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TIME FOR MENTAL HEALTHCARE: EVIDENCE FROM PAID SICK LEAVE MANDATES

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Time for Mental Healthcare: Evidence from Paid Sick Leave Mandates
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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.

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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). 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 laws provide an ideal setting to study the issue of time costs affecting healthcare use due to their staggered implementation over the past 20 years.

Mental healthcare is an interesting setting in which to study the importance of time as this care is often viewed as more discretionary than treatment for physical health conditions – see Frank and McGuire (2000). Moreover, mental health conditions are under-diagnosed and under-treated (Merikangas et al., 2011; Green et al., 2019; Whitney and Peterson, 2019). To the extent that consumers respond to time costs similarly to monetary costs, policies that reduce time costs could have a disproportionately larger effect on mental healthcare use than for general medical care. However, 57% of U.S. counties have been deemed mental healthcare shortage areas (Kaiser Family Foundation, 2023) which may mute that response. Another interesting feature of mental healthcare is that several treatment modalities exist; low-time medication vs. high-time psychotherapy, for example. Many people receive pharmaceutical treatment for mental health conditions that requires minimal time investment outside of an initial clinician visit and periodic medication management appointments. But, 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. Different treatment modalities may be either complements or substitutes for each other depending on a patient's clinical situation. Thus, time may be a binding constraint to mental healthcare for at least some patients, and changes in time costs could affect the margin of overall mental healthcare along with the margin of

which treatment types are used. Specifically, more available time should increase the use of all services, and, shift the treatment mix toward more time-intensive care.

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. In particular, post-mandate dispensed prescriptions decline while both 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). 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 onto 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 telehealth-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

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, 2020). The U.S. is one of the few developed countries that do 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 1993 *Family and Medical Leave Act* (FMLA) provides eligible employees who have worked 1,250 or more hours per year for an employer with at least 50 employees 12 weeks of **unpaid** leave that can be used for pregnancy, own health, or the health of a family member. However, in addition to this Act not providing financially protected time, there are numerous exclusions and nearly half of all employees are ineligible (Appelbaum, 2014). During the onset of the COVID-19 pandemic, the U.S. implemented a temporary federal PSL mandate – the *Families First Coronavirus Response Act* (FFCRA). This Act, which was in place between April and December of 2020, provided up to two weeks of PSL for pandemic-related health and family demands for some workers (Andersen et al., 2023).

18 states, the District of Columbia, and several cities and counties adopted or announced a PSL mandate by November, 2024 (National Partnership of Women & Families, 2023; A Better Balance, 2024). 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.[1] 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 receive all types of care (e.g., transporting patients to treatment, providing childcare), suggesting that PSL mandates could indirectly facilitate both outpatient and inpatient care for dependents. Moreover, 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.

[1]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).

There is a small but growing literature on the impacts of PSL mandates. Maclean et al. (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 measured 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 using survey data post-mandate. There is also some evidence that health may improve following the adoption 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, 2024), 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 examine 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.[2]

[2]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

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.

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 & 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. 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. 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 sub-

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.

stance use diagnoses 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 the 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 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 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 4.6 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. Thus, 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 (2024). 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[3] 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 2024, 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., and Figure 2 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 2024, and the number of employees made newly eligible for PSL as a result of these mandates (National Partnership of Women & Families, 2023). The District of Columbia and Connecticut are excluded from our analysis as these states adopt a PSL mandate prior to the first year of our study and thus 'always treated.'

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 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 4.6).

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. 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).[4] These mandates provide financially

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

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

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.[5] Given these differences, we treat PSL mandates and paid time off mandates as different policies, though we will show in Section 4.6 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 PTO mandates from the analysis sample. Notably, Michigan's paid time off mandate transitioned to a PSL mandate in February 2025.[6]

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 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 adoption using the

Hollywood, California.

[5]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.

[6]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 (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.

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. Furthermore, if PSL mandates impact this mental health metric, our analysis will not allow us to isolate the effect of mental healthcare from other factors such as improved employment outcomes, better work-life balance, and increased use of physical healthcare, among other factors. 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 mandate to a state that never adopts a PSL mandate, and comparing a state treated early 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 early). 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 imputation in the second stage (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 ($),[7] 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, 2023; Flood et al., 2024).[8] Gamma is a vector of state fixed effects and lambda 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 4.6. Standard errors account for both the first-stage imputation and correlation within state and over time.

We select the Gardner (2022) difference-in-differences estimator for our main analyses 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), and the approach performs well in terms of inference relative to other 'new' difference-in-differences estimators (Gardner et al., 2024; Mizushima and Powell, 2025). 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). However, we will show in Section 4.3 that our results are not appreciably different if we use alternative difference-in-differences estimators.

In our analysis of the NCS and BRFSS, we make some modifications as the data are recorded at the individual (job or respondent) level.

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

[8] 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.

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 2024 (measured prior to mandate adoption), and iii) states that do not adopt or announce a PSL mandate by November 2024 (measured in all years). 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. Paid medical and family leave policies are only adopted by states that also implement a PSL mandate while PTO mandates are implemented by non-PSL mandating adopting states only. 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.

Figure 3 reports trends in overall mental healthcare, dispensed prescriptions, outpatient care, and inpatient care over our study period, we log outcomes as we do in our regression analyses. In Figure 4, we report trends in these outcomes, but we stratify the sample based on adoption/announcement of a state PSL mandate by November 2024. At the national level, all outcomes are trending upward over the study period, though increases are more modest for prescriptions than for other metrics. When we separately consider states that will and will not eventually adopt a PSL mandate, the trends follow similar patterns.

4 Results

4.1 First-stage effects

Table 3 reports first-stage results using the NCS data, event-studies are reported in Figure A3. Here we find that, following adoption of a state PSL mandate, there is a 12.2 percentage point 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 5 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 (bot 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.

Tables 4, 5, 6, and 7 report 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 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), 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. Finally, more frequent interac-

tions 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.[9]

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 three patient-level mutually exclusive care patterns over six-month periods of time: i) medications and no outpatient care, ii) no medications and outpatient care, and iii) medications and outpatient 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 8, support the hypothesis that patients shift toward more time-intensive care post-PSL mandate: the number of patients with medications and no outpatient care declines by 7.3% post-mandate while the number of patients with no medications and outpatient care, and medications and outpatient care increase by 21.4% and 18.3%, 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 6, there is no evidence of differential pre-trends between states that will and will not adopt a PSL mandate, which again supports the hypothesis that the data can satisfy parallel trends.

In Figure 7, 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.[10] For brevity, we use patient counts for each of our treatment setting

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

[10]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?

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 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.

`Employee-Assistance-Program-vs.-Behavioral-Health-Coverage-197=&utm_source=chatgpt.com`, website last accessed 7/2/2025.

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.[11]

In Tables A4, A5, A6, A7, and A8 we report results using the Callaway and Sant'Anna (2021) estimator; Tables A9, A10, A11, A12, and A13 the Wooldridge (2023) estimator; and Tables A14, A15, A16, A17, and A18 report results using the Borusyak et al. (2024) estimator. Finally, we report results using TWFE in Tables A19, A20, A21, A22, and A23. 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 mandates, 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 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 lower level of care (using both prescriptions and outpatient care). Our findings hint that, for these 'incumbent' patients, management of mental health outcomes may improve as patients require fewer prescription 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 9 and event-study results in Figure 8.

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

[11] 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).

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.

4.5 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 A24, A25, A26, A27, and A28 report results for substance use disorder treatment metrics. Event-studies are reported in Figures A5 and A6. 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 29.3% and 24.7% respectively, but only inpatient claim counts increase (25.2%). Similar to mental healthcare, we see declines in the least intensive treatment pattern (no outpatient and medications) and increases in more time-intensive patterns (i.e., outpatient without and with medications).

4.6 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 A7 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 (Univer-

sity of Kentucky Center for Poverty Research, 2023)) and proxies for telemedicine availability (state-level indicators for telemedicine coverage and reimbursement in fully insured commercial and Medicaid plans).[12] Controlling for telemedicine 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 PTO mandates during our study period (Maine, Michigan, and Nevada) and we expand our definition of PSL mandates to include PTO mandates.

In Figure A8 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 A8 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 A9 and A10 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

[12]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.

the precision of our first stage prediction. For example, in Figure A11, 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 A12. 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, we show that state PSL mandates do not appear to 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, 2024) 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. Finally, using survey data, we show that self-reported mental health improves following adoption of a state PSL mandate.

Findings for children – for whom we document increases in mental healthcare along the extensive margin and movements towards higher levels of care – may be particularly important. More specifically, there is concern regarding high rates of mental health disorders among children (Haidt, 2024; Currie, 2025). Thus, the ability of parents, following a PSL mandate, to support their children's use of mental healthcare can potentially allow treatment for such conditions. Furthermore, this finding hints 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 a mental health or substance use disorder diagnosis. While this focus allows us to examine patients 'at risk' for using mental healthcare services, we will likely not capture more marginal patients. Second, while our prescription claims data cover roughly 95% 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.

Given the benefits we show in terms of increased healthcare use and improved (self-assessed) mental health, we can account for the costs of PSL mandates to employers who are required to provide the PSL. Maclean et al. (2025) find that the costs to employers of mandated PSL is 6.2 cents per hour and employees work on – on average 1,702 hours per year – which implies that the annual cost to employers is $105.5. In our analysis (see Table 3), we find that the probability that an employer offers PSL to an employee increases by 16.9% relative to a baseline coverage rate of 72.1%. If we assume that the average state without a PSL mandate in place has 2,909,828 employees in 2022,[13] then our estimates imply that the cost to employers is $14,474,737.[14] The annual economic burden to the average U.S. state

[13]Authors' calculation using Bureau of Labor Statistics data maintained by the University of Kentucky Center for Poverty Research (2023).

[14]To calculate this number, we assume that 27.9% of employees do not have access to PSL in non-mandate states (811842). A 16.9% increase in PSL benefits among employees lacking the benefit is 137,201 employees gaining the benefit. If the cost per employee per year is $105.5, then the total cost to employers is $14,474,706

of mental health disorders exceed $5.5 billion per year (Abramson et al., 2024).[15] Thus, without considering the benefits of mandated PSL for other social outcomes, the gains in terms of mental health suggests that these mandates are cost-effective.

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.

per year. Given that we do not account for exemptions, this number likely represents an upper bound on the cost to employers. We do not apply a marginal value of public funds calculation (Hendren and Sprung-Keyser, 2020) as the costs of PSL mandates mainly accrue to private employers and thus, generally, do not use public funds.

[15]The authors report that the costs to the U.S. are $282 billion, we divide this number by 51 to generate a state-level estimate.

