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# THE IMPACT OF STATE PAID SICK LEAVE MANDATES ON MEDICAID-FINANCED PRESCRIPTION MEDICATIONS

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## ABSTRACT

The United States lacks a federal paid sick leave policy. However, 18 states and the District of Columbia have adopted or announced paid sick leave employer mandates to increase access to this benefit, creating a quasi-experimental setting to study whether paid sick leave affects healthcare use. People enrolled in Medicaid are an important population to study in terms of state paid sick leave policies as the majority of non-disabled enrollees are employed, but frequently work in jobs without paid sick leave. Given enrollees' lower incomes, losing earnings to receive healthcare may be a significant barrier to care. In this study, we examine the effect of state paid sick leave policies on Medicaid-financed dispensed prescription medications. Using difference-in-differences methods that are robust to bias associated with a staggered treatment rollout, we show that Medicaid-financed dispensed prescription medications increase by 6.7% following adoption of a state paid sick leave policy. These findings suggest that state paid sick leave policies promote engagement with the healthcare system and use of healthcare services among financially constrained populations.

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# 1 Introduction

Using healthcare requires both financial and time resources, but many studies focus only on financial considerations (Wright et al., 2005; Skipper, 2013; Taubman et al., 2014; Ellis et al., 2017; Ghosh et al., 2019; Maclean et al., 2019; Gruber et al., 2020; Saloner and Maclean, 2020). A relatively understudied resource required for healthcare use is time (Tucker and Davison, 2000; Cheung et al., 2012; Russell et al., 2008), which may represent a particularly important barrier to receiving healthcare for employed people with lower incomes who must often take time off from work to receive care, as many healthcare consultations and procedures occur during standard working hours (ZocDoc, 2013). Unless the employee has access to paid sick leave (PSL), time away from the job for healthcare often results in a loss of pay. The median daily earnings among employees in the United States in the third quarter of 2024 was \$233 (Bureau of Labor Statistics, 2024),[1] suggesting that such losses are non-trivial for many employees. Concerns about employer retaliation associated with taking leave (e.g., job loss, reduced probability of advancement) may further curtail healthcare use.

Most developed countries have established federal policies that provide employees with financially protected sick leave that alleviates some time constraints for healthcare. Despite strong support among Americans (Global Strategy Group & Paid Leave for All Action, 2021), the U.S. does not have such a policy (Pichler and Ziebarth, 2024). Concerns that entitlements to PSL could impose costs on business provide a potential reason for the lack of federal policy action (Vander Weerdt et al., 2023). As a result, access to PSL has historically been largely left to employers. Indeed, according to our analyses of the 2023 National Health Interview Survey (NHIS), 22% of currently employed non-elderly adults do not have access to PSL (Blewett et al., 2024). Additionally, there is disparity across workers: those in jobs that offer high wages and those with higher levels of education are more likely to have PSL than others. Data from the 2022 National Compensation Survey (NCS) indicates that 96% of employees in the highest tercile of the wage distribution have access to PSL, while just 40% in the lowest tercile receive this benefit (Bureau of Labor Statistics, 2024).

The absence of PSL may have implications for Americans. Correlational studies suggest that workers without PSL use less healthcare and have worse health than workers with this benefit (Cook, 2011; DeRigne et al., 2017; Stoddard-Dare et al., 2018; Lamsal et al., 2021).

[1] This average includes wage and salary workers. We convert average weekly earnings – \$1,165 – among full-time workers 25 years and older to daily earnings. People enrolled in Medicaid – the focus of our study as described later in this section – who work potentially have lower earnings than the median for all workers. We can examine earnings for workers with lower levels of education to gain insight as results are not reported by insurance coverage. The median daily earnings for full-time workers with less than a high school degree and a high school degree but no college education were \$147 (=\$734/5) and \$189 (=\$946/5). People enrolled in Medicaid have lower income and thus these losses of financial resources are likely salient to them.

For example, using the NHIS, Lamsal et al. (2021) show that employees who report access to PSL benefits use more outpatient care (odds ratio = 1.16) than employees who do not report this benefit, where outpatient care includes seeing a healthcare professional (e.g., general doctor or nurse practitioner) in the past 12 months in an office-based setting.

At the time of writing, 18 states and the District of Columbia have adopted or announced PSL mandates that require employers in these jurisdictions to provide workers with approximately seven days of PSL per year (National Partnership of Women & Families, 2023; A Better Balance, 2025). This leave can be used for the worker's own health and healthcare use, and for the needs of dependents (children, spouses, parents, etc.). Previous economic research shows that these PSL entitlements are associated with reduced infectious disease spread (including at the workplace), improved health, increased earnings and worker productivity, greater use of healthcare – in particular care that is more time-intensive, and decreases in high-cost care such as emergency department episodes (Ahn and Yelowitz, 2016; Pichler and Ziebarth, 2017; Stearns and White, 2018; Pichler et al., 2021; Callison and Pesko, 2022; Ma et al., 2022; Wolf et al., 2022; Callison et al., 2023; Maclean et al., 2023; Slopen, 2023; Maclean et al., 2024, 2025; Slopen, 2024; Deza et al., 2025; Eisenberg et al., 2025).[2] Further, several studies demonstrate that mandated PSL is used to provide care to dependents including children and older adults (Arora and Wolf, 2024; Maclean and Pabilonia, 2025; Byker et al., 2023; Guo and Peng, 2024), which may be associated with less contagion in schools, overall reduction in missed school days, and improved health and schooling outcomes among children, and increased well-being among older adults. Finally, in contrast to concerns raised by business owners, mandated PSL is relatively inexpensive and does not appear to adversely impact employers and may increase employee productivity (Chunyu et al., 2022; Miller, 2022; Maclean et al., 2025; Slopen, 2024).

People enrolled in Medicaid are an important and policy-relevant group to consider in the context of state PSL policies. Medicaid is a public insurance program for Americans with lower incomes jointly funded by the federal government and states that covers a relatively generous set of healthcare services including preventive care, disease management, prescription medications, hospitalization, emergency, and ambulatory care. In August 2024, Medicaid covered 73 million adults and children (Centers for Medicare & Medicaid Services, 2024). People enrolled in Medicaid generally face low cost-sharing for healthcare, but this population appears to be quite price-responsive. For example, Hartung et al. (2008) find that implementation of relatively small co-payments ($2 for generic and $3 for branded med-

[2] We note that a full consensus on healthcare service use has not been reached. In particular, Guo and Peng (2024) find no evidence that PSL mandates impact preventive healthcare utilization using the Behavioral Risk Factor Surveillance Survey.

ications) leads to a 17.2% reduction in prescriptions, and Subramanian (2011) shows that people with cancer and enrolled in Medicaid reduce prescriptions related to their condition following modest increases in co-payments. Salient to our study – most adults under age 65 and enrolled in Medicaid (61%) work (Guth et al., 2023) and they are often employed in low-wage jobs that do not provide PSL unless the employer is mandated to do so. According to our analyses of the 2023 NHIS, only 49% of adults under 65 years of age and employed at the time of the survey enrolled in Medicaid reported having access to PSL, compared to 79% of adult employees in the same age range and not enrolled in Medicaid (Blewett et al., 2024). Mandated PSL benefits can also be used to support dependents' receipt of healthcare. For instance, in 2023, Medicaid-provided health insurance coverage for 39% of children in the U.S. (Kaiser Family Foundation, 2023), and parents who work may use PSL to seek healthcare for their children. Finally, people enrolled in Medicaid have high rates of chronic conditions and are less likely to receive guideline-concordant care than non-enrollees (Goldman et al., 2007; Chapel et al., 2017), which suggests that people enrolled in this insurance program may benefit disproportionately from access to PSL.

Despite substantial evidence of potential gaps in PSL and unmet medical needs within the Medicaid population, research on the impacts of PSL for healthcare utilization of people enrolled in Medicaid remains limited. Prescription medications are frequently used for the treatment and maintenance of acute and chronic health conditions and are covered by state Medicaid programs. Moreover, prescription medications are often the first line of treatment, and may help avoid more aggressive and costly interventions such as surgery and use of emergency departments for conditions that could be managed in less costly settings. Indeed, the literature documents that improved access to prescription medications is associated with reduced healthcare costs in the Medicaid population (Roebuck et al., 2015). Finally, prescription medications can proxy for access to the healthcare system (Kasper and Wilson, 1983; Lin et al., 2005; Cossman et al., 2010; Mortensen et al., 2021). Only one study to date has examined this question, narrowly focused on a single therapeutic class: Maclean et al. (2024) examine the effect of PSL mandates on Medicaid-financed mental health medications only and find that dispensed prescriptions increase post-mandate. Given the distinct nature of mental healthcare — such as sometimes being viewed as more 'discretionary' than physical healthcare, widespread shortages in mental healthcare professionals that may impede access, and substantial under-diagnosis of conditions among people enrolled in Medicaid (Frank and McGuire, 2000; Beck et al., 2018; Kaiser Family Foundation, 2024a) — investigating the extent to which PSL mandates impact a broader set of dispensed medications is important.

The current study helps to address this gap by studying the effect of state PSL mandates on prescription medications, in all therapeutic classes, dispensed to people enrolled in Medicaid from retail settings over the period 2011 to 2024. Prescription medications dispensed in retail settings capture the ability of people enrolled in Medicaid to access healthcare in non-emergency and potentially less costly care modalities. Our findings suggest that state mandates increase the number of prescriptions financed by Medicaid by 6.7%, with some heterogeneity across medication classes. An interpretation of these findings is that reducing time-related barriers can improve healthcare access among people enrolled in Medicaid.

We make several contributions to the literature. First, we add new evidence on social insurance program spillovers by examining how a new employer mandate (i.e., PSL) may interact with a long-standing public health insurance program (i.e., Medicaid).[3] Understanding such interactions is necessary to assess the value to society of a specific program (Baily, 1978; Chetty, 2006; Hendren, 2016). Earlier work in the U.S. context has investigated possible spillovers across a range of social insurance programs. For instance, Duggan et al. (2007) show that more generous retirement benefits reduce disability insurance enrollment. Two studies demonstrate that cash assistance programs – Aid to Families with Dependent Children (AFDC) and Supplemental Security Income (SSI) – may serve as substitute sources of income/financial resources for populations with lower incomes (Schmidt and Sevak, 2004; Garrett and Glied, 2000). Conversely, increasing the generosity of Workers' Compensation benefits or expanding Medicaid coverage does not impact use of disability insurance (McInerney and Simon, 2012; Schmidt et al., 2020). Second, we provide new evidence to the growing economic literature on the impacts of recent state PSL mandates (summarized above), showing effects for a policy-relevant population with elevated health conditions – people enrolled in Medicaid. Third, we contribute to the broader literature examining factors that impact the demand for healthcare services by studying the importance of the time required to use healthcare services, which has received limited previous research attention.

The paper is organized as follows. Section 2 reviews the paid sick leave landscape in the U.S. Our data and methods are described in Section 3. The main findings are reported in Section 4. Heterogeneity analyses, additional results, and robustness checking are presented in Sections 5 through 7. Finally, Section 8 provides a discussion and conclusion.

## 2 Paid leave in the United States

As noted in Section 1, the U.S. does not have a federal paid sick leave policy in place. There have been efforts – beginning with Senator Ted Kennedy sponsoring the *Healthy Families Act* in 2005 (Pichler and Ziebarth, 2024) and most recently in 2023 (Sanders and DeLauro, 2023) – to implement a federal policy, but none have been successful to date.

[3] Medicaid has been in place since 1965.

The only somewhat related federal leave mandate is the 1993 *Family and Medical Leave Act* (FMLA), which provides employees who have worked 1,250 or more hours per year for an employer with at least 50 employees with rights to 12 weeks of **unpaid** leave that can be used for pregnancy, own health, or the health of a family member. Given exclusions, nearly half of employees are not eligible for FMLA (Appelbaum, 2014). In April 2020, and in response to the COVID-19 pandemic, the U.S. passed a temporary federal PSL policy – the *Families First Coronavirus Response Act* (FFCRA) – which provided up to two weeks of paid leave for pandemic-related health and family demands for some workers (Andersen et al., 2023). That Act was not re-authorized beyond December 2020. We will show in Section 7 that our results are not sensitive to excluding 2020.

States have implemented a variety of paid leave policies, some of which insure against risks that are related to those covered by PSL mandates. Most of the policies are designed for longer-lasting separations. For instance, all states except Texas, South Dakota, and Wyoming require employers to offer Workers' Compensation benefits (Murphy and Wolf, 2022). These benefits are essentially a form of 'no-fault' insurance and provide paid leave, healthcare, rehabilitation services, and death benefits for employees who are injured, become ill or die while working. In return for this insurance, employees are generally prohibited from filing tort lawsuits against their employers for covered work-related injuries and illnesses.

Five states – California, Hawaii, New Jersey, New York, and Rhode Island – adopted Temporary Disability Insurance (TDI) in the 1940s and 1960s.[4] Though there is some heterogeneity across states, TDI provides several weeks of partial wage replacement for medical and family reasons (e.g., childbirth). Beginning with California in 2004, 13 states and the District of Columbia have implemented paid medical and family leave (PMFL) insurance policies that provide paid leave that can be utilized for similar purposes (National Partnership for Women & Families, 2022). There is overlap in the states that adopt the two policies: California, New Jersey, New York, and Rhode Island have both TDI and PMFL.

At the time of writing, 18 states and the District of Columbia have adopted the PSL mandates that are the focus of our study (National Partnership of Women & Families, 2023; Pichler and Ziebarth, 2024; A Better Balance, 2025).[5] The District of Columbia adopted the first such mandate in May 2008, followed by Connecticut in January 2012. Figure 1 displays the geographic distribution of state PSL mandates. Table 1 reports the effective month and year for each mandate adopting state, and (for some states) a lower-bound on the number of people gaining access to PSL from the mandate (National Partnership of Women & Families,

[4] The effective years for TDI policies are as follows: 1942 in Rhode Island, 1946 in California, 1948 in New Jersey, 1949 in New York, and 1969 in Hawaii (Pichler and Ziebarth, 2024).

[5] The District of Columbia is technically a district, but we use the term state for simplicity.

2023). We also indicate in Table 1 whether the state has enacted a PMFL policy, which is the case for most PSL mandate-adopting states. In addition to the numbers cited on the table, employers may have offered some PSL but less than the legislated amount and so, once the mandate is in place, would have to increase benefit generosity. In addition, a newly eligible employee can use benefits to support dependents' use of healthcare, and dependents are not counted in the numbers in the table.

We note that Missouri adopted a PSL mandate in May 2025, but the state legislature later passed legislation that repealed this policy, effective August 2025. Michigan originally adopted a PSL mandate in 2019, but prior to the effective date, provisions were rolled back – in particular protections for workers using leave (e.g., requirements that employees must locate a 'replacement' employee when using leave and protection for employees using mandated leave from employer retailiation). However, in November 2024 the Governor re-instated the original legislation with an effective date of February 2025, thus we code this state as having a PSL mandate as of February 2025 in our analysis. Both the Michigan and Missouri policies occur after our study period ends. Thus, these policy changes do not contribute towards our difference-in-differences findings. However, we will exclude these states from the analysis sample and our event-study findings are nearly identical.

Current state PSL mandates are similar in structure to the originally proposed 2005 *Healthy Families Act*. PSL mandates are individual accounts and employees must work to earn benefits, with accrual typically beginning after a minimum time employed. Employees are provided with, on average, seven days per year of sick leave. All states allow rollover of at least some unused days of PSL. For example, Connecticut mandates that workers can carry forward forty hours of PSL each year. Benefits can be used for own health and healthcare, and the corresponding needs of dependents. Office visits – where a patient could obtain the prescriptions that we study – are eligible healthcare under all state mandates. All programs list spouses and children as eligible dependents, with most states also covering additional dependents such as parents and domestic partners. There is limited monitoring of employee use of PSL. In most states, employees do not need to provide any documentation if the duration of leave is less than three days. Employers are prohibited from retaliating against employees using leave and employees do not need to locate a 'replacement' worker during their job absence. Employees do not have to provide advance notice for emergencies. There are some exemptions for specific industries and for smaller employers in select states. Some states also provide **unpaid** sick leave, in addition to the paid time off work. PSL can be used in combination with other types of leave. For example, PSL could potentially be used during the waiting periods that often exist for receiving Workers Compensation.

