristics. Data are weighted by the state population. In the Callaway and Sant'Anna (2021) procedure we use both the not-yet-treated and never-treated states as the comparison group, and the Sant'Anna and Zhao (2020) doubly robust difference-in-differences estimator based on stabilized inverse probability weighting and ordinary least squares. Standard errors that account for within-state clustering are reported in parentheses. ***,**, and * = statistically different from zero at the 1%, 5%, and 10% level.

Table A10: The effect of a state paid sick leave mandate on dispensed mental health treatment prescriptions utilizing alternative difference-in-differences estimators: IQVIA 2015-2022

| Outcome variable →: | Prescriptions | Patients | Prescription days |
|---|---|---|---|
| Gardner (2022) | -0.043*** | -0.036*** | -0.036*** |
| | (0.011) | (0.010) | (0.010) |
| Callaway and Sant'Anna (2021) | -0.040*** | -0.036*** | -0.036*** |
| | (0.014) | (0.013) | (0.013) |
| Wooldridge (2023) | -0.040*** | -0.036*** | -0.036*** |
| | (0.014) | (0.013) | (0.013) |
| Borusyak et al. (2024) | -0.036*** | -0.031*** | -0.031*** |
| | (0.009) | (0.008) | (0.008) |
| Two-way fixed effects | -0.014 | -0.012 | -0.011 |
| | (0.012) | (0.009) | (0.009) |
| Pre-treatment mean (,000s), level | 1154 | 636 | 18600 |
| Observations | 4704 | 4704 | 4704 |

Notes: This table reports coefficient estimates from a regression of any service utilization on state paid sick leave mandates and other controls. The unit of observation is a state in a month in a year. Connecticut and the District of Columbia are excluded from the sample as they are always treated. Outcomes are log transformed. The regression includes time-varying state characteristics, state fixed effects, and time (month-year) fixed effects. In application of the Callaway and Sant'Anna (2021) procedure we do not control for time-varying state characteristics. Data are weighted by the state population. In the Callaway and Sant'Anna (2021) procedure we use both the not-yet-treated and never-treated states as the comparison group, and the Sant'Anna and Zhao (2020) doubly robust difference-in-differences estimator based on stabilized inverse probability weighting and ordinary least squares.
***,**, and * = statistically different from zero at the 1%, 5%, and 10% level.

Table A11: The effect of a state paid sick leave mandate on outpatient mental health treatment service use utilizing alternative difference-in-differences estimators: IQVIA 2015-2022

| Outcome variable →: | Claims | Patients |
|---|---|---|
| Gardner (2022) | 0.221*** | 0.202*** |
| | (0.060) | (0.069) |
| Callaway and Sant'Anna (2021) | 0.163** | 0.150* |
| | (0.079) | (0.090) |
| Wooldridge (2023) | 0.221*** | 0.202*** |
| | (0.036) | (0.028) |
| Borusyak et al. (2024) | 0.230*** | 0.204*** |
| | (0.041) | (0.038) |
| Two-way fixed effects | 0.147** | 0.093** |
| | (0.056) | (0.045) |
| Pre-treatment mean (,000s), level | 402 | 175 |
| Observations | 4704 | 4704 |

Notes: This table reports coefficient estimates from a regression of any service utilization on state paid sick leave mandates and other controls. The unit of observation is a state in a month in a year. Connecticut and the District of Columbia are excluded from the sample as they are always treated. Outcomes are log transformed. The regression includes time-varying state characteristics, state fixed effects, and time (month-year) fixed effects. In application of the Callaway and Sant'Anna (2021) procedure we do not control for time-varying state characteristics. Data are weighted by the state population. In the Callaway and Sant'Anna (2021) procedure we use both the not-yet-treated and never-treated states as the comparison group, and the Sant'Anna and Zhao (2020) doubly robust difference-in-differences estimator based on stabilized inverse probability weighting and ordinary least squares.
***,**, and * = statistically different from zero at the 1%, 5%, and 10% level.

Table A12: The effect of a state paid sick leave mandate on inpatient mental health treatment service use utilizing alternative difference-in-differences estimators IQVIA 2015-2022

| Outcome variable →: | Claims | Patients |
|---|---|---|
| Gardner (2022) | 0.228** | 0.144* |
| | (0.108) | (0.081) |
| Callaway and Sant'Anna (2021) | 0.166 | 0.108 |
| | (0.126) | (0.089) |
| Wooldridge (2023) | 0.228*** | 0.144*** |
| | (0.040) | (0.036) |
| Borusyak et al. (2024) | 0.230*** | 0.131*** |
| | (0.064) | (0.041) |
| Two-way fixed effects | 0.049 | -0.005 |
| | (0.073) | (0.055) |
| Pre-treatment mean (,000s), level | 35 | 8 |
| Observations | 4704 | 4704 |

Notes: This table reports coefficient estimates from a regression of any service utilization on state paid sick leave mandates and other controls. The unit of observation is a state in a month in a year. Connecticut and the District of Columbia are excluded from the sample as they are always treated. Outcomes are log transformed. The regression includes time-varying state characteristics, state fixed effects, and time (month-year) fixed effects. In application of the Callaway and Sant'Anna (2021) procedure we do not control for time-varying state characteristics. Data are weighted by the state population. In the Callaway and Sant'Anna (2021) procedure we use both the not-yet-treated and never-treated states as the comparison group, and the Sant'Anna and Zhao (2020) doubly robust difference-in-differences estimator based on stabilized inverse probability weighting and ordinary least squares.
***,**, and * = statistically different from zero at the 1%, 5%, and 10% level.