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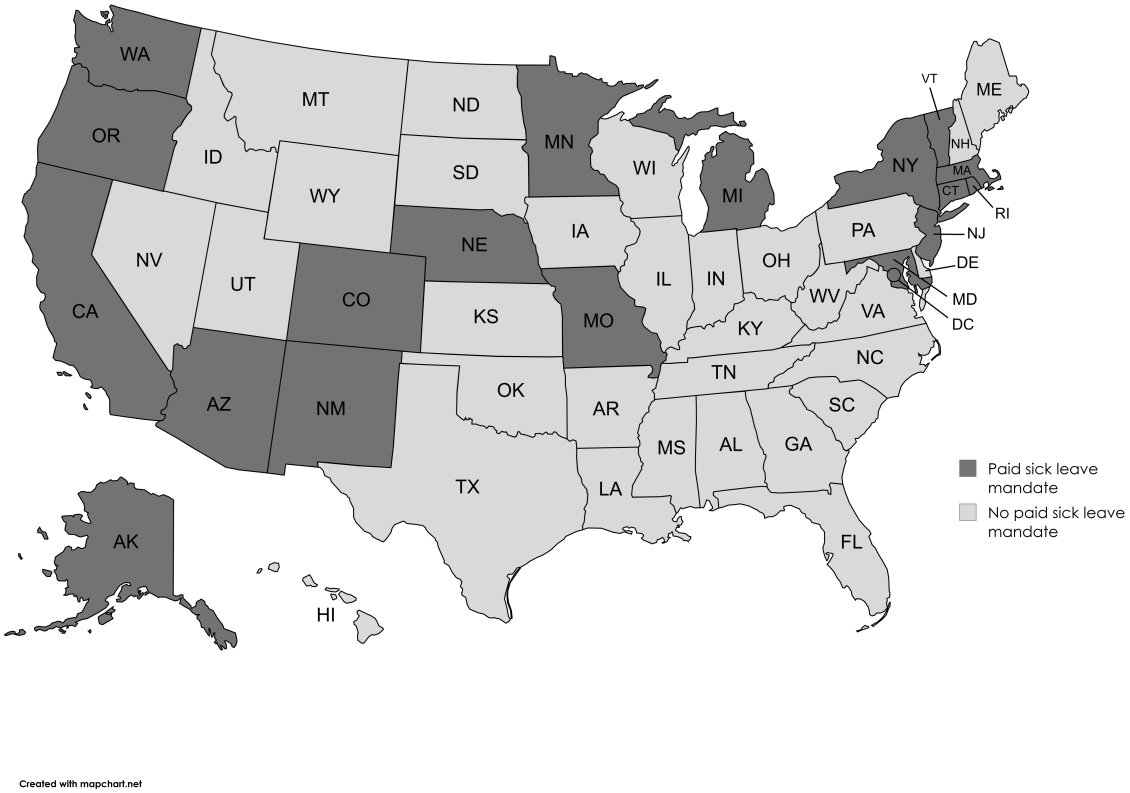
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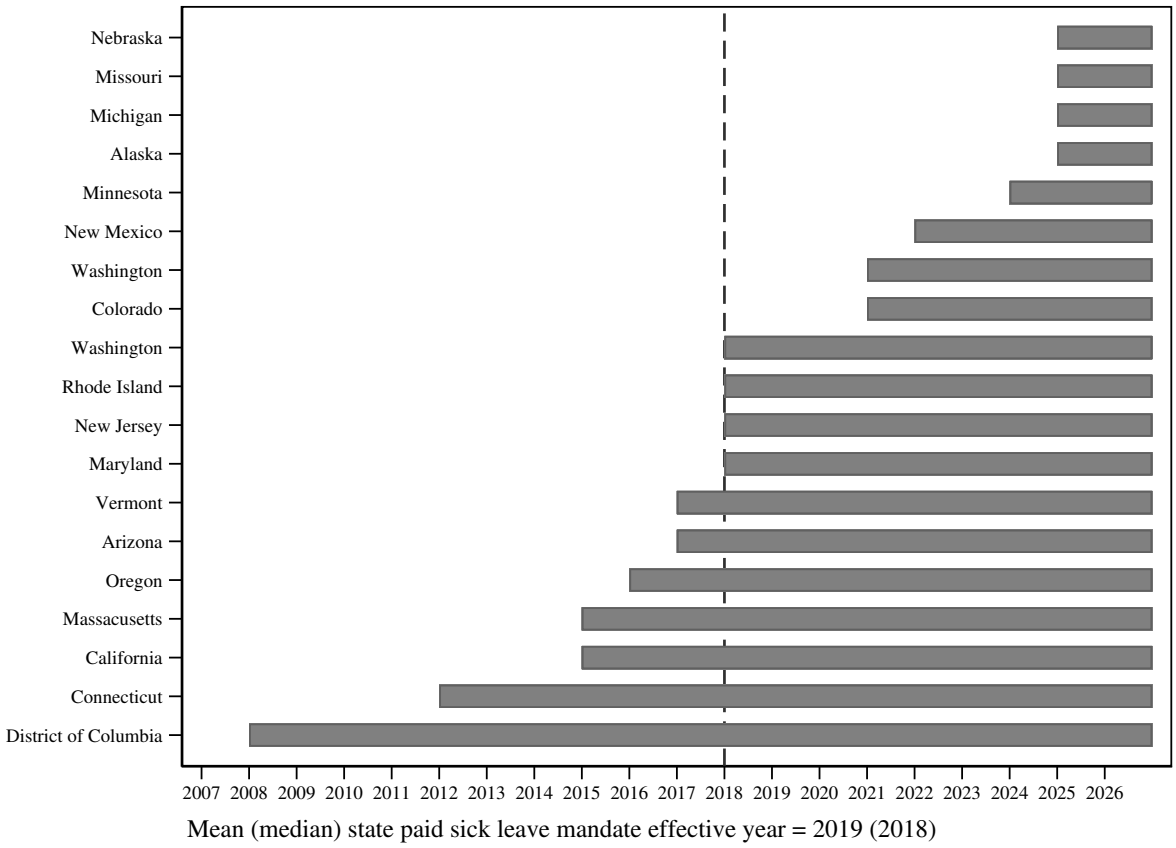
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Figure 1: Geographic distribution of of all adopted or announced state paid sick leave mandates in the United States as of November, 2024



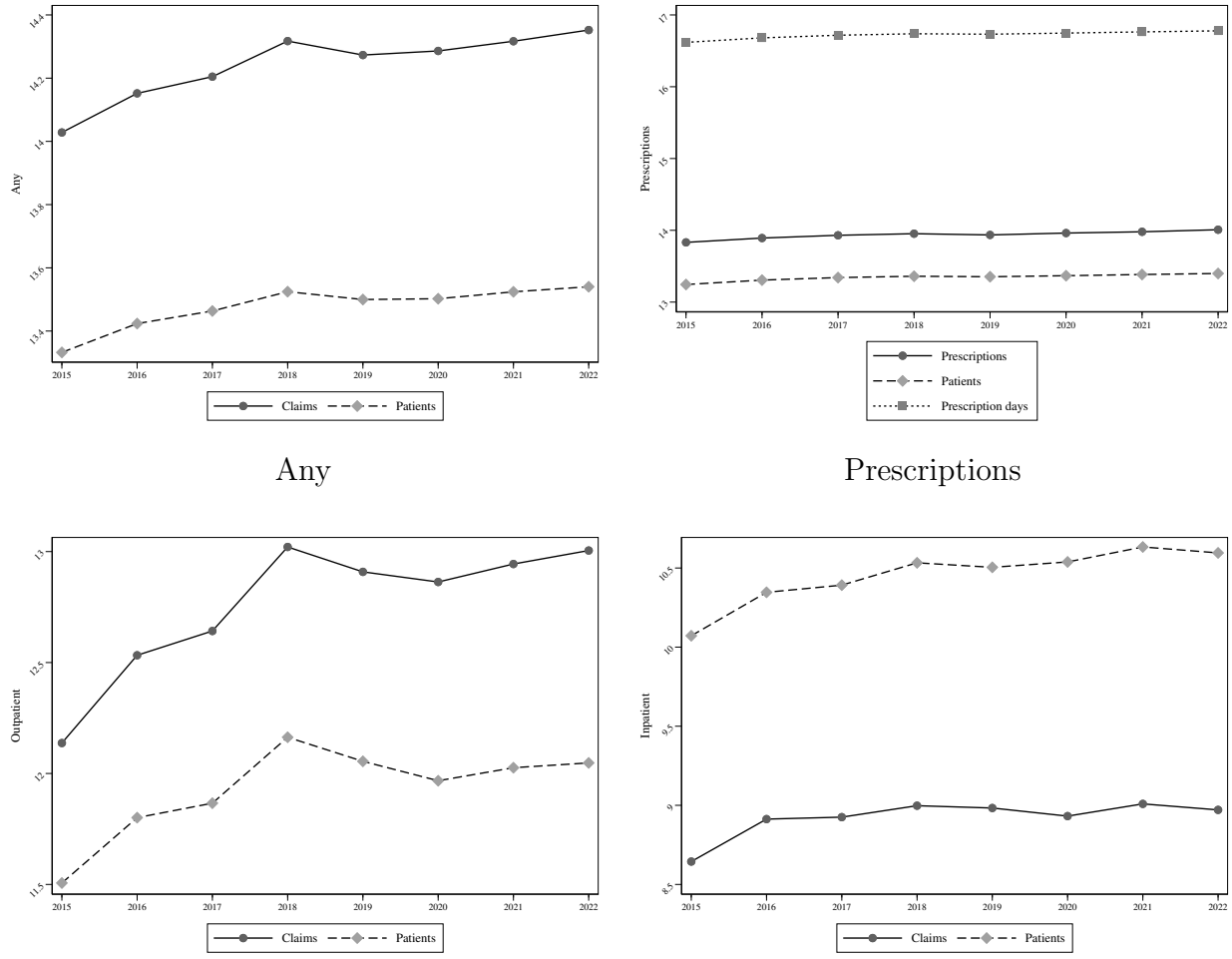
Notes: The data sources are National Partnership of Women & Families (2023) and A Better Balance (2024). All state paid sick leave mandates are effective or announced by November, 2024. See Table 1 for paid sick leave mandate effective dates.

Figure 2: Temporal distribution of of all adopted or announced state paid sick leave mandates in the United States as of November, 2024



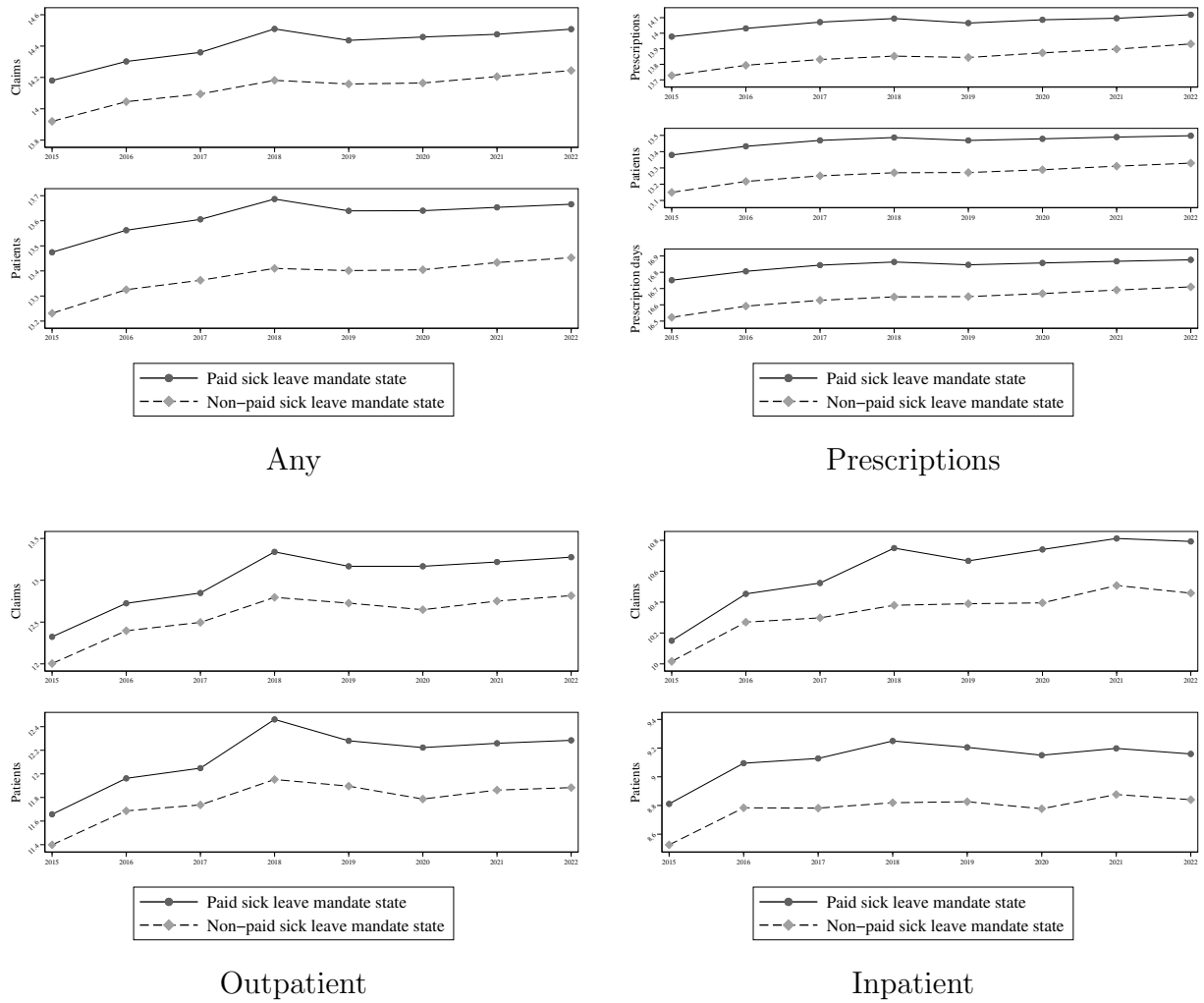
Notes: The data sources are National Partnership of Women & Families (2023) and A Better Balance (2024). All state paid sick leave mandates are effective or announced by November, 2024. See Table 1 for paid sick leave mandate effective dates.