While PMFL policies and PSL mandates both provide financially protected time away from work for health-related reasons to employees, in terms of the prescription medications we study, these policies are not likely direct substitutes for most workers. Though there are differences across states, PMFL policies are designed for longer-term job absences — in particular, eligible leave use in all states includes bonding with a new child, caring for a dependent with a serious illness or injury, and recovering from own serious illness or injury (National Partnership for Women & Families, 2022). PSL mandates, on the other hand, are designed for shorter-term separations associated with interment illness (either own or that of a family member such as a minor child who stays home from school when sick), doctor's appointments, and so forth. For PMFL, most states do not have a waiting period, but for own disability, the modal waiting period ranges from zero to seven, with California having a waiting period of seven days. There is no waiting period for mandated PSL.[6] In terms of the minimum increment of PMFL time that can be used, most states do not have a minimum, but among those that do, the modal minimum value is one day. California's policy does not list a minimum amount of leave that can be utilized. PSL mandates allow employees to use as little as an hour of leave at a time. Moreover, to use PMFL leave, in all states, employees must apply to the state program that administers PMFL and have the application approved. The organization charged with managing the PMLF program is often affiliated with the state's Department of Labor, for example, in California, the program is operated by the Employment Development Department. Finally, in all states, PSL mandates require full wage replacement rates for employees on leave while the replacement rates for PMFL are generally less than one with minimum and maximums established by the state, for example, in California, at the time of writing, workers whose quarterly earnings are at least $929 but less than 1/3 of the state average quarterly wage, the weekly benefit is 70% of the weekly wage. For these reasons, we expect that state PSL mandates to be more likely to be used for activities related to the prescription medications dispensed in retail pharmacies that we study (e.g., doctors' appointment) than state PMLF policies.

Maclean et al. (2025) use the 2009 to 2023 National Compensation Survey (NCS) – a survey of establishments in which human resource managers provide detailed information on wages and benefits – to show that state PSL mandates lead to meaningful changes in employee access to PSL and employee use of these benefits. For example, following a state mandate, the probability that an employee has rights to PSL increases by 32% and use of PSL benefits rises by four hours per quarter or 22%. The gains are largest in positions with low pre-mandate access to PSL, which potentially overlap with the jobs held by people enrolled in Medicaid. Two studies, using the NHIS, indicate that employees report improved

[6] There is a waiting period for new employees: employees must work for a specified period of time, generally 90 days, prior to using PSL, but there is no waiting period at the time of application to use the leave.

access to PSL following adoption of a state mandate (Ahn and Yelowitz, 2016; Callison and Pesko, 2022). Maclean et al. (2025) document that the costs to employers associated with PSL mandates are not overly large: 6.2 cents per employee hour worked, and that employers do not curtail other components of the compensation package following implementation of a state PSL mandate. Instead employers may engage in 'up-scaling' behavior in which they increase benefit offerings perhaps to compete for more qualified employees.

Four states (Illinois, Maine, Michigan, and Missouri) have adopted 'paid time off' (PTO) mandates (National Partnership of Women & Families, 2023). These are similar to PSL mandates in many ways, but offer less protection for employees. For example, the employee may be required to locate a replacement worker and must provide advance notice, thus using leave for emergency purposes is more challenging. PTO mandates also do not explicitly prohibit retaliation by employers, which could discourage employee use of the benefit. For this reason, we treat PSL and PTO mandates as separate policies in our main analysis, but we will report similar findings in Section 7 where we include PTO states in our definition of mandated PSL. Notably, in November 2024, the Governor of Michigan re-instated the originally passed legislation (effective February 2025), leading us to code Michigan as having a PTO mandate from 2019 to 2024, and then as having a PSL mandate in 2025.

Seventeen cities and four counties have also adopted PSL, beginning with San Francisco, California in 2007 (National Partnership of Women & Families, 2023). If a state and sub-state jurisdiction both have a PSL mandate that is in place, the more generous law prevails. Conversely, 23 states have adopted preemption laws that prevent localities from adopting more generous PSL than that set by the state, which can include no mandate (Peters et al., 2020). Our data source (described in Section 3.1) does not include sub-state geographic information and thus we do not directly study the sub-state mandates. However, we will account for these mandates in Section 7 and our findings are not appreciably different.

We will control for state PMFL policies and PTO mandates in our preferred regressions, we do not control for TDI or mandated Workers Compensation benefits as these are time-invariant policies during our study period.

In summary, there are various leave policies in the U.S. for own health or family responsibilities. Though related, the leave provided by these policies are not likely perfect substitutes for all workers and all health needs as they insure against different types of risk.[7] FMLA, TDI, PMFL, and Workers Compensation policies are designed for longer-term separations from work to allow employees to recover from major health events (e.g., a fall at work that

[7] We do not mean to imply that there is no substitution across programs – for example, Dong et al. (2024) provide evidence that some workers may use PSL rather than Workers Compensation following a PSL mandate. Rather, for some workers and some types of healthcare the leave policies are not interchangeable.

requires surgery) or to attend to family responsibilities (e.g., child birth).[8] Conversely, state mandated PSL is designed for short-term separations from work for healthcare and family needs – for example, an office visit with a physician. PTO, as described above, is potentially the closest substitute to PSL and we will include PTO in our definition of PSL in robustness checking (Section 7). While all policies offer leave to at least some employees, these policies are distinct in terms of the duration of the financially protected time away from work they provide, level of financial protection offered, and the types of risks they insure against.

State PSL mandates may allow greater use of both preventive and ambulatory care which could lead to a reduction in, more costly, emergency care. For example, an employee could take time off to see a primary care clinician when they are experiencing cold-like symptoms, and receive an antibiotic (which we would capture in our data, which are described in Section 3.1) and this ambulatory care could prevent the condition from worsening and requiring more costly care (e.g., a hospitalization to treat pneumonia). Ambulatory care that offsets more costly downstream treatment could also extend to an employee's dependents, for example, a parent taking a child with an earache to the a primary care physician in an outpatient setting rather than delaying treatment until the child requires emergency care.

# 3 Data and methods

## 3.1 State Drug Utilization Database

We use administrative data on the universe of dispensed prescription medications obtained in retail settings and financed, fully or partially, by state Medicaid programs in the CMS-administered State Drug Utilization Database (SDUD) over the period 2011 Q1 to 2024 Q4 (the most recent quarter of data available at the time of writing). The SDUD capture prescriptions from retail pharmacies. These prescriptions are likely written by healthcare professionals in outpatient settings such as primary care, clinics, and other office-based settings. Data in the SDUD are recorded as the number of dispensed prescriptions of specific drug types for each state-year-quarter. Included are both prescription and over-the-counter medications, the latter since Medicaid plans often cover over-the-counter medications (Florida Agency for Health Care Administration, 2024; Virginia Medicaid Pharmacy Services, 2024) and, typically, the cost to the patient is lower if insurance is used. The SDUD contain no information on patients or providers, instead supplying aggregate prescription counts financed by Medicaid. Beginning in 1992, to participate in the Medicaid Drug Rebate Program

[8]As described earlier in this section, Workers Compensation benefits, unlike other leave policies, can include fully compensated healthcare, rehabilitation services, and – in the case of a employee's death – benefits to the family.

(MDRP), states must submit data on all dispensed medications financed by fee-for-service Medicaid. As of March 2010, states are also required to submit corresponding data on managed care Medicaid. Some states were slow to start reporting managed care data, thus we follow earlier studies and begin our study period in 2011 (Wen et al., 2017; Maclean et al., 2024). Our primary measure is the number of prescription medications financed by Medicaid per state-year-quarter. Though the SDUD provides prescription counts separately for fee-for-service and managed care Medicaid, we do not distinguish between the two types of programs (Wen et al., 2017). Many states carve-out and carve-in pharmacy benefits in Medicaid (Hinton and Raphael, 2024), which complicates separation of the two types of plans in our settings. Our analysis sample includes 2,800 state-year-quarter observations.

In the SDUD, similar to other federal administrative data sources (Centers for Medicare & Medicaid Services, 2020), cells (medication-state-year-quarters) with less than 11 prescriptions (43% of cells in our data) are suppressed and flagged.[9] In our main analysis, we assign suppressed cells a value of five, the mid-point of suppressed values (Maclean et al., 2024), but we show in robustness checking (Section 7) that results are not sensitive to this choice.

Our main dependent variable is the natural logarithm of dispensed prescription counts, we apply the logarithm to address skewness in this variable. Figure A1 reports a histogram of the untransformed count (Panel A) and logged count (Panel B). We will report results in Section 4 using the unlogged count and least squares regression, and the prescription rate, and the findings are qualitatively similar.

In the SDUD, we measure aggregate counts of dispensed prescription medications. Prescriptions can vary by pack size and dosage, among other factors. Though there are differences across states, for most prescriptions, Medicaid prescription medications dispensed at the retail pharmacies that we capture in the SDUD are capped at approximately one month's supply. Exceptions to this practice include maintenance drugs. However, this feature of the SDUD is a limitation of the study.

In 2019, there were 12.3 million people enrolled in Medicaid and Medicare ('dual eligibles') (CMS Medicare-Medicaid Coordination Office, 2020). For these people, Medicare tends to be the first-payer for services while Medicaid finances care not covered by Medicare. The SDUD do not capture medications dispensed to dual eligibles that are fully financed by Medicare. People enrolled in both Medicaid and Medicare are less likely to work than people enrolled in Medicaid only given the former group's complex medical conditions (Johnston and Joynt Maddox, 2019), thus PSL mandates are less likely to directly affect healthcare-related time constraints among dual elgibibles. For this reason, we do not view the inability to fully capture prescription medications dispensed to dual eligibles as a major limitation.

[9]We weight the data by the number of people enrolled in Medicaid in a state-year-quarter.

In our main analyses, we focus on total dispensed prescriptions, which we view as a proxy for access to healthcare (see Section 1). However, in Section 5 we report results for specific medication classes. Following Ghosh et al. (2019), we examine the following drug classes: antibiotics, birth control, cardiovascular, diabetes, gastrointestinal, HIV and hepatitis, mental health, and respiratory and allergies. To classify medications, we follow the Anatomical Therapeutic Chemical Classification System (ATC), which provides a standardized classifications of drugs based on their therapeutic use and chemical characteristics (World Health Organization, 2024). We add three drug classes likely relevant for people enrolled in Medicaid: 1) chronic pain medications,[10] 2) smoking cessation medications (Maclean et al., 2019), and 3) substance use disorder medications (Maclean and Saloner, 2019).[11] Given co-morbidity across conditions (Kalman et al., 2005), we combine mental health, substance use, and smoking cessation medications into a single prescription group. Finally, we include a catch-all category for all other medications. Notably, there are no zero values for the overall prescription count variable nor for any of the drug classes we study. Thus, taking the logarithm in this setting is reasonable (Chen and Roth, 2024).

A limitation of the heterogeneity analysis is that we may inaccurately classify drugs for several reasons. First, we use both National Drug Codes (NDCs) and medication names to classify drugs, with the latter being particularly susceptible to human entry errors. NDC codes change frequently, sometimes even daily, and although we use a different set of NDCs for each study year sourced from the Food and Drug Administration, we likely miss some changes in the codes.[12] Further, when matching on prescription medication names, there could be errors (including spelling errors) in the drug name provided in the SDUD data. Finally, medications can be used for different purposes, for example, bupropion is used for both smoking cessation and the treatment of depression. Table A1 reports examples of prescription medications included in each of the drug classes; a full list of prescription medications is available on request from the corresponding author.

Whether state PSL mandates afford employees with sufficient time to receive a prescription from a healthcare professional is important to determine for our study. Rui and Okeyode (2016) document, using the 2016 National Ambulatory Medical Care Survey, there were 884,000,000 office visits in the U.S. and 74.3% of these visits involved a mention of a medication.[13] The average time spent with the healthcare professional in 2016 was 22.5

[10] We include the following ATC drug classes M01, M02, M03, M04, M05, M09, N02A, N02B, and N02.

[11] The latter two drug classes are not easily captured in the ATC coding and thus we follow earlier studies using the SDUD.

[12] One exception is that we use 2012 NDC codes for the 2011 SDUD due to changes in file structure in that year. We have excluded 2011 from the heterogeneity analysis and results are not different. Full results and details are available from the corresponding author on request.

[13] See Table 22. According to this report, a drug mention is defined as 'documentation in a patient's record

minutes.[14] If we assume that the average wait time for an office visit was 28.7 minutes (Tak et al., 2014) and that patients faced a travel time of 37.6 minutes (authors' analysis of the 2011-2023 American Time Use Survey (Flood et al., 2023)),[15] then the total time required for an office visit would be 88.8 minutes. In Section 4, we will empirically test whether PSL mandates lead to changes in PSL use that are substantial enough in terms of time to allow for an office visit in which a person enrolled in Medicaid might receive a prescription.

## 3.2 Paid sick leave mandates

We rely on legal coding of state PSL mandates from the National Partnership of Women & Families (NPWF) that includes all mandates adopted or announced as of the end of 2023 (National Partnership of Women & Families, 2023), we supplement this information with data from A Better Balance (2025) for several mandates announced after that date.

The District of Columbia and 18 states have adopted or announced a PSL mandate. We exclude the District of Columbia from our analysis as this jurisdiction adopted a mandate in 2008 and therefore is treated in all years of our study. Our primary explanatory variable is an indicator of whether or not the state has a PSL mandate in place in a specific quarter-year. Though not identical, these mandates largely mirror the provisions proposed in the 2005 *Healthy Families Act* and thus we follow the literature (see Section 1) and do not distinguish between different features of the mandates.[16]

## 3.3 National Compensation Survey

We use restricted-use National Compensation Survey (NCS) data from 2009-2023 to study the impact of PSL mandates on PSL access and use (the 'first-stage' in our analysis).[17] The NCS, a nationally representative dataset of establishments, is administered by the Bureau of Labor Statistics and used to provide official government statistics on employee wages and benefits, and to adjust federal employee compensation. Survey responders are employees familiar with the establishment's human resources policies (e.g., a human resources director). The unit of observation is a probabilistically selected job in an establishment, state and year. We have 727,962 observations in the analysis sample.

of a drug provided, prescribed, or continued at a visit.'

[14]Information on time spent with the healthcare professional is drawn from Table 29 of the report.

[15]We use the activity code 180804 ('Waiting associated with medical services') and take the average time among those with positive values. We are not able to isolate office visits from other healthcare services.

[16]States differ to some extent in terms of accrual rates, definition of dependents, and exemptions for/definitions of small firms.

[17]At the time of writing, the 2023 National Compensation Survey is the most recent year of data available.

Specifically, we follow Maclean et al. (2025) and examine mandate effects on the probability that a job has PSL benefits and use of PSL by employees. We construct an indicator variable that equals one if a job has access to PSL and zero otherwise. This variable is measured once per year and reflects jobs in the first quarter of the year. PSL use is measured each quarter and we aggregate the quarterly data to construct year $t$ PSL use measures by aggregating Q2, Q3, and Q4 for years *t-1* and Q1 in year $t$ following Maclean et al. (2025). Our rationale for taking the average leave use over four quarters, following (Maclean et al., 2025), is that we are interested in average leave use patterns over a year. If we were to use a single quarter (e.g., Q1 which includes January through March), then we could capture seasonality in leave use (e.g., flu season). Using all four quarters offers a more comprehensive picture of leave-taking. The NCS do not include quarterly information on PSL access, thus we do not take the average for that outcome. We analyze the NCS data at the job-establishment-state-year level. Maclean et al. (2025) focuses on private establishments as these establishments are more likely to be impacted by the mandates; however, we include both public and private jobs as we cannot make this distinction in the SDUD.

## 3.4 Annual Social and Economic Supplement to the Current Population Survey

To offer suggestive evidence on whether PSL mandates, and associated changes in prescription medication, impact the health of people enrolled in Medicaid, we turn to the Annual Social and Economic Supplement to the Current Population Survey (Ruggles et al., 2023). This survey – administered by the U.S. Census Bureau on behalf of the Bureau of Labor Statistics – is fielded each year between February and April. Approximately 150,000 respondents are asked detailed questions on income sources, benefit receipt, insurance, labor market outcomes, health, healthcare expenditures, and demographics. We retain adults 19-64 years ($N$=199,792) and children 0-18 years ($N$=247,939) covered by Medicaid at some point in the past year over the period 2011-2024 for our analysis sample.

We construct an indicator for reporting very good or excellent health at the time of survey completion. To complement our analysis of the SDUD, we also investigate the impact of state PSL mandates on past year out-of-pocket medical expenditures.[18] These expenditures should include cost-sharing for dispensed prescription and other healthcare expenditures

[18]More specifically, the Annual Social and Economic Supplement includes total medical expenditures and premium expenditures, we take the difference between the two variables to construct a measure of out-of-pocket expenditures. Because the variables reflect past-year expenditures, we lag the variables one year. For example, in the 2024 survey, the expenditures refer to the year 2023, thus we treat the 2024 survey year as the 2023 'calendar year' following other work using the Annual Social and Economic Supplement to examine variables with this time-frame (Abouk et al., 2023).

