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PAID SICK LEAVE AND MALTREATMENT

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ABSTRACT

Child maltreatment is a major public health concern in the United States. Maltreatment is associated with a range of poor health, developmental, and economic outcomes for child victims. In this study, we examine the impact of recent state paid sick leave mandates on child maltreatment reports over the period 2011-2022. Paid sick leave mandates confer financially protected time that can be used for health and family responsibilities as well as for actions such as court hearings that can reduce exposure to domestic violence. These benefits may also reduce maltreatment reports. Using difference-in-differences and event-study methods, we find that child maltreatment reports decline by 11% following the adoption of a state paid sick leave mandate. An analysis of mechanisms suggests that increases in parental and child health, family economic standing, childcare provision, and healthcare are important channels linking paid sick leave mandates to child maltreatment reports.

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1 Introduction

Child maltreatment is defined in the United States as ‘*Any recent act or failure to act on the part of a parent or caretaker which results in death, serious physical or emotional harm, sexual abuse or exploitation; or an act or failure to act which presents an imminent risk of serious harm*’ (United States Congress, 1974). Children who experience maltreatment are at elevated risk for multiple health and socioeconomic problems as adults, including substance use disorders, mental health disorders, reduced educational attainment, poor labor market outcomes, high-risk sexual behaviors, smoking, and chronic diseases (Centers for Disease Control and Prevention, 2013). Child maltreatment is common with nearly 40% of U.S. children experiencing a child protective services¹ investigation by age 18 (Kim et al., 2017) and in 2023 there were 4.3M child protective service investigations for child maltreatment (U.S. Department of Health and Human Services, 2024a; Children’s Bureau, 2023b).² Given the health and social consequences and high prevalence, the annual economic burden of child maltreatment is estimated to be \$586B in 2025 dollars (Peterson et al., 2018).³ Reducing child maltreatment is a national objective (U.S. Department of Health and Human Services, 2024b).

Governments at all levels have adopted policies to directly reduce child maltreatment. Policies adopted to date are both supportive and punitive in nature. At the federal level, the *Child Abuse Prevention and Treatment Act* of 1974 is the key piece of legislation regarding child maltreatment in the U.S., and includes both punitive and supportive provisions. The Act provides funding for child maltreatment prevention, assessment, data collection, investigation and prosecution of perpetrators, and treatment for states that meet federally set standards. Alternatively, the federal Healthy Families American Program is supportive in nature only. This program offers in-home support to new parents, and visits include building parenting skills, and promoting child development and well-being. Some states provide priority substance use disorder treatment to parents, which supports parents with substance use disorder – a risk factor for child maltreatment (Meinhofer and Angleró-Díaz, 2019; Sanmartin et al., 2020; Meinhofer et al., 2024), while other states have criminalized substance use by pregnant women (Maclean et al.,

¹Child Protective Services is a term used for the state agency that is charged with responding to reports of child maltreatment. Child Protective Services agencies are covered by both state-specific laws and the federal *Child Abuse Prevention and Treatment Act*. Though each state operates its own agencies, generally, the duties and responsibilities of Child Protective Services include investigating reports of child maltreatment, assessing risks to children, providing services to families and, if needed, removing children from unsafe home environments.

²See Exhibit 2D number of referrals.

³We inflate the original estimate of \$428 in 2015 to 2025 terms using the Consumer Price Index-Urban Consumers.

2022), which is a policy more punitive in nature. State and federal policies that provide lower-income families with financial support (e.g., Earned Income Tax Credit, Child Tax Credit, and minimum wages) can reduce maltreatment attributable to inadequate family resources (Spencer et al., 2021; Kovski et al., 2022; Bullinger and Boy, 2023; Bullinger et al., 2025). Policies that expand access to contraception, which potentially allow parents to better time pregnancies, have been linked with reduced child maltreatment (Bitler and Zavodny, 2002; Aslim et al., 2024; Piña et al., 2024).

We study the effect of state paid sick leave (PSL) mandates, a policy designed to support workers and their families, on child maltreatment reports. 17 states and the District of Columbia have adopted or announced a PSL mandate. These mandates provide workers with seven days of financially protected time each year that can be used for the worker’s own health needs or meeting family responsibilities. Mandated PSL can support working people execute their roles as caregivers to children which may, in turn, reduce maltreatment reports. For example, mandated PSL can allow parents to stay home from work during a school closure or when a child is sick, take children to receive healthcare, and — as codified in all state PSL mandates to date — provide financially protected time for those attempting to escape domestic violence (e.g., attend court hearings, moving-related activities). Recent work (discussed in Section 2.1) also suggests that PSL mandates increase employment and economic stability. These changes could potentially reduce risk for child maltreatment by relaxing financial constraints. Finally, state PSL mandates appear to improve health, in particular mental health, which could enhance parenting effectiveness and reduce child maltreatment.

Using administrative data on all child maltreatment reported to Child Protective Services in the U.S. over the period 2011-2022 and difference-in-differences and event-study methods that are robust to bias associated with treatment heterogeneity and dynamics with a staggered policy roll-out, we show that adoption of a state PSL mandate reduces child maltreatment reports by just over 11%. We also study potential mechanisms for the main effects by examining changes in parent and child health, family economic standing, family structure, childcare provision, and healthcare utilization following adoption of a state PSL mandate. In an extension, we also show that PSL mandates may reduce other forms of violence within the family. Our results are robust to numerous sensitivity checks and do not appear to be attributable to differential trends between states that do and do not adopt a paid sick leave mandate. Overall, we document important spillovers from labor market policies to child wellbeing. These findings extend our understanding of PSL mandates’ impacts on workers and their families.

The paper proceeds as follows. Section 2 reviews PSL in the U.S., the related litera-

ture on PSL mandates, and our contributions. Data and methods are reported in Section 3. In Sections 4.1, 4.2, and 4.3 we present evidence on the first-stage, our main results, and an analysis of potential mechanisms. Sections 4.4 and 4.5 report an extension to domestic violence and robustness checking respectively. Finally, Section 5 provides a discussion and conclusion.

2 Background and contributions

2.1 Background on paid sick leave

Most developed countries have federal policies in place that provide workers with PSL. This leave can be used for own health needs and for the health needs of dependents. The U.S. is one of four such countries that does not have a federal PSL policy in place (Pichler and Ziebarth, 2020). The federal void continues despite strong public support (City Health, 2020)⁴ and several legislative efforts, beginning in 2005, to adopt the *Healthy Families Act*. U.S. Senator Theodore Kennedy proposed the initial Act in 2005 (Pichler and Ziebarth, 2020). The most recent attempt to adopt federal PSL policy was pursued by U.S. Senators Rosa DeLauro and Bernie Sanders in 2023 (Sanders and DeLauro, 2023). To date, there is only one federal paid leave policy, the 1993 *Family Medical Leave Act* (FMLA). This Act provides up to 12 weeks of unpaid leave for longer-term illness or bounding with a new child for eligible workers. The Act includes numerous exemptions and only 44% of workers are eligible (U.S. Department of Labor, 2023). Between April and December 2020 – in response to the COVID-19 pandemic – the U.S. adopted the *Families First Coronavirus Response Act* (FFCRA). This Act provided some workers – those deemed ‘non-essential’ and working for firms with 50-500 workers – with up to two weeks of PSL that could be used for COVID-19-related health needs (e.g., quarantining) and family responsibilities (Andersen et al., 2023).

Though there is no permanent federal PSL policy, states and cities have adopted policies designed to provide workers with PSL. These policies are in the form of employer mandates.⁵ San Francisco California adopted the first PSL mandate in 2007, followed by the District of Columbia in 2008 and Connecticut in 2012. Table A1 provides the effective month and year for each state PSL mandate adopted or announced by November, 2024

⁴In May 2020, 78% of American adults agreed with the following statement: ‘Do you favor or oppose a law that guarantees paid sick leave?’

⁵PSL programs are more developed in Europe. Generally, benefit levels are more generous than the state and city mandates that have been adopted in the U.S. For brevity, we focus on the U.S. experience. See Pichler and Ziebarth (2020) for a discussion of European programs.

based on legal data compiled by the [National Partnership for Women & Families \(2023\)](#) and [A Better Balance \(2024\)](#). We mainly analyze state mandates for technical reasons outlined in Section 3.1 and thus focus our discussion on these mandates. The [National Partnership for Women & Families \(2023\)](#) estimates that, by October 2023, roughly 21M employees gained access to PSL for the first time as a result of these mandates (see Table A1 column 3). The true reach of these mandates is likely larger as some employees – whose employers voluntarily offered PSL below the newly mandated level – will experience an increase in benefit generosity as employers comply with the law.

All state mandates have the same structure as the 2005 *Healthy Families Act*. Employers are compelled to provide employees with – on average – seven days of PSL per year at 100% wage replacement (employees must pay tax on leave time earnings). Though there are differences in carry forward rates, unused PSL can be rolled over to future years. Workers must ‘earn’ PSL by working, typically earning one hour of PSL for every 30-40 hours worked. Leave can be used for health needs of the eligible employee and their dependents (e.g., healthcare appointments). Further, all mandates allow employees to use leave for escaping domestic violence situations (‘safe time’). For example, time can be used for attending court hearings, moving, and so forth. All mandates to date include children (biological, adopted, and step), and partners and spouses as eligible dependents. Most mandates additionally include other dependents such as grandparents. Some mandates also confer unpaid sick leave to employees.

Employees do not need to provide substantial details on the use of PSL, though employees must notify the employer in advance, except in the case of emergencies when advanced notification is generally waived ([Pichler and Ziebarth, 2020](#)). Employers must post signage in the workplace to increase employee awareness of the benefit (see Figure A1). Further, employers are prohibited from retaliating against employees using the mandated PSL for allowable purposes, for example, employers cannot fire such employees.⁶ Moreover, employers cannot require leave-using employees to locate replacement employees or require substantial documentation justifying use of leave. These features of PSL mandates differentiate the policies from ‘paid time off’ (PTO) mandates adopted by four states ([National Partnership for Women & Families, 2023](#)) – Illinois (2024), Michigan (2019), Maine (2021), and Nevada (2020). PTO mandates allow employees to take time off work for any reason, but these mandates do not include the above-noted employee protections, which could limit employee use of the benefit. We follow earlier work

⁶We note that most U.S. states are ‘employment at will’ states (Montana is the exception), thus employers can generally fire employees ‘at will,’ that is, at any point and for any reason, as long as the reason is not illegal. This issue applies to most labor market policies, not just PSL mandates.

and code PSL and PTO mandates separately (Maclean et al., 2025, 2024), but we report results in which we expand our definition of PSL to include PSL or PTO mandates in Section 4.5 and our main findings hold, likely as this change only impacts three states.

There is a growing literature on the effect of PSL mandates in the U.S. Using detailed information on private establishments in the 2009-2022 National Compensation Survey (NCS), Maclean et al. (2025) show that following adoption of a state PSL mandate, the probability that an employer offers PSL to an employee increases by 33% and employee use of PSL increases by 22%. Increases in the use of unpaid sick leave roughly double (Maclean et al., 2025). Several other studies – either using the NCS or survey data – similarly document increases in PSL access and utilization (Ahn and Yelowitz, 2016; Callison and Pesko, 2022; Maclean et al., 2023; Dong et al., 2024), though Stearns and White (2018) find that self-reported absences from work decline post-mandate, which the authors attribute to reduced disease spread within the workplace as employees sick with infectious conditions (e.g., influenza) take time off.

Employers do not appear to curtail overall labor costs in response to the newly mandated PSL as would be predicted in a traditional Summers (1989) framework. The lack of response could be due to the low cost of state PSL mandates: Maclean et al. (2025) show that the costs of these mandates are 6.2 cents per employee hour worked. Moreover, Miller (2022) finds no change in employer bankruptcies post-mandate, though consumer bankruptcies decline. Indeed, employers may increase the generosity of their benefit packages (Maclean et al., 2025; Slopen, 2024), these employers (pre-mandate) included PSL in their benefit packages to compete for labor, but post-mandate these employers must improve other aspects of compensation (Boots et al., 2009).

A number of studies suggest that PSL mandates lead to increases in healthcare use – prescriptions, contraception, and vaccinations (Pichler and Ziebarth, 2017; Pichler et al., 2021; Maclean et al., 2023, 2024), reductions in unnecessary care such as emergency department use (Ma et al., 2022), and higher healthcare expenditures (Hebert et al., 2024).⁷ In terms of moral hazard, to date there is no evidence that PSL mandates induce such behaviors: Guo and Peng (2024) observe no change in drinking following adoption of a state PSL mandate. Several studies suggest that state PSL mandates improve health outcomes (Stearns and White, 2018; Slopen, 2023).

Mandated PSL can be used to provide care to dependents and several studies document that the provision of child and eldercare increases following a state PSL mandate (Byker et al., 2023; Arora and Wolf, 2024; Guo and Peng, 2024; Maclean and Pabilonia,

⁷Guo and Peng (2024) find limited evidence using survey data that self-reported preventive healthcare responds to PSL mandates.

2024). For example, [Maclean and Pabilonia \(2024\)](#) show that parents spend approximately 5% more time per day on primary childcare following a state PSL mandate.

To date, to the best of our knowledge, there is limited information on the impact of state PSL mandates on child maltreatment. We note that in a paper written concurrently and independently to our work, [Qiu \(2025\)](#) finds evidence that early adopters of state PSL mandates (before 2020) experience a decrease in child-related victimization reported to local law enforcement in the National Incident-Based Reporting System (NIBRS) – by approximately 10%. Thus, [Qiu \(2025\)](#) studies law enforcement-reported crimes against children while we examine maltreatment overall, and include the pandemic period.

Our use of the NCANDS data — the most comprehensive child maltreatment database in the U.S. and the source used by the federal government to track child maltreatment ([U.S. Department of Health and Human Services, ND](#))⁸ — offers several advantages for our study relative to the NIBRS data. First, we are able to consider all child maltreatment reports to Child Protective Services. Child Protective Services is the state agency that is responsible for investigating all reports of child abuse and neglect, while the law enforcement agencies tracked by NIBRS will only include reports to law enforcement. In our data, we find that 18.9% of all incidents list the reporting source as ‘*legal, law enforcement, or criminal justice personnel*,’ which is likely an upper bound on incidents captured by NIBRS. Thus, by using NCANDS, we are able to study a wider range of maltreatment than afforded by NIBRS. Second, NCANDS contains information on the outcome of the report, that is, did Child Protective Services determine whether there was evidence of maltreatment or not? Third, we can explore heterogeneity by reporting source (e.g., parent, healthcare professional, law enforcement) and type of maltreatment (physical abuse, sexual abuse, neglect, and emotional abuse), as these types of maltreatment can have different implications for children. Moreover, criminal justice scholars raise concerns regarding inconsistent reporting of data to NIBRS ([Kaplan, 2025a](#)), suggesting that these data may not offer a complete picture of law enforcement-reported maltreatment in the U.S. We focus on the NCANDS, and as described in Section 3.1 years in which all states report, and thus our analysis not vulnerable to coverage concerns.⁹ Finally, we can explore changes in family services received (e.g., foster care, mental health, employment) and a wide range of types of maltreatment that occurred – i.e., that Child Protective Services determined have occurred, not simply what civil-

⁸Each year the NCANDS data are used to produce the Administration for Children and Families’ Child Welfare Outcomes Reports to Congress and annual Child Maltreatment reports. NCANDS was established following a 1988 amendment to the *Child Abuse Prevention and Treatment Act*.

⁹As discussed in Section 3.3, we will use NIBRS in an extension to our main analyses. After imposing recommended restrictions on the data, we have just 32% of law enforcement agencies in our sample.

ians report to law enforcement. In summary, these two studies add complementary and independent contributions to our understanding of potential spillovers from state PSL policies to child maltreatment outcomes.

A handful of studies document that state paid medical and family leave policies (described above) reduce child maltreatment reports (Tanis et al., 2024; Bullinger et al., 2025). For example, Bullinger et al. (2025) find that these policies reduce child maltreatment by roughly 14%. However, given differences in the focus and duration of the two leave types, separate consideration of state PSL mandates is necessary.

We expect that state PSL mandates will impact child maltreatment through several different channels, and these channels could decrease or increase reports. First, PSL mandates may facilitate children’s receipt of healthcare, which could reduce reports of neglect as children’s health needs are better met. Moreover, in line with the findings of Ma et al. (2022), PSL mandates reduce emergency department visits, suggesting that children may be less likely to appear in emergency departments experiencing untreated health conditions or accidents, and further reduce the likelihood of a maltreatment report by emergency department healthcare professionals. Healthcare professionals are generally ‘mandatory reporters’ for child maltreatment, meaning that they are legally required to report observed maltreatment of children to Child Protective Services. Alternatively, if children receive more (non-emergency) healthcare following a PSL mandate adoption, the increase in interactions with mandatory reporters such as healthcare professionals could increase maltreatment reports. Second, parents may be better able to take time off to care for children who are sick or have a school closure. To the extent that teachers (who are generally mandatory reporters) may be more likely to file a maltreatment report on a child sent to school sick, PSL may enable parents to take time off to care for their sick children and therefore reduce child maltreatment reports. PSL would also decrease incidents of parents leaving children unattended during school closures if they are able to take time off from work, which could reduce the number of child maltreatment reports. Third, PSL can be used to escape domestic violence (‘safe time’) as described earlier in this section. Paid leave used for this purpose may allow parents to leave unsafe relationships and thus reduce the probability that a child experiences maltreatment. Fourth, PSL mandates improve economic outcomes (e.g., Miller (2022); Slopen (2024)), with wages and employment increasing post-mandate and poverty and consumer bankruptcy rates declining, improved financial standing may also reduce maltreatment reports as families are better able to provide necessities for children. Finally, improvements in health may allow parents to more effectively execute their roles as caregivers to children, thus reducing maltreatment reports. These channels need not operate in isolation and,

instead, there may be interactions across channels. We will test the net effect of state PSL mandates on child maltreatment reports in our main analyses and explore potential mechanisms for the overall effect.