Figure 3: Trends in mental healthcare utilization: IQVIA claims database 2015-2022



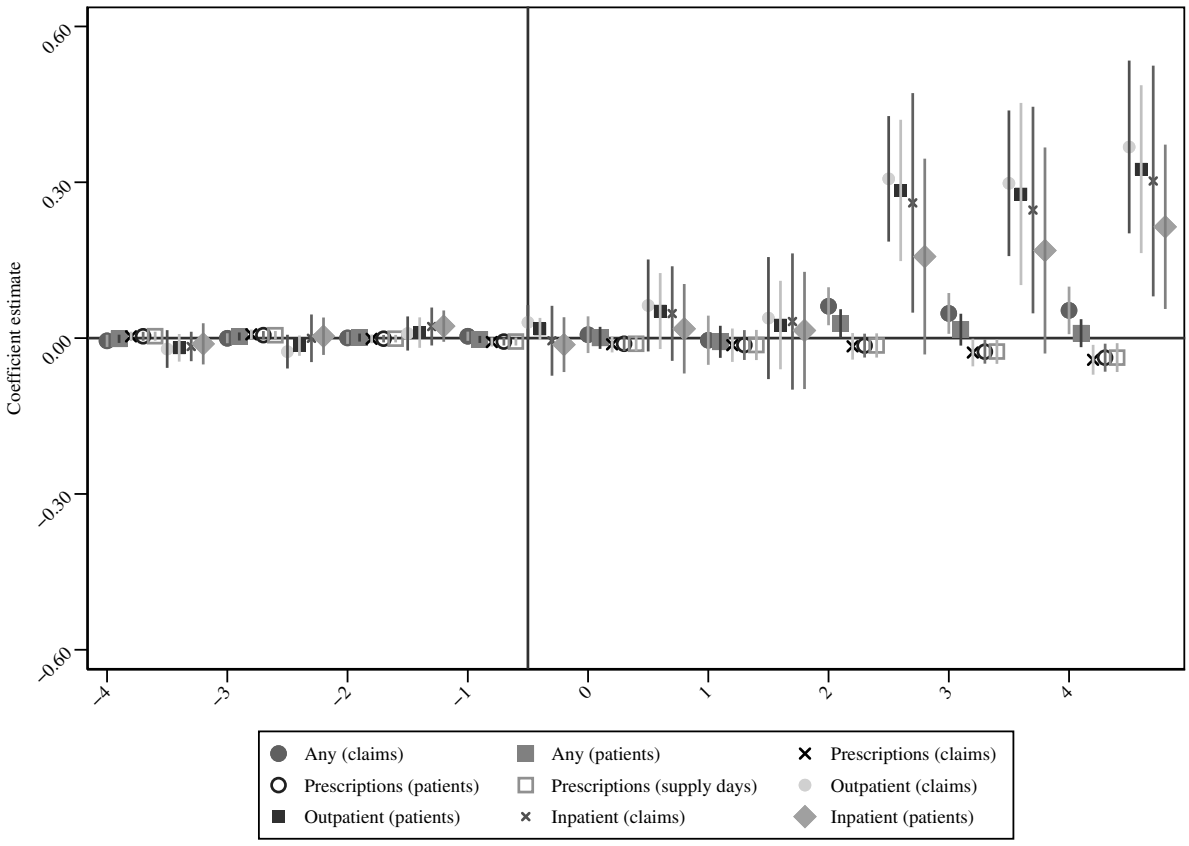
Notes: This figure reports unadjusted trends in mental healthcare use. Outcomes are log transformed. Connecticut and the District of Columbia are excluded from the sample as these jurisdictions are treated in all years 2015-2022. The unit of observation is a year. Data are weighted by the state population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

Figure 4: Trends in mental healthcare utilization in states that do and do not adopt/announce a paid sick leave mandate: IQVIA claims database 2015-2022



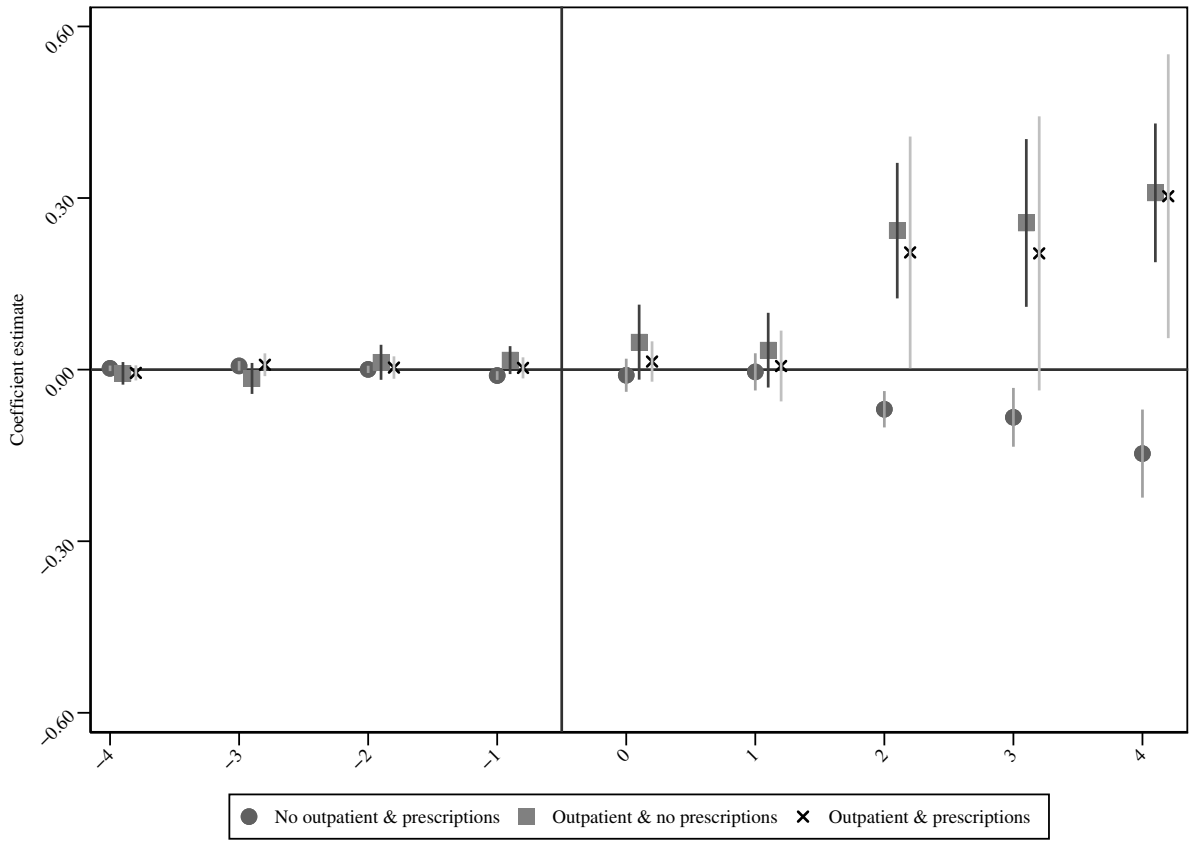
Notes: This figure reports unadjusted trends in mental healthcare use in states that do and do not adopt a paid sick leave mandate by November, 2024. Outcomes are log transformed. Connecticut and the District of Columbia are excluded from the sample as these jurisdictions are treated in all years 2015-2022. 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 5: 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



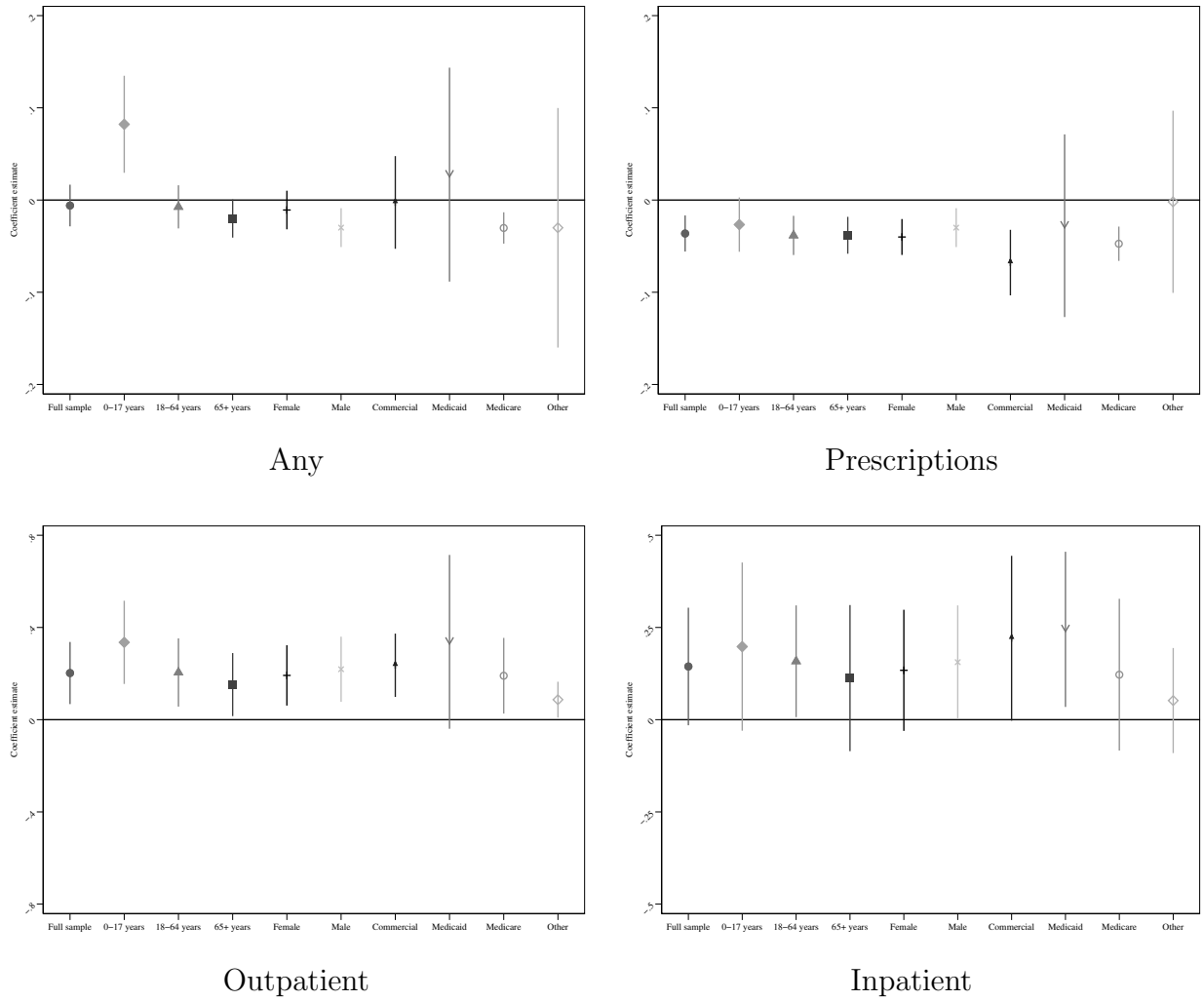
Notes: This figure reports results of a regression of mental healthcare utilization on indicators for time-to-paid sick leave adoption and other controls. Outcomes are log transformed. The unit of observation is a state in a year-month. Connecticut and the District of Columbia are excluded from the sample 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 6: 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