(e.g., co-payments for office visits). We inflate the expenditure values to 2024 dollars using the Consumer Price Index-Urban Consumers.

The Annual Social and Economic Supplement to the Current Population has some limitations. First, though self-reported health measures predict objective health measurements (Lorem et al., 2020), this metric may capture different dimensions of health across respondents (e.g., physical health, mental health) and there could be reporting error (Greene et al., 2015). Second, there are changes in the insurance and healthcare expenditure questions between 2013 and 2014 (Pascale et al., 2016; Hill et al., 2019). Thus, we view our analysis of self-reported health and healthcare expenditures using the Annual Social and Economic Supplement as exploratory only.

## 3.5 Methods

We use difference-in-differences methods to study the impact of state PSL mandates on Medicaid-financed dispensed prescription medications. Our primary estimates are obtained using the two-stage imputation procedure developed by Gardner (2022). In the first stage, we estimate the relationships between the outcome and time-varying covariates and fixed effects using only the untreated observations (we include both the never-treated and the not-yet-treated observations). We then use these estimated parameters to residualize the outcomes for both the treated and untreated observations. In the second stage of the procedure, we regress the residualized outcomes on the treatment variable (i.e., state PSL mandate) for both treated and untreated observations. The primary specification takes the form:

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

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

where $P_{st}(0)$ captures an untreated Medicaid-financed prescription medication outcome for a state $s$ in period (quarter-year) $t$, where equation (1) is estimated using only observations for untreated states (where $PSL_{s,t-1}$ an indicator variable that takes on a value of one if a state has a PSL mandate in place at any point in period $t-1$ and zero otherwise).[19] PSL mandates are lagged one year to allow Medicaid enrollees to accrue and learn about benefits, though, we will show in Section 7 that results are not sensitive to using the current mandate.

Equation (1) includes state ($\alpha_s$) and period ($\gamma_t$) fixed effects to account for pre-existing state differences in Medicaid-financed prescriptions and common secular shocks. We also

[19]Thus, changes that do not become effective on the first day of a quarter are coded as in place in their first partial quarter.

control for time-varying state characteristics ($\boldsymbol{X}_{st}$), including: PMFL policies (National Partnership for Women & Families, 2022), PTO mandates (National Partnership of Women & Families, 2023), Affordable Care Act (ACA) Medicaid expansions (Kaiser Family Foundation, 2024b), demographics (Ruggles et al., 2023), and the number of people enrolled in Medicaid (University of Kentucky Center for Poverty Research, 2023). Demographics include age (0-15 years, 16-44 years, and 45+ years, with 0-15 years as the omitted category), sex (male and female, with male as the omitted category), race (White, Black, and race, with White as the omitted category), ethnicity (Hispanic and non-Hispanic, with non-Hispanic as the omitted category), and education (less than high school, high school, some college, and a college degree, with less than high school as the omitted category). We use predictions from equation (1) as counterfactuals ($\widehat{P}_{st}(0)$) for the treated observations (i.e., observations with a state PSL mandate in place). We estimate our parameter of interest ($\delta$) in equation (2). Standard errors are clustered at the level of the state and account for the first-stage imputation (Bertrand et al., 2004; Butts and Gardner, 2021; Gardner, 2022).

We select the Gardner (2022) procedure for our main analyses as this procedure has several attractive features. Coefficient estimates obtained using the Gardner (2022) procedure are not vulnerable to bias from a staggered treatment roll-out 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 procedure performs well in terms of inference relative to other difference-in-differences estimators (Gardner et al., 2024). Finally, the Gardner (2022) procedure relies on regression, which is a concept familiar to many micro-economists, and is computationally efficient. However, we will show in Section 7 that our results are not appreciably different if we use alternative difference-in-differences estimators employed in the economics literature.

Data are weighted by the number of people enrolled in Medicaid per state-quarter (University of Kentucky Center for Poverty Research, 2023).[20] We choose to weight the data because our objective is to recover the average treatment effect on the treated for the average person enrolled in Medicaid, and weighting in this manner allows us to estimate our target parameter. Weighting by the number of people enrolled in Medicaid in the state upweights the treatment effect for larger (in terms of Medicaid program) states such as California and downweights the treatment effect for smaller states like Alaska, which is in line with our objective. Alternatively, unweighted regression places equal weight on large and

[20]Some time-varying covariates are not available through 2024, the most recent period of SDUD data at the time of writing. For such variables, we linearly impute separately by state any missing values. Full details are available on request.

small states (in terms of the Medicaid program), and thus gives us the average treatment effect on the treated across states, which is not our target parameter.

We make some modifications to equations (1) and (2) in our analyses of the National Compensation Survey and the Annual Social and Economic Supplement to the Current Population Survey given the different data structures of these survey datasets. For example, in analyses of the survey datasets, we use Bureau of Labor Statistics-provided survey weights and we include year fixed effects rather than quarter-year fixed effects, as the survey data are recorded annually and not quarterly.

In our analyses of medical expenditures in the Annual Social and Economic Supplement to the Current Population Survey, we use a difference-in-differences procedure proposed by Wooldridge (2023) that is robust to bias associated with a staggered policy rollout and can accommodate a Poisson distribution (Hegland, 2023). Roughly 15% of the sample reports no medical expenditures each year, suggesting that a log-linear specification would not be appropriate (Chen and Roth, 2024), thus, for this outcome, we use a Poisson regression. Estimates of treatment effects are reported as differences in linear predictions.

A key assumption of difference-in-differences methods is parallel trends, that is, PSL adopting and non-adopting states would have followed the same trends in outcomes absent the policy shock. This assumption, while not testable as counterfactual outcomes are not observed, allows the researcher to use untreated states as a valid comparison for treated states. We estimate event-studies to provide suggestive evidence on the ability of our data to satisfy the parallel trends assumption. Specifically, we decompose our difference-in-differences variable into a series of indicator variables taking the value of one if a treatment state is observed -6 through +6 years to PSL mandate adoption. There is no omitted category in the Gardner (2022) procedure event-study. States that do not adopt/announce a PSL mandate by the end of 2024 are coded as zero for all event-time indicators. We exclude treated states more than six years pre- and post-event.

As discussed in Section 3.2, the District of Columbia adopted a PSL mandate in May 2008, thus this jurisdiction is treated throughout our study period (2011-2024). Therefore, we exclude the District of Columbia from our analysis.

### 3.6 Summary statistics and trends

Trends in the (unlogged) counts of dispensed medications financed by Medicaid for states that do and do not adopt a PSL mandate are reported in Figure 2. We display both raw trends (top panel) and trends centered around the PSL adoption year (bottom panel); for non-adopting states we center data on 2018 which is the median adoption year. We scale counts by 100,000 for readability. Raw trends depart in 2015, with prescription counts rising faster in PSL adopting states, which coincides with the year that California and Massachusetts implemented PSL policies. Examination of the centered trends shows a stark rise in dispensed prescriptions in the year of the mandate in adopting vs. non-adopting states. These trends foreshadow the results of our difference-in-differences analysis.

Table 2 reports summary statistics for all states from 2011 Q1 through 2024 Q4, as well as for PSL-adopting states pre-policy, and non-PSL-adopting states over the entire period. The mean number of dispensed medications in the full sample (scaled by 100,000) is 85.03, and the counts are higher in states that adopt a PSL mandate (92.14) than in states that do not (52.01). This difference is likely attributable to heterogeneity in the size of the Medicaid program; adopting states have a larger number of people enrolled in this program (3,892,018) than non-adopting states (2,398,384). For this reason, we adjust for the number of people enrolled in Medicaid in the regressions. 21.8% of state-year-quarter observations have a PSL mandate in place. There are differences in terms of other policies across the two groups of states. States adopting PSL mandates are more likely to have paid medical and family leave policies in place (30.0% vs. 0%) and to have expanded Medicaid with the ACA (62.7% vs. 38.8%). Conversely, PTO mandates are only implemented in states that do not adopt PSL mandates (7.0% vs 1.7%). While not identical, the two groups of states are broadly similar in terms of demographics that we include in our regressions, though states that will adopt the mandate have higher shares of people who are of Hispanic ethnicity (21.2% vs. 13.6%) and other races (11.7% vs. 7.4%) than other states.

# 4 Results

## 4.1 Evidence on the first-stage

We report first-stage results in Table 3, we consider an indicator variable for a job having access to PSL and hours of PSL used. Here, we replicate earlier work (see Section 2) and show that adoption of a state PSL mandate leads to substantial gains in employee access to and use of PSL. In particular, post-mandate the probability that a job has access to PSL increases by 12.2 percentage points and use of PSL rises by 2.3 hours per quarter. Comparing these changes to the baseline (i.e., the value in PSL adopting states prior to adoption) implies relative effects of 16.9% for access and 9.8% for use. Results from event-studies are reported in Figure A2 and provide no evidence that our findings are driven by differential trends in

PSL use and access across states that do and do not adopt a PSL mandate.[21]

Our findings are slightly smaller in size than those reported in Maclean et al. (2025), which makes sense since we include jobs in public establishments (e.g., state government) while Maclean et al. (2025) do not.[22] Maclean et al. (2025) also show that the gains in terms of both access to and use of PSL are larger in groups with lower baseline values. For example, access to PSL is estimated to increase by 13 percentage points in full-time jobs but by more than three times as much (38 percentage points) in part-time jobs following adoption of a state PSL mandate (see Table 2 in that paper). Similarly, gains in non-unionized jobs and small establishments are on par, or larger than, gains in unionized jobs and large establishments, and with particularly large increases in access to and use of PSL in construction; administration, support, and waste management; accommodation and food services; and food preparation and serving jobs. Guth et al. (2023) show that 47% of people enrolled in Medicaid who work are employed in the agricultural or service industry, which includes many of the jobs identified as experiencing substantial gains in both PSL coverage and use in Maclean et al. (2025). Thus, the benefits from state PSL mandates are likely passed on to the jobs that people enrolled in Medicaid might hold.

As described in Section 3.1, we estimate that an office visit with a healthcare professional in which a prescription is obtained requires 88.8 minutes. Thus, the increase in employee PSL time we estimate in Table 3 is larger than the time required for an office visit (2.2 hours per quarter, which corresponds to 8.8 hours per year), suggesting that the financially protected time made available by state PSL mandates and used by employees is sufficient to receive a prescription that would later appear as one or multiple dispensed prescriptions in our main SDUD dataset. We view these findings as confirming earlier work providing evidence of a strong first-stage. With these findings in hand, we turn to our main analyses of Medicaid-financed dispensed prescription medications.

[21] Our event-studies using the NCS data include five leads, the period of the mandate, and five lags. Moreover, we include job-specific controls in our NCS analysis (both difference-in-differences and event-study specifications) following Maclean et al. (2025): unionization and part-time work status. The NCS, maintained by the Bureau of Labor Statistics, are no longer available to external researchers. Thus, we are not able to estimate event-studies that more closely mirror our SDUD event-studies. Full details are available on request.

[22] Public employers are more likely to voluntarily offer PSL benefits, which likely leads to the somewhat smaller mandate effects that we estimate.

## 4.2 The effect of state PSL mandates on Medicaid-financed dispensed prescription medications

Our main results are reported in Table 4. Our primary outcomes are logged, thus we convert the coefficient estimates to the percent change using the following formula: $(exp^{\hat{\beta}} - 1) * 100$. Here we find that there is an 6.7% increase in Medicaid-financed dispensed prescriptions post-PSL mandate. We also report results in which we use the rate of dispensed medications per 1,000 Medicaid enrollees (logged),[23] and the (unlogged) count of the number of prescriptions using least squares. Findings are robust.

Event-study results are reported in Figure 3. We trim the data in event-time for treatment states, we exclude observations more than six years pre- and post-mandate, we do not trim the data for untreated states. Coefficient estimates on the policy leads are small and statistically indistinguishable from zero, which offers suggestive evidence that our data satisfy the parallel trends assumption. Policy lags show that there is a substantial increase in Medicaid-financed dispensed prescription medications post-mandate and that the effect is observable through our six-year window.

In Figure A3, we report event-studies using alternative specifications. In particular, we exclude the four states that adopt a PSL mandate after the end of our study period (Alaska, Michigan, Missouri, and Nebraska, see Table 1), remove time-varying covariates, and do not trim the data in event-time for states that adopt PSL mandates. Results are generally robust, though there is some evidence of differential trends (or potentially anticipation effects) when we do not include time-varying covariates in the regression.

# 5 Heterogeneity by prescription medication class

In Table 5, we examine whether estimated PSL effects differ across medication classes, and obtain some evidence of heterogeneous responses. We also report findings graphically in Figure A4. Event-studies for each drug class are reported in Figure A5, similar to overall prescriptions, we do not observe evidence of differential pre-trends for any of the classes.

Post-mandate, dispensed prescription medications for antibiotics are estimated to increase by 8.0%, behavioral health by 12.7%, respiratory and allergy by 18.5%, and pain by 20.7%. There is no observable change in dispensed prescription medications for cardiovascular health, diabetes, gastrointestinal health, or HIV or hepatitis. Potentially these conditions are viewed as less discretionary among people enrolled in Medicaid than medica-

[23] We do not control for the number of people enrolled in Medicaid in this specification as this variable serves as the denominator in the outcome variable.

tions for behavioral health, chronic pain, and so forth, thus patients receive care for these latter conditions regardless of PSL availability.

Our heterogeneity analysis offers us an opportunity to think about the marginal healthcare encounters that occur following PSL mandate adoption among people enrolled in Medicaid. More specifically, one could interpret the findings of our heterogeneity analyses to imply that mandated PSL allows patients to receive more 'discretionary' healthcare. That is, people prioritize physical conditions that they may perceive as more serious (e.g., heart problems, high insulin levels which can cause blindness and amputations, or HIV). Thus – absent a PSL mandate – people enrolled in Medicaid receive such 'non-discretionary' healthcare, but only after time and financial constraints are relaxed post-mandate do enrollees seek care for 'discretionary' conditions (e.g., pain or mental health conditions).

Interestingly, we observe that prescription medications for birth control decline post-mandate. While we lack the data to explore this finding, one potential explanation is that PSL may allow people enrolled in Medicaid to substitute from prescription pills to other, more time-intensive, contraception methods not captured in the SDUD prescriptions. For example, PSL may allow people enrolled in Medicaid to utilize long-acting reversible contraception (LARC), which is more effective than oral contraception (Stoddard et al., 2011), but is more time-intensive as a specialist clinician must insert the device in an outpatient setting and the patient must typically receive a follow-up appointment with the specialist to ensure that the device is correctly inserted. Moreover, prescriptions for LARC are generally dispensed by specialty pharmacies and not the retail pharmacies that are captured in the SDUD.

We note that our finding for behavioral health medications – 12.7% increase post-mandate – is larger than the 6% increase reported by Maclean et al. (2024). We include substance use and smoking cessation medications while Maclean et al. (2024) focus exclusively on mental health medications which may lead to differences in effect sizes. However, the 95% confidence interval surrounding our coefficient estimate includes a 4.8% increase in behavioral health medications (i.e., a value less than the coefficient estimate reported by Maclean et al. (2024)), thus we are reluctant to over-interpret differences across the two studies.

# 6 Self-reported health of people enrolled in Medicaid following adoption of a state paid sick leave mandate

In Table 6 we report our analyses of self-reported health using the Annual Social and Economic Supplement to the Current Population Survey.[24] In these analyses, we lag the

[24] We replace state-level demographics with individual-level demographics: age; sex (male and female, with female as the omitted group; race (White, Black, and other race, with White race as the omitted group);

PSL mandate by two years to allow for additional time for changes in dispensed prescription medications to translate into changes in health. We estimate the regression for all people enrolled in Medicaid, and separately for those 19-64 years who reported working and reported not working in the past year, and children (0-18 years). Results (available on request) are very similar, though somewhat less precise, if we use a one-year lag.

Here we find some suggestive evidence that self-reported health among Medicaid enrollees improves following adoption of a state PSL mandate. In particular, post-mandate, among all non-elderly enrollees, there is a 2.2 percentage point increase in the probability that a person enrolled in Medicaid reports very good or excellent health, comparing this coefficient estimate to the pre-mandate mean in PSL mandate-adopting states (58.7%) implies a 3.7% increase. Among adults 19-64 years (children 0-18 years) enrolled in Medicaid, the increase is 2.1 percentage points or 4.8% (2.4 percentage points or 3.2%). In terms of the sample of adults who are enrolled in Medicaid who work and do not work, we see increases in the probability of self-reporting very good or excellent health, though only the coefficient estimate for the non-working sample is statistically distinguishable from zero. This pattern of results hints that state PSL mandates may allow employed people enrolled in Medicaid to support their family members' healthcare. Additionally, there may be spillovers within the family, for example if a person enrolled in Medicaid who works is better able to meet family responsibilities post-PSL mandate, this balancing may positively impact the self-assessed health of dependents. We note that earlier work suggests that employment may increase post-PSL mandate (Maclean et al., 2025), thus stratifying the sample on a potentially endogenous variable (past-year work) may lead to biased coefficient estimates. Event-studies for self-reported health (Figure A6), though somewhat noisy, do not display evidence of systematic pre-treatment differences between states that will and will not adopt a PSL mandate.