Table A13: The effect of a state paid sick leave mandate on substitution mental health treatment service use utilizing alternative difference-in-differences estimators: IQVIA 2015-2022

| Outcome variable →: | No outpatient & prescriptions | Outpatient no prescriptions | Outpatient & prescriptions |
|---|---|---|---|
| Gardner (2022) | -0.076*** | 0.194*** | 0.168* |
| | (0.023) | (0.049) | (0.092) |
| Callaway and Sant'Anna (2021) | -0.066** | 0.168** | 0.146 |
| | (0.029) | (0.073) | (0.094) |
| Wooldridge (2023) | -0.076*** | 0.194*** | 0.168*** |
| | (0.010) | (0.023) | (0.023) |
| Borusyak et al. (2024) | -0.069*** | 0.186*** | 0.155*** |
| | (0.011) | (0.033) | (0.043) |
| Two-way fixed effects | -0.023* | 0.086** | 0.029 |
| | (0.012) | (0.040) | (0.035) |
| Pre-treatment mean (,000s), level | 742 | 210 | 282 |
| Observations | 784 | 784 | 784 |

Notes: This table reports coefficient estimates from a regression of any service utilization on state paid sick leave mandates and other controls. The unit of observation is a state in a month in a year. Connecticut and the District of Columbia are excluded from the sample as they are always treated. Outcomes are log transformed. The regression includes time-varying state characteristics, state fixed effects, and time (month-year) fixed effects. In application of the Callaway and Sant'Anna (2021) procedure we do not control for time-varying state characteristics. Data are weighted by the state population. In the Callaway and Sant'Anna (2021) procedure we use both the not-yet-treated and never-treated states as the comparison group, and the Sant'Anna and Zhao (2020) doubly robust difference-in-differences estimator based on stabilized inverse probability weighting and ordinary least squares.
***,**, and * = statistically different from zero at the 1%, 5%, and 10% level.

Table A14: The effect of a state paid sick leave mandate on patient demographics utilizing a two-stage difference-in-differences procedure: IQVIA 2015-2022

| Outcome variable →: | Age | Female | Rural |
|---|---|---|---|
| State paid sick leave mandate | -0.491*** | -0.003*** | 0.000 |
| | (0.123) | (0.001) | (0.002) |
| Pre-treatment mean | 47.027 | 0.624 | 0.223 |
| Observations | 4704 | 4704 | 4704 |

Notes: This table reports coefficient estimates from a regression of patient demographics on state paid sick leave mandates and other controls. The unit of observation is a state in a month in a year. Connecticut and the District of Columbia are excluded from the sample as they are always treated. Outcomes are log transformed. The regression includes state characteristics, state fixed effects, and time (month-year) fixed effects. Data are weighted by the state population. Regressions are estimated with a two-stage difference-in-differences procedure proposed by Gardner (2022). Standard errors that account for within-state clustering are reported in parentheses.
***,**, and * = statistically different from zero at the 1%, 5%, and 10% level.

Table A15: The effect of a state paid sick leave mandate on any substance use disorder treatment use utilizing a two-stage difference-in-differences procedure: IQVIA 2015-2022

| Outcome variable →: | Claims | Patients |
|---|---|---|
| State paid sick leave mandate | 0.032 | 0.172** |
| | (0.117) | (0.078) |
| Pre-treatment mean (,000s), level | 148 | 44 |
| Observations | 4704 | 4704 |

Notes: This table reports coefficient estimates from a regression of any service utilization on state paid sick leave mandates and other controls. The unit of observation is a state in a month in a year. Connecticut and the District of Columbia are excluded from the sample as they are always treated. Outcomes are log transformed. The regression includes state characteristics, state fixed effects, and time (month-year) fixed effects. Data are weighted by the state population. Regressions are estimated with a two-stage difference-in-differences procedure proposed by Gardner (2022). Standard errors that account for within-state clustering are reported in parentheses.
***,**, and * = statistically different from zero at the 1%, 5%, and 10% level.

