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ABSTRACT

The U.S. does not have a federal paid sick leave (PSL) policy; however, multiple states have adopted PSL mandates which compel employers to provide employees with, on average, seven days of PSL per year. PSL can facilitate healthcare use among women of child-bearing ages, including use of family planning services. We combine administrative, health insurance claims, and survey data with difference-in-differences methods to shed light on these possibilities. Our findings indicate that state PSL mandates reduce birth rates, potentially through increased use of contraceptive services, and changes in employment and marital status post-mandate.

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1 Introduction

The United States is one of three Organisation for Economic Co-operation and Development countries that does not have a permanent federal paid sick leave (PSL) policy (Raub et al., 2018), leaving large portions of the U.S. workforce without the ability to take time off work for their own or a family member's health needs without foregoing wages. In 2023, 22% of civilian employees in the U.S. reported that they did not have access to PSL benefits through their employer (Blewett et al., 2024).[1] Moreover, there is substantial heterogeneity in PSL access across employees (Bartel et al., 2019; DeSilver, 2020; Maclean et al., 2025). Generally, employees in jobs with high wages and generous benefit packages have access to PSL while employees in other jobs do not. Given that the median daily earnings among U.S. employees in 2024 was \$233 (Bureau of Labor Statistics, 2024),[2] lost wages associated with leave-taking for sickness are likely non-trivial for many Americans.

The available evidence suggests that employees without PSL forego healthcare for themselves and their dependents (DeRigne et al., 2016). They also work while sick: surveys show that 90% of employees report working while sick at some point (Acutemps, 2019). Susser and Ziebarth (2016) document that each week three million Americans work while sick. Fear of losing income or the job are potential reasons for these patterns. On the other hand, employees with PSL benefits use more healthcare, particularly primary and preventive care, than employees without such benefits (Kaiser Family Foundation, 2021). These associations suggest that the lack of a federal PSL policy may negatively impact many Americans.

In the absence of federal legislation, U.S. states and localities have begun to mandate employer-provided PSL. At the time of writing, 18 states plus the District of Columbia (referred to as a 'state' below) have adopted or announced a PSL mandate (National Partnership for Women & Families, 2023; A Better Balance, 2025). These mandates require employers to provide, on average, seven days of PSL per year (Kaiser Family Foundation, 2021), granting access to PSL benefits to well over 21 million employees (National Partnership for Women & Families, 2023),[3] and increasing generosity of the benefit for many other employees. Previous economic studies show that these mandates increase PSL access and use, increase healthcare received, and improve health (e.g., by reducing infectious disease), but do not lead employers to curtail wages or other valuable benefits (see Section 2.2). In contrast to concerns sometimes raised by critics of requiring provision of PSL benefits

[1] Authors' analysis of the 2023 National Health Interview Survey.

[2] This average includes wage and salary workers. We convert average weekly earnings – \$1,165 – among full-time workers 25 years and older to daily earnings.

[3] Assuming each employee has two dependents, these PSL mandates would result in over 60 million persons (employees and dependents) living in households covered through PSL for the first time.

(Copland, 2013), these mandates are not overly costly to employers (Maclean et al., 2025).

We extend the literature examining U.S. PSL mandates by investigating their impact on birth rates. Understanding these effects is important because, while birth rates in the U.S. have been declining for decades (Buckles et al., 2025; Kearney et al., 2022), 40% of pregnancies occur earlier than intended or when no pregnancy is desired (Kost and Lindberg, 2015). Such pregnancies most commonly reflect a failure to use effective contraception (Centers for Disease Control and Prevention, 2023b). On the other hand, 19% of married women 15-49 years old experience infertility each year (Centers for Disease Control and Prevention, 2023a)[4] and nearly 13% of reproductive age women receive fertility treatment (Carson and Kallen, 2021). Finally, despite virtually universal support from healthcare professionals, 2.1% of pregnant mothers received no prenatal care in 2021 and 12.5% received inadequate prenatal care (Martin and Osterman, 2023).[5] These statistics highlight substantial barriers in access to family planning, fertility, and prenatal care, underscoring the need for improved policies to ensure better birth outcomes for women.

Policies increasing the access of women[6] to healthcare (including family planning services) and that allow improved pregnancy timing may have benefits both for women and for their children. For example, mistimed or unwanted ('unintended') pregnancies can be costly to women in terms of labor market outcomes, health, family decisions, and educational attainment (Goldin and Katz, 2002; Bailey, 2006; Bailey et al., 2012; Biggs et al., 2017; Buckles et al., 2025; Miller et al., 2023). Births resulting from unintended pregnancies can lead to both immediate (e.g., low birthweight and birth complications) and longer-term (e.g., worse health and increased risk of poverty) consequences (Mohllajee et al., 2007; Ananat and Hungerman, 2012; Bailey, 2013; Kost and Lindberg, 2015; Lin et al., 2020). Prenatal care[7] is beneficial for both mothers and their children (U.S. Department of Health and Human Services, 2021), but requires time investments including regular healthcare professional appointments which often occur during the standard workday.