2.2 Contributions

Our work connects to several literatures. First, we study the role of public policy in child development generally and maltreatment specifically (Almond et al., 2018; Aizer et al., 2022). Childhood includes critical periods of development, and shocks – positive or negative – experienced during this stage can have persistent impacts across the lifecourse (Cunha and Heckman, 2007; Currie and Almond, 2011). Previous work shows that programs providing financial resources to families (e.g., Earned Income Tax Credit) reduce child maltreatment (Spencer et al., 2021; Kovski et al., 2022; Bullinger and Boy, 2023; Bullinger et al., 2025). Similarly, policies that increase access/decrease financial costs to healthcare reduce child maltreatment (Brown et al., 2019; Maclean et al., 2022). We study the impact of providing parents with financially protected time away from work to attend to own-health and child-rearing responsibilities.

Second, we add to the small but growing literature investigating the social impacts of state PSL mandates discussed in Section 2.1. We are the first to study the effects of these policies on all types of child maltreatment.

Third, we add to the literature specifically examining determinants of child maltreatment. Parental economic resources are well-documented determinants of child maltreatment, as measured by parental employment, income, and education, (Paxson and Waldfogel, 2002; Sedlak et al., 2010; Lindo et al., 2013), as well as parental receipt of government benefits (Sedlak et al., 2010). For example, mass layoffs in male-dominated industries increase child maltreatment, while mass layoffs in female-dominated industries decrease child maltreatment (Lindo et al., 2013).¹⁰ There are several non-economic determinants of child maltreatment, including parental time - both quantity and quality (Paxson and Waldfogel, 2002), and family structure (Paxson and Waldfogel, 2002; Sedlak et al., 2010; van Berkel et al., 2024). PSL could potentially affect not only the quantity of parental time, but also quality if parents are better able to tend to their health needs and a healthier parent may be able to invest higher quality time with their children than sick parents. Mandated PSL could lead to changes in family structure, potentially through safe-time provisions which support employees seeking to leave rela-

¹⁰Previous literature has documents sex differences between paternal and maternal propensity for child maltreatment (Guterman and Lee, 2005; Dubowitz, 2006; Francis and Wolfe, 2008; Lee et al., 2009; Lindo et al., 2013), which likely lead to the heterogeneous effects of mass layoffs.

tionships that involve domestic violence or by alleviating financial strain is a determinant of divorce (Hawkins et al., 2012).

3 Data and methods

3.1 Child maltreatment data

We use the National Child Abuse and Neglect Data System (NCANDS) (Children’s Bureau, 2023a) as our main source of data. Beginning in 1995, these administrative data provide the most detailed information available on Child Protective Services investigated reports for child maltreatment that receive a disposition each fiscal year. Many states initially did not contribute data to NCANDS as participation is voluntary. We start our study period in 2011 as all states report data as of that year and we end our study period in 2022 as that the most recent (full) year of data available at the time of writing. Though data for fiscal year 2023 are available at the time of writing, reports initiated in 2023 may not have received a final disposition by Child Protective Services, thus we exclude this year of data from our main analysis sample. However, we will show in Section 4.5 that our results are robust to including earlier years, including 2023, and sequentially excluding each year 2011-2022 from the sample.

We focus on maltreatment reports for children 0-18 years of age at the time of the initial Child Protective Services report. The NCANDS can include more than one report per child-incident. In such cases, we use the age at the initial report, but we use disposition information (i.e., if Child Protective Services determines that maltreatment occurred) from the final NCANDS entry associated with the child-incident. Children can experience more than one – separate – maltreatment incident. As a specific example, a child could experience a maltreatment incident that is reported to Child Protective Services in 2011 and then, for a different report to Child Protective Services, in 2017. We treat these incidents as two separate incidents. Notably, we are not able to link such reports over time. Finally, more than one child can be associated with any given maltreatment incident, e.g., a parent can be reported to Child Protective Services for potentially maltreating three children, and in such a case we would observe each child-incident separately, thus leading to three incidents.¹¹ We use NCANDS procedures to

¹¹The vast majority of child maltreatment perpetrators are parents – 89% in fiscal year 2023 (Children’s Bureau, 2023a), and thus use the term ‘parent.’ Still, we note that other people can also be perpetrators of maltreatment. These include other relatives (non-foster parents), relative foster parents, nonrelative foster parents, group home or residential facility staff, child daycare providers, unmarried partners of parents, legal guardians, other professionals, and friends or neighbors.

aggregate the child-report case-level data to the level of state-year ([Children’s Bureau, 2023a](#)). Thus, the unit of analysis in estimation is a state in a year.

Though the NCANDS includes county information, we do not use that information to study the sub-state PSL mandates in our analysis. From the perspective of PSL mandates, what is important is the location of employment and not the location of the Child Protective Services maltreatment report. We are not aware of any data that could be used to determine differences between the location of parental employment and the location of Child Protective Services reporting. However, we can use location of residence as a proxy for Child Protective Service reporting location, this approach will likely provide a lower bound estimate on potential measurement error as residence may differ from Child Protective Service reporting location for many families. Using the 2019 American Community Survey ([Ruggles et al., 2023](#)), we find that 97% of employed adults 25 to 62 years of age live and work in the same state, while only 77% of such adults live and work in the same county. Thus, there is likely to be substantially more measurement error in linking reports to policies when relying on sub-state mandates rather than state mandates. Moreover, in terms of the NCANDS data, counties with fewer than 1,000 reports per year are suppressed. [Ali et al. \(2024\)](#) document that roughly 60% of counties are suppressed, suggesting that we may miss many counties if we use data at the county level. However, in robustness checking (Section 4.5), we report analyses in which we incorporate sub-state mandates into our analysis, and results are not appreciably different.

We consider three measures (counts) of child maltreatment: total, substantiated, and unsubstantiated or alternative response reports per state-year. A substantiated report occurs when a Child Protective Services case worker determines that a child has been maltreated. In unsubstantiated reports, the case worker determines that no maltreatment has occurred. While substantiated reports potentially capture higher risk situations for affected children than unsubstantiated reports, unsubstantiated reports potentially impose hardship on families as well. For example, undergoing a Child Protective Services investigation is stressful and costly for families. Moreover, medical research suggests that the differences in outcomes for children with substantiated and unsubstantiated maltreatment may not be clinically meaningful. For example, risk of subsequent maltreatment ([Kohl et al., 2009](#)), behavioral outcomes ([Hussey et al., 2005](#)), human capital outcomes ([Leiter et al., 1994](#)), and substance use outcomes ([Kugler et al., 2019](#)) are quite similar across children with substantiated and unsubstantiated maltreatment. Thus, we consider both types of maltreatment reports.

In some states, low-risk reports can receive an ‘alternative response’ rather than a

standard Child Protective Services investigation. In these situations, a Child Protective Services caseworker partners with the family to connect the family with services (e.g., mental healthcare, housing, employment) and develop strategies to reduce risks to children and prevent family separation (Maclean et al., 2022). We include alternative response reports with unsubstantiated reports in our analysis (i.e., we take the sum of the two types of reports), as not all states provide alternative responses, though we will report these two types of reports separately in Section 4.5. We convert counts of each reporting metric to a yearly rate per 1,000 children 0-18 years in the state (Surveillance, Epidemiology, and End Results, 2022).

We also consider younger and older children separately. In particular, we construct the three maltreatment metrics for children 0-5 years and 6-18 years. The 0-5 period is a critical period for child development (Currie and Almond, 2011), and older and younger children have different care needs. For example, younger children are more likely to need primary care, such as feeding. On the other hand, older children are more likely to only need supervisory care (Maclean and Pabilonia, 2024). We use the population 0-18 to construct rates for the age-specific outcomes to maintain a common denominator.

DC adopted a mandate prior to the beginning of our study period (2008). We exclude DC from our analysis as this state is always treated 2011-2022. However, in robustness checking (Section 4.5), we will extend the study period back to 2004 and, in this analysis, we include DC and results are largely unchanged. Our final analysis sample is balanced and includes 600 state-year observations.

3.2 Paid sick leave data

We rely on the National Partnership for Women & Families (2023) and A Better Balance (2024) for information on PSL mandates. Table A1 provides the effective month and year for each state that adopts or announces a PSL mandate by November, 2024. As described in Section 3.1, our NCANDS data are aggregated to the state-year and we match state PSL mandates on the first partial year in which a mandate is in place, this coding will likely yield muted effects in the year of the policy change, but creates a clean (untreated) pre-mandate period. In event-studies reported in Section 4.2, we will code states that adopt a PSL mandate after 2022 (i.e., Alaska, Minnesota, Missouri, and Nebraska) as in their pre-treatment period, though we will report results in which we i) exclude these states and ii) code these states as zero for all time-to-event periods, and our results are robust to these alternative approaches (Section 4.2).

Figures 1 and 2 report the temporal and geographic distribution of PSL mandates

cross U.S. states. The mean adoption year is 2019. There is some geographic clustering across the U.S., for example, there are few PSL mandates in the South region, we will include region-by-year fixed effects in regressions to account for this distribution.

3.3 Additional datasets

We rely on several auxiliary datasets that we use to i) test for ‘first-stage’ effects, ii) explore mechanisms, and iii) consider related extensions to the main analyses.

National Compensation Survey: Following earlier work ([Maclean et al., 2025](#)), we use the NCS 2009-2022 to study ‘first-stage’ effects of PSL mandates on access to PSL and employee use of PSL.¹² The NCS, maintained by the Bureau of Labor Statistics, is a nationally representative sample of establishments. The data are used by the federal government for two key purposes: i) to produce official government statistics on labor costs, compensation, and benefit offering, and ii) to adjust the wages of federal employees. As such, the data include a wide range of benefit information (including PSL) and substantial efforts are made to ensure data quality. The unit of observation is a job in an establishment, thus while the NCS includes extensive information on benefits, there is no information on the demographics of people who hold jobs, which is a limitation. We construct a measure of access to PSL – whether or not the job has any PSL benefits, and the average quarterly hours of PSL used in a job. The first measure (access) is measured in the first quarter of the year. The second measure (use) is the average over four quarters of the year, we follow [Maclean et al. \(2025\)](#) and construct the average over quarter one of the focal year and quarters two to four over the year prior to the focal year. For example, in 2019, we take the average over 2019 quarter 1, 2018 quarter 2, 2018 quarter 3, and 2018 quarter 4. Focusing on a single quarter (e.g., quarter 1) could offer a biased estimate of PSL use due to seasonal fluctuations in need for PSL (e.g., influenza season). We include both private and government jobs in our sample, there are 691,388 observations in our analysis sample.

Behavioral Risk Factor Surveillance System: A mechanism through which PSL could affect child maltreatment could be through parental physical and mental health. To the extent that parents may have more flexibility to more effectively manage and prevent

¹²We note that our analysis period is slightly different for the NCS than for our other data sources. At the time of writing, the Bureau of Labor Statistics is transitioning access to the NCS for external researchers and the data are not currently available, thus we were not able to match the samples exactly, but we will update when the NCS data are available again (expected summer 2025). We note that our NCANDS findings are not different if we use the study period 2009-2022, results are available on request from the corresponding author. We also note that we include for year fixed effects in the NCS analysis for this same reason.

physical and mental illnesses, PSL may improve parental overall health and, in turn, equip parents better to handle childcare, thereby reducing child maltreatment risk. To study parental physical and mental health, we use the Behavioral Risk Factor Surveillance System (BRFSS) 2011-2021. The BRFSS is a publicly available individual-level national survey which is conducted annually by the Centers for Disease Control and Prevention (CDC) with the goal to provide a nationally representative dataset on health behaviors. From the 4,906,381 respondents in the 50 states and DC we observe between 2011-2022, we restrict the analysis to adults between ages 22 and 59 who reside with at least one child younger than 18 years of age (the manner in which the household children information is collected prevents us from including respondents with children age 18 years), which leaves us with 1,112,488 observations. The BRFSS asks respondents whether they would say their general health is excellent, very good, good, fair, or poor. We use this question to construct two indicator variables: i) reporting one’s health as excellent or very good, and ii) reporting one’s health as fair or poor. Respondents are also asked the number of days in the past month in which the respondent’s health was not good, separately for physical health and mental health. The BRFSS further asks respondents the number of days in which their poor physical or mental health kept them from doing usual activities such as self-care, work, or recreation. In addition to the general health questions, we use month and year of interview, as well as the sex, age, race, and education of the respondent as controls.

American Time Use Survey: To study time spent with children, we use the American Time Use Survey (ATUS) 2011-2022. The ATUS is administered by the U.S. Census on behalf of the Bureau of Labor Statistics. The ATUS sample is nationally representative and includes respondents to the Current Population Survey who are interviewed five to eight months after the final Current Population Survey interview. We use a harmonized version of the ATUS prepared by IPUMS (Flood et al., 2023). The ATUS uses a time diary format in which respondents are asked to record all activities for a 24-hour period, respondents are also asked to report the location in which an activity occurred and the people present. To focus on families with children, we restrict the sample to adults ages 22 to 59 years of age with children 18 years and under, leaving us with 41,305 observations. We construct two child care metrics using Bureau of Labor Statistics-prepared variables: primary childcare of household children and secondary childcare of household children.¹³ Primary childcare is defined as an activity that includes time

¹³We use the following IPUMS variables: primary childcare for a household child (BLS CAREHH KID) and secondary childcare for household children (SCC HH). We also include respondent sex, age, race, and education.

spent on the direct care of children. This care may include physical care, child-related healthcare, reading to children, playing with children, educational activities, talking with children, and so forth. Secondary childcare information is only collected for households with a child under 13 years. Secondary childcare captures time spent engaged in a primary activity (other than childcare provision) but the respondent reports that a child is in their care. For example, a respondent may report preparing a meal as the primary activity, but the respondent may also be supervising a child who is doing homework.

Annual Social & Economic Supplement to the Current Population Survey: We use the Annual and Social Economic Supplement to the Current Population Survey (ASEC) 2012-2023 to study the impact of PSL mandates on labor market, family structure, and health outcomes. In the ASEC, the labor market variables we study refer to the past calendar year, thus data from the 2012-2023 ASEC files refer to the calendar years 2011-2022. Hoover, family structure and health outcomes reflect status at the time of the interview, thus we use survey years 2011-2022 for these outcomes. We use a harmonized version provided by [Flood et al. \(2024\)](#). Each year, the ASEC is fielded between February and April by the U.S. Census Bureau on behalf of the Bureau of Labor Statistics. Annually, the ASEC includes approximately 150,000 respondents. Respondents are asked a range of questions related to income sources, benefit receipt, health insurance, labor market participation, and demographics. Our final analysis includes 520,538 adults 22 to 59 years of age with minor children in the household. We consider the following labor market and economic outcomes: any work in the past year, weeks worked in the past year, usual hours worked per week, full-time employment, income from wages and salary (we take the logarithm of earnings, but add a value of one prior to logging), and family income below the Federal Poverty Level. We also measure marital status (measured at the time of the interview) to proxy family structure. We examine four indicator variables that capture all marital status patterns reported in the ASEC: i) married or living as married, ii) divorced or separated, iii) widowed, and iv) never married. We have 535,365 observations on adults ages 22-59 with minor children in the household. Finally, we use the ASEC to study changes in reported health among children 0-18 years, we construct measures of reporting excellent or very good, and fair or poor health. A knowledgeable adult in the household (e.g., a parent) provides this health information for children. We have 624,663 children ages 0-18.¹⁴

MarketScan commercial claims: We draw on the Merative™ MarketScan® Research Database ('MarketScan Data'), a longitudinal panel of employer-sponsored health insurance claims, to analyze emergency department (ED) utilization. MarketScan links

¹⁴We use information on respondent age, sex, race, and education.

paid claims and encounter records with monthly enrollment files and basic demographics for the period 2016–2022. The MarketScan Database includes aggregated person-level clinical use, expenditures, and enrollment across inpatient, outpatient, prescription-drug, and carve-out services. Contributed by roughly 350 payers (large self-insured employers), these data capture adjudicated paid claims and capitated encounters for each service rendered. In total, our sample comprises roughly 75 million enrollees, of whom about 53 million are planholders (employees) and 17 million are child dependents. For our study, we extract and analyze all claims and encounters occurring in an ED setting. We aggregate past-year ED episodes, as a summary measure of healthcare use, at the state-year level. More specifically, many ED episodes are avoidable with appropriate preventive and ambulatory care, thus, if ED episodes decline post-PSL mandate – as shown by [Ma et al. \(2022\)](#), then this pattern of results is suggestive that PSL adoption allows parents and children to receive more appropriate care. Because PSL mandates can be used in the case of domestic violence, we might expect that injuries related to domestic violence are less likely to occur and lead to an ED episode. Furthermore, in the case of children, state PSL mandates may allow parents to better supervise their children when they are sick or there is a school closure, thereby reducing the risk of injuries.