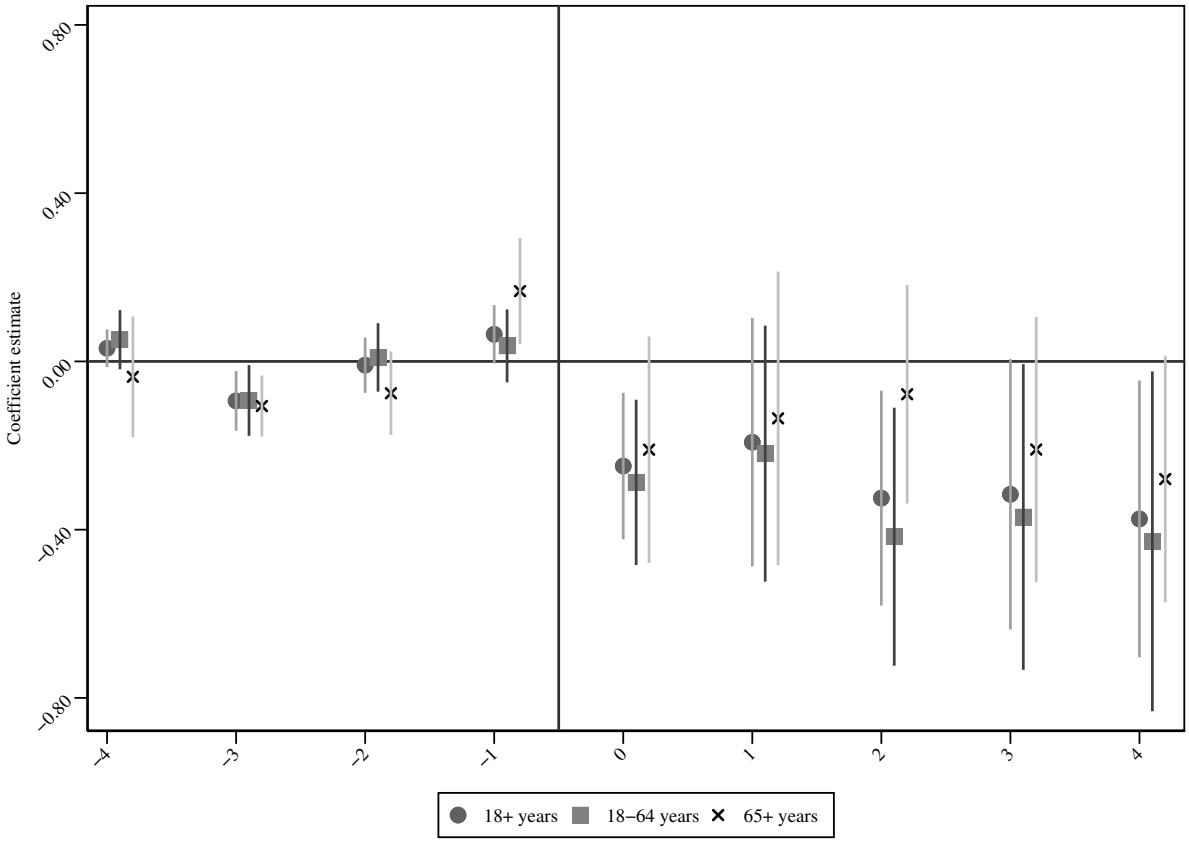
Notes: This figure reports results of a regression of mental healthcare utilization on indicators for time-to-paid sick leave adoption and other controls. Outcomes are log transformed. The unit of observation is a state in a year-six-month bin. Connecticut and the District of Columbia are excluded from 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 7: Heterogeneity in the effect of a state paid sick leave mandate on mental healthcare utilization using 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 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 8: 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 (2024)

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 (2024). Dates are in the format of month/year. State paid sick leave mandates adopted or announced as of November, 2024. 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 District of Columbia is excluded from the analysis sample as this jurisdiction is treated in all years but is reported here for completeness.
†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'). 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 = $\hat{\beta}$/per-treatment mean value in states that adopt a paid sick leave mandate × 100%.

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

Outcome variable →:	Claims	Patients
State paid sick leave mandate	0.014	-0.006
	(0.018)	(0.012)
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 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 5: The effect of a state paid sick leave mandate on dispensed mental health prescriptions utilizing a two-stage difference-in-differences procedure: IQVIA 2015-2022

Outcome variable →:	Prescriptions	Patients	Prescription days
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

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

Outcome variable →:	Claims	Patients
State paid sick leave mandate	0.221***	0.202***
	(0.060)	(0.069)
Pre-treatment mean (,000s), level	402	175
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 7: The effect of a state paid sick leave mandate on inpatient mental health service use utilizing a two-stage difference-in-differences procedure: IQVIA 2015-2022

Outcome variable →:	Claims	Patients
State paid sick leave mandate	0.228**	0.144*
	(0.108)	(0.081)
Pre-treatment mean (,000s), level	35	8
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 8: The effect of a state paid sick leave mandate on on substitution mental health service use utilizing a two-stage difference-in-differences procedure: IQVIA 2015-2022

Outcome variable →:	No outpatient & prescriptions	Outpatient no prescriptions	Outpatient & prescriptions
State paid sick leave mandate	-0.076***	0.194***	0.168*
	(0.023)	(0.049)	(0.092)
Pre-treatment mean (,000s), level	742	210	282
Observations	784	784	784

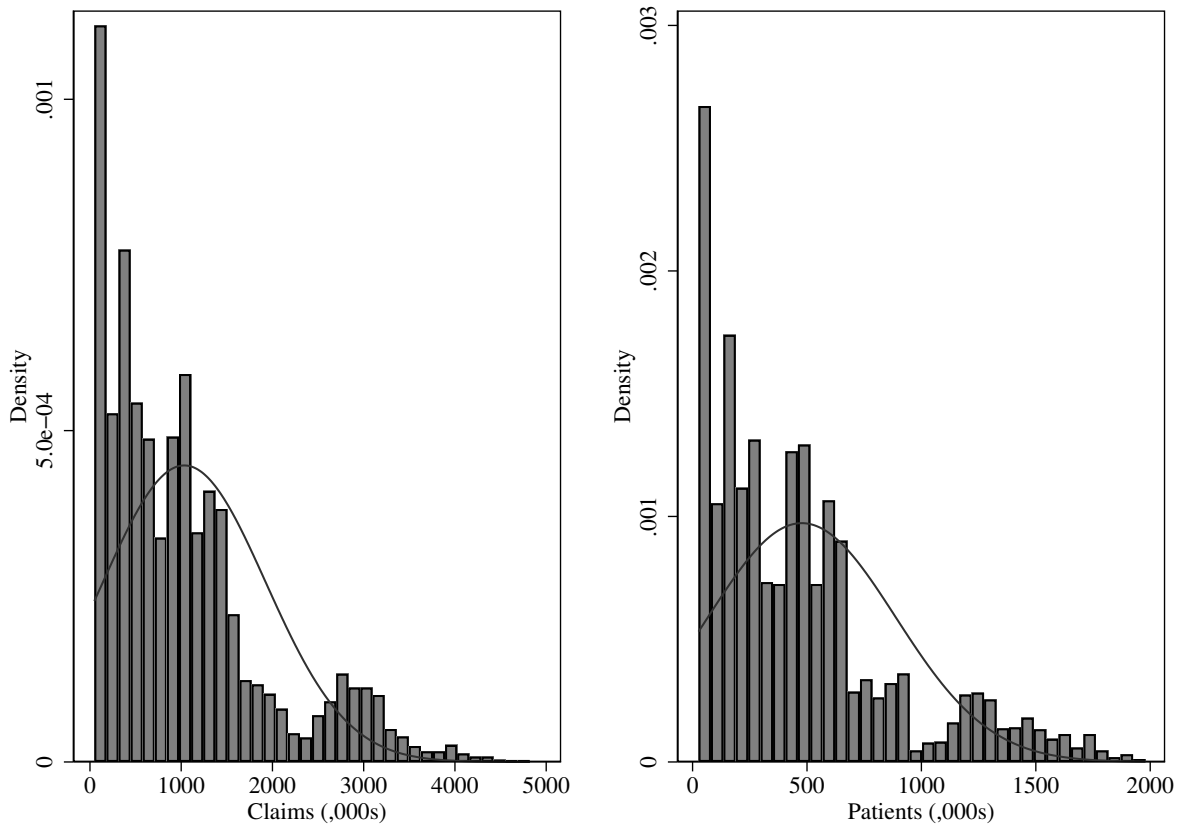
Notes: This table reports coefficient estimates from a regression of service utilization on state paid sick leave mandates and other controls. The unit of observation is a state in a six-month period in a year. Connecticut and the District of Columbia are excluded from the sample as they are always treated. 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 9: 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 →	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

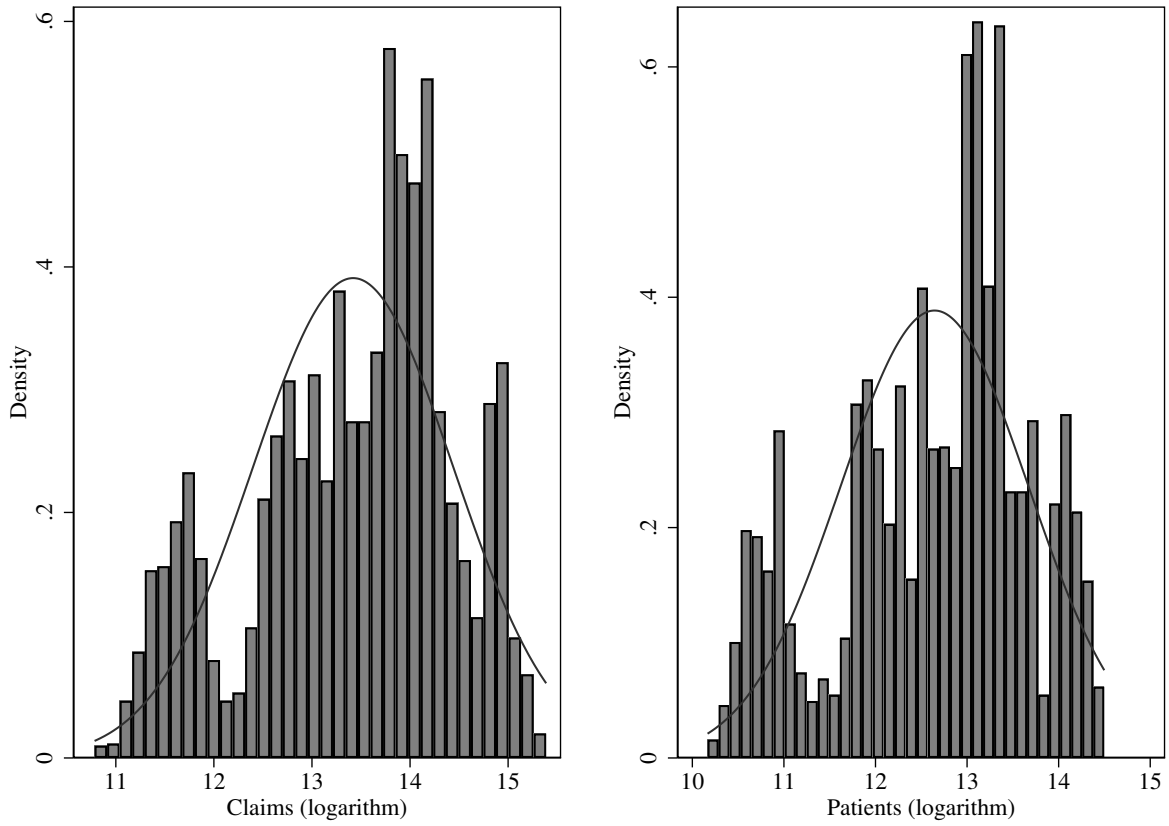
Notes: This table reports coefficient estimates from a regression of the number of days in bad mental health in the past 30 days on state paid sick leave mandates and other controls. The unit of observation is a respondent in state in a six-month period in a year. Connecticut and the District of Columbia 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 = $\hat{\beta}$/per-treatment mean value in states that adopt a paid sick leave mandate × 100%.