In this study, we combine difference-in-differences and event-study methods with administrative, health insurance claims, and survey data from 2010 to 2022 – a period in which PSL mandates were implemented in multiple states – to comprehensively study the impact

[4]Infertility is defined as women with no prior births who were not able to become pregnant after one year of attempts.

[5]Inadequate prenatal care is defined as '...care beginning after the fourth month of pregnancy or care that includes less than 50% of the recommended number of visits' (Martin and Osterman, 2023).

[6]We use the term 'women' for brevity in this paper. We recognize that all persons biologically able to have children could use PSL to receive family planning services.

[7]Prenatal care can include screening and treatment for medical conditions, and interventions designed to address risk factors associated with poor birth outcomes such as maternal smoking or substance use, mental health conditions, and poor nutrition.

of state PSL mandates on birth rates. We obtain five key findings. First, PSL mandates raise access to and use of PSL in a national survey of establishments and survey data. Second, the use of both contraception and fertility treatments increases among women of child-bearing age increases post-mandate. Third, we observe modest increases in any work, full-time employment, hours worked, hourly wages, and weekly earnings among women of child-bearing age following adoption of a state PSL mandate. Earlier work documents that PSL mandates improve women's economic standing (Slopen, 2024) and increases in wages may be attributable to job 'up-scaling' by employers who now must improve other aspects of the offered benefit package to attract employees (Maclean et al., 2025). Fourth, we find some evidence that the probability of marriage declines post-mandate, and this decline is offset by an increase in remaining unmarried (i.e., not divorce). This pattern of results may suggest that PSL may offer women independence from sub-optimal romantic relationships. Finally, birth rates decline post-mandate by 2.5% following adoption of a state PSL mandate. Overall, our findings suggest that mandated PSL facilitates the reproductive choices of women, with the net effect of reducing birth rates.

2 Background and prior research

2.1 Paid sick leave in the United States

The only permanent federal leave policy in the U.S., the *Family and Medical Leave Act* (FMLA) of 1993, provides eligible employees with up to 12 weeks of <u>unpaid</u> leave in a 12-month period for prenatal care and incapacity related to pregnancy, for the birth of a child and to care for the newborn, and for own serious health condition following the birth of a child (U.S. Department of Labor, ND). Benefits can also be used to care for children, spouses, or parents; however, FMLA benefits are not available for short-term absences attributable to 'acute health problems' (Stoddard-Dare et al., 2018). For example, FMLA leave cannot be used for healthcare professional visits to obtain prescriptions for contraceptives, though some legal scholars argue that these benefits can be used for abortion services and medically necessary prenatal care (Nowak, 2022).

While FMLA provides unpaid leave to some employees (including child-bearing age women), many employees are ineligible for coverage because they work for small employers, who are exempt, or do not meet the Act's work history requirements.[8] Moreover, large portions of the U.S. workforce are unable to take time off for healthcare needs without

[8]The Department of Labor states that 44% of employees are ineligible for FMLA benefits (Heymann et al., 2021).

losing earnings. As mentioned in Section 1, 22% of U.S. employees report that they lack access to paid sick leave benefits (Blewett et al., 2024). The inability to take time off without losing earnings may prevent some individuals from seeking treatment for themselves or their dependents. Despite the lack of a federal provision, paid sick leave is popular, with 84% of Americans supporting policies that would mandate PSL (Global Strategy Group & Paid Leave for All Action, 2021). U.S. Senators Rosa DeLauro and Bernie Sanders recently sponsored the *Healthy Families Act* of 2023 which, if implemented, would provide nearly all employees with seven days of PSL per year (Sanders, 2023).[9]

Given the absence of federal action, multiple states have adopted PSL mandates. Table A1 presents data on the 19 states with PSL mandates in place or announced as of July 2025, using legal data prepared by the National Partnership for Women & Families (2023) and A Better Balance (2025). We note that the Missouri legislature – although the state adopted a PSL mandate in 2024 through a ballot initiative (Proposition A) supported by 57.6% of voters – repealed the state's mandate within one year of implementation (Lieb, 2025). This table also shows the number of employees estimated to have gained PSL coverage for the first time due to these mandates. Because some employees will gain additional benefits post-mandate (e.g., those working for employers who provide PSL on less generous terms than those mandated by the state policy) and many employees have dependents who could indirectly benefit from expanded PSL, the full number of individuals experiencing improved access to PSL is likely larger than the numbers shown on the table. Figure A1 displays the geographic distribution of these mandates across states.