A concern with our analysis is that state PSL mandates may impact the probability of enrolling in Medicaid, for example, an employee could take time off work to apply for Medicaid or certify program eligibility for continued coverage as these administrative processes can be time-consuming, which could complicate interpretation of the coefficient estimates. To test this hypothesis, we conduct three exercises. First, returning to the Annual Social and Economic Supplement to the Current Population Survey and using respondents (regardless of insurance status) ages 0-64 years, we regress past-year Medicaid coverage on state PSL mandates and other covariates (see Table 7). The coefficient estimate is not statistically distinguishable from zero. With 95% confidence, we can rule out declines (increases) in the probability of Medicaid coverage greater than -3.6% (+10.9%). We replicate this finding us-

ethnicity (Hispanic and non-Hispanic, with non-Hispanic as the omitted group); and education (less than high school, high school, some college, and college degree, with less than high school as the omitted category).

ing a two-year lag in the PSL mandate variable, here we again see no statistically significant evidence that the probability of Medicaid coverage changes following mandate adoption, indeed the coefficient estimate is somewhat smaller in size. Second, using data from the Centers for Medicare and Medicaid Services (University of Kentucky Center for Poverty Research, 2023), we regress the logarithm of the number of people enrolled in Medicaid on state PSL mandates and other controls. There are no states with zero people enrolled in Medicaid in any year.[25] We weight the data by the number of people enrolled in Medicaid in the first year of our study (2011). Results are reported in Panel A of Table 8. Similarly to our Medicaid coverage result, we find no evidence that PSL mandate adoption impacts these outcomes. With 95% confidence, we can rule out decreases (increases) in the number of people enrolled in Medicaid greater than 0.2% (0.4%). We replicate this analysis weighting by the state-year population, results are very similar. Third, again using the Annual Social and Economic Supplement to the Current Population Survey, we calculate the average age of all people under 65 years of age enrolled in Medicaid in each state-year and regress this average on state PSL mandates and other controls (Panel C of Table 8). Examining this outcome allows us to offer some suggestive evidence of a potential change in the composition of people enrolled in Medicaid that may be overlooked when focusing only on 'extensive margin' outcomes. The age of people enrolled in Medicaid does not vary with state PSL mandate adoption, with 95% confidence we can rule out decreases (increases) greater than -3.7% (+1.7%). In Figure A9 we report findings from event-studies for the three outcomes in Table 8, we do not observe any evidence of differential pre-trends between states that will and will not adopt a PSL mandate.

We interpret the results from these analyses as evidence that our findings from the SDUD reflect changes within a relatively stable sample of beneficiaries, with minimal impact from sample fluctuations associated with state PSL mandates.[26]

We note that PSL could impact self-reported health of people enrolled in Medicaid through various pathways beyond accessing prescription medications. For example, mandated PSL could be used to better balance family and work responsibilities, which could improve self-reported health. Further, PSL days could be used to recover from a short-term illness (e.g., cold) or to take a mental health day. These, and likely other, channels could link state PSL mandates to self-reported health among people enrolled in Medicaid. We leave

[25] The number of people enrolled in Medicaid is available at the annual level, thus we modify equations 1 and 2 accordingly. For example, we replace quarter-year fixed effects with year fixed effects.

[26] We do not mean to imply that there are no changes in enrollees – for example the ACA Medicaid expansions increased the number of enrollees over our study period (see, e.g., Miller and Wherry (2017)). Rather, the adoption of state PSL mandates does not appear to drive changes in Medicaid participation. Importantly, we control for ACA Medicaid expansion in our regressions.

exploration of these avenues for future work.

# 7 Robustness checking

We conduct a number of robustness checks on our main findings using an alternative data source, as well as different specifications, samples, and estimators.

To complement to our examination of Medicaid-financed prescription medications in the SDUD, we explore overall out-of-pocket healthcare expenditures captured in the Annual Social and Economic Supplement to the Current Population Survey (Table A2), including co-payments among people enrolled in Medicaid for prescription medications and other healthcare treatment spending. We include all non-elderly adults enrolled in Medicaid in the analysis sample. Post-mandate, out-of-pocket expenditures increase among people enrolled in Medicaid. The coefficient implies a 10.2% increase in expenditures. Figure A7 reports an event-study for out-of-pocket medical expenditures.[27] We note that there is some evidence of differential pre-trends for states that will and will not adopt a PSL mandate, which suggests that some of the post-treatment effect we estimate could be attributable to such trends rather than the impact of the policy on out-of-pocket medical expenditures.

Figure A8 summarizes the results from robustness checking using different samples and specifications, with findings from our main specification and sample shown in the left-most panel, the coefficient estimate is indicated with a black circle.

First, to further probe the parallel trends assumption, we de-trend the data. More specifically, for PSL-adopting states, we use pre-policy data and separately by state estimate a linear trend in Medicaid-financed prescriptions; for non-PSL-adopting states, we estimate (again separately by state) a linear trend over the full study period. We then remove the estimated trend from our outcome and use the de-trended variable as our dependent variable. Second, we change the included covariates to: 1) exclude time-varying covariates, 2) control for an extended set of time-varying covariates, and 3) add an indicator for whether there is a sub-state mandate in place in that state-period (National Partnership of Women & Families, 2023). The additional time-varying covariates include: the effective minimum wage (the higher value of the state or federal value), Temporary Assistance for Needy Families monthly benefit for a family of four, poverty rates, number of general physician offices, political party of the Governor, unemployment rate, and an indicator for whether the state borders another state with a PSL mandate (Ruggles et al., 2023; University of Kentucky Center for Poverty Research, 2023). Third, we alter how we operationalize PSL mandates by: 1) not lagging the mandate one year; 2) expanding our definition of a PSL mandate to include both PSL

[27] We omit the period prior to PSL mandate adoption.

and PTO mandates; 3) excluding treatment states that we do do not observe for three years pre- and post-mandate (i.e., states that adopt before 2015 and after 2018); and 4) excluding states where a substantial percentage of people work and live in different states (i.e., Maryland and Virginia, and Connecticut, New Jersey, and New York). Fourth, we drop 2020 – the COVID-19 pandemic year when a temporary PSL mandate was in place (FFCRA) – and, alternatively, census divisions without a state that adopts a PSL mandate. Fifth, we first estimate unweighted regression, and then use the i) state population and ii) number of people enrolled in Medicaid in 2011 as the weight. Sixth, we assign suppressed cells with a value of zero (smallest possible value) or ten (highest possible value), instead of the value of five used in our primary model. Seventh, we aggregate the data to the state-year level to smooth out noise in quarterly reporting by states.

The findings are generally robust to these alternatives, although using unweighted data leads to imprecise results. Further, we suspect – due to the suppression imposed by CMS on the SDUD data (see Section 3.1) – there is more error in states with smaller Medicaid programs. 43% of cells are suppressed by CMS, but the share is 52% in observations at or below the median number of Medicaid enrollees and 41% in observations above the median, and the correlation between the share suppressed and the number of Medicaid enrollees is -0.55 ($p$-value $< 0.0000$). Another possibility is that PSL mandates are more effective — in terms of allowing people enrolled in Medicaid to access dispensed prescription medications, which generally require a consultant with a healthcare professional — in more populous areas. Indeed, the well-established healthcare shortages across the U.S., are particularly severe in more rural areas (Arredondo et al., 2023). To dig deeper into this question, we stratify the sample based on the number of people enrolled in Medicaid in the first year of our study (2011), and estimate the effect of state PSL mandates on the logarithm of prescriptions for states that have Medicaid populations at or above the median value and below the median value. We also exclude California, the most populous state (University of Kentucky Center for Poverty Research, 2023)), in some specifications to see if it drives the findings for the sample of larger states. Results are reported in Table A3. Here, we see that our findings are driven by larger states (both with and without California in the large state sample), which supports the conjecture that PSL mandates are more effective in larger states. We note that coefficient estimate in the above-median Medicaid population excluding California is noisy.

In Figure A10, we report results from a 'leave-one-out' analysis, where we sequentially drop each treated state and re-estimate our difference-in-differences approach. Results are similar, though excluding California and New York, with their large Medicaid populations, leads to some attenuation. As documented in Table A3, the effect of state PSL mandates on prescriptions appears to be larger in more populous states. Further, California and New

York have the two lowest shares of suppressed data (34% and 31% respectively). For these reasons, the fact that these states are empirically important is not suprising. In Figure A11 we exclude each time period (i.e., quarter-year) and re-estimate our main regression. Results are not sensitive to excluding time periods.

We rely on a two-stage difference-in-difference procedure developed by Gardner (2022) in our main analyses. In Table A4, we report results from several common alternative estimators: two-way fixed effects, the Callaway and Sant'Anna (2021) procedure, the Wooldridge (2023) procedure, stacked difference-in-differences (Cengiz et al., 2019), and the Borusyak et al. (2024) procedure.[28] Table notes include specifics on the implementation of each procedure. Each of these procedures relies on somewhat different assumptions, but – other than two-way fixed effects – a common feature is that these estimators prohibit 'forbidden comparisons' in which later treated units (e.g., Minnesota which adopted a PSL mandate in 2024) are compared with earlier treated units (e.g., Connecticut which adopted a PSL mandate in 2012). Further, these estimators do not 'variance' weight the data, that is place the most weight on states that 'turn on' their PSL mandate in the mid-point of the panel (2018). Results are generally robust, though results are somewhat sensitive to the use of the Callaway and Sant'Anna (2021) procedure. This procedure does not accommodate time-varying covariances and will not converge when we include all covariates. We believe that time-varying covariates are important in our context.

With regard to the Callaway and Sant'Anna (2021) findings reported in Table A4, we note that this approach reweights the group-time average treatment effects based on the share of units treated at each period, not by the number of people enrolled in Medicaid in each state.[29] This distinction is important given that our research objective is to estimate the effect of PSL mandate for the average person enrolled in Medicaid. The lack of statistical significance in those estimates does not contradict our main findings but reflects this difference in weighting structure and target population. Moreover, the Callaway and Sant'Anna (2021) procedure has been shown to under-reject the null while the Gardner (2022) approach is less vulnerable to this issue (Mizushima and Powell, 2025).

[28] We implement two-way fixed effect using -reghdfe- (Correia, 2016), Callaway and Sant'Anna (2021) using -csdid2- (Rios-Avila et al., 2023), Wooldridge (2023) using -wooldid- (Hegland, 2023), and Borusyak et al. (2024) using -didimputation- (Borusyak, 2023) in Stata.

[29] We also apply the number of peopled enrolled in Medicaid as the weight which further complicates how to envision weighting in our context.

# 8 Conclusion

People who are enrolled in Medicaid have high rates of chronic conditions and unmet need for medical care (Chapel et al., 2017). A potential barrier in accessing treatment for people enrolled in Medicaid, who generally have lower income levels, is the ability to take time-off from work to receive care. For employed people enrolled in Medicaid, and their dependents, one potential policy approach to address this unmet need is to provide financially protected time away from work. Though the U.S. does not have a federal policy providing such benefits to working Americans, 18 states and the District of Columbia have adopted mandates compelling employers to provide paid sick leave. We study the impact of these mandates on an important measure of access to care — dispensed prescription medications obtained in retail pharmacies (Ellis et al., 2017; Ghosh et al., 2019) – within a policy relevant population – people enrolled in Medicaid. Prescription medications proxy access to healthcare and account for roughly 25% of U.S. healthcare expenditures (Mortensen et al., 2021). These medications are generally prescribed in outpatient settings (e.g., primary care) and thus proxy access to lower-level and less costly care settings. We find that Medicaid-financed prescription medications dispensed in retail pharmacies rise by 6.7% after a state adopts a PSL mandate, with somewhat heterogeneous responses across the specific classes of medications that we examine.

Our findings suggest that there are potential spillovers from state PSL employer mandates to Medicaid. That is, a policy designed to improve employee's ability to tend to their health and family needs, increases the use of healthcare services that are at least partially financed by states and the federal government. The extent to which the spillover we document reflects a cost or benefit is not immediately clear. On the one hand, Medicaid finances a larger number of prescriptions post-PSL mandate, which could be viewed as a cost. Alternatively, the ability to access non-emergency care could lead to future health benefits and potentially cost savings to Medicaid. Indeed, our findings, combined with a recent study by Ma et al. (2022), which shows a decline in emergency department visits following adoption of a state PSL mandate, suggest that PSL not only improves access to healthcare – as indicated by the increase in prescriptions dispensed in retail settings – but also enables people enrolled in Medicaid to better manage conditions and engage in preventive care, reducing the future need for more costly and intensive care. Similarly, our work aligns with an earlier study documenting that increases in prescription use among people enrolled in Medicaid is associated with reduced expenditures on non-prescription medication healthcare (Roebuck et al., 2015). Care provided in emergency departments is significantly more expensive than the primary care settings where these medications are typically prescribed. Moreover, supporting people enrolled in Medicaid who work could increase paid employment, possibly yielding important economic benefits for both enrollees and society.

Earlier research suggests that there may be spillovers from state PSL mandates to Workers Compensation programs. Dong et al. (2024) provide evidence that Workers Compensation and PSL may be substitute sources of insurance, at least for some injured workers. Post-PSL mandate, Dong et al. (2024) find a 15% reduction in the probability of past-year income from Workers Compensation. Collectively, our study and the work of Dong et al. (2024) suggest that state PSL mandates likely interact with other components of the U.S. safety net, including Medicaid. This information is crucial for understanding the costs and benefits of social insurance programs, and for optimal program design.

In our analysis of heterogeneity by medication class, we consider physical and mental health dispensed prescription medications. Mental healthcare is often viewed as more discretionary than other forms of care, which might imply different elasticities with respect to time. We find a 6.7% increase in overall prescriptions post-mandate and a 12.7% increase in medications for mental health and other behavioral health conditions. The difference across the two estimates hints at higher time elasticities of mental than general healthcare. However, we also find large estimated effects (8.0 to 20.7%) for medications for allergies, chronic pain, and respiratory problems.

We can compare our estimated treatment effects with other policies to assess whether our findings are reasonable, in terms of size. For example, Ghosh et al. (2019) find that Medicaid-paid dispensed prescription medications increased by 19% following ACA Medicaid expansion. Thus, effects of the ACA Medicaid expansion – which expanded insurance coverage to lower-income Americans – are nearly three times as large as those we estimate, which offers suggestive evidence that our findings are reasonable in terms of magnitude.

The study has limitations. First, we cannot distinguish between prescriptions received by working people enrolled in Medicaid or their dependents. However, we note that a majority of Medicaid insurance recipients are working or children of working adults (Guth et al., 2023). Second, we lack data on patients and providers in the SDUD, and so are unable to study the extent to which the dispensed medications result in improved health or other clinically relevant outcomes. Third, some medications can be used for multiple purposes and thus we may not accurately classify drugs when we separately examine PSL effects across drug classes. Fourth, the SDUD provide aggregate counts of prescriptions and we are not able to account for differences in package size or dosage. Finally, though the Gardner (2022) approach performs well in terms of inference relative to other estimators (Mizushima and Powell, 2025), we note that some of our coefficient estimates are only marginally statistically significant, which is a study limitation.

Findings from this research suggest that recent state policies mandating paid time off for health needs are effective in enabling some of the most vulnerable populations to access dispensed prescription medications for chronic and acute health conditions. Extrapolating from our findings, recent proposals for a federal PSL policy -– such as the *Healthy Families Act* of 2023 -– could offer benefits, potentially including long-run cost-savings for Medicaid.