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



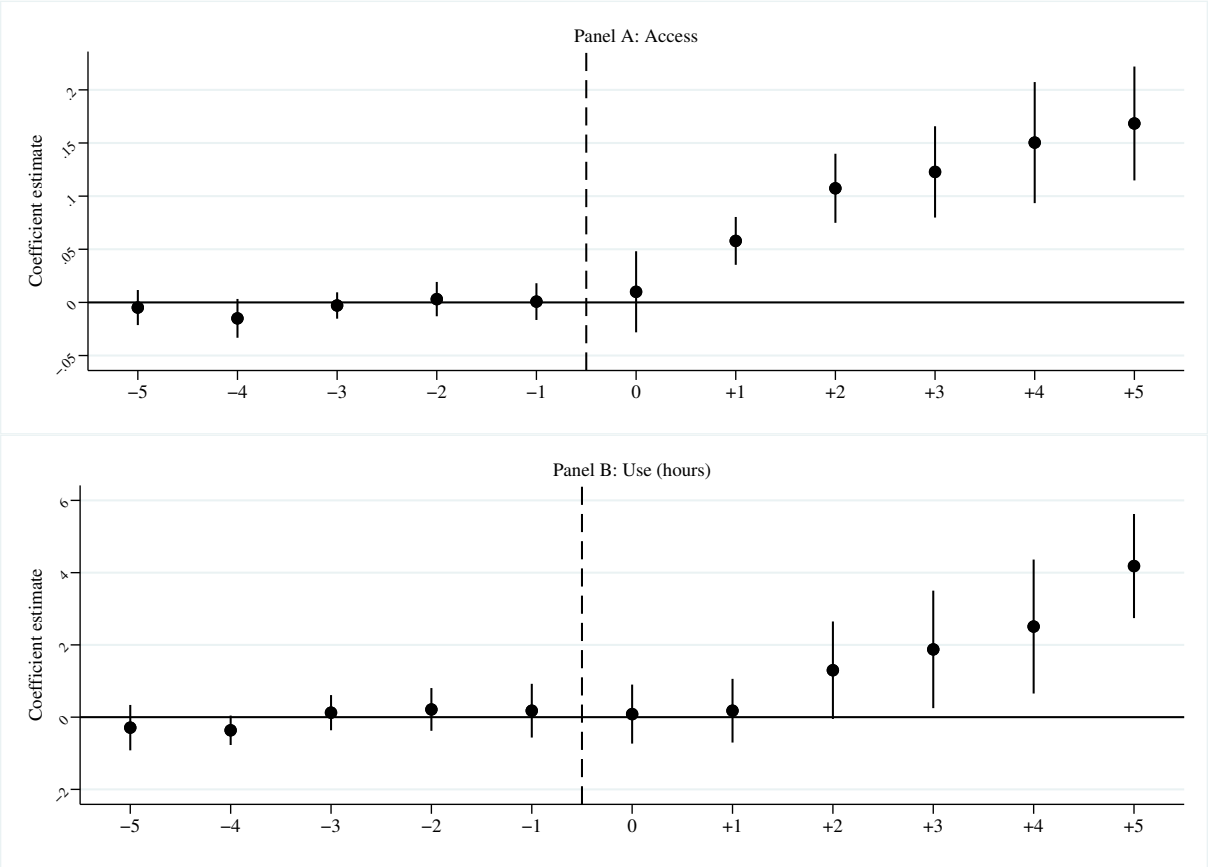
Notes: This figure reports the distribution of mental healthcare use measured by claims for mental health and patients with a mental health condition. The outcomes are divided by 1,000 for readability. The unit of observation is a state in a month-year. Connecticut and the District of Columbia are excluded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



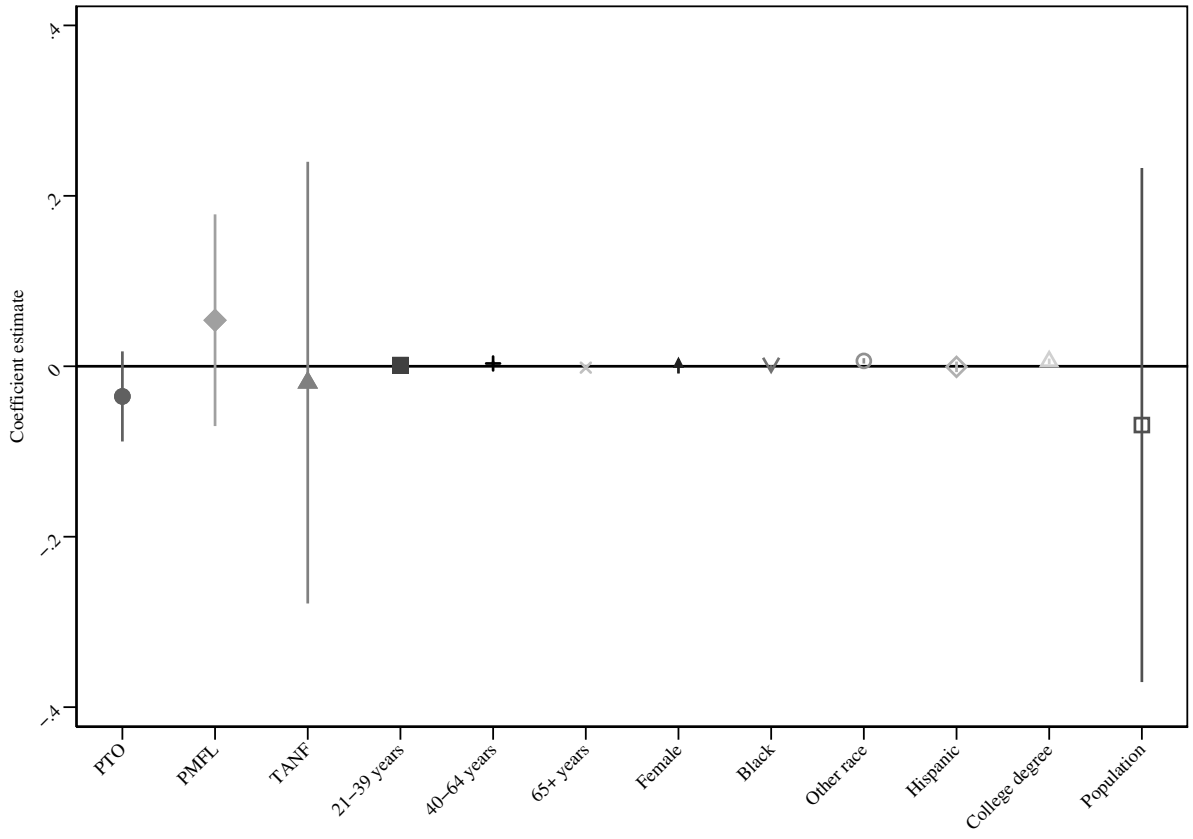
Notes: This figure reports the distribution of mental healthcare use measured by claims for mental health and patients with a mental health condition. The outcomes log-transformed. The unit of observation is a state in a month-year. Connecticut and the District of Columbia are excluded from the sample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

Figure A3: 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



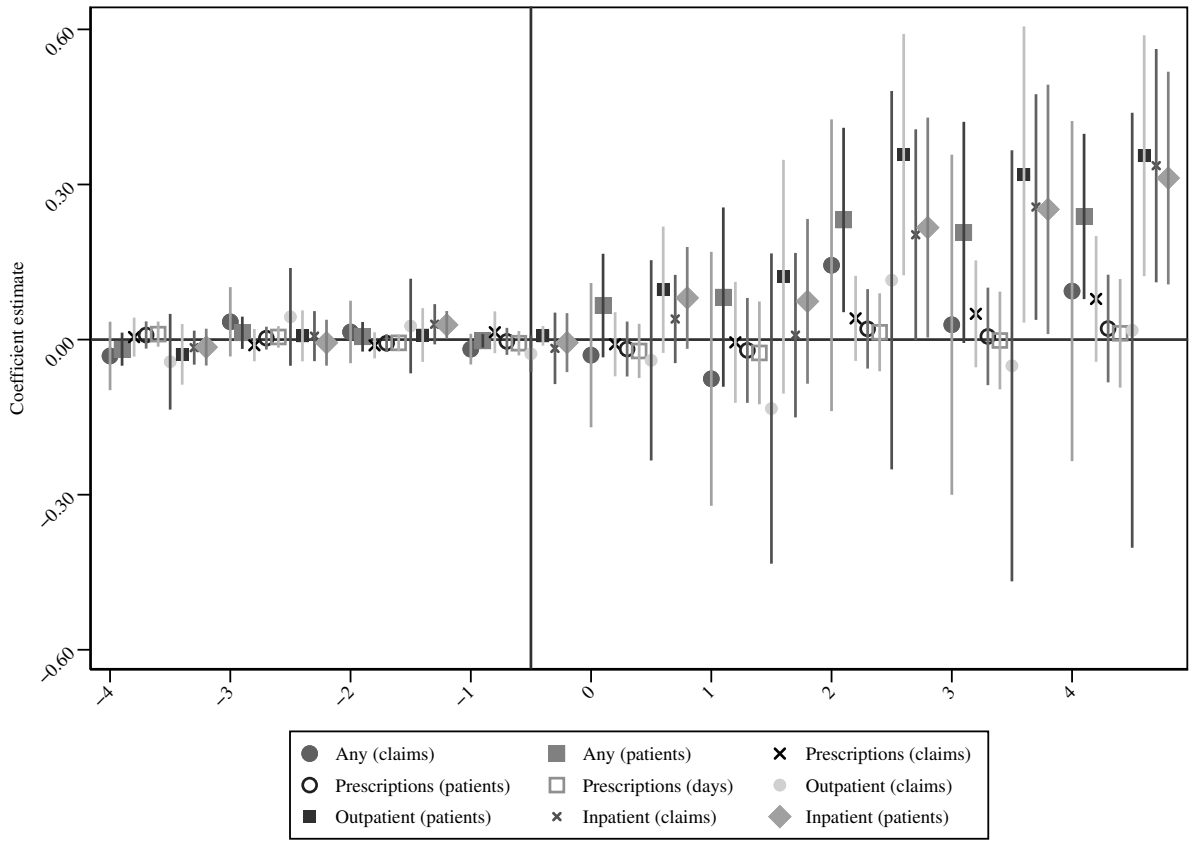
Notes: This figure reports results of a regression of paid sick leave access and utilization on indicators for time-to-paid sick leave adoption and other controls. We have a longer study period in the National Compensation Survey than in the IQVIA data, thus we are able to include additional leads and 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 A4: Test of covariate balance: Various data sources 2015-2022