Most often, these mandates require employers to allow employees (on average) seven days of PSL per year, with unused benefits generally available to be rolled over to the following year. All U.S. state PSL mandates to date cover employee time off for sickness of/caring responsibilities for the employee's spouse and children; most also apply to sickness of parents, domestic partners, and some other family members. PSL benefits are financed by employers, who are also required to post benefit information at the worksite. For example, Figure A2 provides the information that Massachusetts requires employers to post. Generally, there is limited monitoring of employee PSL use. For instance, employees are typically not required to state specifically whether they are using PSL for themselves or dependents.[10] PSL policies prohibit employer retaliation against employees who use PSL entitlements.

In addition to states, some cities and counties have adopted PSL mandates (e.g., San Francisco, California adopted a PSL policy in 2007). When a state and a sub-state juris-

[9]The *Healthy Families Act* was first introduced by U.S. Senator Ted Kennedy in 2005.

[10]For example, in Massachusetts, employees must complete a form requesting use and attest that they will utilize the PSL for an allowable activity, but the employee does not have to list specific activity. Please see `https://www.mass.gov/info-details/earned-sick-time`, last accessed May 26, 2023.

diction both adopt a PSL, the most generous policy is binding. In our main analyses, for reasons described in Section 3.2, we focus on state PSL mandates only. However, our results are not appreciably different if we incorporate sub-state policies (see Section 5).

Like many state laws, there are variations in details of the PSL mandates, that could lead to heterogeneous impacts. For example, some mandates compel employers to provide unpaid leave, in addition to paid leave (National Partnership for Women & Families, 2023). Further, PSL mandates generally exempt some firms (e.g., smaller firms) and some types of workers (e.g., independent contractors), see the National Partnership for Women & Families (2023) for details. However, due to the recency of these policies, we examine the impact of any PSL mandate, without accounting for these differences.

Three states (Illinois, Maine, and Nevada) have adopted 'paid time off' (PTO), but not PSL, mandates (National Partnership for Women & Families, 2023). PTO mandates require employers to provide a certain amount of paid time off work each year, regardless of the purpose. Legal scholars view PTO laws as less generous than PSL mandates and recommend separate classifications for each type of law (National Partnership for Women & Families, 2023). In particular, PSL mandates provide employee protection that is not often codified in PTO mandates.[11] In our main analyses, we focus on PSL mandates while separately controlling for PTO mandates; however, in a robustness check we include PTO in our definition of a PSL mandate, with no appreciable change in the results. Notably, Michigan adopted a PSL mandate in 2019, but the state's legislature removed employee protections and thus, when the mandate became effective in 2019, the law was a PTO mandate according to the National Partnership for Women & Families (2023). However, in 2024, the Governor re-instated the PSL provisions, thus we consider Michigan to have a PTO mandate between 2019 and 2024, and then a PSL mandate from 2025 onward. As described in Section 3, this policy change will only impact our event-study analysis.

During the COVID-19 pandemic, the U.S. federal government adopted a temporary PSL policy under the *Families First Coronavirus Response Act* or 'FFCRA' which, from April 1 2020 through December 31 2020, offered a sub-set of employees up to two weeks of PSL for COVID-19 related illness, exposure, or family responsibilities. We exclude 2020 from our main analyses but we show that including this year does not appreciably affect our findings.

The effect of mandated PSL on birth rates is ex ante ambiguous. At first glance, drawing

[11] PTO mandates generally offer limited or no protection against employer retaliation for employees who request or use PTO; do not include a limit on the employer's ability to require the employee to locate a replacement employee during the leave period; do not offer protected ability to take leave without advance notice; and impose no limitations on documentation or requirements needed to be granted paid leave. (This footnote is based on the authors' personal conversations with a senior policy analyst at the National Partnership for Women & Families. Full details available on request.)