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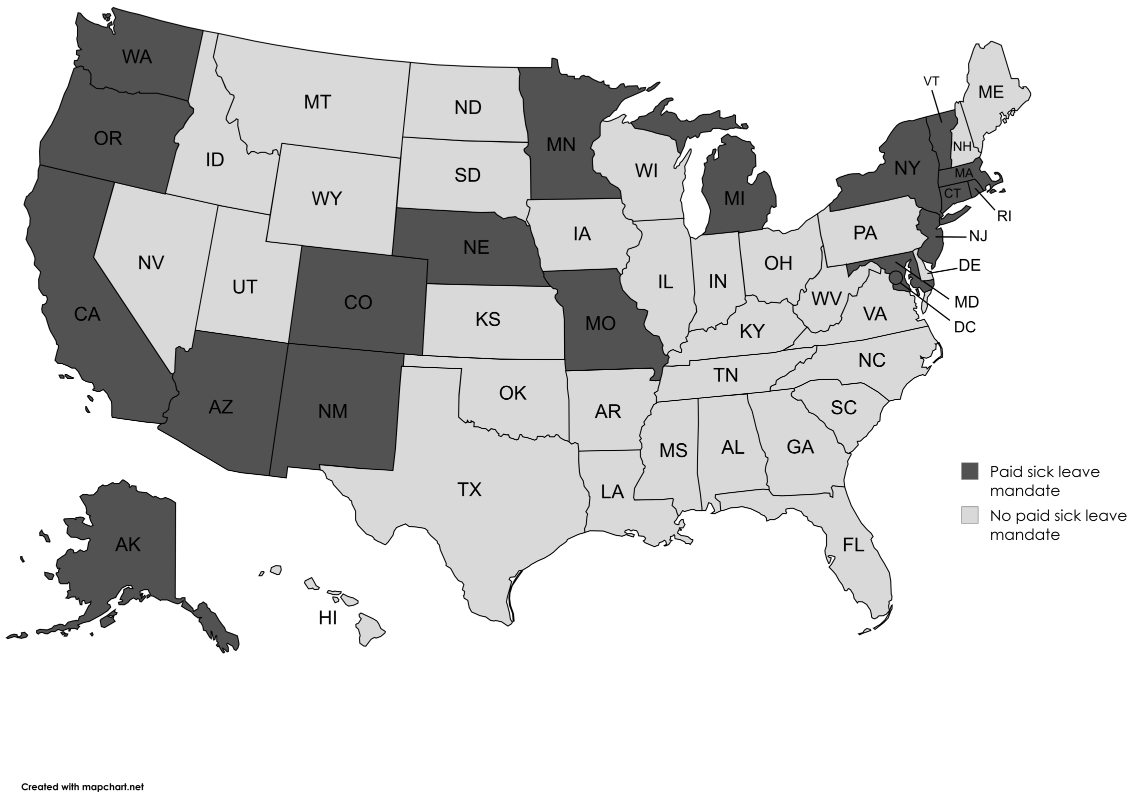
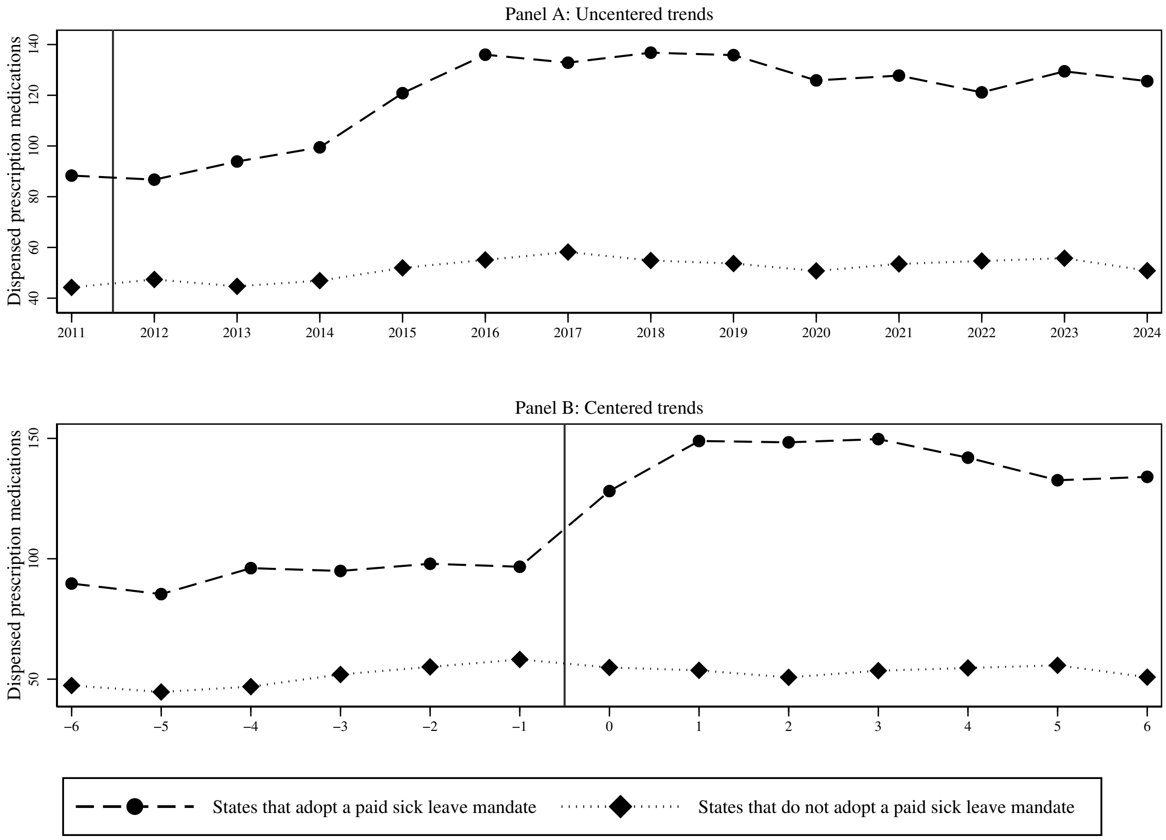
Figure 1: Geographic distribution of of all adopted or announced state paid sick leave mandates in the United States as of August, 2025



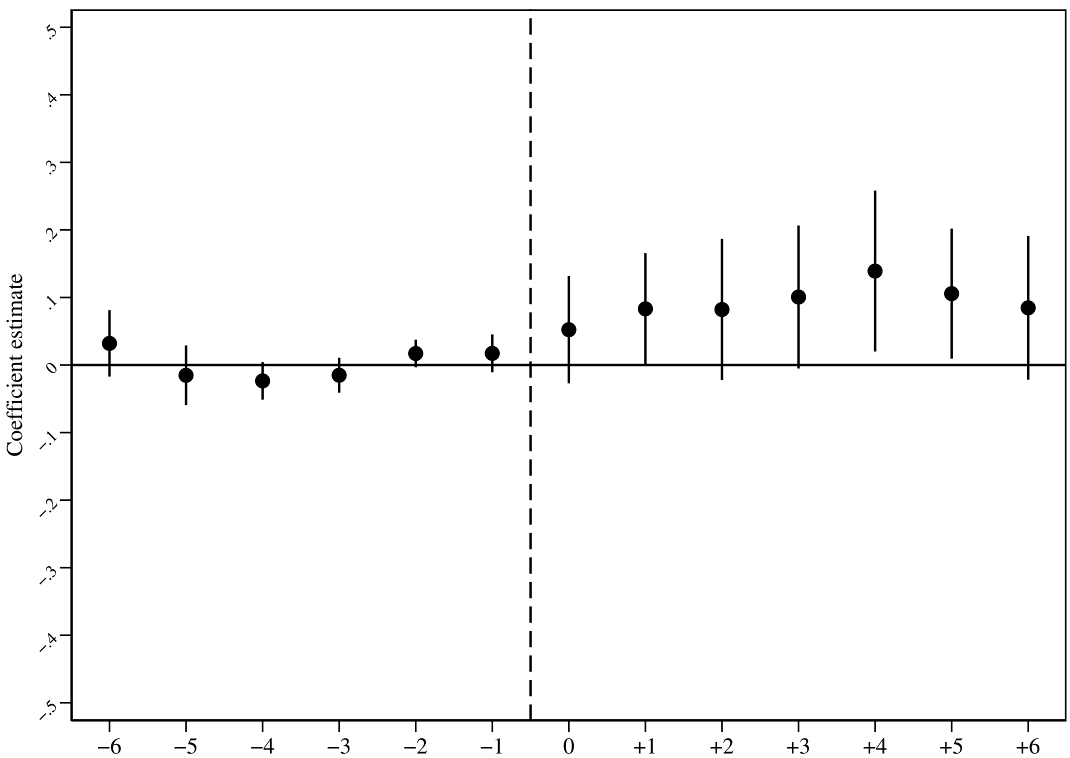

Notes: The data sources are National Partnership of Women & Families (2023) and A Better Balance (2025). All state paid sick leave mandates are effective or announced by December, 2024. Paid sick leave mandate adopting states (effective month/year) are as follows: Alaska (7/2025), Arizona (7/2017), California (7/2015), Colorado (1/2021), Connecticut (1/2012), District of Columbia (5/2008). Massachusetts (7/2015), Maryland (2/2018), Michigan (2/2025), Minnesota (1/2024), Missouri (5/2025), New Mexico (7/2022), Nebraska (10/2025), New York (1/2021), New Jersey (10/2018), Oregon (1/2016), Rhode Island (7/2018), Vermont (1/2017), & Washington (1/2018).

Figure 2: Trends in Medicaid-financed dispensed prescription medications: State Drug Utilization Database 2011 Q1 - 2024 Q4




Notes: Author's calculation of raw trends (top panel, Panel (A)) and trends centered around the state paid sick leave adoption year (bottom panel, Panel (B)) in the counts of dispensed medications per 100,000, financed by Medicaid for states that do and do not adopt a paid sick leave mandate. Data are weighted by the number of people enrolled in Medicaid per state-year prior to aggregation. The unit of observation in Panel (A) is the year. The unit of observation in Panel (B) is the year-to-event. In Panel (B), for non-adopting states data are centered at the the median adoption year of 2018. The vertical line in Panel (A) breaks the study period into the period before and after the first state paid sick leave adopted in our study period (Connecticut in 2012). The vertical line in Panel (B) breaks the centered study period into the period before and after adoption of a state paid sick leave mandate. The District of Columbia is excluded from the sample as this jurisdiction is treated in all years.

Figure 3: The effect of a state paid sick leave mandate on the logarithm of Medicaid-financed dispensed prescriptions using an event-study: State Drug Utilization Database 2011 Q1-2024 Q4




Notes: The outcome variable is the logarithm of Medicaid-financed dispensed prescription medications. The unit of observation is a state in a year-quarter. The District of Columbia is excluded from the sample as this jurisdiction is treated in all years. We utilize the Gardner et al. (2024) 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 (quarter-year) fixed effects. There is no omitted category in the Gardner (2022) procedure. For treated states, observations more than six years pre-mandate or six years post-mandate are excluded. The data for untreated states are not trimmed. Data are weighted by the number of people enrolled in Medicaid per state-year. 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

| 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: The data sources are National Partnership of Women & Families (2023) and A Better Balance (2025). Dates are in the format of month/year. State paid sick leave mandates adopted or announced as of August 2025. Estimates of employees gaining paid sick leave coverage for the first time based on National Partnership of Women & Families (2023) '*Law/Bill Number and Impact*.' The District of Columbia is excluded from the analysis sample as this jurisdiction is treated in all years but is reported here for completeness.

*State also adopts a paid medical and family leave policy by 2024 Q4.

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

Table 2: Summary statistics: State Drug Utilization Database 2011 Q1-2024 Q4

| Sample: | All states | States that adopt a mandate, pre-mandate | States that do not adopt a mandate |
|---|---|---|---|
| Dispensed prescription medications (/100,000) | 85.03 | 92.14 | 52.01 |
| Paid sick leave mandate (lagged one year) | 0.218 | 0 | 0 |
| Paid time off mandate | 0.025 | 0.070 | 0.017 |
| Paid family & medical leave policy | 0.257 | 0.300 | 0 |
| ACA Medicaid expansion* | 0.594 | 0.627 | 0.388 |
| People enrolled in Medicaid | 4155725 | 3892018 | 2398384 |
| 0-15 years | 0.200 | 0.202 | 0.203 |
| 16-44 years | 0.383 | 0.387 | 0.377 |
| 45+ years | 0.417 | 0.411 | 0.419 |
| Female | 0.490 | 0.490 | 0.488 |
| Men | 0.510 | 0.510 | 0.512 |
| White race | 0.762 | 0.773 | 0.768 |
| Black race | 0.130 | 0.110 | 0.158 |
| Other race | 0.108 | 0.117 | 0.074 |
| Hispanic | 0.188 | 0.190 | 0.141 |
| Less than high school | 0.150 | 0.153 | 0.149 |
| High school | 0.278 | 0.263 | 0.302 |
| Some college | 0.262 | 0.270 | 0.264 |
| College degree | 0.310 | 0.314 | 0.284 |
| Observations** | 2800 | 629 | 1792 |

Notes: The unit of observations is a state in a year-quarter. The District of Columbia is excluded from the sample as this jurisdiction is treated in all years. Data are weighted by the number of people enrolled in Medicaid per state-year.
* ACA = Affordable Care Act.
** Columns (2) and (3) do not sum to column (1) – 2421 vs. 2800 – 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 2023

| Outcome variable →: | Access | Utilization |
|---|---|---|
| Paid sick leave mandate (lagged one year) | 0.122*** | 2.268*** |
| | (0.028) | (0.656) |
| Percent change† | 16.9% | 9.8% |
| Pre-treatment mean, treatment states | 0.7214 | 23.1269 |
| Observations | 727962 | 727962 |

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. 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. Moreover, we include job-specific controls in our NCS analysis following Maclean et al. (2025): unionization and part-time work status. 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 = (coefficient estimate) ÷ (pre-treatment mean, treatment state) × 100%.

Table 4: The effect of a state paid sick leave mandate (lagged one year) on the logarithm of Medicaid-financed dispensed prescription medications: State Drug Utilization Database 2011 Q1-2024 Q4

| Outcome variable ↓: | Coefficient estimate (Standard error) |
|---|---|
| Logarithm of dispensed prescription medications | 0.065** |
| | (0.032) |
| Logarithm of the rate of dispensed prescription medications | 0.079*** |
| | (0.029) |
| Count of dispensed prescription medications (not logged) | 12.073*** |
| | (4.330) |
| Pre-treatment mean (not logged, scaled), treatment states | 92.144 |
| Observations | 2800 |

Notes: The outcome variables are Medicaid-financed dispensed prescription medications. Pre-treatment means for dispensed prescription medications are scaled by 100,000. The unit of observations is a state in a year-quarter. The District of Columbia is excluded from the sample as this jurisdiction is treated in all years. We utilize the Gardner et al. (2024) 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 (quarter-year) fixed effects. Data are weighted by the number of people enrolled in Medicaid per state-year. 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: Heterogeneity by drug class in the effect of a state paid sick leave mandate (lagged one year) on the logarithm of Medicaid-financed dispensed prescriptions: State Drug Utilization Database 2011 Q1-2024 Q4

| Prescription medication class ↓: | Coefficient estimate (Standard errors) |
|---|---|
| All medications | 0.065** |
| | (0.032) |
| Pre-treatment mean (not logged, scaled), treatment states | 92.144 |
| Antibiotics | 0.077** |
| | (0.031) |
| Pre-treatment mean (not logged, scaled), treatment states | 2.671 |
| Birth control | -0.326* |
| | (0.183) |
| Pre-treatment mean (not logged, scaled), treatment states | 0.462 |
| Cardiovascular | 0.003 |
| | (0.053) |
| Pre-treatment mean (not logged, scaled), treatment states | 8.288 |
| Diabetes | -0.026 |
| | (0.064) |
| Pre-treatment mean (not logged, scaled), treatment states | 1.721 |
| Gastrointestinal | -0.016 |
| | (0.052) |
| Pre-treatment mean (not logged, scaled), treatment states | 7.898 |
| HIV & hepatitis | 0.131 |
| | (0.106) |
| Pre-treatment mean (not logged, scaled), treatment states | 0.328 |
| Behavioral health† | 0.120*** |
| | (0.037) |
| Pre-treatment mean (not logged, scaled), treatment states | 7.650 |
| Respiratory & allergies | 0.170*** |
| | (0.048) |
| Pre-treatment mean (not logged, scaled), treatment states | 7.219 |
| Pain | 0.188*** |
| | (0.061) |
| Pre-treatment mean (not logged, scaled), treatment states | 4.427 |
| Other | 0.052 |
| | (0.035) |
| Pre-treatment mean (not logged, scaled), treatment states | 51.479 |
| Observations | 2800 |

Notes: The outcome variables are the logarithm of Medicaid-financed dispensed prescription medications, for all medications and by drug class. The unit of observations is a state in a year-quarter. The District of Columbia is excluded from the sample as this jurisdiction is treated in all years. 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 (quarter-year) fixed effects. Data are weighted by the number of people enrolled in Medicaid per state-year. Standard errors that account for within-state clustering are reported in parentheses.
***,**, and * = statistically different from zero at the 1%, 5%, and 10% level.
†Behavioral health = mental health, substance use, and smoking cessation prescription medications.