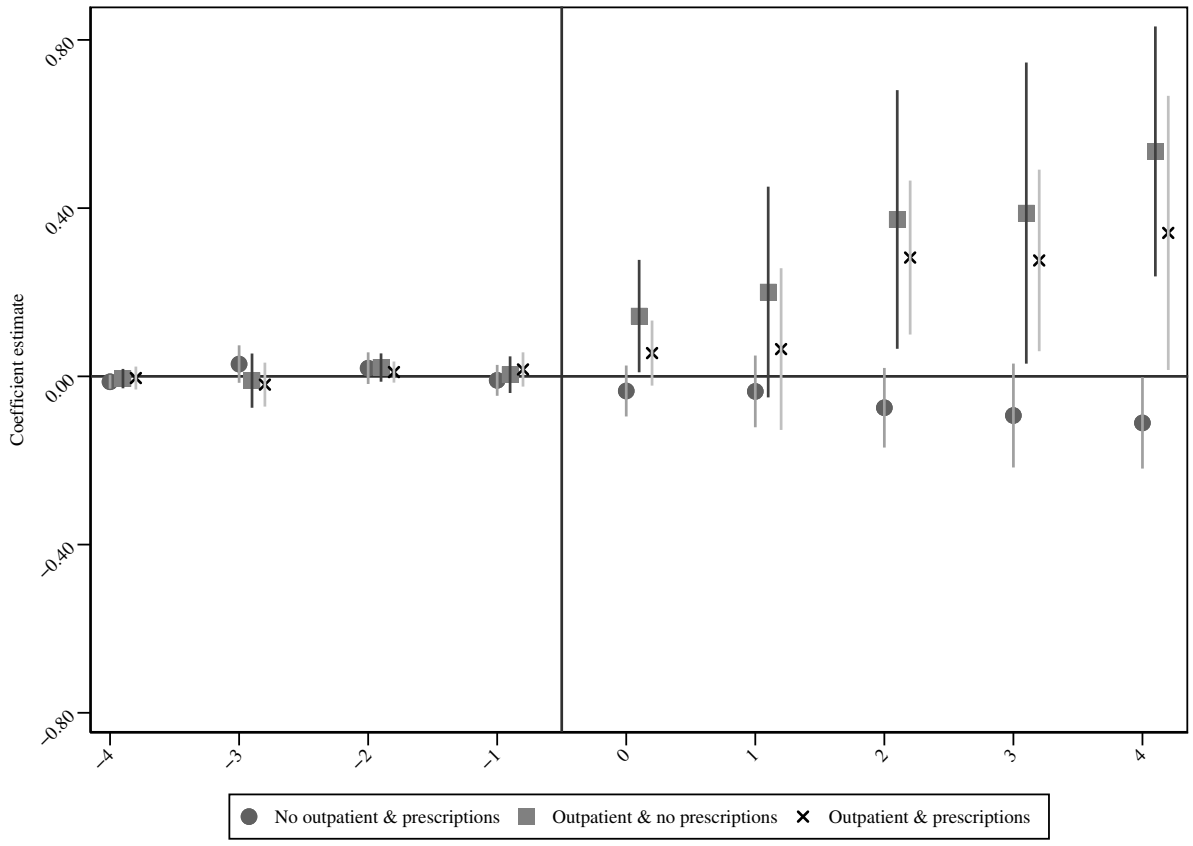
Notes: See Section 3 for information on data sources. This figure reports results from a regression of each covariate included in equation (1) on state and month-year fixed effects. The legend lists the outcome variable in each regression. The unit of observation is a state in a month-year. Connecticut 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 A5: 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



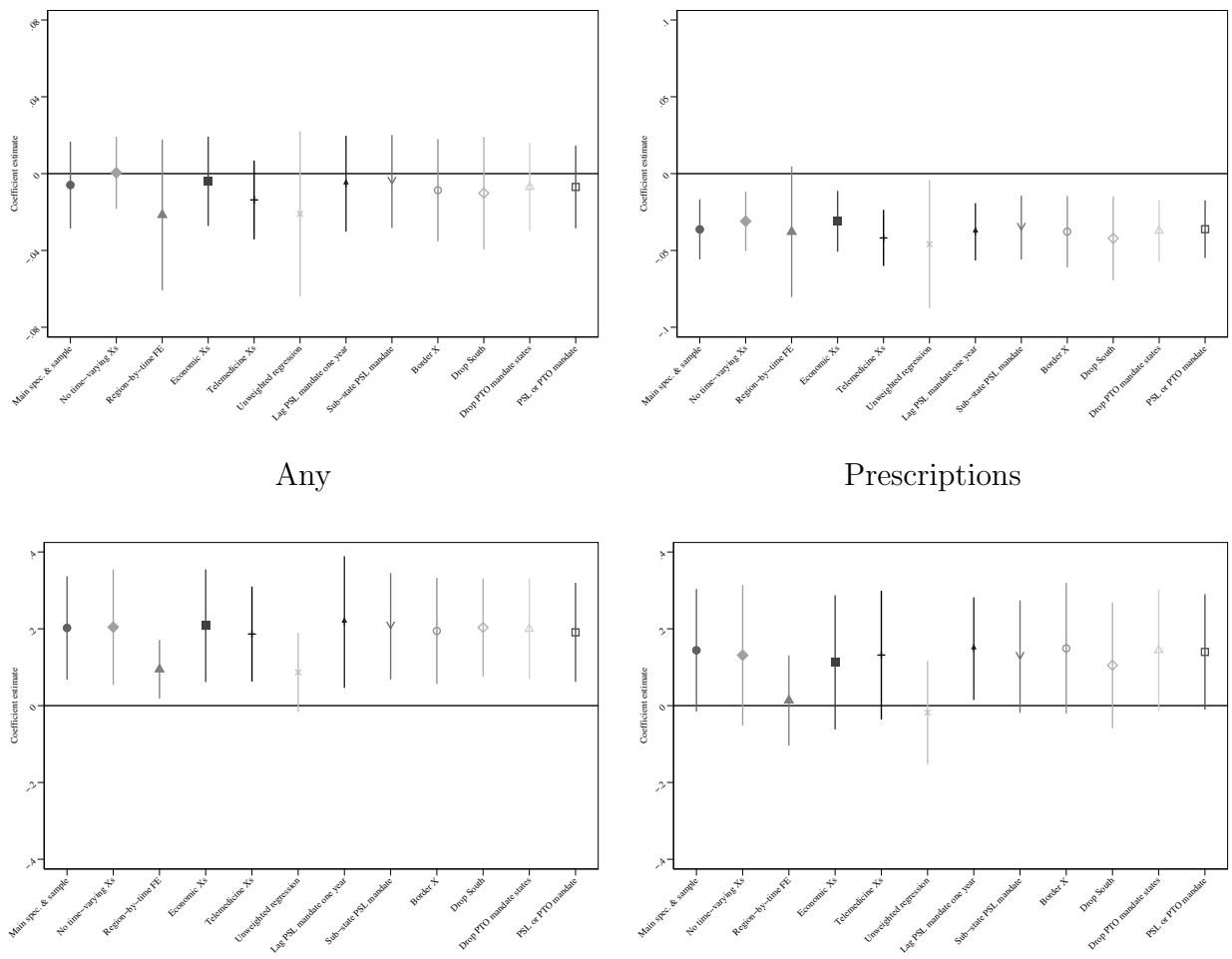
Notes: This figure reports results of a regression of substance use disorder utilization on a state paid sick leave mandate and other controls across different samples of patients. The sample is reported in the legend. Outcomes are patient counts and are log transformed. The unit of observation is a 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 A6: 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



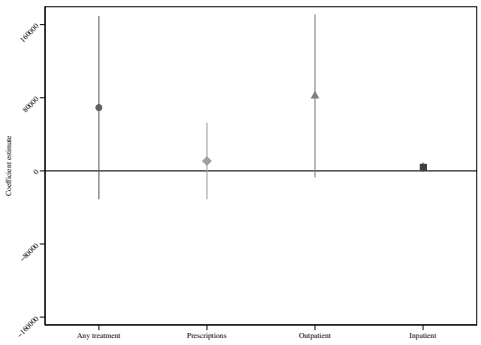
Notes: This figure reports results of a regression of substance use disorder utilization on indicators for time-to-paid sick leave adoption and other controls. Outcomes are log transformed. The unit of observations is a state in a year-month. Connecticut and the District of Columbia are excluded from 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 A7: 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

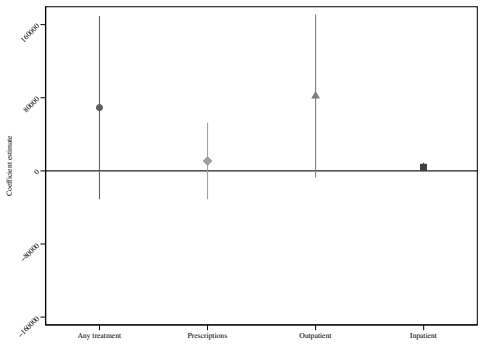


Notes: Spec. = specifications. FE = fixed effects. This figure reports results of a regression of mental healthcare utilization on a state paid sick leave mandate and other controls using different samples and specifications to assess the robustness of our findings. Outcomes are patient counts and are 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.

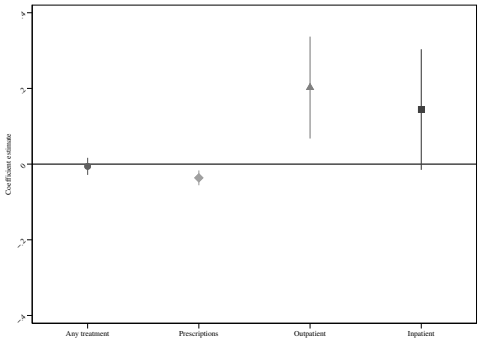
Figure A8: The effect of a state paid sick leave mandate on mental healthcare utilization not logging the outcome utilizing a two-stage difference-in-differences procedure: IQVIA claims database 2015-2022