Table A16: The effect of a state paid sick leave mandate on dispensed substance use disorder treatment prescriptions utilizing a two-stage difference-in-differences procedure: IQVIA 2015-2022

| Outcome variable →: | Prescriptions | Patients | Prescription days |
|---|---|---|---|
| State paid sick leave mandate | 0.010 | -0.019 | -0.024 |
| | (0.049) | (0.042) | (0.042) |
| Pre-treatment mean (,000s), level | 25 | 17 | 455 |
| Observations | 4704 | 4704 | 4704 |

Notes: This table reports coefficient estimates from a regression of dispensed prescription medications utilization on state paid sick leave mandates and other controls. The unit of observation is a state in a month in a year. Connecticut and the District of Columbia are excluded from the sample as they are always treated. Outcomes are log transformed. The regression includes state characteristics, state fixed effects, and time (month-year) fixed effects. Data are weighted by the state population. Regressions are estimated with a two-stage difference-in-differences procedure proposed by Gardner (2022). Standard errors that account for within-state clustering are reported in parentheses.
***,**, and * = statistically different from zero at the 1%, 5%, and 10% level.

Table A17: The effect of a state paid sick leave mandate on outpatient substance use disorder treatment service use utilizing a two-stage difference-in-differences procedure: IQVIA 2015-2022

| Outcome variable →: | Claims | Patients |
|---|---|---|
| State paid sick leave mandate | -0.023 | 0.257** |
| | (0.153) | (0.106) |
| Pre-treatment mean (,000s), level | 105 | 28 |
| Observations | 4704 | 4704 |

Notes: This table reports coefficient estimates from a regression of outpatient service utilization on state paid sick leave mandates and other controls. The unit of observation is a state in a month in a year. Connecticut and the District of Columbia are excluded from the sample as they are always treated. Outcomes are log transformed. The regression includes state characteristics, state fixed effects, and time (month-year) fixed effects. Data are weighted by the state population. Regressions are estimated with a two-stage difference-in-differences procedure proposed by Gardner (2022). Standard errors that account for within-state clustering are reported in parentheses.
***,**, and * = statistically different from zero at the 1%, 5%, and 10% level.

Table A18: The effect of a state paid sick leave mandate on inpatient substance use disorder treatment service use utilizing a two-stage difference-in-differences procedure: IQVIA 2015-2022

| Outcome variable →: | Claims | Patients |
|---|---|---|
| State paid sick leave mandate | 0.225** | 0.221** |
| | (0.109) | (0.101) |
| Pre-treatment mean (,000s), level | 19 | 5 |
| Observations | 4704 | 4704 |

Notes: This table reports coefficient estimates from a regression of inpatient service utilization on state paid sick leave mandates and other controls. The unit of observation is a state in a month in a year. Connecticut and the District of Columbia are excluded from the sample as they are always treated. Outcomes are log transformed. The regression includes state characteristics, state fixed effects, and time (month-year) fixed effects. Data are weighted by the state population. Regressions are estimated with a two-stage difference-in-differences procedure proposed by Gardner (2022). Standard errors that account for within-state clustering are reported in parentheses.
***,**, and * = statistically different from zero at the 1%, 5%, and 10% level.

Table A19: The effect of a state paid sick leave mandate on substitution substance use disorder treatment service use utilizing a two-stage difference-in-differences procedure: IQVIA 2015-2022

| Outcome variable ↓: | Coefficient estimate (Standard error) |
|---|---|
| Prescriptions, outpatient, inpatient | 0.120 |
| | (0.094) |
| Pre-treatment mean (,000s), level | 1.541 |
| No prescriptions, outpatient, inpatient | 0.314** |
| | (0.131) |
| Pre-treatment mean (,000s), level | 7.677 |
| Prescriptions, outpatient, no inpatient | 0.260* |
| | (0.137) |
| Pre-treatment mean (,000s), level | 7.850 |
| No prescriptions, outpatient, no inpatient | 0.374** |
| | (0.155) |
| Pre-treatment mean (,000s), level | 66.561 |
| Prescriptions, no outpatient, inpatient | 0.133** |
| | (0.066) |
| Pre-treatment mean (,000s), level | 0.770 |
| Prescription, no outpatient, no inpatient | -0.075* |
| | (0.040) |
| Pre-treatment mean (,000s), level | 16.136 |
| No prescriptions, no outpatient, inpatient | 0.183* |
| | (0.106) |
| Pre-treatment mean (,000s), level | 12.669 |
| Observations | 784 |

Notes: This table reports coefficient estimates from a regression of service utilization on state paid sick leave mandates and other controls. The unit of observation is a state in a six-month period in a year. Connecticut and the District of Columbia are excluded from the sample as they are always treated. Outcomes are log transformed. The regression includes state characteristics, state fixed effects, and time (six-month-year) fixed effects. Data are weighted by the state population. Regressions are estimated with a two-stage difference-in-differences procedure proposed by Gardner (2022). Standard errors that account for within-state clustering are reported in parentheses.
***,**, and * = statistically different from zero at the 1%, 5%, and 10% level.