Table 6: Effect of a state paid sick leave mandate (lagged two years) on self-reported very good or excellent health among people 0-64 years enrolled in Medicaid: Annual Social & Economic Supplement to the Current Population Survey 2011-2024

| Sample of people enrolled in Medicaid ↓: | Coefficient estimate (Standard errors) |
|---|---|
| Sample: People 0-64 years | 0.022* |
| | (0.011) |
| Pre-treatment proportion, treatment states | 0.587 |
| Observations | 240838 |
| Sample: Adults 19-64 years | 0.021* |
| | (0.012) |
| Pre-treatment proportion, treatment states | 0.434 |
| Observations | 195108 |
| Sample: Adults 19-64 years | 0.021 |
| (work in the past year) | (0.016) |
| Pre-treatment proportion, treatment states | 0.558 |
| Observations | 100994 |
| Sample: Adults 19-21 years | 0.030*** |
| (do not work in the past year) | (0.010) |
| Pre-treatment proportion, treatment states | 0.311 |
| Observations | 94114 |
| Sample: Children 0-18 years | 0.024** |
| | (0.010) |
| Pre-treatment proportion, treatment states | 0.745 |
| Observations | 45730 |

Notes: The outcome variable is an indicator for reporting very good or excellent health. The unit of observations is a respondent 0-64 years of age enrolled in Medicaid in a state in a year. The District of Columbia is excluded from the sample as this jurisdiction is treated in all years. We utilize the Gardner et al. (2024) two-stage difference-in-differences procedure. All regressions estimated with ordinary least squares and control for enrollee demographics, time-varying state policies, state fixed effects, and year fixed effects. Data are weighted by Annual Social & Economic Supplement to the Current Population Survey 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.

Table 7: Effect of a state paid sick leave mandate on the probability of being enrolled in Medicaid: Annual Social & Economic Supplement to the Current Population Survey 2011-2023

| Paid sick leave mandate variable ↓: | Coefficient estimate (Standard errors) |
|---|---|
| Panel A: Lag paid sick leave mandate one year | 0.008 |
| | (0.008) |
| Panel B: Lag paid sick leave mandate two years | 0.005 |
| | (0.008) |
| Pre-treatment proportion, treatment states | 0.218 |
| Observations | 1450894 |

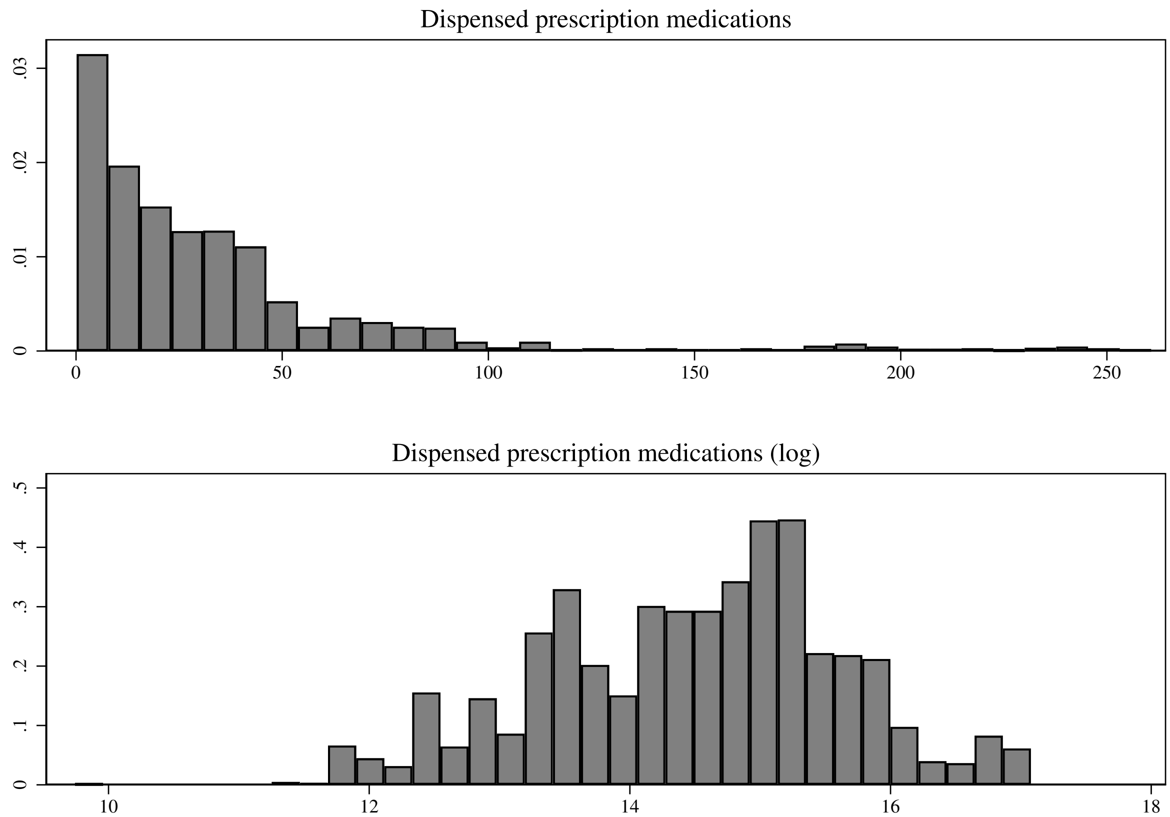
Notes: The outcome variable is an indicator for Medicaid coverage in the past year. In Panel A the state paid sick leave mandate is lagged one year. In the Panel B the state paid sick leave mandate is lagged two years. The unit of observations is a respondent 0-64 years of age in a state in a year. The District of Columbia is excluded from the sample as this jurisdiction is treated in all years. We utilize the Gardner et al. (2024) two-stage difference-in-differences procedure. All regressions estimated with ordinary least squares and control for respondent demographics, time-varying state policies, state fixed effects, and year fixed effects. Data are weighted by Annual Social & Economic Supplement to the Current Population Survey 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.

Table 8: Effect of a state paid sick leave mandate on the logarithm of the number of people enrolled in Medicaid & average age of non-elderly people enrolled in Medicaid: Centers for Medicare & Medicaid Services, & Annual Social & Economic Supplement to the Current Population Survey 2011-2024

| Outcome variable ↓: | Coefficient estimate (Standard errors) |
|---|---|
| Panel A: Number of people enrolled in Medicaid | 0.053 |
| (weight by the number of people enrolled in Medicaid in 2011) | (0.058) |
| Pre-treatment mean (not logged, scaled), treatment states | 39.973 |
| Panel B: Number of people enrolled in Medicaid | 0.031 |
| (weight by the population) | (0.048) |
| Pre-treatment mean (not logged, scaled), treatment states | 34.255 |
| Panel C: Average age of non-elderly Medicaid enrollees | -0.242 |
| | (0.324) |
| Pre-treatment proportion, treatment states | 23.727 |
| Observations | 700 |

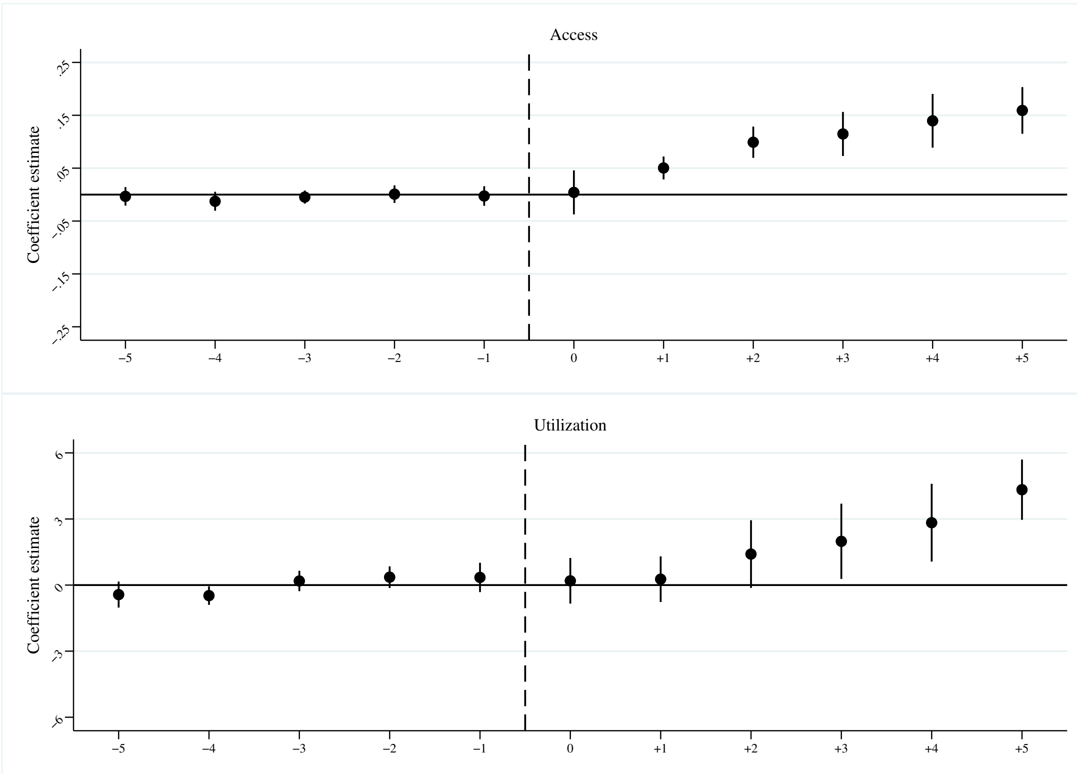
Notes: The outcome variable in Panels A and B and is the logarithm of the number of people enrolled in Medicaid, and the outcome in Panel C is the average age of people 0-64 years of age enrolled in Medicaid. The unit of observations is a state in a year in Panels A and B, and a respondent 0-64 years in Panel C. The District of Columbia is excluded from the sample as this jurisdiction is treated in all years. We utilize the Gardner et al. (2024) two-stage difference-in-differences procedure. All regressions estimated with ordinary least squares and control for respondent demographics, time-varying state policies, state fixed effects, and year fixed effects. Data are weighted by the the number of people enrolled in Medicaid in the state in 2011 Panel, the state-year population in Panel B, and Annual Social & Economic Supplement to the Current Population Survey sample weights in Panel C. Standard errors that account for within-state clustering are reported in parentheses.
***,**, and * = statistically different from zero at the 1%, 5%, and 10% level.

Figure A1: Histogram of the number of Medicaid-financed dispensed medications unlogged and logged: State Drug Utilization Database 2011 Q1 to 2024 Q4



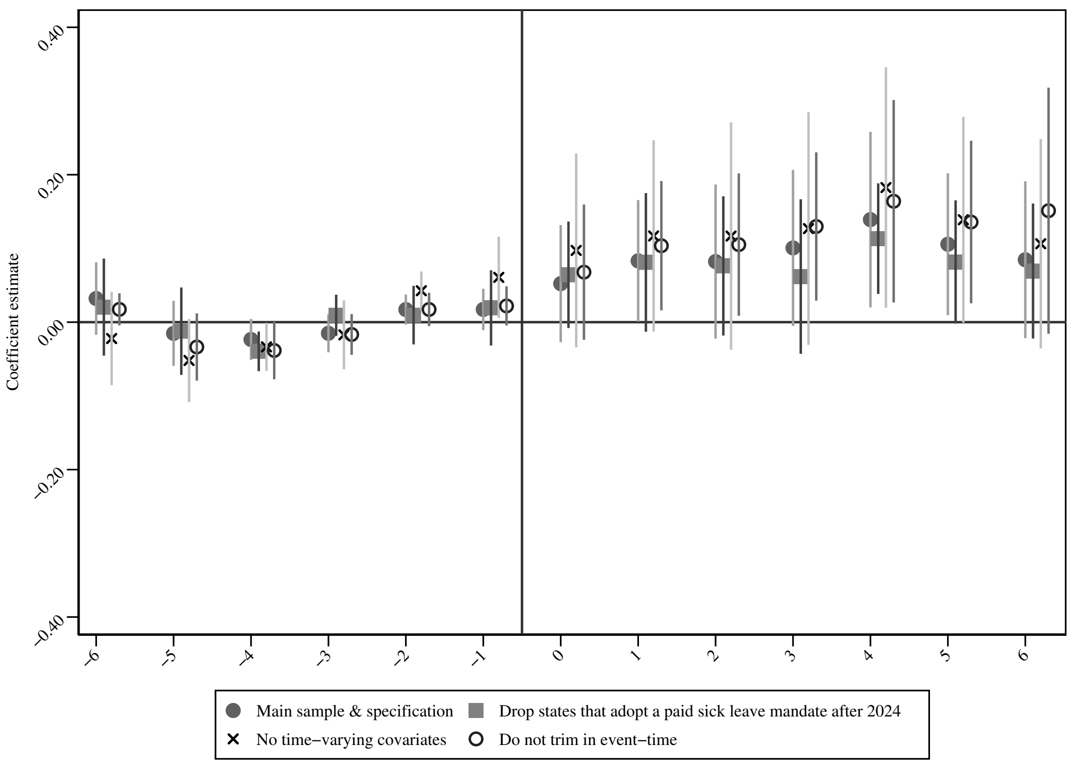

Notes: The unit of observation is the state-year-quarter. Data are unweighted.

Figure A2: The effect of a state paid sick leave mandate on access to and use of paid sick leave using an event-study: National Compensation Survey 2009 to 2023



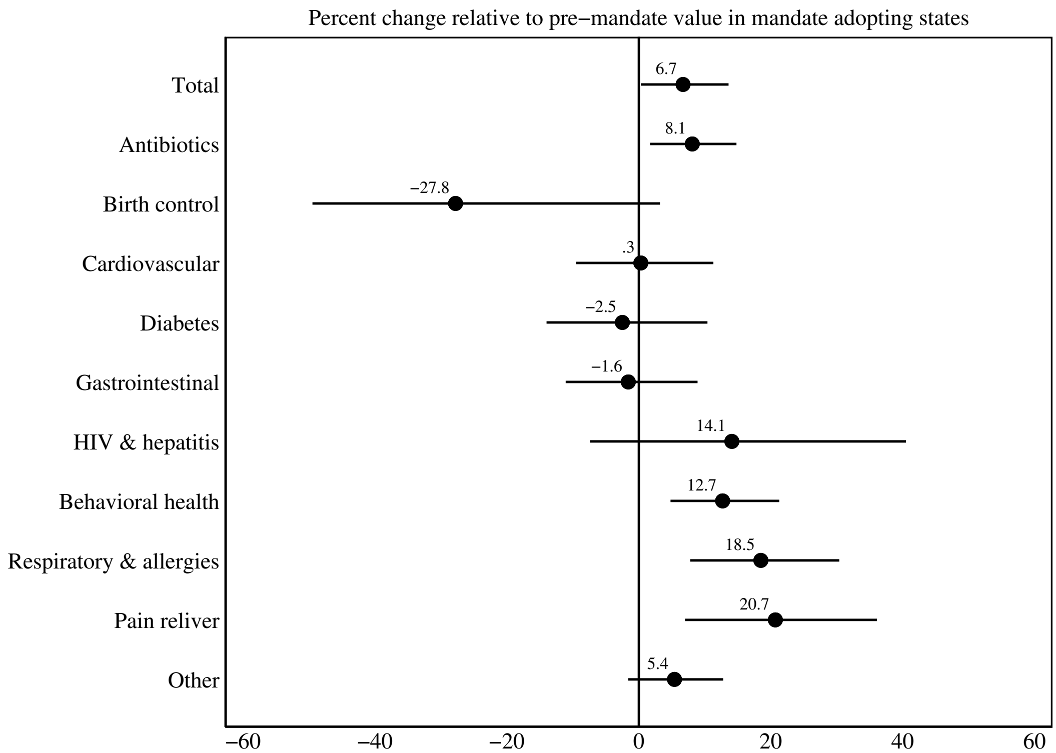

Notes: The outcome variables are i) an indicator for a job having paid sick leave benefits ('Access') and ii) the quarterly hours of paid sick leave used ('Utilization'). The unit of observation is an establishment in a job in a state in a year. The District of Columbia is excluded from the sample as this jurisdiction is treated in all years. 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. Moreover, we include job-specific controls in our NCS analysis (both difference-in-differences and event-study specifications) following Maclean et al. (2025): unionization and part-time work status. There is no omitted category in the Gardner (2022) procedure. For treated states, observations more than five years pre-mandate or five years post-mandate are excluded. The data for untreated states are not trimmed. 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 A3: The effect of a state paid sick leave mandate on the logarithm of Medicaid-financed dispensed prescriptions using alternative event-study specifications and samples: State Drug Utilization Database 2011 Q1-2024 Q4