Panel A: Ordinary least squares



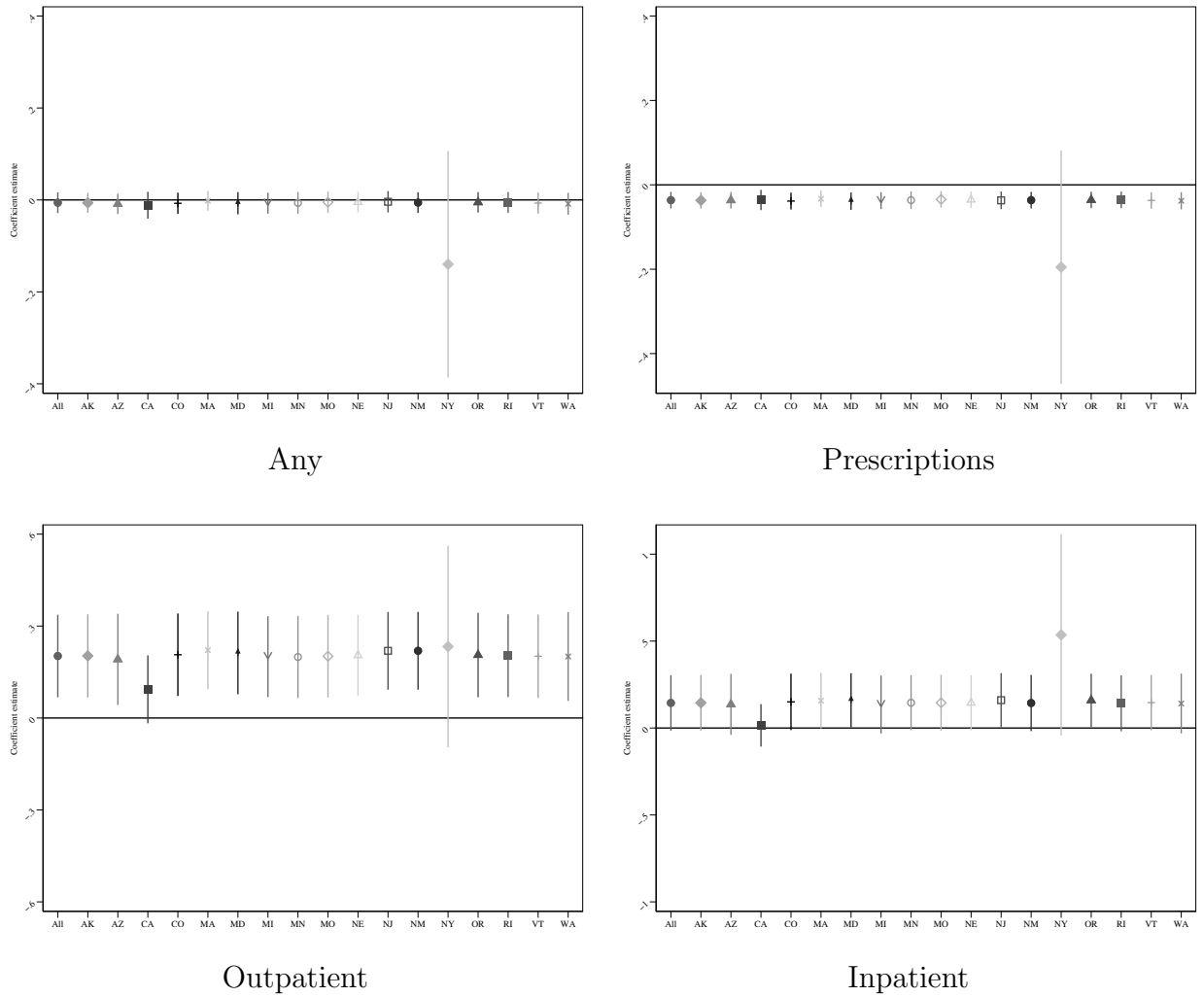
Panel B: Poisson



Panel C: Inverse hyperbolic sign

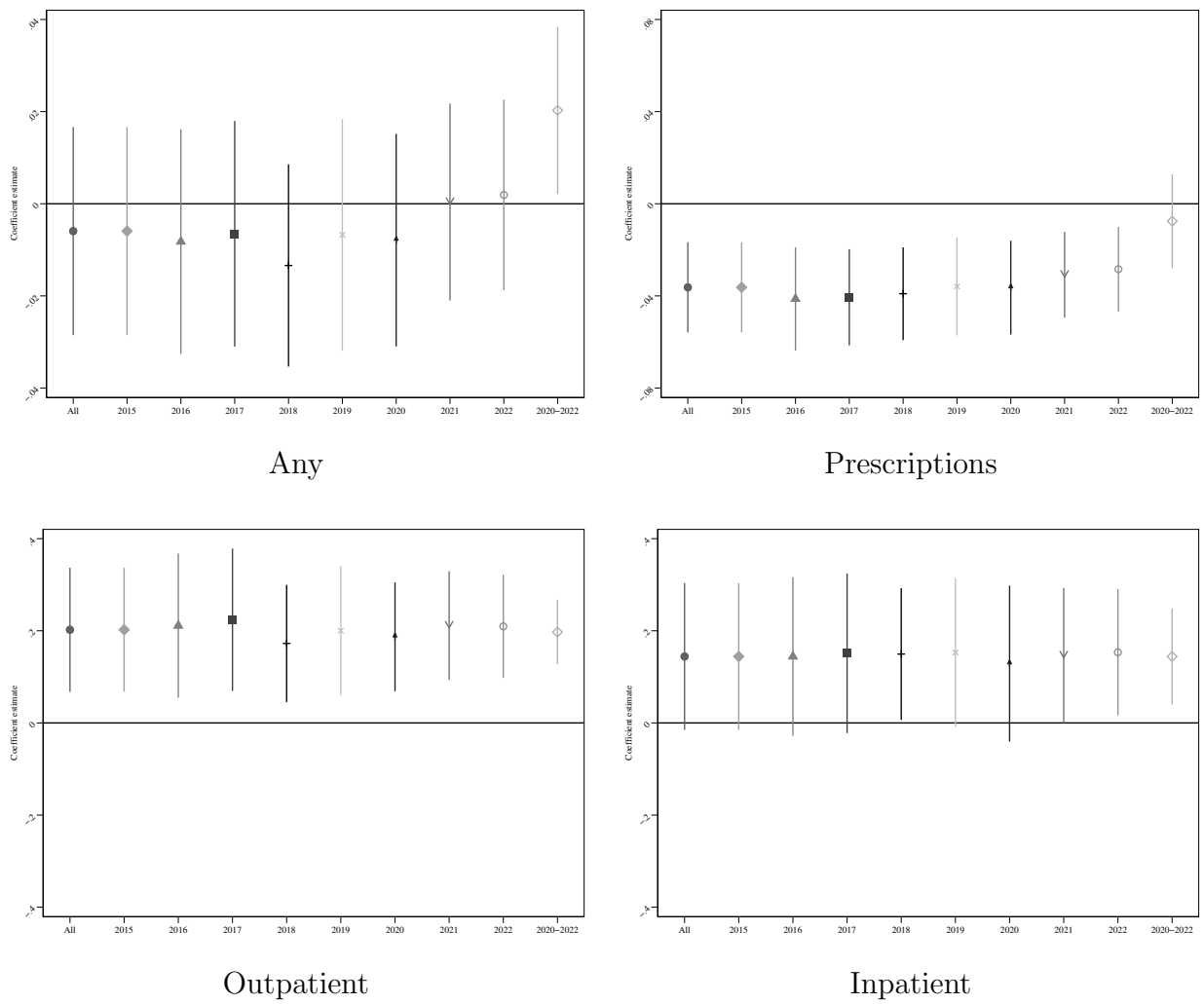
Notes: This figure reports results of a regression of mental healthcare utilization on a state paid sick leave mandate and other controls. Outcomes are patient counts. In panel A, outcomes are not logged and are scaled by 100,000 for readability. In panel B, a Poisson specification is employed, treatment 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 A9: 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



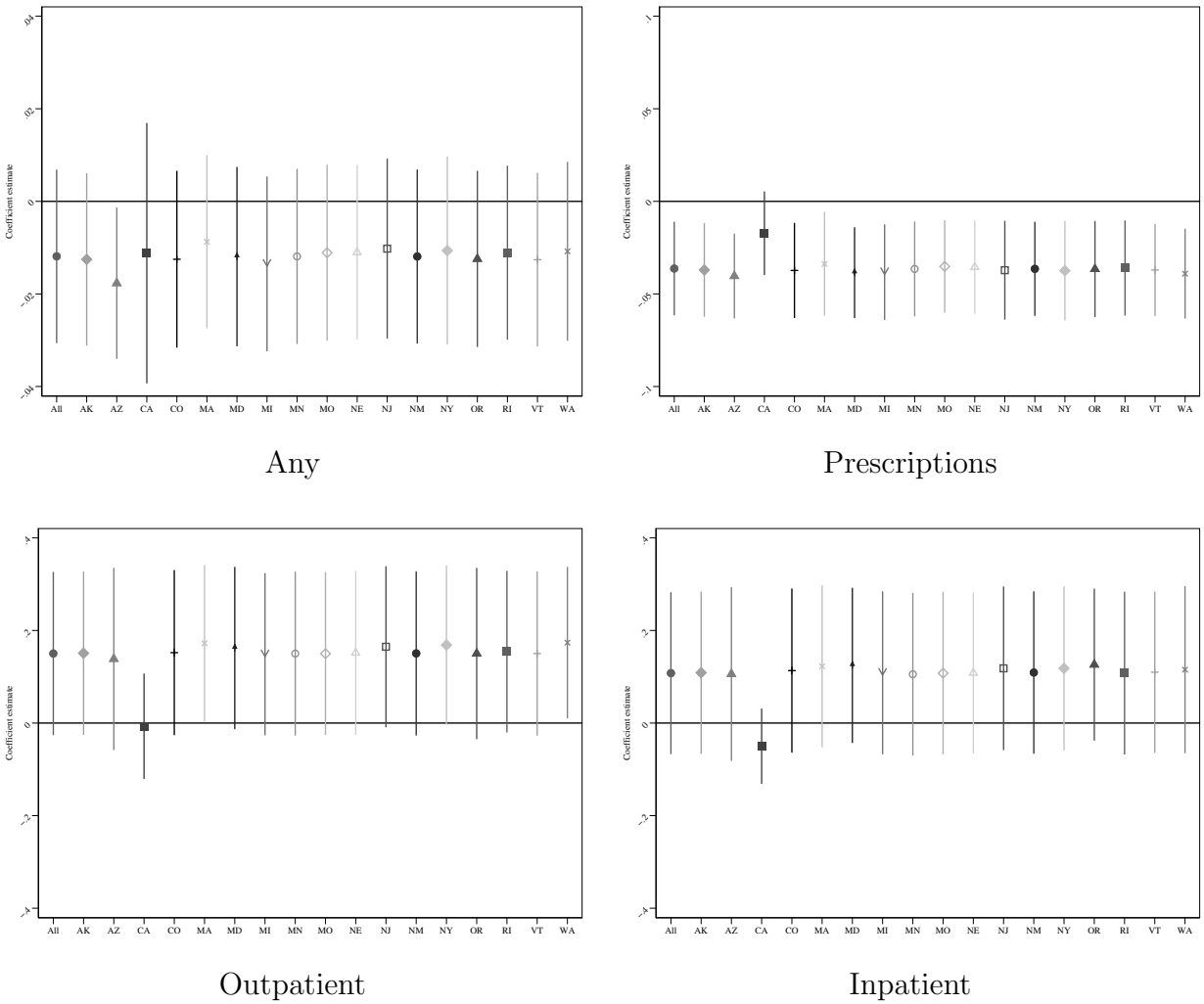
Notes: This figure reports results of a regression of mental healthcare utilization on a state paid sick leave mandate and other controls sequentially excluding each state that adopts or announces a paid sick leave mandate by November, 2024 from the sample, the excluded state is listed in the legend. 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 A10: 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



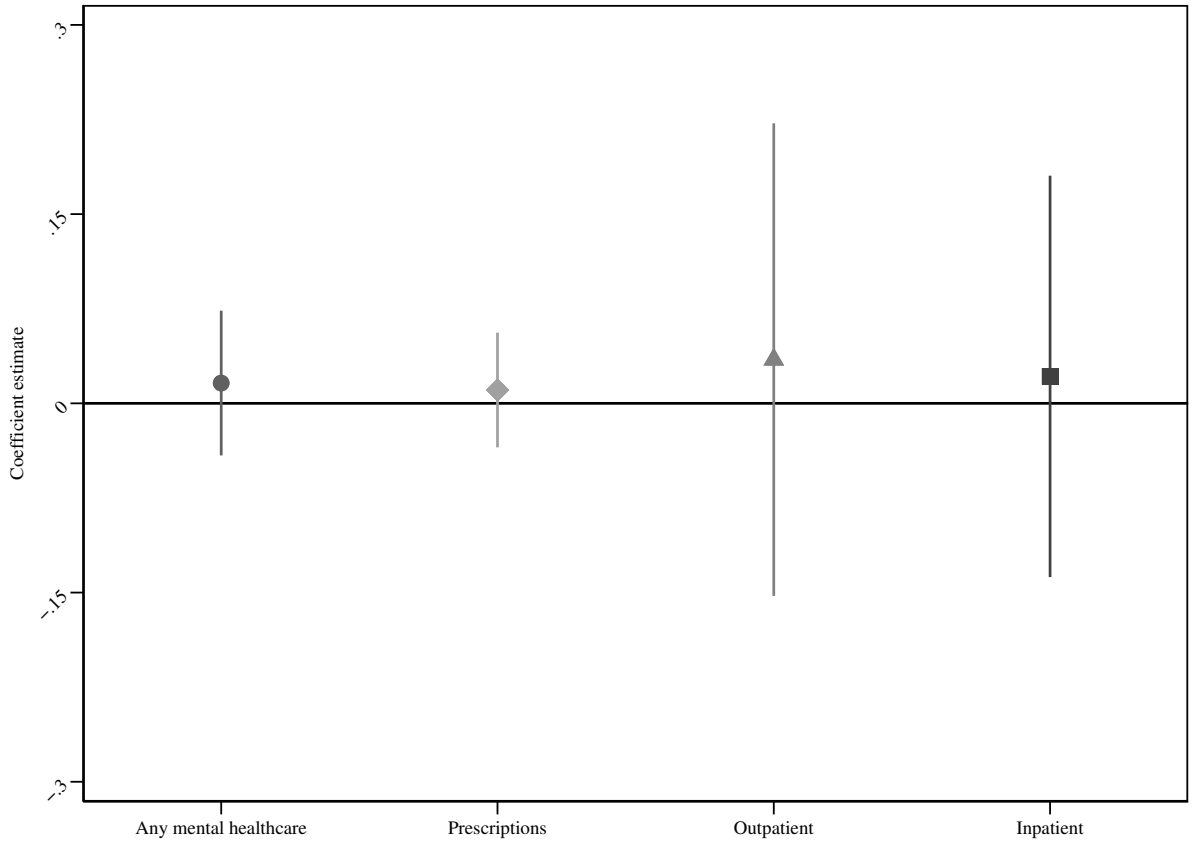
Notes: This figure reports results of a regression of mental healthcare utilization on a state paid sick leave mandate and other controls sequentially excluding each year from the sample, the excluded year is listed in the legend. Outcomes are patient counts and are log transformed. The unit of observations 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 A11: 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