Intimate partner violence: In an extension to the main analyses, we investigate whether state PSL mandates may reduce intimate partner violence. Previous studies indicate that increased earnings and employment security can materially lower domestic violence ([Aizer, 2010](#); [Anderberg et al., 2016](#); [Bhalotra et al., 2021](#)). Health-related shocks may similarly strain households, and policies that alleviate medical and income stresses could, in theory, reduce intimate partner violence. Motivated by these insights, we examine whether state PSL mandates produce a comparable spillover by lowering intimate partner violence. We collect administrative data from the NIBRS Victim Segment ([Kaplan, 2025b](#)). We identify assaults, sexual offenses, and homicide incidents in which the offender is recorded as a spouse, boyfriend, or partner, and compare patterns before and after state PSL mandate adoption over the period 2011–2022.¹⁵ Many local municipalities do not consistently report crime data as part of NIBRS over time ([Kaplan, 2021](#)). To overcome potentially selective reporting in the data, we follow previous research and conduct this analysis at the police agency-level and restrict the analysis sample to agencies that report crimes to NIBRS at least once in every year 2011–2022 ([Barbos and Sun,](#)

¹⁵We categorize assaults as aggravated assault, intimidation, or simple assault. Sexual offenses include fondling, incest, rape, and sexual assault. We categorize murder/nonnegligent manslaughter and negligent manslaughter as homicides. Some incidents contain more than one offense code we classify a crime as an assault, sexual offense, or homicide if each respective crime appears as one of the first two offenses recorded in the data.

2025; Deza et al., 2024). Our final sample includes 4,593 out of 14,263 (32%) agencies that ever reported data to the NIBRS during our study period 2011-2022.¹⁶

State Youth Risk Behavior Surveillance System: We use individual level data on teens from the 2011-2022 State Youth Risk Behavior Survey (SYRBS), which are collected biennially by the CDC and state health departments, with the goal to gather information on health and health behaviors among U.S. high school students. The data are designed to be representative at the level of the state. The 2011-2022 SYRBS include 861,645 high school students, and we restrict the analysis sample to the 774,221 observations of students under the age of 18 with non-missing age and non-missing sex (the SYRBSS collects age categories where the oldest category corresponds to respondents 18 and older). We focus on four past 12-month mental health metrics: i) ever felt sad or hopeless almost every day for two weeks or more in a row to the point that stopped doing some usual activities, ii) considered suicide, iii) planned suicide, and iv) attempted suicide in the past 12 months.¹⁷ These measures, while not clinical metrics, likely capture more severe mental health conditions. We also include respondent age, sex, and race.

3.4 Methods

We use difference-in-differences methods to study the impact of state PSL mandates on child maltreatment reports. Recent research shows that standard two-way fixed effects (TWFE) regressions are vulnerable to bias associated with dynamic or heterogeneous treatment effects when treatment follows a staggered rollout, with different units adopting treatment at different points in times (Goodman-Bacon, 2021). PSL mandates display such a rollout (see Figure 1). Thus, we use a two-stage imputation procedure developed by Gardner (2022). This approach is robust to both of these sources of bias. Equations (1) and (2) outline our primary difference-in-differences application:

$$M_{s,t}(0) = \alpha_s + \gamma_t + \mathbf{X}'_{s,t}\boldsymbol{\beta} + \epsilon_{s,t}, \quad (1)$$

$$M_{s,t}(1) - \widehat{M}_{s,t}(0) = \delta PSL_{s,t-1} + \mu_{s,t}, \quad (2)$$

where $M_{s,t}(1)$ and $M_{s,t}(0)$ are measures of child maltreatment outcome in state s in year t in the treated (i.e., PSL mandate in place) and untreated (i.e., no PSL mandate in place) states. We estimate equation (1) using untreated observations, i.e., $PSL_{s,t-1} = 0$.

¹⁶The 4,593 account for 58% of all reporting years between 2011-2022.

¹⁷The SYRBS have limited information on physical health, thus we focus on mental health outcomes. Our use of self-reported general health in the ASEC complements these mental health measures.

$PSL_{s,t-1}$ is an indicator variable that is coded one if a state has a PSL mandate in place at any point in year $t - 1$ and coded zero otherwise. We impute $\widehat{M}_{s,t}(0)$ (our missing counterfactual) using untreated observations. We lag the PSL mandate variable by one year to allow employees to learn about and earn benefits, though we will show in Section 4.5 that using alternative lag structures does not change the findings.

Equation (1) includes state (α_s) and year (γ_t) fixed effects to account for pre-existing state differences and secular trends in the child maltreatment outcomes. We adjust for the following time-varying state characteristics ($\mathbf{X}_{s,t}$): paid medical and family leave policies (National Partnership for Women & Families, 2022), PTO mandates (National Partnership for Women & Families, 2023), Temporary Aid to Dependent Families monthly benefit for a family of four University of Kentucky Center for Poverty Research (2024),¹⁸ Affordable Care Act Medicaid expansion and Medicaid income eligibility thresholds for parents and children ages 6-18 years (Kaiser Family Foundation, 2025, ND), poverty rates (University of Kentucky Center for Poverty Research, 2024), and demographics which we construct from the basic monthly Current Population Survey (Flood et al., 2024). Data are weighted by the state population 0-18 years (Flood et al., 2024; University of Kentucky Center for Poverty Research, 2024)

In the first-stage of the Gardner (2022) procedure, relationships between the outcome (child maltreatment variables in our study) and the included covariates (fixed effects and time-varying covariates) are estimated using untreated observations only, that is both not-yet-treated observations (i.e., states that adopt/announce PSL mandates prior to adoption) and never-treated observations (i.e., states that do not adopt or announce a PSL mandate by November, 2024). Because no treated observations are used in this estimation, parameter estimates are not vulnerable to bias associated with a staggered policy rollout. The outcomes (i.e., child maltreatment report rates) are residualized using these estimated parameters. In the second stage – equation (2), the residualized outcomes are regressed on the treatment variable (i.e., state PSL mandates). The Gardner (2022) approach accounts for both the two-stage procedure and within-unit (here, state) clustering in estimation of standard errors.

We select the Gardner (2022) difference-in-differences estimator for our primary analysis given attractive features of this procedure – in addition to being robust to bias associated with treatment effect dynamics and heterogeneity as described above. First, this procedure is not vulnerable to bias in the estimated coefficient estimates when treatment effect heterogeneity is correlated with the time-varying covariates (e.g., Medicaid expansions) included in the regression (Caetano et al., 2022; Powell, 2021). Moreover,

¹⁸We inflate this variable to 2022 dollars using the Consumer Price Index-Urban Consumers.

in terms of inference, the [Gardner \(2022\)](#) preforms well relative to other difference-in-differences estimators, for example, the approach is less likely to over-reject the null hypothesis ([Gardner et al., 2024](#); [Mizushima and Powell, 2025](#)). Finally, the [Gardner \(2022\)](#) procedure is based on regression, a concept familiar to many micro-economists, and is efficient in terms of run-time. However, we will report results using alternative difference-in-differences methods in [Section 4.2](#) and findings are similar.

We make some modifications to equations (1) and (2) when we use alternative data sources (see [Section 3.3](#)). For example, we use the data at the respondent/job-level when we analyze the ASEC, ATUS, BRFSS, NCS, and SYRBS, and we use survey weights provided by the respective data administrators to weight the data.

3.5 Summary statistics and trends

[Figure 3](#) display unadjusted trends in in our three child maltreatment report rates. Trends for all three reports increase nearly monotonically between 2011 and 2019. There is a noticeable decline in 2020 – potentially attributable to the COVID-19 pandemic and temporary shutdown of many government services – and an increase in 2021 and 2022. [Figure 4](#) displays these trends for states that will and will not adopt a PSL mandate. These unadjusted trends suggest that total and unsubstantiated reports appear to depart at the same time that states adopt PSL mandates – in 2012 Connecticut adopts the first state PSL mandate and in 2015 both California and Massachusetts adopt mandates, but trends are more muted for substantiated reports.

[Table A2](#) reports summary statistics for states that adopt or announce a PSL mandate by November, 2024 (measured prior to mandate adoption) and states that do not adopt or announce such a mandate by November, 2024. Rates of total child maltreatment reports are modestly higher in states that do not adopt PSL mandates than in states that do. For example, the rate per 1,000 of total child maltreatment reports for children 0-18 years is 47.2 in states that do adopt/announce a mandate and 52.8 in states that do not. However, there are differences in terms of substantiated and unsubstantiated reports: rates of substantiated reports are generally higher in states that do adopt/announce a PSL mandate while rates of unsubstantiated reports are generally higher in states that do not adopt a PSL mandate. In terms of time-varying covariates that we include in our regressions, only states that do not adopt a PSL mandate have a PTO mandate in place and paid family and medical leave policies are only observed in states that will also adopt a PSL mandates. Other social insurance policies are more generous in states that do vs. do not adopt a PSL mandate, for example, 50% of states adopting a PSL

mandate have implemented an Affordable Care Act Medicaid expansion while only 31% of other states have implemented this expansion. Demographics are fairly similar across the two groups of states, though states that adopt/announce a PSL mandate have higher shares of Hispanic people than other states – 20% vs. 15%.

4 Results

4.1 First-stage evidence: State paid sick leave mandates increase paid sick leave use and access

In Table 1 we report evidence on first-stage effects, that is, to what extent do state PSL mandates lead to changes in PSL access and use among employees? We observe a 12.2 percentage point increase in the probability that the employer offers PSL to employees and employees use 2.3 additional hours per quarter or just over one day per year. Comparing these coefficients to the baseline mean (defined as the mean value in comparison states in the median year of state PSL adoption among treated states, 2018), these effects suggest a 16.9% and a 9.9% increase respectively.

These findings are similar to other studies using the NCS to study first order PSL mandate effects (Maclean et al., 2023; Dong et al., 2024; Maclean et al., 2025). We note that our effect sizes are somewhat smaller than those reported by Maclean et al. (2025). We suspect that the difference in effect sizes is due to our inclusion of government jobs in the NCS, while Maclean et al. (2025) focus exclusively on private jobs. Government workers are more likely to have PSL benefits pre-mandate and are therefore less impacted. Our research objective differs from that of Maclean et al. (2025), and we include government workers in this first-stage analysis because we are interested in spillovers to child maltreatment reports, regardless of the parent’s class of job.¹⁹ With evidence on the first-stage in hand, we turn to our primary research objective, that is estimating the effects of state PSL mandates on child maltreatment reports.

4.2 Paid sick leave mandates reduce child maltreatment reports

Table 2 reports our main results. Panel A includes total reports, while panels B and C present findings for substantiated reports and unsubstantiated reports respectively. Columns, moving left to right, report findings for children 0-18, 0-5, and 6-18 years. Overall, our findings suggest that child maltreatment reports decline following adoption

¹⁹Further, we do not have information on parental class of job in the NCANDS data.

of a state PSL mandate, and this pattern is observed for all report types and age groups that we consider. In particular, post-PSL mandate, total reports decline by 6.6, 7.0, and 6.5 per 1,000 children 0-18, 0-5, and 6-18 years. Comparing these coefficient estimates to the baseline, our findings suggest a 9.7% to 12.7% decline. We observe similarly sized effects for both substantiated and unsubstantiated maltreatment reports. Table A3 replicates Table 2, but excludes time-varying covariates, and the findings are largely unchanged, though the magnitudes of the coefficient estimates are somewhat larger.

The key assumption of our difference-in-differences approach is ‘parallel trends.’ That is, we must assume that the pattern of untreated outcomes would have been the same post-PSL mandate for both treated states and untreated states. However, we do not observe treated states post-mandate in the untreated state, thus this assumption is untestable. To shed some light on the ability of our data to satisfy the parallel trends assumption, we estimate an event-study. More specifically, we decompose our static difference-in-differences variable into a series of indicators for time-to-event interacted with being a state that adopts/announces a PSL mandate by November, 2024. We include five leads, the period of the event (i.e., state PSL mandate effective year), and five lags. We use the first partially treated year as the effective year and we code states that adopt their PSL mandate after 2022 (the last year of our study period) as in their pre-treatment period (e.g., Minnesota adopts a state PSL mandate in January, 2024 and we code that state as -2 in 2022). We trim the data in event-time. Figure 5 reports event-studies for total, substantiated, and unsubstantiated reports among children 0-18 years. We observe no evidence of differential trends between states that will and will not adopt/announce a PSL mandate – coefficient estimates on the policy leads are all small in size and statistically indistinguishable from zero. Following mandate adoption, maltreatment reports of all three types decline, though effects are not observable immediately and instead appear over time. The delayed effects are reasonable as the year of the event is only partially treated for states where the mandate becomes effective mid-year and employees must work to earn their benefits (see Section 2.1).

In Figure 6 we report event-studies using alternative specifications and samples. For brevity, we report results for total reports only, we also report our main specification and sample for comparison. We i) exclude time-varying covariates, ii) drop states that adopt/announce a PSL mandate after 2022, iii) code states that adopt/announce a PSL mandate after 2022 as zero for all time-to-event indicators (i.e., treat these states as comparison states), iv) use NCANDS data 2004-2002 (including DC), and v) do not trim the data in event-time. Overall, the pattern of results does not change across these alternative specifications and samples, which supports the validity of our design.

In Figure 7 we regress each time-varying covariate included in equation 1 on the state PSL mandate (lagged one year), state fixed effects, and region-by-year fixed effects. This analysis evaluates the extent to which included covariates are (conditionally) balanced across adopting and non-adopting states. We have reasonable balance for most outcomes, although there is some imbalance in terms of monthly Temporary Aid to Needy Families and Medicaid income thresholds for parents and children ages 0-18 years. However, results are robust to excluding these controls (see Table A3), which suggests that any imbalance does not lead to substantial bias.

We rely on the Gardner (2022) approach to study the effect of state PSL mandates on child maltreatment reports. We next report results (for total reports) using alternative difference-in-differences approaches utilized within the economic literature (Table A4). In particular, we report results using methods proposed by Callaway and Sant’Anna (2021), Borusyak et al. (2024), Wooldridge (2023), and de Chaisemartin and d’Haultfoeuille (2020). We also report results estimated using a stacked difference-in-differences estimator (Cengiz et al., 2019) and two-way fixed effects. Details on the estimators are located in the table notes. Overall, while not identical, the pattern of results is highly consistent across the different approaches, which suggests that findings are not driven by our use of the Gardner (2022) approach. We note that results from two-way fixed effects are smaller and imprecise. In Table A5, we report results from a Goodman-Bacon (2021) decomposition. Two patterns emerge. First, the coefficients using different two-by-two contrasts are similar. Second, only 3.6% of comparisons that contribute to the overall estimate of the average treatment on the treated (ATT) are ‘forbidden,’ in that later treated units are contrasted with earlier treated units.

We next explore heterogeneity in state PSL mandate effects across child demographics (Figure A2), reporting sources (Figure A3) – where the reporting source is the person who makes the allegation of child maltreatment to Child Protective Services, and type of maltreatment (Figure A4). We also consider services received by families (Figure A5), service variables are missing for many observations, thus we include only state-year pairs with no more than 25% of the information missing.²⁰ We present total reports for children 0-18 years for brevity. In terms of child demographics, our overall findings appear to be driven by Black and Hispanic children. We observe similar declines in child maltreatment reports among boys and girls post-mandate. Declines in maltreatment reports initiated by mental health providers and parents are particularly steep post-PSL mandate. This pattern of results suggest that declines attributable to both professional and non-professional sources (Evans et al., 2022). Figure A4 reports substantiated reports by

²⁰Missingness varies across outcome, thus sample sizes vary across the service variables.

type of maltreatment, while coefficient estimates are negative across the types, only the coefficient in the sexual abuse regression rises to the level of statistical significance. In Figure A5, coefficient estimates are generally native across the service types, but findings are often imprecise, likely due to missingness in these variables (see Section 3.1).

4.3 Mechanisms

State PSL mandates do not likely directly affect child neglect, rather these mandates potentially influence parental outcomes and behaviors, family economic standing, and child health. In this section, we explore several possible mechanisms.

A first potential mechanism is parental health, in particular mental health. Previous research shows that parental mental health increase the risk of child maltreatment (Ali et al., 2024) and studies suggest that health outcomes improve post-PSL mandate adoption (see Section 2.1). More specifically, healthy parents are likely better able to execute their parenting roles. We test whether state PSL mandates impact health outcomes among parents in the 2011-2022 BRFSS (Table 3). We find that PSL mandate adoption decreases the probability of reporting having fair or poor health by 1.5 ppts, a 10% decrease relative to the baseline. This change is accompanied by 0.34 fewer days of bad physical health in the past month, a 11% decrease relative to the baseline. In terms of mental health outcomes, we find that a PSL mandate leads to 0.325 fewer days of bad mental health in the past 30 days, which is a 7.9% decrease relative to the baseline. Taken together, our findings document that mandated PSL improves parents' health, both mental and physical, and this improved health ultimately translates into a 7.6% fewer days experiencing health impediments.

PSL mandates may allow parents to better balance family and work responsibilities, indeed several studies show that child care-giving increases following a PSL mandate adoption (Maclean and Pablonia, 2024). Parents who are able to take time off work for child illnesses, child healthcare, school closures, and so forth may be less likely to be reported to Child Protective Services for child maltreatment. We next examine the extent to which state PSL mandates impact three measures of time allocated to primary childcare for household children, non-household children, and all children (i.e., the sum of household and non-household children). Results reported in Table 4 suggest that time spent in primary childcare increases by 8.7 minutes per day or 11.3% relative to the baseline. Findings for secondary childcare are imprecise.