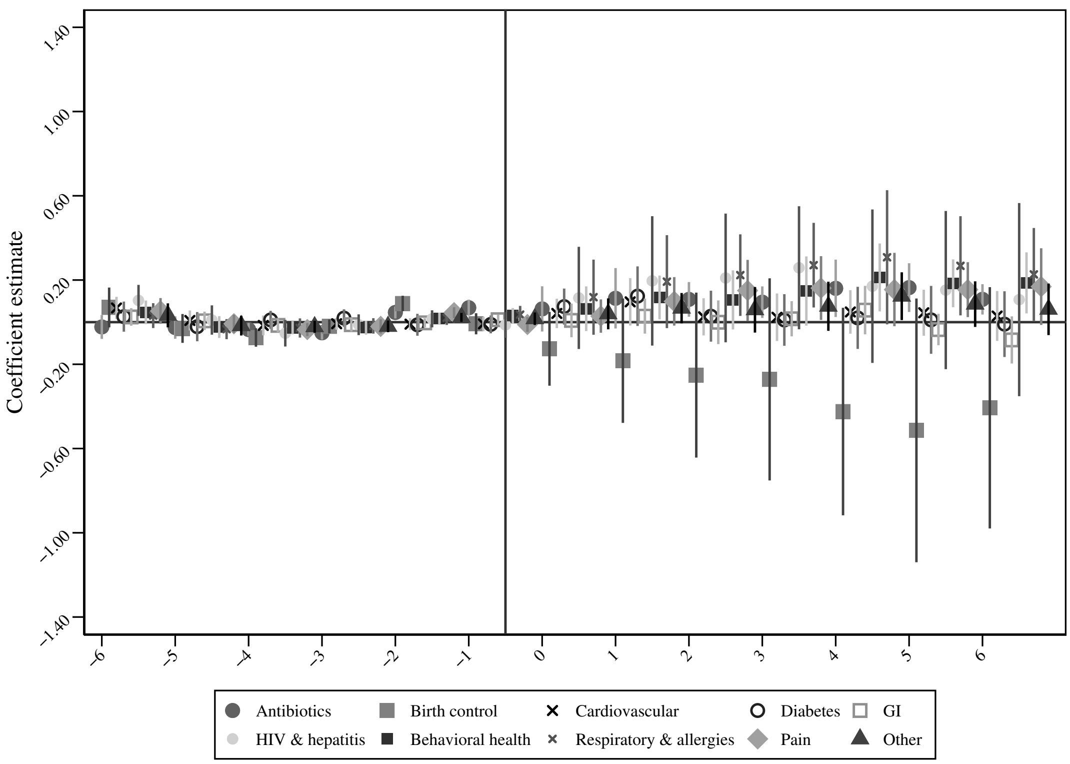

Notes: The specification or sample is listed in the legend. The outcome variable is the logarithm of Medicaid-financed dispensed prescription medications. The unit of observations is a state in a year-quarter. The District of Columbia is excluded from the sample as this jurisdiction is treated in all years. We utilize the Gardner et al. (2024) 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 (quarter-year) fixed effects unless otherwise noted. There is no omitted category in the Gardner (2022) procedure. For treated states, observations more than six years pre-mandate or six years post-mandate are excluded unless otherwise noted. Data are weighted by the number of people enrolled in Medicaid 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 A4: Heterogeneity in the effect of a state paid sick leave mandate on the logarithm of Medicaid-financed dispensed prescriptions: State Drug Utilization Database 2011 Q1-2024 Q4



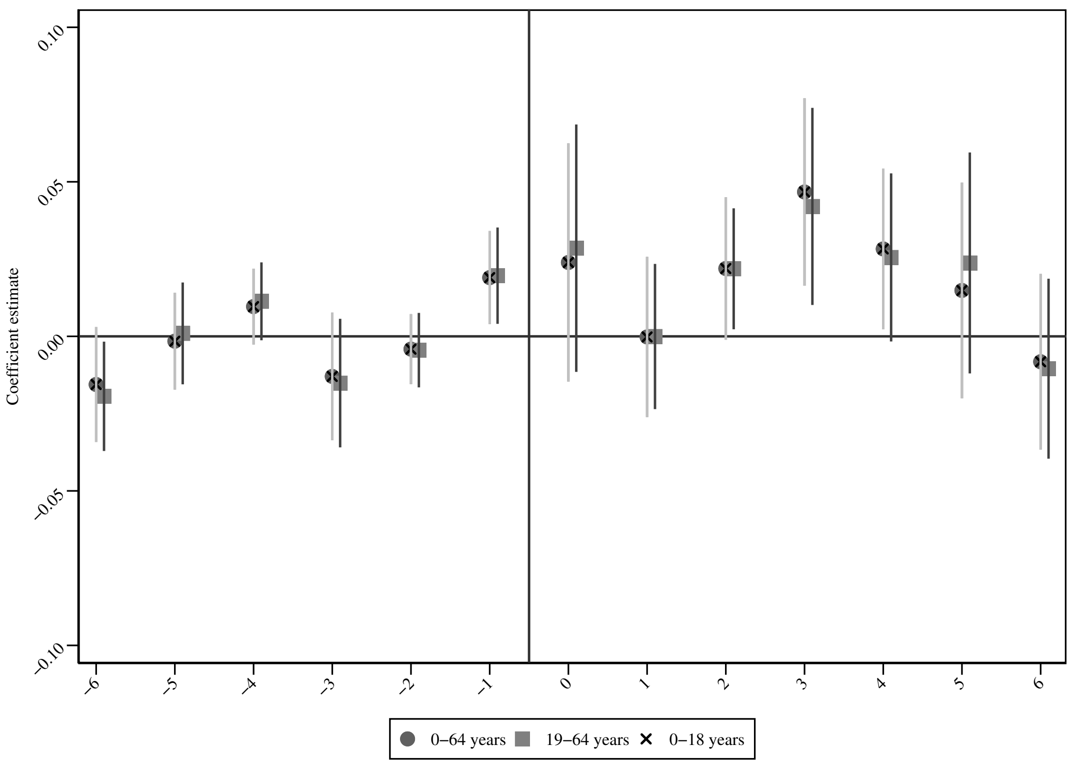

Notes: The drug class is listed on the y-axis. The outcome variable is the logarithm of Medicaid-financed dispensed prescription medications. The unit of observations is a state in a year-quarter. The District of Columbia is excluded from the sample as this jurisdiction is treated in all years. We utilize the Gardner et al. (2024) 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 (quarter-year) fixed effects unless otherwise noted. Data are weighted by the number of people enrolled in Medicaid per state-year. Coefficient estimates are reported in circles and 95% confidence intervals that account for within-state clustering are reported with horizontal lines.

Figure A5: Heterogeneity in the effect of a state paid sick leave mandate on the logarithm of Medicaid-financed dispensed prescriptions using an event-study: State Drug Utilization Database 2011 Q1-2024 Q4



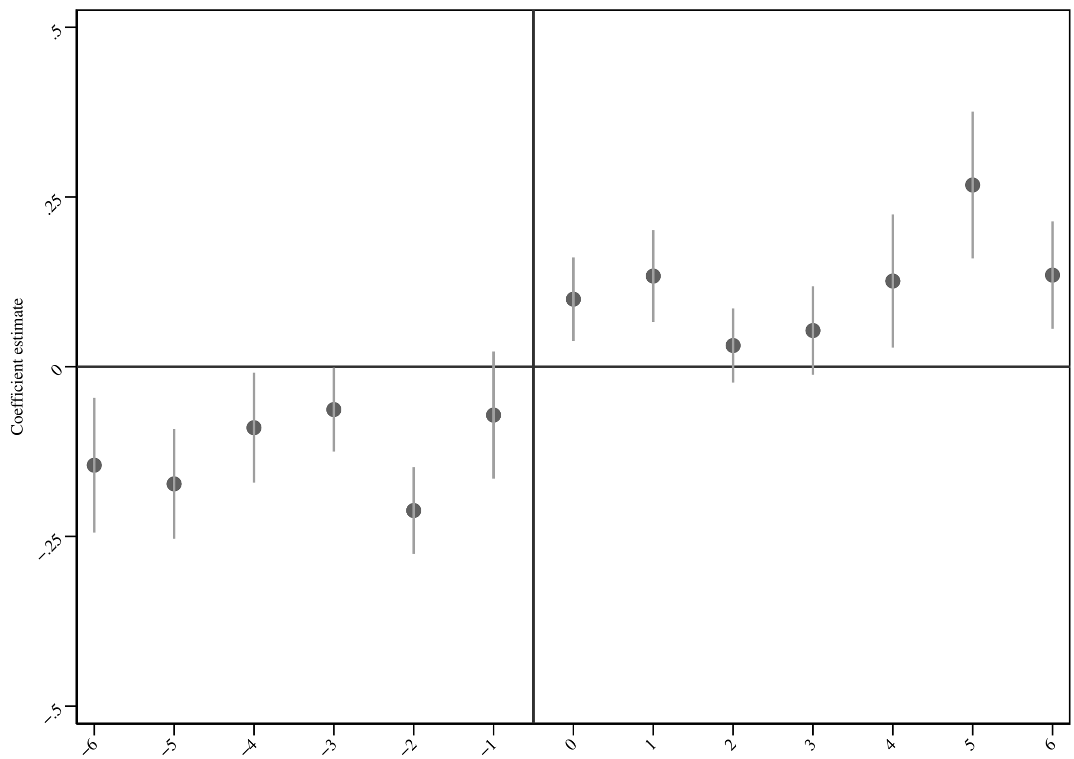

Notes: The drug class is listed in the legend. The outcome variable is the logarithm of Medicaid-financed dispensed prescription medications. The unit of observations is a state in a year-quarter. The District of Columbia is excluded from the sample as this jurisdiction is treated in all years. We utilize the Gardner et al. (2024) 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 (quarter-year) fixed effects unless otherwise noted. There is no omitted category in the Gardner (2022) procedure. For treated states, observations more than six years pre-mandate or six years post-mandate are excluded unless otherwise noted. Data are weighted by the number of people enrolled in Medicaid 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 A6: Effect of a state paid sick leave mandate on self-reported very good or excellent health among people 0-64 years of age enrolled in Medicaid using an event-study: Annual Social & Economic Supplement to the Current Population Survey 2011-2024



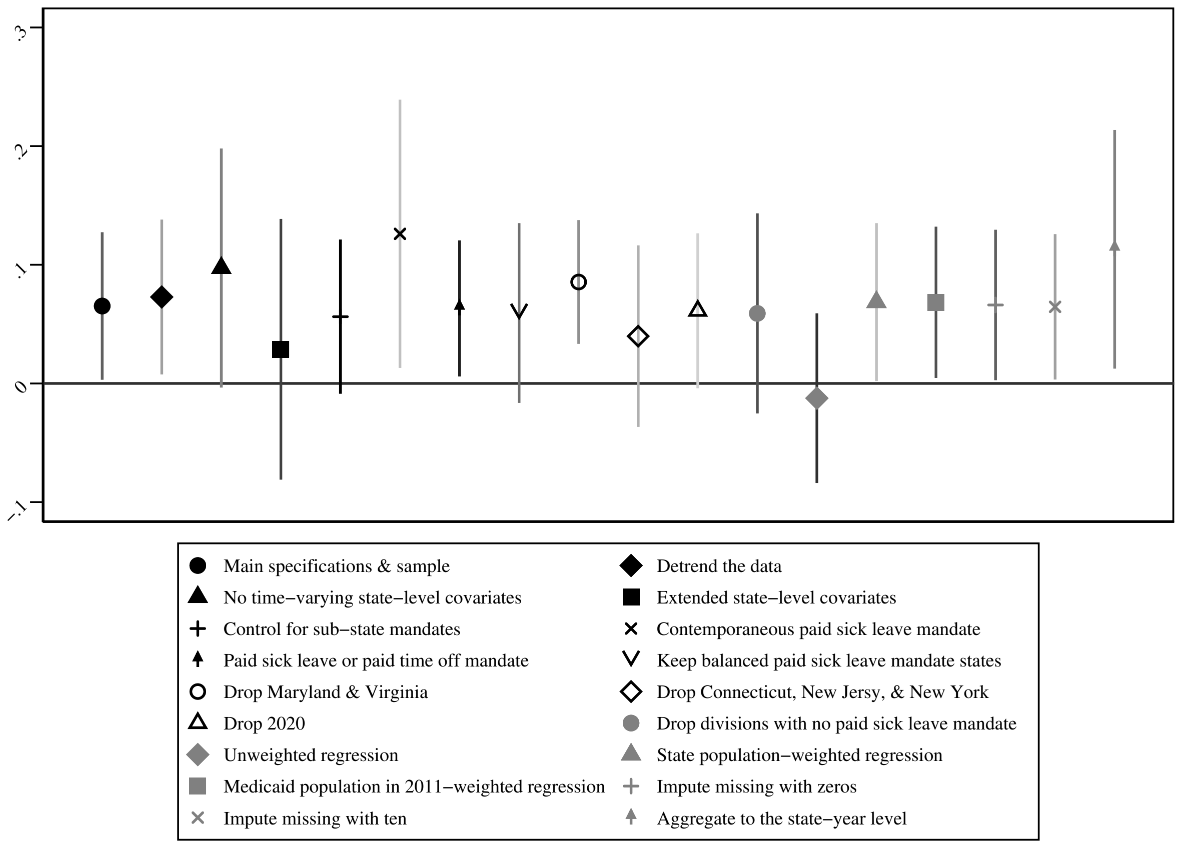

Notes: The sample is listed in the legend. The outcome variable is an indicator for reporting very good or excellent health. The unit of observations is a respondent 0-64 years enrolled in Medicaid in a state in a year. The District of Columbia is excluded from the sample as this jurisdiction is treated in all years. We utilize the Gardner et al. (2024) 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 (quarter-year) fixed effects unless otherwise noted. There is no omitted category in the Gardner (2022) procedure. For treated states, observations more than six years pre-mandate or six years post-mandate are excluded unless otherwise noted. Data are weighted by Annual Social & Economic Supplement to the Current Population 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 A7: Effect of a state paid sick leave mandate on annual medical expenditures among people 0-64 years of age enrolled in Medicaid using an event-study: Annual Social & Economic Supplement to the Current Population Survey 2011-2024



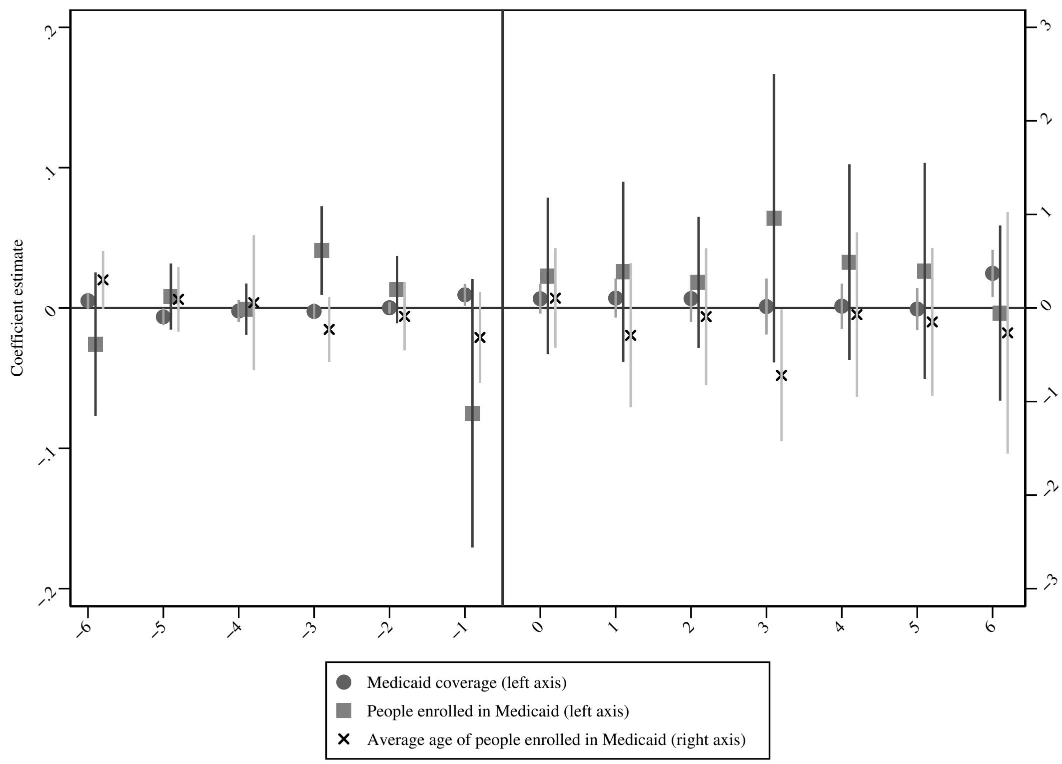

Notes: The outcome variable is out-of-pocket medical expenditures. The unit of observations is a respondent 0-64 years enrolled in Medicaid in a state in a year. The District of Columbia is excluded from the sample as this jurisdiction is treated in all years. We utilize the Wooldridge (2023) difference-in-differences procedure. All regressions estimated with a Poisson model and control for time-varying state characteristics, state fixed effects, and period (quarter-year) fixed effects unless otherwise noted. Estimates of treatment effects are reported as differences in predictions. The omitted category is -1. For treated states, observations more than six years pre-mandate or six years post-mandate are excluded unless otherwise noted. Data are weighted by Annual Social & Economic Supplement to the Current Population Survey sample weights. Coefficient estimates are reported in circles 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 the logarithm of Medicaid-financed dispensed prescriptions using alternative specifications and samples: State Drug Utilization Database 2011 Q1-2024 Q4