predictions from investigations of state paid family and medical leave (PFML) policies may seem useful. While findings are mixed, several studies suggest that PFML policies may increase birth rates (Bana et al., 2020; Golightly and Meyerhofer, 2022; Bailey et al., 2019). However, the effect sizes are not arguably large given the duration of leave afforded by these policies. For example, Golightly and Meyerhofer (2022) find a 2.5% increase in the birth rate in California following PFML policy adoption. The California policy provides six weeks of paid leave for workers who take time off to bond with a new child. A likely rationale for the potential rise in fertility following enactment of a state PFML is that, by allowing parents to take several weeks off work around the time of birth or adoption, PFML substantially reduces these large initial costs and so increases birth rates. There could be additional effects resulting from increases in job continuity, where parents do not have to quit their jobs to obtain time at home with infants due to the availability of paid leave. This benefit, potentially, preserves job-specific human capital leading to higher levels of future employment and wages which, if children are normal goods, could increase fertility rates as well. However, the reasoning behind PFML policies increasing birth rates does not necessarily translate to PSL. The facilitated periods of leave provided by state PSL mandates to parents with infants in relatively short (seven days per year) and so are unlikely to have the effects just described in the context of PMFL policies. On the other hand, a variety of other mechanisms could either increase or decrease birth rates. PSL may increase healthcare access among women of child-bearing age, including access to use of birth control which can be obtained in office visits with a clinician. Thus, expanding PSL access could *reduce* birth rates, a pattern that may be reinforced if PSL increases the receipt of abortion services. Conversely, PSL may allow women to receive fertility treatment, which would *increase* birth rates.

2.2 Prior evidence on U.S. paid sick leave mandates

Studies of U.S. PSL mandates unsurprisingly show that employers increase their offering of paid sick leave to employees once the policy is in place (Colla et al., 2014; Ahn and Yelowitz, 2016; Schneider, 2020; Maclean et al., 2025; Callison and Pesko, 2022).[12] Using detailed national establishment data, Maclean et al. (2025) find that the probability that a job offers PSL rises by 32% and employee use of PSL grows by 22% following adoption of a state mandate.[13] Gains are largest for jobs with the least prior access to PSL: part-time and

[12]PSL policies are both more ubiquitous and more comprehensive in European countries than in U.S. states and localities. For this reason, and given differences in both healthcare and labor markets, we focus our attention on U.S. PSL mandates in our review of the literature. Rho et al. (2020) compares and contrasts PSL policies across advanced economies.

[13]While there are substantial increases in the probability a job provides PSL, the coverage rate does not rise to 100%. The authors suggest that employer non-compliance, legal issues (several major employers

non-union jobs, and jobs in construction; administration, support, and waste management; accommodation and food services; and food preparation and serving sectors. As described in Section 2.1, some state PSL mandates also provide unpaid leave to employees (National Partnership for Women & Families, 2023) and Maclean et al. (2025) show that employee use of unpaid sick leave roughly doubles post-mandate. Similarly, using survey data, Callison and Pesko (2022) and Ahn and Yelowitz (2016) document that employee reports of access to PSL increase following adoption of a state mandate. Maclean et al. (2025) find that costs of PSL to employers are 6.2 cents per hour worked post-mandate. Employers do not cut back on benefits – potentially because the costs associated with PSL mandates are low – and instead may engage in 'job-upscaling' where compensation packages become more generous as employers must add more benefits to compete for workers. In particular, employers increase offerings of health insurance and wages (Maclean et al., 2025) and Slopen (2024) shows that women's employment and earnings increase by 1.7% and 8.5% respectively following adoption of a state PSL mandate, while their poverty rates decline by 6.7% post-mandate.

Examining early PSL mandates in Connecticut and Washington DC, Stearns and White (2018) provide evidence of declines in reported sickness absence post-mandate using survey data. The authors attribute this finding to reduced disease spread at the workplace and other factors. Previous research also suggests that mandated PSL increases preventive and ambulatory healthcare use such as vaccinations and screenings (Pichler and Ziebarth, 2017; Pichler et al., 2021; Callison and Pesko, 2022; Callison et al., 2023, 2025),[14] while reducing unnecessary service use (Ma et al., 2022), and improving health status (Callison and Pesko, 2022; Slopen, 2023). Pichler et al. (2020) and Andersen et al. (2023) show that the PSL, temporarily provided in 2020 during the COVID-19 pandemic through FFCRA, reduced the spread of COVID-19. Finally, several studies indicate that PSL mandate adoption leads to increases in caregiving, both for children and older adults (Byker et al., 2023; Arora and Wolf, 2024; Guo and Peng, 2024; Maclean and Pabilonia, 2024).

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sued states and localities over these mandates, for example, American Airlines sued the Commonwealth of Massachusetts over that state's PSL mandate. Please see `https://tinyurl.com/mryk949e`, last accessed May 24, 2023.) and lack of benefit knowledge as potential reasons for less than full compliance.

[14] We note that Guo and Peng (2024) find no evidence that self-reported preventive care changes following a PSL mandate in survey data.