Several studies suggest that – counter to predictions from a basic Summers (1989) model – wages and employment outcomes may increase post-PSL mandate (see Section

2.1). Such changes could increase available economic resources in the household, which could reduce child maltreatment reports. To explore changing economic resources, we examine the impact of state PSL mandate adoption on economic outcomes among adults 22-59 with minor children in the household (Table 5). Our findings suggest, similar to earlier work (Boots et al., 2009; Slopen, 2024; Maclean et al., 2025), that employment outcomes increase post-mandate. In particular, we find that past-year any work, weeks worked, usual hours worked, full-time employment, and earnings from wages and salaries by 1.4%, 1.4%, 1.0%, 1.7%, and 14.7%²¹ relative to the baseline mean. We also observe that the probability of family income below the Federal poverty level declines by 7.8%. In Table 6 we report results of the impact of state PSL mandates on marital status – a proxy for family structure. We observe no change in any of the outcomes we consider following adoption of a state PSL mandate.

Using commercial health insurance claims data (Table 7), we find that ED episodes decline by 11.6% among children following adoption of a state PSL mandate, though coefficient estimates are only statistically different from zero for children 6-18 years. Similarly, in Table A6, we report comparable results for adults 22-59 years of age and adults 22-59 years of age with a child listed as a dependent on the plan, the latter allows us to proxy parents. Here, we observe an approximately 11% decline in ED visits post-mandate. We use ED episodes as a summary measure for access to adequate healthcare and mitigated domestic violence within the household that state PSL mandates may confer to both adults and children.

In the SYRBS, we explore the impact of state PSL mandates on teen mental health (Table 8). We observe no change in any of the outcomes we consider, suggesting that changes in measures of very poor teen mental health – at least using the metrics we consider – is not an empirically important channel linking PSL mandates to reductions in child maltreatment reports. Of note, the SYRBS includes high school students and thus we are not able to explore this channel using younger children or children who may have dropped out of school. In the ASEC, we examine the effect of state PSL mandates on children’s reported health (Table 9). Here, we see that the probability that a child has very good or excellent health increases by 1.8 ppts, or 2.2% relative to the baseline mean. Collectively, the SYRBS and ASEC results suggest that state PSL mandates improve moderate mental health outcomes, but perhaps not more severe measures.

In summary, our analysis of potential mechanisms linking state PSL mandate adoption to reported child maltreatment suggests that improvements in parental and child health – though not severe measures of mental health problems among children; and fam-

²¹We convert the coefficient to the percent changes as follows: $e^{\beta} - 1$

ily economic standing; increased childcare; and greater use of ambulatory and preventive healthcare are important. Alternatively, changes in family structure do not appear to link state PSL mandate adoption with reports of child maltreatment.

4.4 Extension to domestic violence

Given that state PSL mandates significantly lower child maltreatment, we next examine their impact on a possibly related outcome – intimate partner violence. The columns in Table 10 report the estimated effects on assaults, sexual offenses, and homicides. We restrict incidents to those where a partner or spouse perpetrated the victimization. The first column shows that post-PSL mandate, there are 35 fewer incidents of intimate partner violence or about a 8.5% reduction. This decrease is driven by a decline in assaults and sex offenses. The average police agency reports about 31.6 fewer intimate partner assaults (per 100,000), an 8% decrease relative to the baseline. In column (3), we see that there is about a 33% decline in sexual violence from partners or spouses. The large decline is likely attributable to low baseline of these incidents as scholars note that these metrics are under-reported (Koss et al., 1987; Tjaden and Thoennes, 2006; Stricot, 2021). Our coefficient estimates imply a 2.8 per 100,000 reduction in sexual assault crimes. In column (4), although the coefficient estimate is negative, the effects of PSL mandates on intimate partner homicides is not statistically significant.²²

4.5 Robustness checks

In this section, we report results of our primary finding – state PSL mandate adoption reduces child maltreatment reports – from a series of robustness analyses that use different samples and alternative specifications. The purpose of this exercise is to ensure that our findings are not driven by a specific sample or specification. For brevity, we focus on total maltreatment reports for children ages 0-18 years in robustness checking.

First, we evaluate whether the effects are robust to the chosen sample (Figure A6). We estimate the main regression using the following alternative subsamples: i) include 2023 (which likely includes some incomplete reports) and ii) use 2004-2022. Second, we vary our approach to weighting the data: i) estimate unweighted regression, ii) use the 2011 population as the weight, and iii) utilize the average population 2011-2022 as the weight. Third, we operationalize the state PSL mandate variable in different ways: i) use the current mandate; ii) lag the mandate two years; iii) include an indicator for

²²The results are similar if we use the Federal Bureau of Investigation’s Supplemental Homicide Reports. These results are available upon request from the corresponding author.

sub-state mandates in localities with populations great than 1,000,000 based on the 2010 Census ([National Partnership for Women & Families, 2023](#); [U.S. Census Bureau, 2022](#)); iv) include an indicator for bordering a state with a PSL mandate in place (lagged one year); v) expand the definition of PSL mandate to include PSL or PTO mandates (i.e., the indicator variable tables on a value of one if either of these mandates is in place); and vi) exclude states that adopt PSL mandates during the pandemic and post-pandemic period (i.e, 2020-2022). Fourth, we change the included covariates: i) replace region-by-year fixed effects with year fixed effects and ii) include an extended set of time-varying covariates – unemployment rates, poverty rates, political party of the state Governor, effective minimum wage (inflated to 2022 dollars using the Consumer Price Index-Urban Consumers), and the ratio of the state-to-federal Earned Income Tax ratio ([University of Kentucky Center for Poverty Research, 2024](#)). Results are stable across the alternative specifications. We lose some precision when we do not weight the data, though the coefficient estimate is largely unchanged suggesting that weighting gives us an efficiency gain as we upweight more populous states and downweight less populous states. Fifth, we report results using different functional forms (Table A7). We use the report count (controlling for the state population 0-18 years) and we take the logarithm of the report rate. Finally, we turn to unsubstantiated reports, as noted in Section 3.1, these reports include both unsubstantiated reports and alternative response reports. In Table A4, we separate out the two types of reports, and we observe that our findings are driven by alternative responses, though coefficient estimates are negative in all specifications.

Next, we turn to evaluating the extent to which our findings are driven by any particular state or year. In Figures A7 and A8 we report results in which we sequentially exclude each state that adopts or announces a PSL mandate by November, 2024 and each year included in our study period (2011-2022). Findings are robust across these various ‘leave-one-out’ samples, though we do lose some precision when we exclude California, this state is large in terms of population – California is the largest state ([University of Kentucky Center for Poverty Research, 2024](#)) – and has generous PSL mandate ([National Partnership for Women & Families, 2022](#)), which may explain the drop in precision, though we are re-assured that the effect size remains stable to excluding this state.

5 Discussion and conclusion

Child maltreatment is documented to affect long-term health, labor market, and economic outcomes of child victims, and the estimated cost of nonfatal child maltreatment

per-victim is \$836,494 (Peterson et al., 2018).²³ Given the large social cost of child maltreatment, research on determinants of child maltreatment is crucial for optimal policy design to reduce this burden. We estimate the effect of state PSL mandates on child maltreatment over the period 2011 to 2022. By guaranteeing seven days of PSL (on average) for personal or family health needs, state PSL mandates enable parents to take financially protected time off from work to tend to their own health needs as well as their family responsibilities. This financially protected time among parents could potentially affect several determinants of child maltreatment, such as improved mental and physical parental health, financial stability, and time constraints faced by caregivers. Particularly relevant for child maltreatment, state PSL mandates include ‘safe time’ as an eligible activity, where safe time can be used to attend court hearings related to domestic violence situations and moving, which may allow parents to leave unsafe home environments and, in turn, reduce child maltreatment reports.

Our study finds that state PSL mandates confer meaningful protective benefits for children. Our staggered difference-in-differences estimates indicate that state-paid sick leave mandates lead to an 11.2% reduction in child maltreatment reports. This decrease is comparable for younger (0-5 years) and older (6-18) years. This effect is also similar for reports in which a Child Protective Services caseworker determines that maltreatment has occurred (‘substantiated’) and for reports where no evidence of maltreatment is determined (‘unsubstantiated’). We consider possible mechanisms and find improvements in parental economic well-being, health, and caregiving capacity, moreover, children are less likely to require emergency care, suggesting that mandated PSL is used for children’s healthcare needs. In addition to fewer child maltreatment reports, state PSL mandates may also lead to declines in other forms of family violence – specifically, assaults and sexual offenses perpetrated by victims’ partners or spouses. In sum, our findings suggest that state PSL mandates can correct the negative externality imposed by maltreatment of children, and potentially that imposed by family violence more broadly defined.

To put in context the cost-effectiveness of state PSL mandates, we quantify the effect of these policies on child maltreatment with the following back-of-the-envelope calculation. The passage of a state PSL mandate prevents 6.6 reports of child maltreatment to Child Protective Services per 1,000 children (see Table 2). Given that, on average, a state has 1,581,119 individuals between the age of 0 and 18 years, this finding suggests that mandated PSL prevents 10,435 ($= [6.6 \times 1,581,119]/1000$) incidents of child maltreatment in any given state-year. Using the estimated cost of nonfatal child maltreatment

²³We inflate the original estimate – \$830,928 per victim in 2015 – to 2025 dollars using the Consumer Price Index-Urban Consumers.

per-victim lifetime is \$836,494 in 2025 dollars (Peterson et al., 2018), this increase in PSL translates into a \$8,728,814,890 ($=10,435 \times \$836,494$) in any given state-year. These benefits would be an additional positive spillover effect from PSL in addition to the health and labor market benefits such as those described in Section 2.1.

We can compare these benefits to the cost of PSL mandates. Earlier work shows that the costs of PSL mandates to employers is 6.2 cents per hour worked (Maclean et al., 2025).²⁴ To put this number in context, Maclean et al. (2025) estimate that people work on average 1,702 hours per year, which would result in a cost of \$105.5 per worker per year. Our coefficient estimates indicate that the implementation of PSL increases the probability that any given job has access to PSL by 12.2 ppts, which is equivalent to a 16.9% increase in access to PSL. An average state has 2,880,979 workers, and 72.1% of them have PSL prior to the implementation of PSL. That is, there are approximately 2,078,338 workers with PSL coverage prior to the implementation of PSL. If there is an increase of 16.9% of PSL access, this leads to 351,446 ($= 2,078,338 \times 0.1691$) additional people with PSL. Given that each of these individuals works on average 1,702 hours per year, we would expect a cost increase of \$35,889,665 ($= 351,446 \times 1,702 \times 0.06$). Even without taking into account the additional benefits outside child maltreatment, this back-of-the-envelope calculation indicates that the benefits outweigh the costs.

Finally, we contextualize the magnitude of our findings relative to the estimated effect of other similar policies on child maltreatment. Our study indicates that PSL decreases child maltreatment by 11.2%. A similar policy to PSL is publicly-funded paid family leave (PFL), which allows workers to take financially protected time from work in order to care for new children and long-term health and care giving responsibilities. PFL reduces infant maltreatment by 14% (Bullinger et al., 2025). A potential reason why PFL has larger effects than PSL is that PSL could be used to care for children of any age, while PFL is particularly targeted at infants as the benefits can be used for bonding with a new child (National Partnership for Women & Families, 2022), who may need much higher level of care and supervision, and therefore their exposure to maltreatment and neglect is more responsive to parental ability to take time off from work. Financial compensation through wage replacement is among the components of PSL that decreases child maltreatment; therefore, we compare our findings with other policies that provide financial support to lower income households, such as Temporary Aid to Needy Families, Earned Income Tax Credit, and expanded Child Tax Credit. Our estimated 11.2% decrease in child maltreatment is comparable to the effect of of a 20

²⁴We do not conduct a marginal value of public funds analysis as we study employer mandates. Thus, the costs of these mandates are largely borne by employers and not government.

ppts increase in a refundable state EITC program ([Kovski et al., 2022](#)).²⁵

Our findings contribute to the limited literature on the positive spillovers of PSL, in this case, on child maltreatment. This study suggests that, in addition to providing financial security by reducing fear of losing one’s job due to health shocks, state PSL mandates also had positive spillovers on a socially valuable outcome, namely child maltreatment. Future research could examine spillovers on other demographic outcomes related to household stability, household formation, and the relevance of government policies that relax time constraints to address own health issues and family obligations in improving children outcomes.

²⁵[Kovski et al. \(2022\)](#), find that a ten ppt increase in a refundable state Earned Income Tax Credit benefit leads to a 5% decline in rates of reported child maltreatment.

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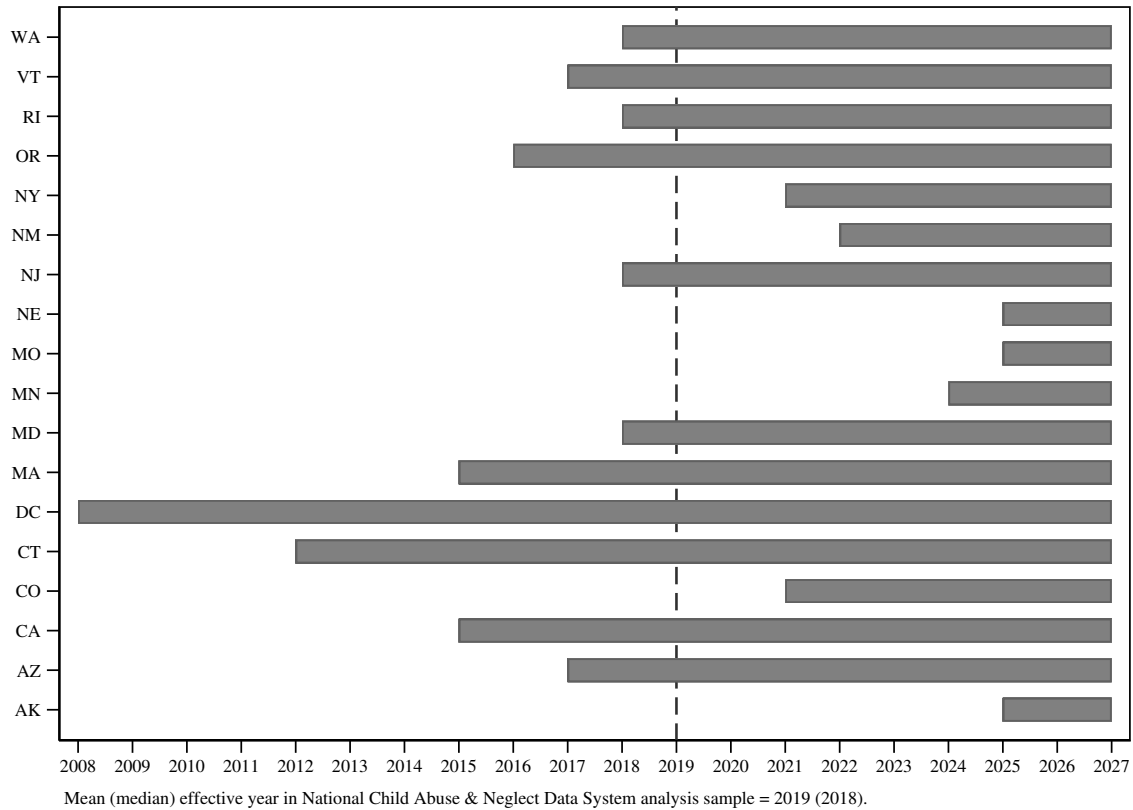
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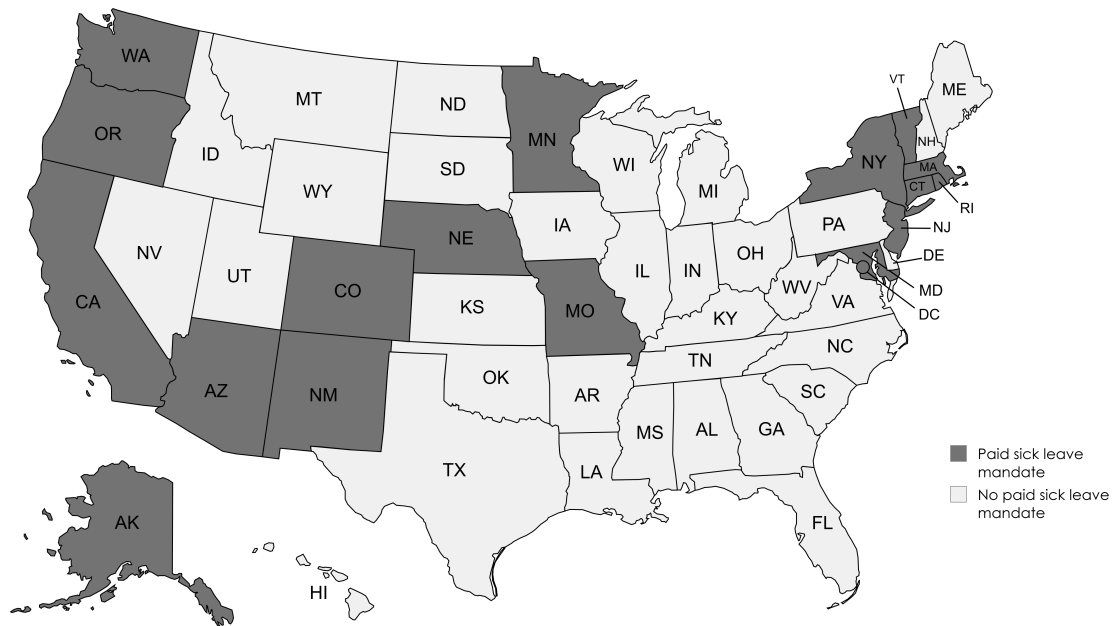
6 Figures and tables

Figure 1: Temporal distribution of state paid sick leave mandates adopted or announced by November, 2024



Notes: The data sources are [National Partnership for Women & Families \(2023\)](#) and [A Better Balance \(2024\)](#).