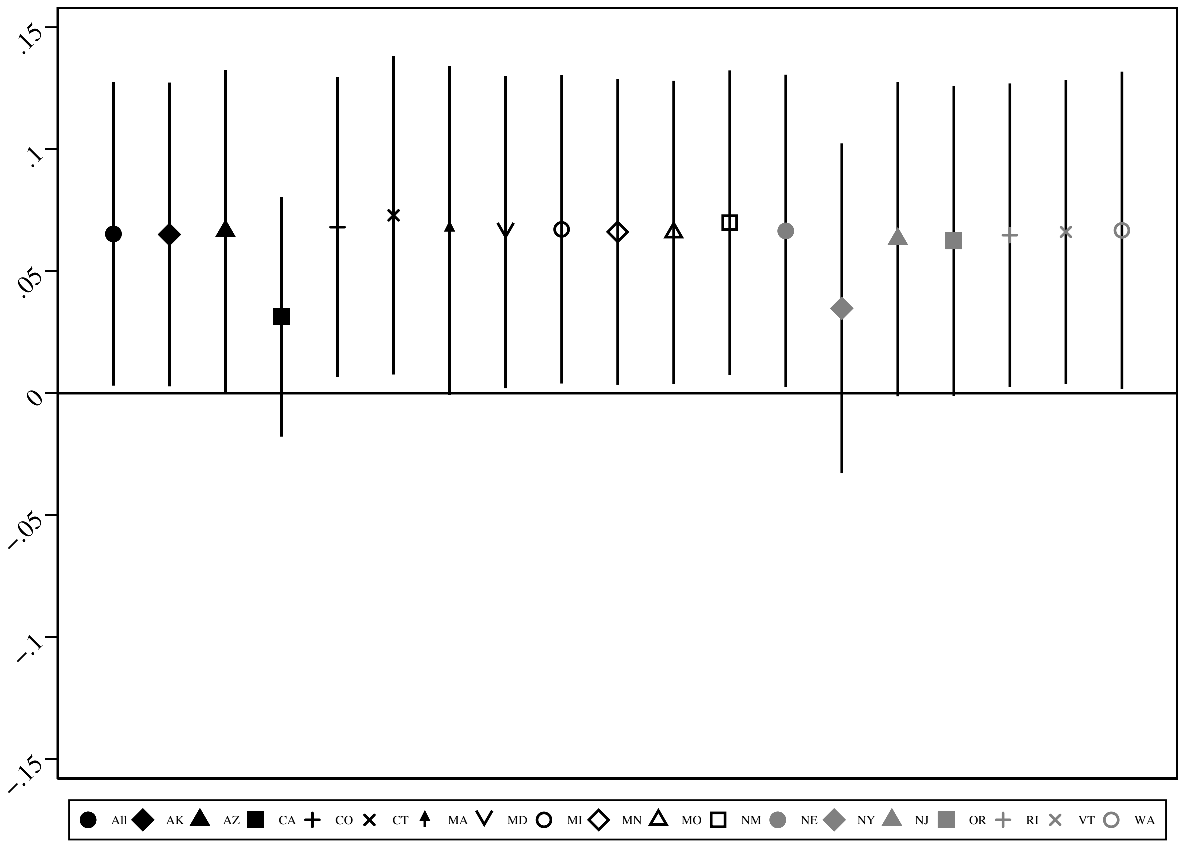

Notes: The specification or sample is listed in the legend. The outcome variable is the logarithm of Medicaid-financed dispensed prescription medications. The unit of observations is a state in a year-quarter unless otherwise noted. The District of Columbia is excluded from the sample as this jurisdiction is treated in all years. We utilize the Gardner et al. (2024) two-stage difference-in-differences. All regressions estimated with ordinary least squares and control for time-varying state characteristics, state fixed effects, and period (quarter-year) fixed effects unless otherwise noted. Data are weighted by the number of people enrolled in Medicaid per state-year unless otherwise noted. Coefficient estimates are reported in circles and 95% confidence intervals that account for within-state clustering are reported with vertical lines.

Figure A9: Effect of a state paid sick leave mandate on the number of people enrolled in Medicaid & the average age of people 0-64 years of age enrolled in Medicaid using an event-study: Centers for Medicare & Medicaid Services, & Annual Social & Economic Supplement to the Current Population Survey 2011-2024



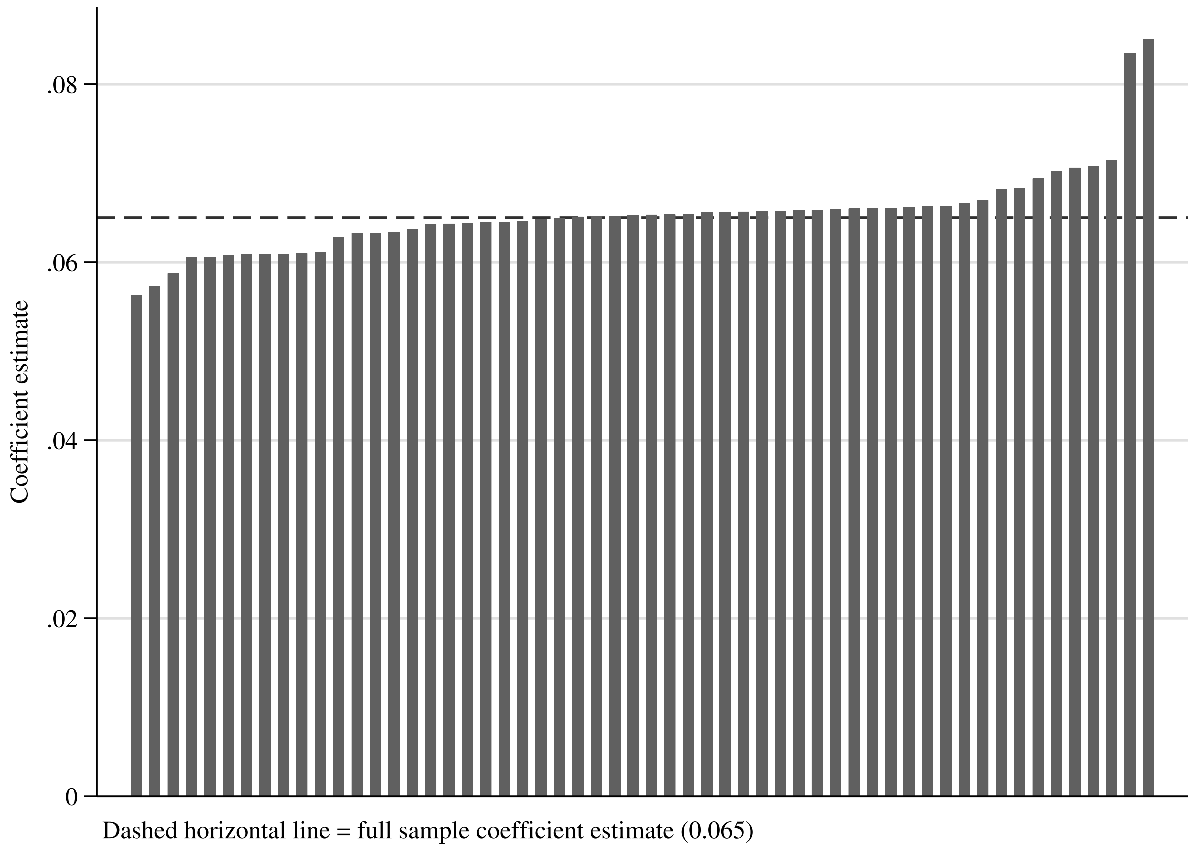

Notes: The outcome variable is listed in the legend. The outcome variables are i) the logarithm of the number of people enrolled in Medicaid and ii) the average age of people 0-64 years of age enrolled in Medicaid. The unit of observations is i) a state in a year and ii) a respondent 0-64 years of age in a state in a year-quarter. The District of Columbia is excluded from the sample as this jurisdiction is treated in all years. We utilize the Gardner et al. (2024) 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 (quarter-year) fixed effects unless otherwise noted. There is no omitted category in the Gardner (2022) procedure. For treated states, observations more than six years pre-mandate or six years post-mandate are excluded unless otherwise noted. Data are weighted by the number of people enrolled in Medicaid in 2011 when the outcome is the number of people enrolled in Medicaid and Annual Social & Economic Supplement to the Current Population Survey sample weights when the outcome is the average age of non-elderly people enrolled in Medicaid. Coefficient estimates are reported in circles and 95% confidence intervals that account for within-state clustering are reported with vertical lines.

Figure A10: The effect of a state paid sick leave mandate (lagged one year) on the logarithm of Medicaid-financed dispensed prescriptions sequentially excluding each treatment state: State Drug Utilization Database 2011 Q1-2024 Q4




Notes: The excluded state is listed in the legend. The outcome variable is the logarithm of Medicaid-financed dispensed prescription medications. The unit of observations is a state in a year-quarter. The District of Columbia is excluded from the sample as this jurisdiction is treated in all years. We utilize the Gardner et al. (2024) 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 (quarter-year) fixed effects unless. Data are weighted by the number of people enrolled in Medicaid per state-year. Coefficient estimates are reported in circles and 95% confidence intervals that account for within-state clustering are reported with vertical lines.

Figure A11: The effect of a state paid sick leave mandate (lagged one year) on the logarithm of Medicaid-financed dispensed prescriptions sequentially excluding each time period: State Drug Utilization Database 2011 Q1-2024 Q4




Notes: The coefficient estimates from each leave-one-out sample are ordered from the smallest (left) to the highest (right) values. 95% confidence intervals that account for within-state clustering are suppressed for readability and the specific excluded time period, but all coefficient estimates are statistically different from zero at the 10% level of better. Full results are available from the corresponding author on request. The outcome variable is the logarithm of Medicaid-financed dispensed prescription medications. The unit of observations is a state in a year-quarter. The District of Columbia is excluded from the sample as this jurisdiction is treated in all years. We utilize the Gardner et al. (2024) 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 (quarter-year) fixed effects unless. Data are weighted by the number of people enrolled in Medicaid per state-year.

Table A1: Example medication names in each drug class

| **Medication class** | **Example medications** |
|---|---|
| Antibiotics | Acanya, evoclin, macrobid, & rosadan |
| Birth control | Curae, kyleena, levonorgestrel, & norgestimate |
| Cardiovascular | Barnidipine, flosequinan, piretanide, & trimetazidine |
| Diabetes | Duetact, exenatide, semaglutide, & wegovy |
| Gastrointestinal | Balsalazide, homatropine, mazindol, & oxyquinoline |
| HIV & hepatitis | Harvoni, nevirapine, prezcobix, & telaprevir |
| Behavioral health | Ambien, buporion, naltrexone, & pristiq |
| Respiratory & allergies | Binosto, demerol, relafen, & viltolarsen |
| Pain medications | Hydroquinine, morphine, phenylbutazone, & oxycodonee |

Notes: Data reflect Anatomical Therapeutic Chemical classification. The data source is World Health Organization (2024). We include substance use disorder and smoking cessation medications from Maclean et al. (2019) and Maclean and Saloner (2019) in the behavioral health class. The full list of medications are available on request from the corresponding author.

Table A2: Effect of a state paid sick leave mandate (lagged one year) on out-of-pocket medical expenditures among people 0-64 years of age enrolled in Medicaid: Annual Social & Economic Supplement to the Current Population Survey 2011-2024

| Paid sick leave mandate variable ↓: | Coefficient estimate (Standard errors) |
|---|---|
| State paid sick leave mandate | 0.097*** |
| | (0.026) |
| Pre-treatment proportion, treatment states | $1288.771 |
| Observations | 240312 |

Notes: The outcome variable is out-of-pocket payments. The unit of observations is a respondent 0-64 years of age enrolled in Medicaid in a state in a year. The District of Columbia is excluded from the sample as this jurisdiction is treated in all years. We utilize the Wooldridge (2023) difference-in-differences procedure. Estimates of treatment effects are presented in terms of differences in linear predictions. All regressions estimated with a Poisson model and control for enrollee demographics, time-varying state policies, state fixed effects, and year fixed effects. Data are weighted by Annual Social & Economic Supplement to the Current Population Survey 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.

Table A3: Heterogeneity by size of the Medicaid population in the effect of a state paid sick leave mandate (lagged one year) on the logarithm of Medicaid-financed dispensed prescriptions: State Drug Utilization Database 2011 Q1-2024 Q4

| Sample ↓: | Coefficient estimate (Standard errors) |
|---|---|
| Full sample | 0.065** |
| | (0.032) |
| Pre-treatment mean (not logged, scaled), treatment states | 92.144 |
| Observations | 2800 |
| At or above median number of people enrolled in Medicaid in 2011 | 0.077** |
| (including California) | (0.037) |
| Pre-treatment mean (not logged, scaled), treatment states | 104.661 |
| Observations | 1400 |
| At or above median number of people enrolled in Medicaid in 2011 | 0.051 |
| (excluding California) | (0.035) |
| Pre-treatment mean (not logged, scaled), treatment states | 92.592 |
| Observations | 1344 |
| Below the median number of people enrolled in Medicaid in 2011 | -0.082 |
| | (0.050) |
| Pre-treatment mean (not logged, scaled), treatment states | 13.971 |
| Observations | 1400 |

Notes: The outcome variables are the logarithm of Medicaid-financed dispensed prescription medications. The unit of observations is a state in a year-quarter. The District of Columbia is excluded from the sample as this jurisdiction is treated in all years. 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 (quarter-year) fixed effects. Data are weighted by the number of people enrolled in Medicaid per state-year. Standard errors that account for within-state clustering are reported in parentheses.
***,**, and * = statistically different from zero at the 1%, 5%, and 10% level.
†Behavioral health = mental health, substance use, and smoking cessation prescription medications.

Table A4: The effect of a state paid sick leave mandate (lagged one year) on the logarithm of Medicaid-financed dispensed prescription medications using alternative estimators: State Drug Utilization Database 2011 Q1-2024 Q4

| Difference-in-differences estimator ↓: | Coefficient estimate (Standard error) |
|---|---|
| Gardner (2022) | 0.065** |
| | (0.032) |
| Two-way fixed effects | 0.065*** |
| | (0.022) |
| Callaway and Sant'Anna (2021)† | 0.042 |
| | (0.118) |
| Wooldridge (2023) | 0.126** |
| | (0.054) |
| Borusyak et al. (2024) | 0.081*** |
| | (0.029) |
| Observations | 2800 |
| Pre-treatment mean (not logged, scaled), treatment states | 95.95 |
| Stacked difference-in-differences (Cengiz et al., 2019)†† | 0.053 |
| | (0.034) |
| Observations | 4820 |
| Pre-treatment mean (not logged, scaled), treatment states | 103.58 |

Notes: The outcome variables are the logarithm of Medicaid-financed dispensed prescription medications. Pre-treatment means for dispensed prescription medications are scaled by 100,000. The unit of observations is a state in a year-quarter. The District of Columbia is excluded from the sample as this jurisdiction is treated in all years. All regressions estimated with ordinary least squares and control for time-varying stage characteristics, state fixed effects, and period (quarter-year) fixed effects. Data are weighted by the number of people enrolled in Medicaid per state-year. Standard errors that account for within-state clustering are reported in parentheses.

***,**, and * = statistically different from zero at the 1%, 5%, and 10% level.

†We use the never-treated and not-yet-treated as the comparison group and a doubly robust regression suggested by Callaway and Sant'Anna (2021). We do not control for time-varying covariates.

†† We use the never-treated and not-yet-treated as the comparison group. We include four cohorts of treated states, those treated in 2015, 2016, and 2017. Untreated states can appear in the comparison group for more than one cohort. For treatment states, we include only those observations within three years of the paid sick leave mandate. For comparison states, we (separately for each cohort) assign a 'placebo' effect year which is the effective year for treated cohort (e.g., cohort one is treated in 2015, in that cohort untreated states are assigned a placebo effective year of 2015) and include only those observations within three years of the 'event.'