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 A12: 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, 306 307, 308, 309, 311, 312, 313, 314, 316
(suicide-related)	E950, E951, E952, E953, E954, E955, E956, E957, E958, V6284
ICD-10 codes	
(general mental health)	F20, F21, F22, F23, F24, F25, F28, F29 F30, F31, F32, F33, F34, F39, F40, F41 F42, F43, F44, F45, F48, F50, F53, F54, F59, F60, F63, F68, F69, F84, F90, F91, F93, F94, F95, F98, F99
(suicide-related)	R45851, T1491, X71-X83, T36-T50 with the 6th character of 2† T51–T65 with the 6th character of 2†† 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: The effect of a state paid sick leave mandate on any mental health treatment use utilizing the Callaway and Sant'Anna (2021) procedure: IQVIA 2015-2022

Outcome variable →:	Claims	Patients
State paid sick leave mandate	0.005	-0.012
	(0.014)	(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 state fixed effects and time (month-year) fixed effects. Data are weighted by the state population. We use the Callaway and Sant'Anna (2021) procedure using 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 A5: The effect of a state paid sick leave mandate on dispensed mental health treatment prescriptions utilizing the Callaway and Sant'Anna (2021) procedure: IQVIA 2015-2022

Outcome variable →:	Prescriptions	Patients	Prescription days
State paid sick leave mandate	-0.040***	-0.036***	-0.036***
	(0.014)	(0.013)	(0.013)
Pre-treatment mean (,000s), level	1154	636	18600
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 fixed effects and time (month-year) fixed effects. Data are weighted by the state population. We use the Callaway and Sant'Anna (2021) procedure using 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 A6: The effect of a state paid sick leave mandate on outpatient mental health treatment service use utilizing a the Callaway and Sant'Anna (2021) procedure: IQVIA 2015-2022

Outcome variable →:	Claims	Patients
State paid sick leave mandate	0.163**	0.150*
	(0.079)	(0.090)
Pre-treatment mean (,000s), level	402	175
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 fixed effects and time (month-year) fixed effects. Data are weighted by the state population. We use the Callaway and Sant'Anna (2021) procedure using 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 A7: The effect of a state paid sick leave mandate on inpatient mental health treatment service use utilizing the Callaway and Sant'Anna (2021) procedure: IQVIA 2015-2022

Outcome variable →:	Claims	Patients
State paid sick leave mandate	0.166	0.108
	(0.126)	(0.089)
Pre-treatment mean (,000s), level	35	8
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 fixed effects and time (month-year) fixed effects. Data are weighted by the state population. We use the Callaway and Sant'Anna (2021) procedure using 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 A8: The effect of a state paid sick leave mandate on substitution mental health treatment service use utilizing the Callaway and Sant'Anna (2021) procedure: IQVIA 2015-2022

Outcome variable →:	No outpatient & prescriptions	Outpatient no prescriptions	Outpatient & prescriptions
State paid sick leave mandate	-0.066**	0.168**	0.146
	(0.029)	(0.073)	(0.094)
Pre-treatment mean (,000s), level	742	210	282
Observations	784	784	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 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 fixed effects and time (month-year) fixed effects. Data are weighted by the state population. We use the Callaway and Sant'Anna (2021) procedure using 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 A9: The effect of a state paid sick leave mandate on any mental health treatment use utilizing the Wooldridge (2023) procedure: IQVIA 2015-2022

Outcome variable →:	Claims	Patients
State paid sick leave mandate	0.014	-0.006
	(0.014)	(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. Data are weighted by the state population. We use the Wooldridge (2023) procedure. 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 the Wooldridge (2023) procedure: IQVIA 2015-2022

Outcome variable →:	Prescriptions	Patients	Prescription days
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

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 time-varying state characteristics, state fixed effects, and time (month-year) fixed effects. Data are weighted by the state population. We use the Wooldridge (2023) procedure. 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 A11: The effect of a state paid sick leave mandate on outpatient mental health treatment service use utilizing a the Wooldridge (2023) procedure: IQVIA 2015-2022

Outcome variable →:	Claims	Patients
State paid sick leave mandate	0.221***	0.202***
	(0.036)	(0.028)
Pre-treatment mean (,000s), level	402	175
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 time-varying state characteristics, state fixed effects, and time (month-year) fixed effects. Data are weighted by the state population. We use the Wooldridge (2023) procedure. 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 A12: The effect of a state paid sick leave mandate on inpatient mental health treatment service use utilizing the Wooldridge (2023) procedure: IQVIA 2015-2022

Outcome variable →:	Claims	Patients
State paid sick leave mandate	0.228***	0.144***
	(0.040)	(0.036)
Pre-treatment mean (,000s), level	35	8
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 time-varying state characteristics, state fixed effects, and time (month-year) fixed effects. Data are weighted by the state population. We use the Wooldridge (2023) procedure. 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 A13: The effect of a state paid sick leave mandate on substitution mental health treatment service use utilizing the Wooldridge (2023) procedure: IQVIA 2015-2022

Outcome variable →:	No outpatient & prescriptions	Outpatient no prescriptions	Outpatient & prescriptions
State paid sick leave mandate	-0.076***	0.194***	0.168***
	(0.010)	(0.023)	(0.023)
Pre-treatment mean (,000s), level	742	210	282
Observations	784	784	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 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. Data are weighted by the state population. We use the Wooldridge (2023) procedure. 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 A14: The effect of a state paid sick leave mandate on any mental health treatment use utilizing the Borusyak et al. (2024) procedure: IQVIA 2015-2022

Outcome variable →:	Claims	Patients
State paid sick leave mandate	0.024**	0.000
	(0.011)	(0.008)
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. 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 fixed effects and time (month-year) fixed effects. Data are weighted by the state population. We use the Borusyak et al. (2024) procedure. 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 dispensed mental health treatment prescriptions utilizing the Borusyak et al. (2024) procedure: IQVIA 2015-2022

Outcome variable →:	Prescriptions	Patients	Prescription days
State paid sick leave mandate	-0.036***	-0.031***	-0.031***
	(0.009)	(0.008)	(0.008)
Pre-treatment mean (,000s), level	1154	636	18600
Observations	4704	4704	4704

Notes: This table reports coefficient estimates from a regression of dispensed prescription medications utilization on state paid sick leave mandates. 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 fixed effects and time (month-year) fixed effects. Data are weighted by the state population. We use the Borusyak et al. (2024) procedure. 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 outpatient mental health treatment service use utilizing a the Borusyak et al. (2024) procedure: IQVIA 2015-2022

Outcome variable →:	Claims	Patients
State paid sick leave mandate	0.230***	0.204***
	(0.041)	(0.038)
Pre-treatment mean (,000s), level	402	175
Observations	4704	4704

Notes: This table reports coefficient estimates from a regression of outpatient service utilization on state paid sick leave mandates. 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 fixed effects and time (month-year) fixed effects. Data are weighted by the state population. We use the Borusyak et al. (2024) procedure. 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 inpatient mental health treatment service use utilizing the Borusyak et al. (2024) procedure: IQVIA 2015-2022

Outcome variable →:	Claims	Patients
State paid sick leave mandate	0.230***	0.131***
	(0.064)	(0.041)
Pre-treatment mean (,000s), level	35	8
Observations	4704	4704

Notes: This table reports coefficient estimates from a regression of inpatient service utilization on state paid sick leave mandates. 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 fixed effects and time (month-year) fixed effects. Data are weighted by the state population. We use the Borusyak et al. (2024) procedure. 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 substitution mental health treatment service use utilizing the Borusyak et al. (2024) procedure: IQVIA 2015-2022

Outcome variable →:	No outpatient & prescriptions	Outpatient no prescriptions	Outpatient & prescriptions
State paid sick leave mandate	-0.069***	0.186***	0.155***
	(0.011)	(0.033)	(0.043)
Pre-treatment mean (,000s), level	742	210	282
Observations	784	784	784

Notes: This table reports coefficient estimates from a regression of service utilization on state paid sick leave mandates. 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 fixed effects and time (month-year) fixed effects. Data are weighted by the state population. We use the Borusyak et al. (2024) procedure. 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 any mental health treatment use utilizing two-way fixed effects regression: IQVIA 2015-2022

Outcome variable →:	Claims	Patients
State paid sick leave mandate	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. Data are weighted by the state population. We use two-way fixed effects regression. 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 A20: The effect of a state paid sick leave mandate on dispensed mental health treatment prescriptions utilizing two-way fixed effects: IQVIA 2015-2022

Outcome variable →:	Prescriptions	Patients	Prescription days
State paid sick leave mandate	-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 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 time-varying state characteristics, state fixed effects, and time (month-year) fixed effects. Data are weighted by the state population. We use two-way fixed effects regression. 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 A21: The effect of a state paid sick leave mandate on outpatient mental health treatment service use utilizing two-way fixed effects: IQVIA 2015-2022

Outcome variable →:	Claims	Patients
State paid sick leave mandate	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 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 time-varying state characteristics, state fixed effects, and time (month-year) fixed effects. Data are weighted by the state population. We use two-way fixed effects regression. 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 A22: The effect of a state paid sick leave mandate on inpatient mental health treatment service use utilizing two-way fixed effects: IQVIA 2015-2022

Outcome variable →:	Claims	Patients
State paid sick leave mandate	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 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 time-varying state characteristics, state fixed effects, and time (month-year) fixed effects. Data are weighted by the state population. We use two-way fixed effects regression. 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 A23: The effect of a state paid sick leave mandate on substitution mental health treatment service use utilizing two-way fixed effects: IQVIA 2015-2022

Outcome variable →:	No outpatient & prescriptions	Outpatient no prescriptions	Outpatient & prescriptions
State paid sick leave mandate	-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 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. Data are weighted by the state population. We use two-way fixed effects regression. 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 A24: 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 A25: 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 A26: 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 A27: 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 A28: 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 →:	No outpatient & prescriptions	Outpatient no prescriptions	Outpatient & prescriptions
State paid sick leave mandate	-0.075*	0.220**	0.358**
	(0.040)	(0.110)	(0.152)
Pre-treatment mean (,000s), level	16	10	87
Observations	784	784	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.