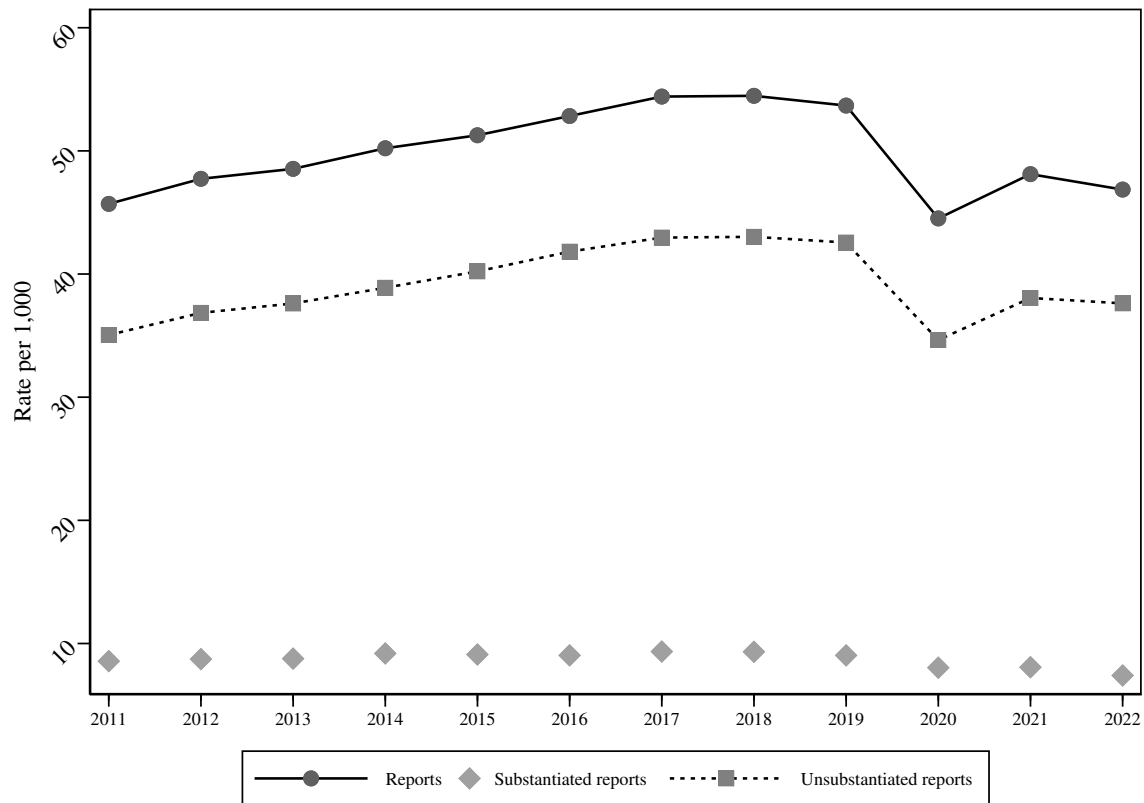
Figure 2: Geographic distribution of state paid sick leave mandates adopted or announced by November, 2024



Created with mapchart.net

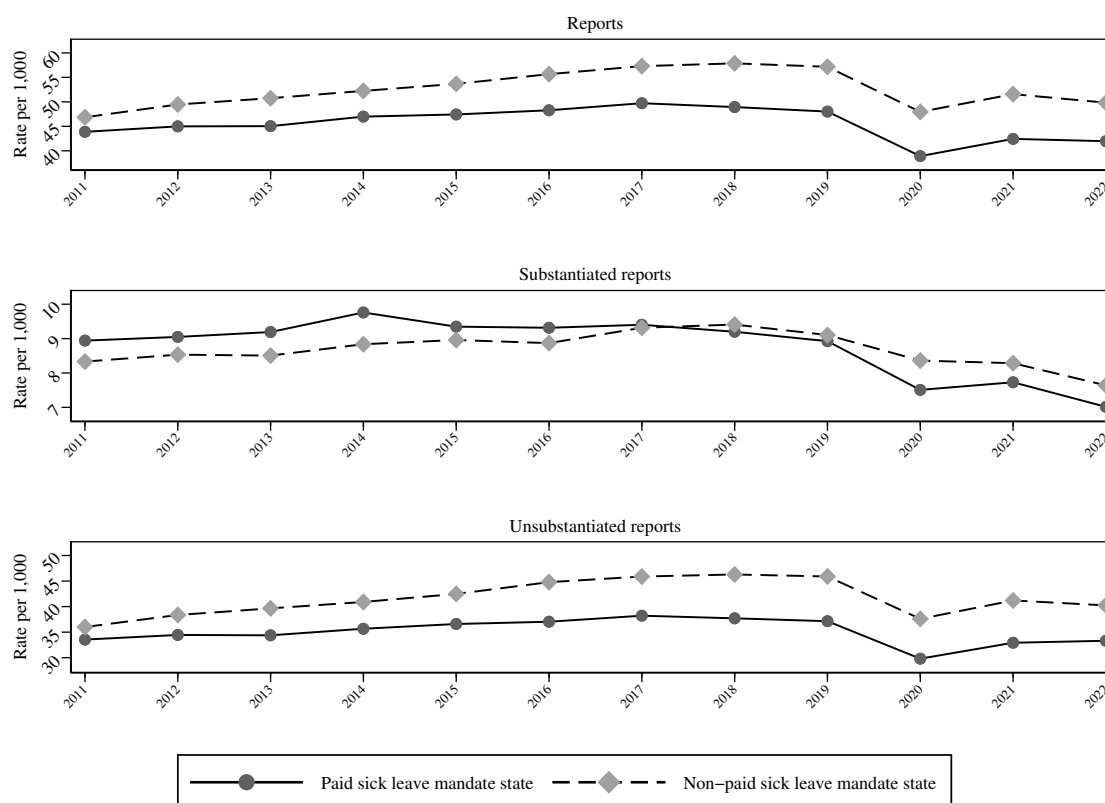
Notes: The data sources are [National Partnership for Women & Families \(2023\)](#) and [A Better Balance \(2024\)](#). Figure created by the authors using MapChart.

Figure 3: Trends in child maltreatment reports among children 0-18 years per 1,000: National Child Abuse & Neglect Data System 2011-2022



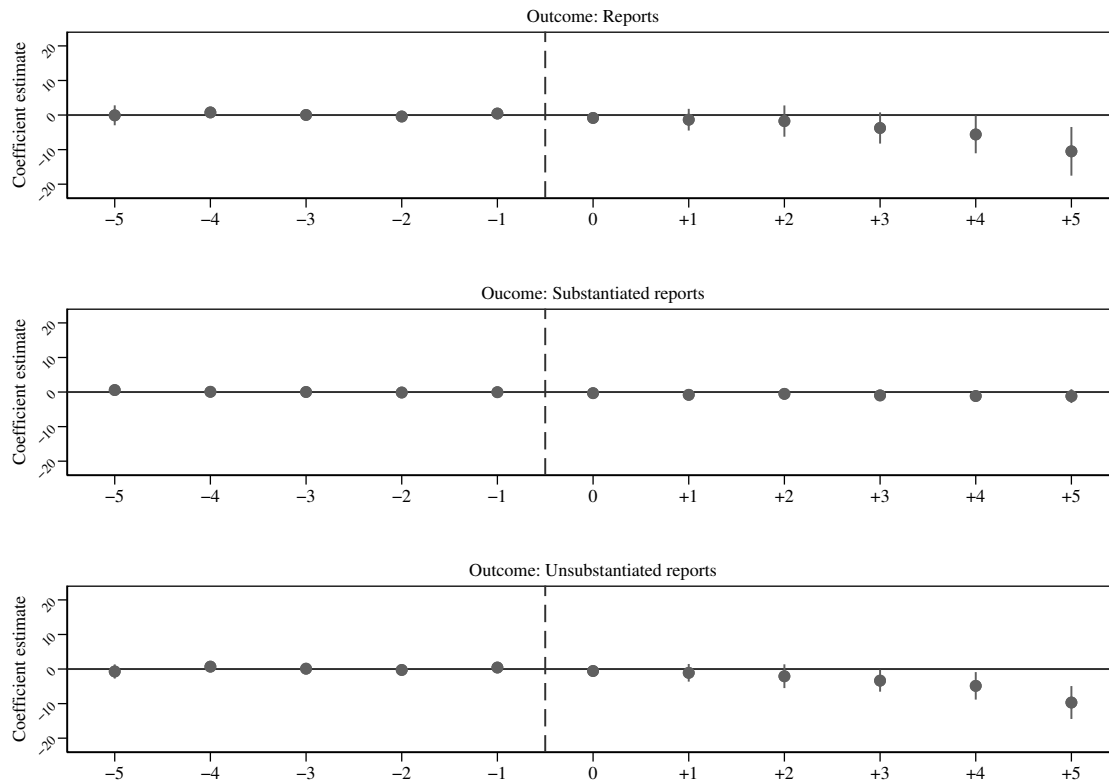
Notes: The figure reports trends in outcomes over time. The outcomes are total, substantiated, and unsubstantiated maltreatment reports for children 0-18 years per 1,000 state residents 0-18 years. The data source is the National Child Abuse & Neglect Data System 2011-2022. The unit of observation is year. Data are weighted by the state population 0-18 years prior to aggregating to the year-level.

Figure 4: Trends in child maltreatment reports among children 0-18 years per 1,000 in states that do and do not adopt a paid sick leave mandate: National Child Abuse & Neglect Data System 2011-2022



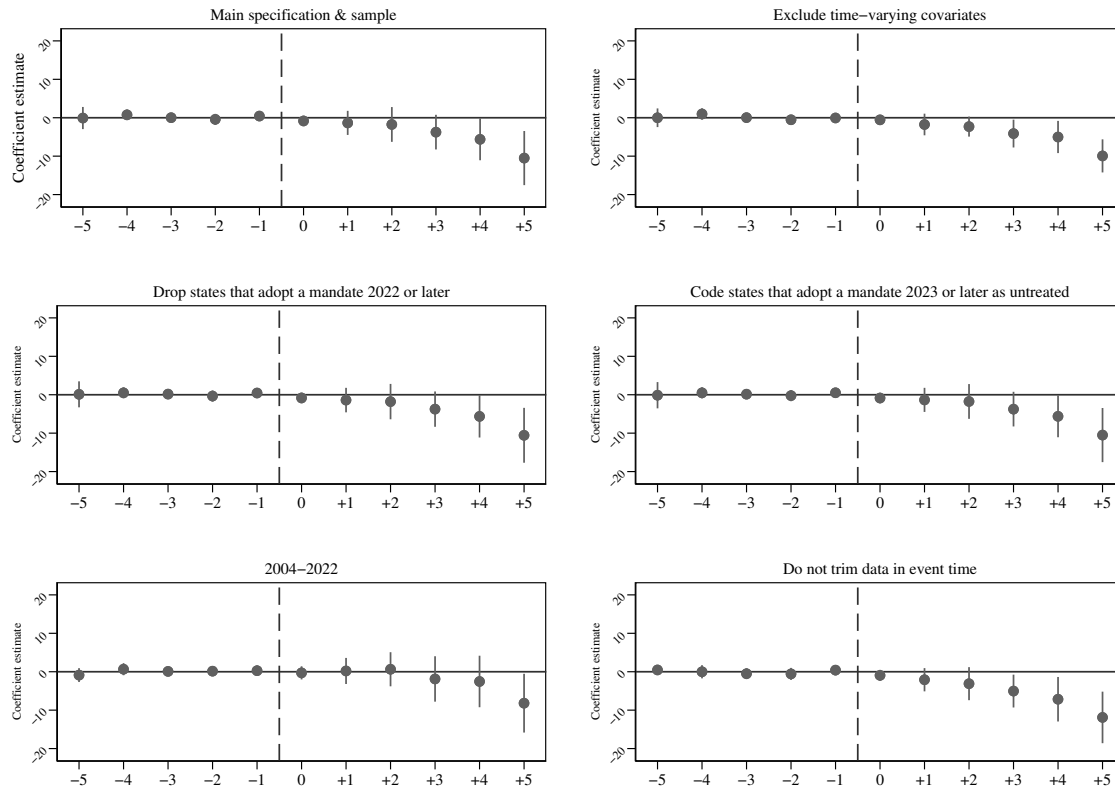
Notes: The figure reports trends in outcomes over time by states that do and do not adopt or announce a paid sick leave mandate by November, 2024. The data source is the National Child Abuse & Neglect Data System 2011-2022. The outcomes are total, substantiated, and unsubstantiated maltreatment reports for children 0-18 years per 1,000 state residents 0-18 years. The unit of observation is year-treatment group where treatment = adopt a state paid sick leave and comparison group = do not adopt a state paid sick leave mandate. Data are weighted by the state population 0-18 years prior to aggregating to the treatment-year-level.

Figure 5: Effect of a state paid sick leave mandate on child maltreatment reports among children 0-18 years per 1,000 using an event-study: National Child Abuse & Neglect Data System 2011-2022



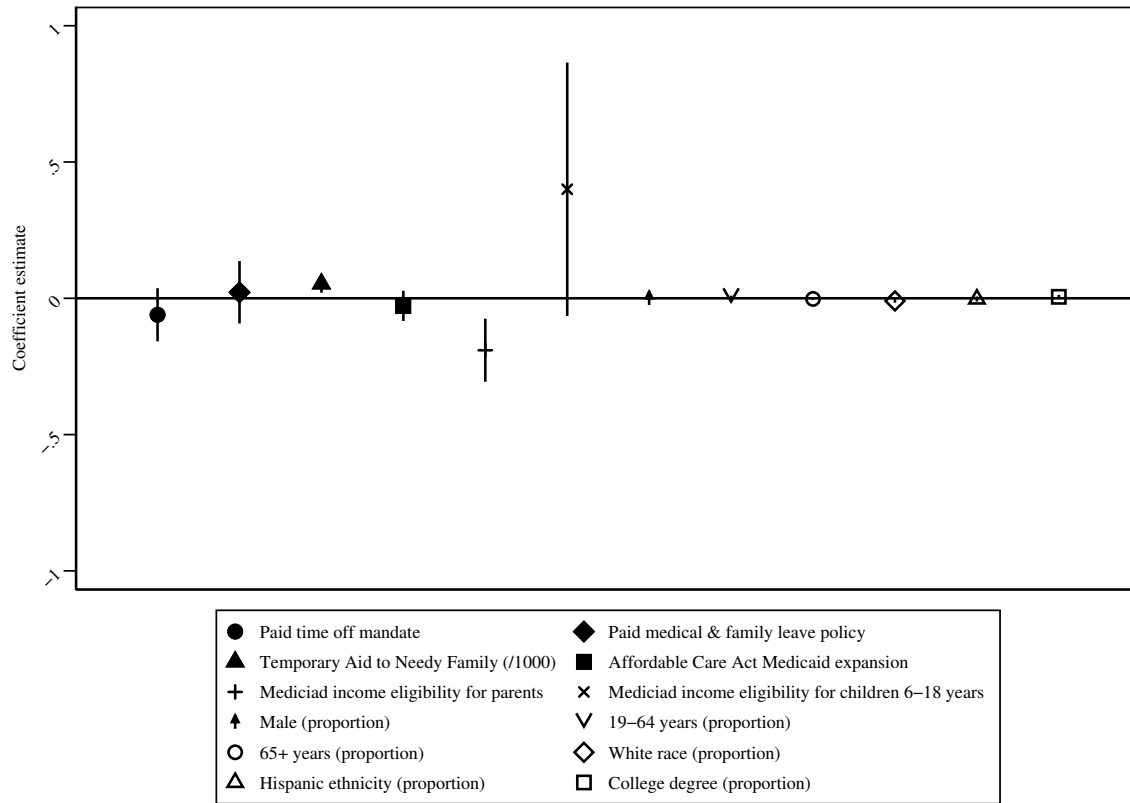
Notes: This figure reports the results of a regression of total reports, substantiated reports, and unsubstantiated reports on lead and lag variables relative to state paid sick leave mandate adoption and controls. The data source is the National Child Abuse & Neglect Data System 2011-2022. We use [Gardner \(2022\)](#) for estimation. The unit of observation is a state in a year. Regressions control for state-level characteristics, state fixed effects, and region-by-year fixed effects. Data are weighted by the state population 0-18 years. Data are trimmed in event-time for paid sick leave adopting states: paid sick leave mandate adopting state periods more than five years before and after the mandate are excluded. There is no trimming for states that do not adopt a paid sick leave mandate. The [Gardner \(2022\)](#) procedure does not require normalizing a specific estimate to zero (all coefficient estimates are implicitly normalized to the pre-period average). Shapes represent coefficient estimates. 95% confidence intervals that account for within-state clustering are reported with vertical lines.

Figure 6: Effect of a state paid sick leave mandate on child maltreatment reports among children 0-18 years per 1,000 using an event-study with alternative specifications & samples: National Child Abuse & Neglect Data System 2011-2022



Notes: This figure reports the results of a regression of total reports on lead and lag variables relative to state paid sick leave mandate adoption and controls. The data source is the National Child Abuse & Neglect Data System 2011-2022. We use [Gardner \(2022\)](#) for estimation. The unit of observation is a state in a year. Regressions control for state-level characteristics, state fixed effects, and region-by-year fixed effects unless noted. Data are weighted by the state population 0-18 years. Data are trimmed in event-time for paid sick leave adopting states (unless otherwise noted): paid sick leave mandate adopting state periods more than five years before and after the mandate are excluded. There is no trimming for states that do not adopt a paid sick leave mandate. The [Gardner \(2022\)](#) procedure does not require normalizing a specific estimate to zero (all coefficient estimates are implicitly normalized to the pre-period average). Shapes represent coefficient estimates. 95% confidence intervals that account for within-state clustering are reported with vertical lines.

Figure 7: Covariate balance across states that do and do not adopt a paid sick leave mandate



Notes: This figure reports the results of a regression of time-varying covariates on state and year fixed effects. The outcome variable is listed in the legend. See Section 3 for data sources. We use [Gardner \(2022\)](#) for estimation. The unit of observation is a state in a year. Regressions control for state fixed effects and region-by-year fixed effects unless noted. Data are weighted by the state population 0-18 years. Shapes represent coefficient estimates. 95% confidence intervals that account for within-state clustering are reported with vertical lines.

Table 1: Effect of a state paid sick leave mandate on access to and use of paid sick leave using difference-in-differences: National Compensation Survey 2009-2022

Outcome →	Access	Use
State paid sick leave mandate	0.122*** (0.028)	2.268*** (0.656)
Baseline mean†	0.7214	23.1269
Percent change††	16.91	9.81
Observations	691388	691388

Notes: This table reports the results of a regression of an indicator for paid sick leave access and quarterly hours of paid sick leave use (hours per quarter) on a state paid sick leave mandate (lagged one year) and controls. The data source is the National Compensation Survey 2009-2022. We use [Gardner \(2022\)](#) for estimation. The unit of observation is a job in an establishment in state in a year. Regressions control for state characteristics, state fixed effects, and year fixed effects. We note that we do not include region-by-year fixed effects in this analysis, see footnote 12 for details. Data are weighted by National Compensation Survey-provided weights. Standard errors account for within-state clustering and are reported in parentheses.

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

†Baseline mean = mean value in comparison states in 2018.

††Percent change = coefficient estimate / baseline mean $\times$ 100%.

Table 2: Effect of a state paid sick leave mandate on child maltreatment reports among children 0-18 years per 1,000 using difference-in-differences: National Child Abuse & Neglect Data System

Sample →	0-18 years	0-5 years	6-18 years
Panel A: Total reports			
State paid sick leave mandate	-6.643*** (2.516)	-7.013** (3.354)	-6.474*** (2.385)
Baseline mean†	57.871	72.094	51.141
Present change††	-11.479	-9.728	-12.659
Panel B: Substantiated reports			
State paid sick leave mandate	-1.160** (0.588)	-1.957** (0.972)	-0.821 (0.514)
Baseline mean†	9.409	14.813	6.985
Present change††	-12.329	-13.211	-11.754
Panel C: Unsubstantiated reports			
State paid sick leave mandate	-6.071*** (1.760)	-5.642** (2.385)	-6.213*** (1.649)
Baseline mean†	46.291	54.383	42.331
Present change††	-13.115	-10.375	-14.677
Observations	600	600	600

Notes: This table reports the results of a regression of total reports, substantiated reports, and unsubstantiated reports on a state paid sick leave mandate (lagged one year) and controls. The data source is the National Child Abuse & Neglect Data System 2011-2022. We use [Gardner \(2022\)](#) for estimation. The unit of observation is a state in a year. Regressions control for state characteristics, state fixed effects, and region-by-year fixed effects. Data are weighted by the state population 0-18 years. Standard errors account for within-state clustering and are reported in parentheses.

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

†Baseline mean = mean value in comparison states in 2018.

††Percent change = coefficient estimate / baseline mean × 100%.

Table 3: Effect of a state paid sick leave mandate on self-reported health status among adults 22-59 years of age with minor children in the household: Behavioral Risk Factor Surveillance Survey

Outcome ↓	Coefficient estimate (Standard error)
Excellent or very good health	-0.002 (0.005)
Baseline mean†	0.540
Present change††	-0.370
Observations	1110381
Fair or poor health	-0.015*** (0.005)
Baseline mean†	0.139
Present change††	-10.791
Observations	1110381
Days bad physical health	-0.337*** (0.123)
Baseline mean†	3.054
Present change††	-11.035
Observations	1099194
Days bad mental health	-0.325** (0.162)
Baseline mean†	4.090
Present change††	-7.946
Observations	1099048
Days health impediments	-0.308*** (0.116)
Baseline mean†	4.030
Present change††	-7.643
Observations	589357

Notes: This table reports coefficient estimates from a regression of indicators for whether the respondent’s general health is excellent/very good or fair/poor (out of one = excellent to five = poor), as well as the number of days in the last month with bad physical health, bad mental health, and the number of days where physical or mental health affected self-care, work, or recreation on state paid sick leave mandates. The data source is the Behavioral Risk Factor Surveillance Survey 2011-2022. The unit of observation is a respondent in a state, month, and year. We use [Gardner \(2022\)](#) for estimation. The regression includes state characteristics, state fixed effects, and region-by-year fixed effects. We also include month-of-interview, sex, and age fixed effects. The paid sick leave mandate is lagged one year. Data are weighted by the Behavioral Risk Factor Surveillance Survey-provided weights. Standard errors that account for within-state clustering are in parentheses.

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

†Baseline mean = mean value in comparison states in 2018.

††Percent change = coefficient estimate / baseline mean $\times$ 100%.

Table 4: Effect of a state paid sick leave mandate on childcare (average minutes per day) among adults 22-59 years of age with minor children in the household: American Time Use Survey

Type of childcare →	Primary	Secondary
State paid sick leave mandate	8.654** (4.090)	-15.141 (10.870)
Baseline mean†	76.878	326.403
Present change††	11.257	-4.639
Observations†††	41305	34277

Notes: This table reports the results of a regression of total reports on a state paid sick leave mandate (lagged one year) and controls. The data source is the American Time Use Survey 2011-2022. The unit of observation is a respondent in a state in a month-year. We use [Gardner \(2022\)](#) for estimation. The regression includes respondent characteristics, state characteristics, state fixed effects and region-by-year-month fixed effects. The paid sick leave mandate is lagged one year. Data are weighted by American Time Use Survey-provided sample weights. Standard errors that account for within-state clustering are reported in parentheses.

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

†Baseline mean = mean value in comparison states in 2018.

††Percent change = coefficient estimate / baseline mean $\times$ 100%.

†††Secondary childcare is only available for respondents living in households with a child under the age of 13 years, thus the sample size for this outcome is reduced.

Table 5: Effect of a state paid sick leave mandate on economic outcomes: Annual Social and Economic Supplement to the Current Population Survey 2011-2022

Outcome ↓	Coefficient estimate (Standard error)
Any work in past year	0.012*** (0.004)
Baseline mean†	0.831
Present change††	1.444
Observations	520538
Weeks worked in past year	0.565** (0.251)
Baseline mean†	40.623
Present change††	1.391
Observations	520538
Usual hours per week in past year	0.403*** (0.125)
Baseline mean†	40.806
Present change††	0.988
Observations	431109
Full-time work in past year	0.015*** (0.004)
Baseline mean†	0.874
Present change††	1.716
Observations	431109
Wage & salary earnings (logarithm) in past year	0.137*** (0.045)
Baseline mean (unlogged)†	\$56112.360
Present change ($= e^{\beta} - 1$)	14.683
Observations	520538
Family income below Federal Poverty Level past year	-0.009** (0.004)
Baseline mean†	0.115
Present change††	-7.826
Observations	520538

Notes: This table reports coefficient estimates from a regression of economic outcomes on state paid sick leave mandates. The data source is the Annual Social & Economic Supplement to the Current Population Survey 2012-2023, economic outcomes refer to the year prior to the survey year, thus economic outcomes refer to 2011-2022. The unit of observation is a respondent in a state and year. We use [Gardner \(2022\)](#) for estimation. The regression includes state characteristics, state fixed effects, and region-by-year fixed effects. We also include respondent sex and age fixed effects. The paid sick leave mandate is lagged one year. Data are weighted by the Annual Social & Economic Supplement to the Current Population Survey-provided weights. Standard errors that account for within-state clustering are in parentheses.

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

†Baseline mean = mean value in comparison states in 2018.

††Percent change = coefficient estimate / baseline mean $\times 100\%$.

Table 6: Effect of a state paid sick leave mandate on marital status outcomes: Annual Social and Economic Supplement to the Current Population Survey 2011-2022

Outcome ↓	Coefficient estimate (Standard error)
Married	0.005 (0.004)
Baseline mean†	0.757
Present change††	0.661
Observations	535365
Divorced or separated	-0.0001 (0.003)
Baseline mean†	0.100
Present change††	0.000
Observations	535365
Widowed	-0.001 (0.001)
Baseline mean†	0.009
Present change††	-11.111
Observations	535365
Never married	-0.004 (0.004)
Baseline mean†	0.134
Present change††	-2.985
Observations	535365

Notes: This table reports coefficient estimates from a regression of economic outcomes on state paid sick leave mandates. The data source is the Annual Social & Economic Supplement to the Current Population Survey 2011-2022. The unit of observation is a respondent in a state and and year. We use [Gardner \(2022\)](#) for estimation. The regression includes state characteristics, state fixed effects, and region-by-year fixed effects. We also include respondent sex and age fixed effects. The paid sick leave mandate is lagged one year. Data are weighted by the Annual Social & Economic Supplement to the Current Population Survey-provided weights. Standard errors that account for within-state clustering are in parentheses.

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

†Baseline mean = mean value in comparison states in 2018.

††Percent change = coefficient estimate / baseline mean $\times$ 100%.

Table 7: Effect of a state paid sick leave mandate on emergency department episodes among children 0-18 years: MarketScan commercial claims 2016-2022

Age group →	0-18 years	0-5 years	6-18 years
State paid sick leave mandate	-23.000* (12.002)	-23.802 (15.051)	-22.361** (10.838)
Baseline mean†	198.289	226.172	186.085
Present change††	-11.599	-10.524	-12.017
Observations	322	322	322

Notes: This table reports coefficient estimates from regressions of emergency department visits among children (per 1,000 enrollees) on state paid sick leave mandates. The unit of observation is a state-year. The data source is MarketScan commercial claims data 2016-2022. Estimation uses the method proposed by [Gardner \(2022\)](#), which is robust to bias from dynamic and heterogeneous treatment effects. All regressions include state characteristics, state fixed effects, and region-by-year fixed effects. The paid sick leave mandate is lagged one year. Data are weighted by the number of enrollees in each age group. Standard errors clustered by state are in parentheses.

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

† Baseline mean = mean value in comparison states in 2018.

†† Percent change = coefficient estimate / baseline mean $\times$ 100%.

Table 8: Effect of a state paid sick leave mandate on self-reported health status among children under age 19: State Youth Risk Factor Surveillance Survey 2011-2022

Outcome ↓	Coefficient estimate (Standard error)
Sad	0.020 (0.013)
Baseline mean†	0.321
Present change††	6.231
Observations	760234
Consider suicide	0.006 (0.008)
Baseline mean†	0.175
Present change††	3.429
Observations	683313
Plan suicide	0.016 (0.014)
Baseline mean†	0.145
Present change††	11.034
Observations	737353
Attempt suicide	0.006 (0.009)
Baseline mean†	0.092
Present change††	6.522
Observations	515376

Notes: This table reports coefficient estimates from a regression of indicators for whether the respondent reports they were sad, considered suicide, planned suicide, or attempted suicide in the past year. The unit of observation is a respondent in a state and year. The data source is the State Youth Risk Factor Surveillance Survey 2011-2022. We use [Gardner \(2022\)](#) for estimation. The regression includes state characteristics, state fixed effects, and region-by-year fixed effects. The paid sick leave mandate is lagged one year. Data are weighted by the State Youth Risk Factor Surveillance Survey-provided weights. Standard errors clustered by state are in parentheses.

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

†Baseline mean = mean value in comparison states in 2018.

††Percent change = coefficient estimate / baseline mean $\times$ 100%.

Table 9: Effect of a state paid sick leave mandate on health outcomes among children: Annual Social and Economic Supplement to the Current Population Survey 2011-2022

Outcome →	Fair or poor health	Very good or excellent health
State paid sick leave mandate	0.002 (0.002)	0.018** (0.008)
Baseline mean†	0.018	0.837
Percent change††	11.111	2.151
Observations	624663	624663

Notes: This table reports the results of a regression of indicators for fair or poor health and very good or excellent health on a state paid sick leave mandate (lagged one year) and controls. The data source is the Annual Social & Economic Supplement 2011-2022. We use [Gardner \(2022\)](#) for estimation. The unit of observation is a child in an establishment in state in a year. Regressions control for state characteristics, state fixed effects, and region-by-year fixed effects. Data are weighted by Annual Social & Economic Supplement to the Current Population Survey-provided weights. Standard errors account for within-state clustering and are reported in parentheses.

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

†Baseline mean = mean value in comparison states in 2018.

††Percent change = coefficient estimate / baseline mean $\times$ 100%.

Table 10: Effect of a state paid sick leave mandate on intimate partner violence: National Incident-Based Reporting System 2011–2022

Offense Type →	All	Assault	Sex offense	Homicide
State paid sick leave mandate	-34.615** (17.319)	-31.685* (16.989)	-2.833*** (0.405)	-0.064 (0.060)
Baseline mean †	405.160	396.251	8.401	0.553
Present change ††	-8.544	-7.996	-33.722	-11.573
Observations	49339	49339	49339	49339

Notes: This table reports coefficient estimates from a regression of intimate partner violence rates per 100,000 individuals on state paid sick leave mandates. The unit of observation is a police agency within a state-year. The data source is the National Incident-Based Reporting System. The analysis is restricted to agencies that report crimes annually between 2011 and 2022. The regression is estimated with the two-stage imputation procedure of [Gardner \(2022\)](#), which is robust to dynamic and heterogeneous treatment effects. All regressions include state characteristics, police agency fixed effects, and region-by-year fixed effects; the paid sick leave mandate is lagged one year. Data are weighted by the population covered by each agency. Standard errors clustered by state in parentheses.

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

†Baseline mean = mean value in comparison states in 2018.

††Percent change = coefficient estimate / baseline mean $\times$ 100%.

Appendix

Figure A1: Paid sick leave signage in the Commonwealth of Massachusetts

EARNED SICK TIME Notice of Employee Rights

Beginning July 1, 2015, Massachusetts employees have the right to earn and take sick leave from work.

WHO QUALIFIES?

All employees in Massachusetts can earn sick time.

This includes full-time, part-time, temporary, and seasonal employees.

HOW IS IT EARNED?

- Employees earn 1 hour of sick time for every 30 hours they work.
- Employees can earn and use up to **40 hours per year** if they work enough hours.
- Employees with unused earned sick time at the end of the year can **rollover up to 40 hours**.
- Employees **begin earning** sick time on their first day of work and **may begin using** earned sick time 90 days after starting work.

WILL IT BE PAID?

- If an employer has 11 or more employees, sick time must be paid.
- For employers with 10 or fewer employees, sick time may be unpaid.
- Paid sick time must be paid on the same schedule and at the same rate as regular wages.

WHEN CAN IT BE USED?

- An employee can use sick time when the employee or the employee's child, spouse, parent, or parent of a spouse is sick, has a medical appointment, or has to address the effects of domestic violence.
- The smallest amount of sick time an employee can take is one hour.
- Sick time cannot be used as an excuse to be late for work without advance notice of a proper use.
- Use of sick time for other purposes is not allowed and may result in an employee being disciplined.

CAN AN EMPLOYER HAVE A DIFFERENT POLICY?

Yes. Employers may have their own sick leave or paid time off policy, so long as employees can use at least the same amount of time, for the same reasons, and with the same job-protections as under the Earned Sick Time Law.

RETALIATION

- Employees using earned sick time cannot be fired or otherwise retaliated against for exercising or attempting to exercise rights under the law.
- Examples of retaliation include: denying use or delaying payment of earned sick time, firing an employee, taking away work hours, or giving the employee undesirable assignments.

NOTICE & VERIFICATION

- Employees must **notify** their employer before they use sick time, except in a emergency.
- Employers may require employees to **use a reasonable notification system** the employer creates.
- If an employee is out of work for 3 consecutive days **OR** uses sick time within 2 weeks of leaving his or her job, an employer may require documentation from a medical provider.

DO YOU HAVE QUESTIONS?

Call the Fair Labor Division at 617-727-3465 **Visit** www.mass.gov/ago/earned sick time



Commonwealth of Massachusetts
Office of the Attorney General
English - July 2016

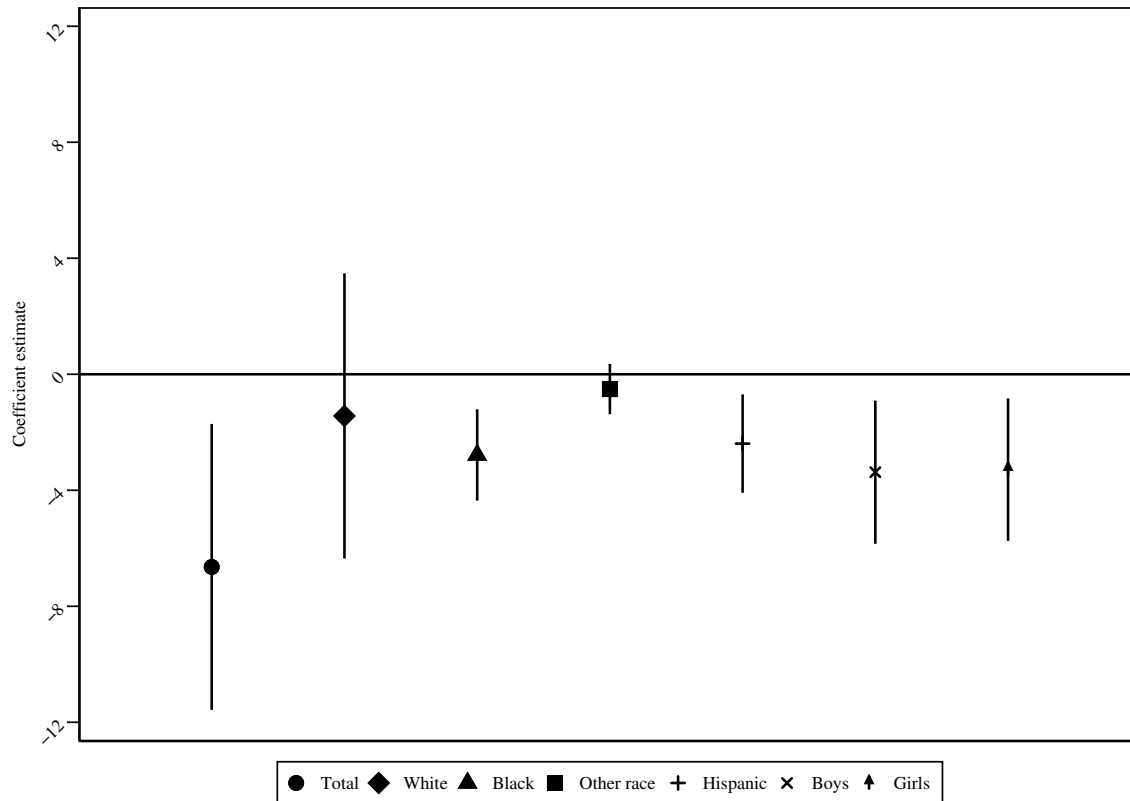
The Attorney General enforces the Earned Sick Time Law and regulations.

It is unlawful to violate any provision of the Earned Sick Time Law.
Violations of any provision of the Earned Sick time law, M.G.L. c. 149, §148C, or these regulations, 940 CMR 33.00 shall be subject to paragraphs (1), (2), (4), (6) and (7) of subsection (b) of M.G.L. c. 149, §27C(b) and to §150.
This notice is intended to inform.

Full text of the law and regulations are available at www.mass.gov/ago/earned sick time.

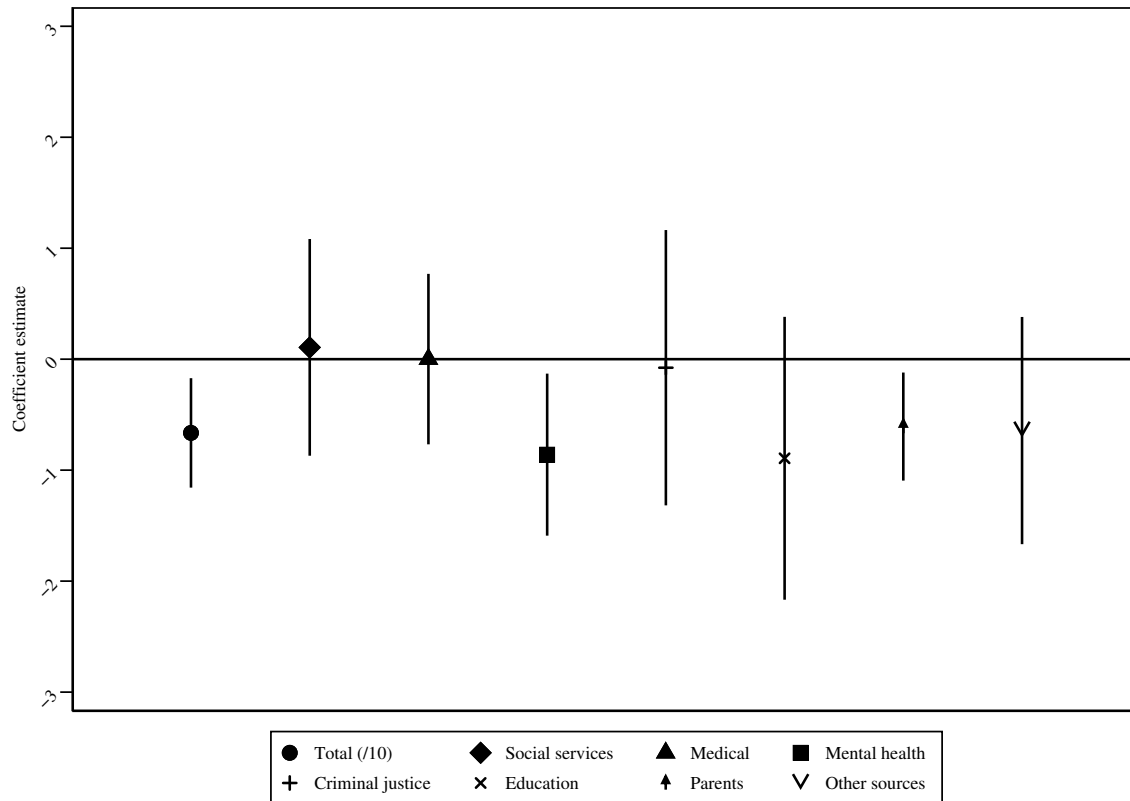
Notes: This figure depicts signage that employers must post in the workplace in Massachusetts. Source: Commonwealth of Massachusetts.

Figure A2: Heterogeneity in the effect of a state paid sick leave mandate on child maltreatment reports among children 0-18 years per 1,000 by child characteristics: National Child Abuse & Neglect Data System 2011-2022



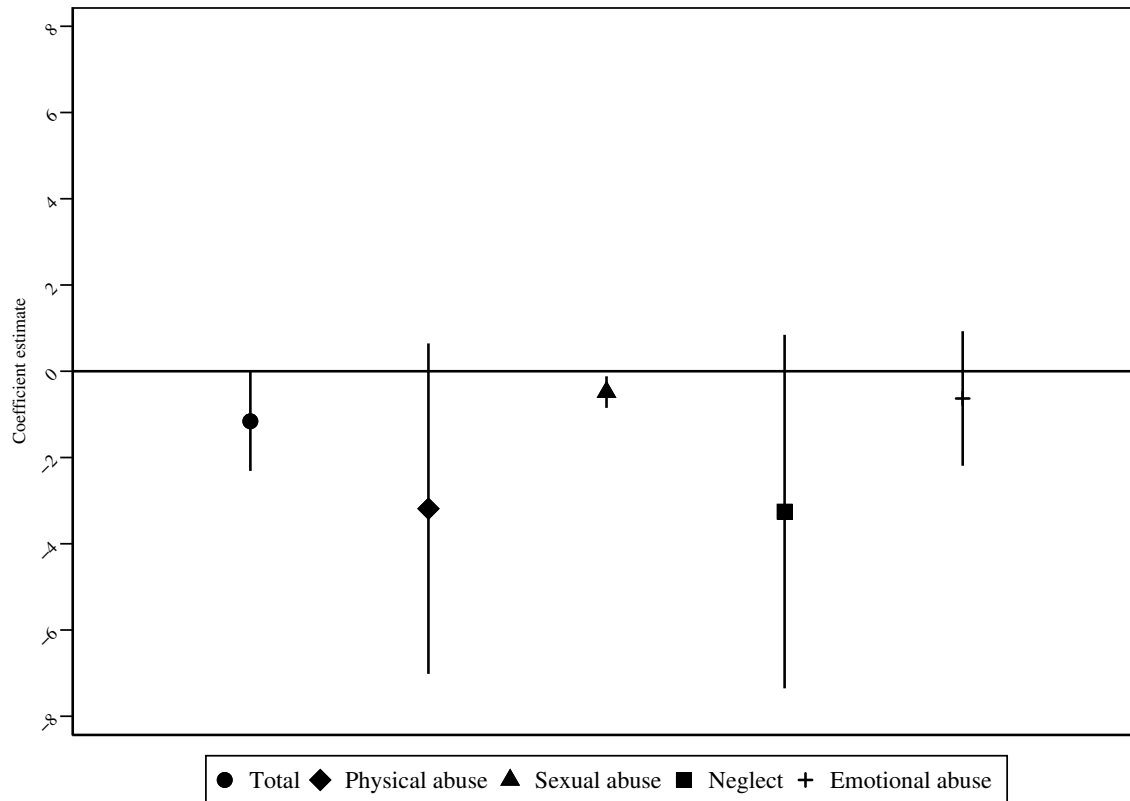
Notes: This figure reports the results of a regression of total reports on a state paid sick leave mandate (lagged one year) and controls. The child sample is listed in the legend. The data source is the National Child Abuse & Neglect Data System 2011-2022. We use [Gardner \(2022\)](#) for estimation. The unit of observation is a state in a year. Regressions control for state-level characteristics, state fixed effects, and region-by-year fixed effects. Data are weighted by the state population 0-18 years. Shapes represent coefficient estimates. 95% confidence intervals that account for within-state clustering are reported with vertical lines.

Figure A3: Heterogeneity in the effect of a state paid sick leave mandate on child maltreatment reports among children 0-18 years per 1,000 by reporting source: National Child Abuse & Neglect Data System 2011-2022



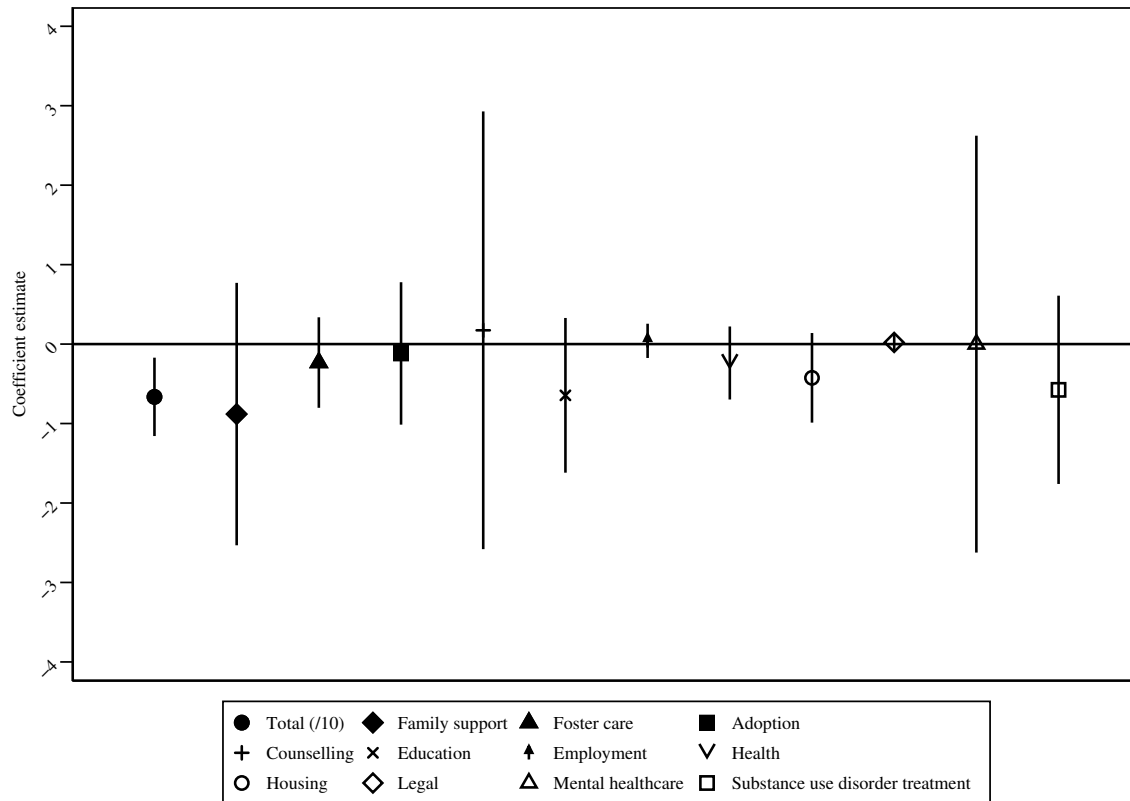
Notes: This figure reports the results of a regression of total reports on a state paid sick leave mandate (lagged one year) and controls. The child sample is listed in the legend. The data source is the National Child Abuse & Neglect Data System 2011-2022. We use [Gardner \(2022\)](#) for estimation. The unit of observation is a state in a year. Regressions control for state-level characteristics, state fixed effects, and region-by-year fixed effects. Data are weighted by the state population 0-18 years. Shapes represent coefficient estimates. 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 substantiated child maltreatment reports among children 0-18 years per 1,000 by type of maltreatment: National Child Abuse & Neglect Data System 2011-2022



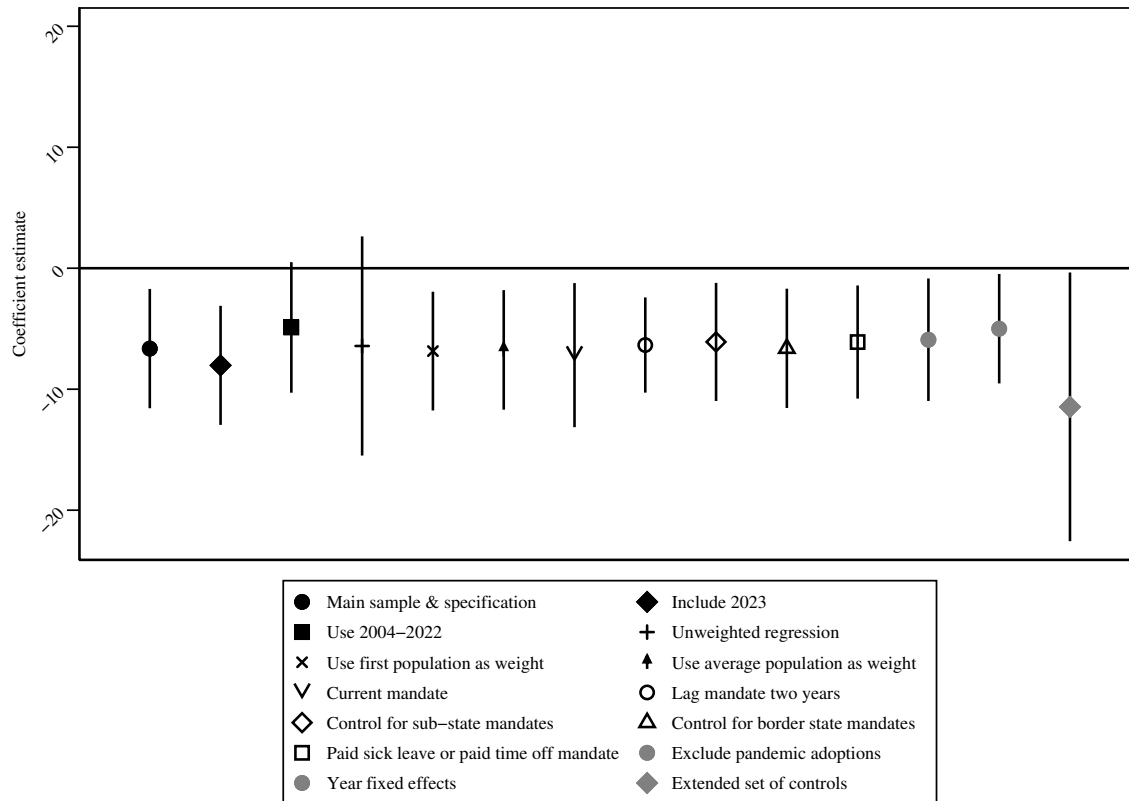
Notes: This figure reports the results of a regression of total substantiated reports on a state paid sick leave mandate (lagged one year) and controls. The type of maltreatment is listed in the legend, only substantiated reports have a type of maltreatment listed. The NCANDS includes up to four types of maltreatment, we classify a report a particular type of maltreatment if that type is reported as the first, second, third, or fourth maltreatment type. The data source is the National Child Abuse & Neglect Data System 2011-2022. We use [Gardner \(2022\)](#) for estimation. The unit of observation is a state in a year. Regressions control for state-level characteristics, state fixed effects, and region-by-year fixed effects. Data are weighted by the state population 0-18 years. Shapes represent coefficient estimates. 95% confidence intervals that account for within-state clustering are reported with vertical lines.

Figure A5: Heterogeneity in the effect of a state paid sick leave mandate on child maltreatment reports among children 0-18 years per 1,000 by type of service received: National Child Abuse & Neglect Data System 2011-2022



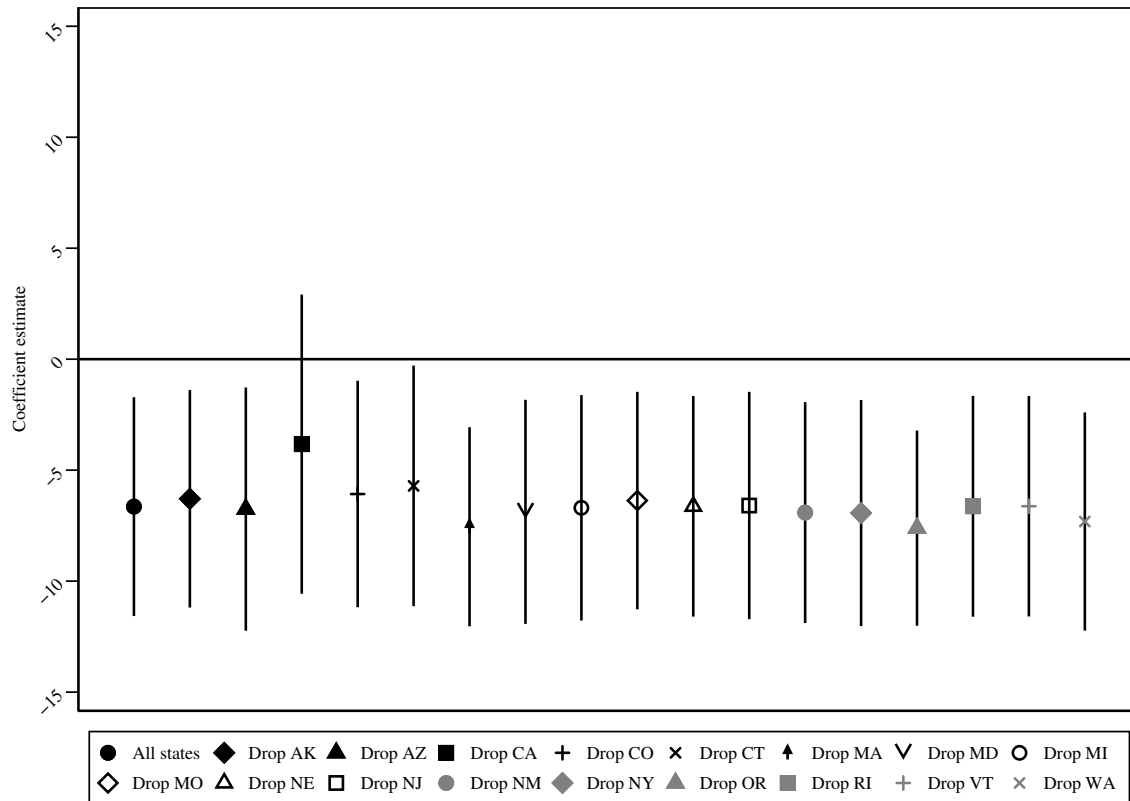
Notes: This figure reports the results of a regression of total reports on a state paid sick leave mandate (lagged one year) and controls. The type of services received listed in the legend. Service variables are missing for many observations, thus we include only state-year pairs with no more than 25% of the information missing. The data source is the National Child Abuse & Neglect Data System 2011-2022. We use [Gardner \(2022\)](#) for estimation. The unit of observation is a state in a year. Regressions control for state-level characteristics, state fixed effects, and region-by-year fixed effects. Data are weighted by the state population 0-18 years. Shapes represent coefficient estimates. 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 child maltreatment reports among children 0-18 years per 1,000 using alternative samples & specifications: National Child Abuse & Neglect Data System 2011-2022



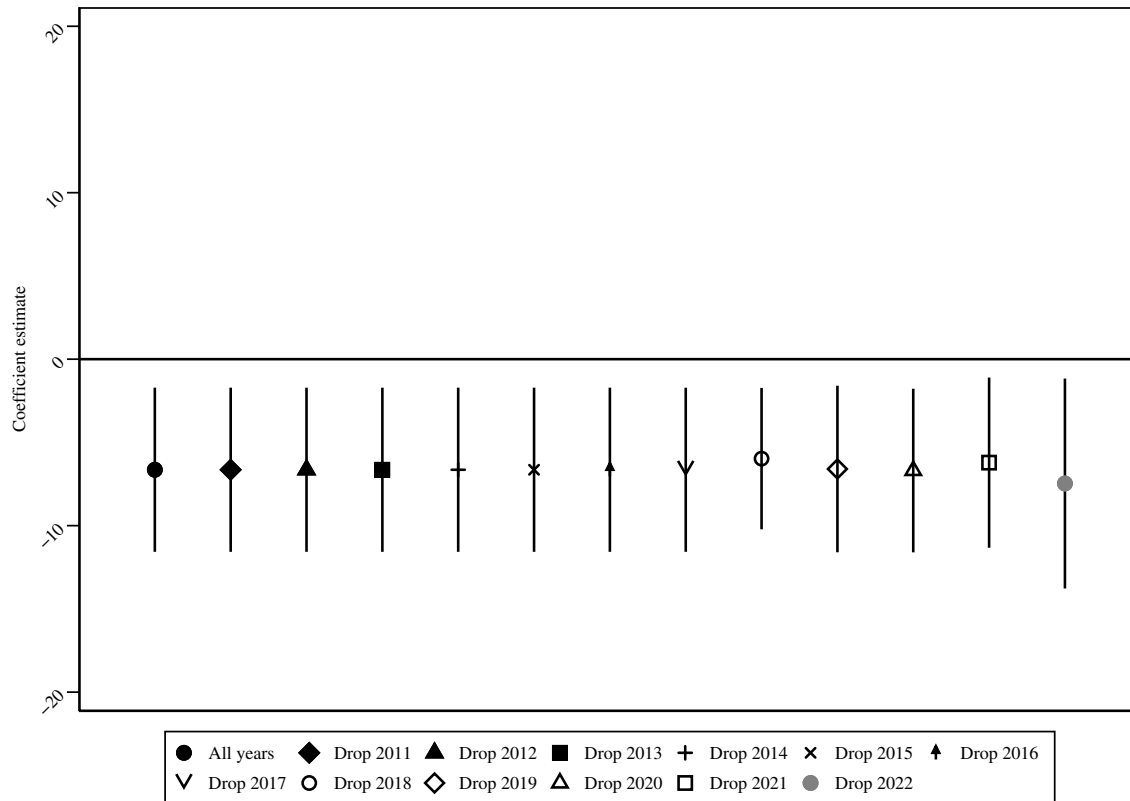
Notes: See Section 4.5 for full details. This figure reports the results of a regression of total reports on a state paid sick leave mandate (lagged one year) and controls unless otherwise noted. The sample & specification is listed in the legend. The data source is the National Child Abuse & Neglect Data System 2011-2022. We use [Gardner \(2022\)](#) for estimation. The unit of observation is a state in a year. Regressions control for state-level characteristics, state fixed effects, and region-by-year fixed effects unless otherwise noted. Data are weighted by the state population 0-18 years unless otherwise noted. Shapes represent coefficient estimates. 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 child maltreatment reports among children 0-18 years per 1,000 sequentially dropping each state that adopts a paid sick leave mandate: National Child Abuse & Neglect Data System 2011-2022



Notes: See Section 4.5 for full details. This figure reports the results of a regression of total reports on a state paid sick leave mandate (lagged one year) and controls. The excluded state is listed in the legend. The data source is the National Child Abuse & Neglect Data System 2011-2022. We use [Gardner \(2022\)](#) for estimation. The unit of observation is a state in a year. Regressions control for state-level characteristics, state fixed effects, and region-by-year fixed effects. Data are weighted by the state population 0-18 years. Shapes represent coefficient estimates. 95% confidence intervals that account for within-state clustering are reported with vertical lines.

Figure A8: Effect of a state paid sick leave mandate on child maltreatment reports among children 0-18 years per 1,000 sequentially dropping each year: National Child Abuse & Neglect Data System 2011-2022



Notes: See Section 4.5 for full details. This figure reports the results of a regression of total reports on a state paid sick leave mandate (lagged one year) and controls. The excluded year is listed in the legend. The data source is the National Child Abuse & Neglect Data System 2011-2022. We use [Gardner \(2022\)](#) for estimation. The unit of observation is a state in a year. Regressions control for state-level characteristics, state fixed effects, and region-by-year fixed effects. Data are weighted by the state population 0-18 years. Shapes represent coefficient estimates. 95% confidence intervals that account for within-state clustering are reported with vertical lines.

Table A1: State paid sick leave mandate effective dates and number of employees gaining access for the first time in the United States: [National Partnership for Women & Families \(2023\)](#) & [A Better Balance \(2024\)](#)

State	Effective date	Employees gaining coverage for the first time
Alaska	7/2025	N/A [†]
Arizona	7/2017	934,000
California	7/2015	6,900,000
Colorado	1/2021	813,000
Connecticut	1/2012	200,000
District of Columbia	5/2008	220,000
Massachusetts	7/2015	900,000
Maryland	2/2018	750,000
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 for Women & Families \(2023\)](#) and [A Better Balance \(2024\)](#). Dates are in the format of month/year. State paid sick leave mandates adopted or announced as of November 2024. Estimates of employees gaining paid sick leave coverage for the first time based on [National Partnership for 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 included here for completeness.

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

Table A2: Summary statistics: National Child Abuse & Neglect Data System 2011-2022

Sample:	Paid sick leave mandate states, pre-mandate	Non-paid sick leave mandate states
Total reports per 1,000		
0-18 years	47.2	52.5
0-5 years	57.0	67.5
6-18 years	42.7	45.5
Substantiated reports per 1,000		
0-18 years	9.37	8.68
0-5 years	13.1	13.7
6-18 years	7.70	6.47
Unsubstantiated reports per 1,000		
0-18 years	36.5	41.6
0-5 years	42.4	50.9
6-18 years	33.8	37.1
State-level covariates		
Paid sick leave mandate (lagged one year)	0	0
Paid time off mandate	0	0.020
Family & medical leave policy	0.33	0
Temporary Aid to Needy Families monthly benefit for a family of four (\$)	851.8	497.0
Medicaid expansion	0.50	0.31
Parent Medicaid parent income thresholds	1.22	0.75
Child 6-18 years Medicaid income thresholds	1.72	1.52
Female	0.51	0.51
Male	0.49	0.49
0-18 years	0.25	0.25
19-64 years	0.61	0.60
65+ years	0.14	0.15
White	0.78	0.78
Non-White	0.22	0.22
Hispanic	0.20	0.15
Non-Hispanic	0.80	0.85
No college degree	0.68	0.72
College degree	0.32	0.28
Observations	133	396

Notes: This table reports summary statistics. The unit of observation is a state in a year. The data source is the National Child Abuse and Neglect Data System 2011-2022. Data are weighted by the state population 0-18 years.

Table A3: Effect of a state paid sick leave mandate on child maltreatment reports among children 0-18 years per 1,000 using difference-in-differences not controlling for time-varying covariates: National Child Abuse & Neglect Data System 2011-2022

Sample →	0-18 years	0-5 years	6-18 years
Panel A: Total reports			
State paid sick leave mandate	-6.008*** (2.027)	-6.534*** (2.489)	-5.787*** (1.998)
Baseline mean†	57.871	72.094	51.141
Present change††	-10.382	-9.063	-11.316
Panel B: Substantiated reports			
State paid sick leave mandate	-1.411*** (0.388)	-2.192*** (0.574)	-1.062** (0.418)
Baseline mean†	9.409	14.813	6.985
Present change††	-14.996	-14.798	-15.204
Panel C: Unsubstantiated reports			
State paid sick leave mandate	-5.065*** (1.426)	-4.840** (1.900)	-5.148*** (1.344)
Baseline mean†	46.291	54.383	42.331
Present change††	-10.942	-8.900	-12.161
Observations	600	600	600

Notes: This table reports the results of a regression of total reports, substantiated reports, and unsubstantiated reports on a state paid sick leave mandate (lagged one year) and controls. The data source is the National Child Abuse & Neglect Data System 2011-2022. We use [Gardner \(2022\)](#) for estimation. The unit of observation is a state in a year. Regressions control for state fixed effects and region-by-year fixed effects. Data are weighted by the state population 0-18 years. Standard errors account for within-state clustering and are reported in parentheses.

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

†Baseline mean = mean value in comparison states in 2018.

††Percent change = coefficient estimate / baseline mean × 100%.

Table A4: Effect of a state paid sick leave mandate on child maltreatment reports among children 0-18 years per 1,000 using alternative difference-in-differences estimators: National Child Abuse & Neglect Data System 2011-2022

Sample →	0-18 years	0-5 years	6-18 years
Gardner (2022)	-6.643*** (2.516)	-7.013** (3.354)	-6.474*** (2.385)
Present change†	-11.479	-9.728	-12.659
Callaway and Sant’Anna (2021)	-3.461** (1.658)	-3.354 (2.091)	-3.560** (1.614)
Present change†	-5.981	-4.652	-6.961
Borusyak et al. (2024)	-4.717*** (1.715)	-3.122 (2.379)	-5.240*** (1.582)
Present change†	-8.151	-4.330	-10.246
Wooldridge (2023)	-4.717*** (1.589)	-3.122 (2.372)	-5.240*** (1.362)
Present change†	-8.150	-4.330	-10.247
de Chaisemartin and d’Haultfoeuille (2020)	-2.938*** (0.993)	-1.535 (1.708)	-3.396*** (1.185)
Present change†	-5.0768	-2.1292	-6.6405
Stacked difference-in-differences	-17.866** (7.995)	-26.235** (11.044)	-13.790** (6.800)
Present change†	-30.8721	-36.3900	-26.9647
Two-way fixed effects	-2.818 (2.262)	-1.449 (2.891)	-3.303 (2.131)
Present change†	-4.869	-2.010	-6.458
Baseline mean††	57.871	72.094	51.141
Observations	600	600	600

Notes: The estimator is reported in column (1). This table reports the results of a regression of total reports on a state paid sick leave mandate (lagged one year) and controls. The data source is the National Child Abuse & Neglect Data System 2011-2022. The unit of observation is a state in a year. Regressions control for state characteristics, state fixed effects, and region-by-year fixed effects. [Callaway and Sant’Anna \(2021\)](#) does not include time-varying covariates. The stacked difference-in-differences replaces state and year fixed effects with cohort specific state and year fixed effects, includes the 2015, 2016, 2017, and 2018 cohorts, and the event-window includes three years before mandate adoption, year of adoption, and three years following mandate adoption. Data are weighted by the state population 0-18 years. Standard errors account for within-state clustering and are reported in parentheses.

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

†Percent change = coefficient estimate / baseline mean × 100%.

††Baseline mean = mean value in comparison states in 2018.

Table A5: [Goodman-Bacon \(2021\)](#) decomposition of the effect of a state paid sick leave mandate on child maltreatment reports among children 0-18 years per 1,000 using alternative difference-in-differences estimators: National Child Abuse & Neglect Data System 2011-2022

Two-by-two comparison:	ATT	Weight
Early treated versus late treated	-3.848	0.061
Treated versus never treated	-3.496	0.903
Late treated versus early treated	3.343	0.036
Re-weighted ATT	-3.100	-
Pre-treatment mean,	57.871	-
mandate states		-
Observations	600	-

Notes: ATT = average treatment on the treated. This table reports results from a [Goodman-Bacon \(2021\)](#) decomposition. The data source is the National Child Abuse & Neglect Data System 2011-2022. The unit of observation is a state in a year. No time-varying covariates are included to isolate the two-by-two comparisons. Data are unweighted to isolate the two-by-two comparisons.

Table A6: Effect of a state paid sick leave mandate on adult emergency department visits: MarketScan commercial claims 2016-2022

Group →	All adults	Adults with dependents
State paid sick leave mandate	-26.213** (12.823)	-24.348* (13.687)
Baseline mean†	267.823	220.219
Present change††	-9.787	-11.056
Observations	322	322

Notes: This table reports coefficient estimates from regressions of emergency department visits among those age 22-59 (per 1,000 enrollees) on state paid sick leave mandates. The data source is MarketScan commercial claims 2016–2022. Estimation uses the two-stage imputation procedure of [Gardner \(2022\)](#), robust to dynamic and heterogeneous treatment effects. All regressions include state characteristics, state fixed effects, and region-by-year fixed effects; the mandate variable is lagged one year. Data are weighted by the number of enrollees in each age bin. Those with dependents include adults within a family that also contains a dependent child 18 years of age or younger. Standard errors clustered by state in parentheses.

†Baseline mean = mean value in comparison states in 2018.

††Percent change = coefficient estimate / baseline mean $\times$ 100%.

Table A7: Effect of a state paid sick leave mandate on child neglect reports using different functional forms: National Child Abuse & Neglect Data System 2011-2022

Functional form $\rightarrow$	Rate	Count (/1000)	Logarithm(rate)
State paid sick leave mandate	-6.643*** (2.516)	-36.013** (16.587)	-0.182*** (0.054)
Baseline mean†	57.871	164.092	164.092
Present change††	-11.479	-21.947	-16.640†††
Observations	600	600	600

Notes: Notes: This table reports coefficient estimates from a regression of child neglect outcomes on state paid sick leave mandates. The data source is the National Child Abuse and Neglect Data System 2011-2022. The unit of observation is a state in a year. We use [Gardner \(2022\)](#) for estimation. The regression includes state characteristics, state fixed effects, and region-by-year fixed effects. The paid sick leave mandate is lagged one year. Data are weighted by the state population 0-18 years. Standard errors that account for within-state clustering are reported in parentheses.

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

†Baseline mean = mean value in comparison states in 2018.

††Percent change = coefficient estimate / baseline mean $\times$ 100%.

†††Percent change = $e^\beta - 1$.

Table A8: Effect of a state paid sick leave mandate on unsubstantiated and alternative response child maltreatment reports among children 0-18 years per 1,000 using difference-in-differences: National Child Abuse & Neglect Data System

Sample →	0-18 years	0-5 years	6-18 years
Panel A: Unsubstantiated reports			
State paid sick leave mandate	-1.538 (2.546)	-1.115 (2.943)	-1.704 (2.413)
Baseline mean†	36.981	45.003	33.173
Present change††	-4.159	-2.478	-5.137
Panel B: Alternative responses			
State paid sick leave mandate	-4.532** (2.122)	-4.527** (2.185)	-4.508** (2.067)
Baseline mean†	9.311	9.380	9.158
Present change††	-48.674	-48.262	-49.225
Observations	600	600	600

Notes: This table reports the results of a regression of unsubstantiated reports and alternative responses on a state paid sick leave mandate (lagged one year) and controls. The data source is the National Child Abuse & Neglect Data System 2011-2022. We use [Gardner \(2022\)](#) for estimation. The unit of observation is a state in a year. Regressions control for state characteristics, state fixed effects, and region-by-year fixed effects. Data are weighted by the state population 0-18 years. Standard errors account for within-state clustering and are reported in parentheses.

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

†Baseline mean = mean value in comparison states in 2018.

††Percent change = coefficient estimate / baseline mean × 100%.