3 Data and methods

3.1 Birth records

Primary data are drawn from the restricted use National Center for Health Statistics (NCHS) administrative birth records database. Our main analysis sample includes a near universe of recorded U.S. births occurring between 2010 and 2022, excluding 2020 due to the COVID-19 pandemic and the temporary federal PSL policy (though we report some results including this year). We begin the analysis in 2010 to avoid potential confounding from the Great Recession 2007-2009 and given that the Affordable Care Act (ACA), implemented in 2010, removed cost-sharing for many contraceptives (Tschann and Soon, 2015). However, we show in robustness checking reported in Section 5 that our results are not sensitive to i) beginning the sample in earlier years, or ii) excluding each individual year of the study period (2010-2022). The birth record data include information on location of birth, mothers' characteristics (e.g., residence location, age and race), maternal behaviors (prenatal care use and smoking), features of the birth (e.g., vaginal vs. Cesarean birth), and infant outcomes (e.g., birth weight). We exclude births for which the mother's location residence is a U.S. territory. For computational ease, we collapse the microdata to mother's residence state-year of birth-level, corresponding to the state variation in PSL mandates that we study.[15] Aggregating in this manner leaves us with 600 observations. We exclude the District of Columbia from the analysis sample as that location adopted a PSL mandate in May 2008 and is therefore treated in all years of our study period.

Our primary outcome is the annual state birth rate per 1,000 women ages 16-44 years (i.e., the general fertility rate). Using data from other sources, described below, we test for first stage effects, mechanisms for our main findings, and threats to identification, selecting women 16-44 years as closely as the data allow.

3.2 Paid sick leave mandates

We use data on state PSL mandates prepared by the National Partnership for Women & Families (2023). This organization maintains a database of PSL mandates that is current as of October 2023. We supplement this source with information from A Better Balance (2025) for four states (Alaska, Michigan, Missouri, and Nebraska) that adopt or announce a PSL mandate after that time period. Using these two data sources allows us to track all states that adopt or announce a PSL mandate by July 2025.

[15] The micro-data (i.e., birth-date of birth-level) over our study period includes 50,090,895 observations. The substantial computing time required to analyze data at this level leads us to aggregate the data.

States adopt PSL mandates at various points of the year, and we code the first partial year in which the mandate is in place as the effective year. Figure A1 reports the geographic and temporal variation in PSL mandates in the U.S. Table A1 reports the effective date (month/year) for all states that adopted or announced a PSL mandate adoption by July 2025, as well as the number of employees gaining access to PSL for the first time due to the mandates we study (National Partnership for Women & Families, 2023).

3.3 Other data sources

We complement our primary birth records dataset with other data sources that allow us to study 'first-stage' effects and potential mechanisms. We utilize restricted-use National Compensation Survey (NCS) data for 2009-2022 to study the effect of PSL mandates on PSL coverage and use. The NCS, which is maintained by the Bureau of Labor Statistics (BLS) and provides a nationally-representative sample of establishments,[16] is used to produce official government statistics on compensation and labor costs in the U.S., and to adjust federal government employee wages. We follow Maclean et al. (2025) and construct annual measures using Q1 in year *t* and Q2, Q3, and Q4 for years *t-1* for the year *t* PSL utilization. Information is collected by trained BLS economists who interview human resources administrators at each establishment each quarter.[17] The unit of observation is a job in an establishment, BLS administrators sample jobs probabilistically within a surveyed establishment. We weight data by NCS-provided survey weights.

We use in the American Time Use Survey (ATUS) to study how PSL mandates impact self-reported PSL access and PSL utilization in a 'Leave Module' included in the 2011, 2017, and 2018 surveys (Flood et al., 2023). Respondents in the ATUS reflect a sub-set of Current Population Survey (CPS) respondents who are interviewed five to eight months after completing the eighth (final) month of the CPS survey.[18] In the leave module, respondents are asked if they have paid leave that can be used for their own health needs (we cannot isolate PSL from PMFL or PTO, which likely leads to measurement error) at their current job and, among those who report such access, whether any leave was taken in the past five days and the number of hours of leave used, we cannot separate paid and unpaid leave in the use measures. Given the very limited data in the leave module (just three years of data) we do not estimate an event-study. These data are a useful complement to the NCS as we

[16]The BLS defines an establishment as '...a single physical location where one predominant activity occurs' (Sadeghi et al., 2016).

[17]Establishments remain in the sample for three to five years. The NCS economists typically collect baseline data from each establishment the year prior to the establishment entering the survey.

[18]CPS respondents are interviewed each month for four months, not interviewed for eight months, and interviewed again for four months.

are able to isolate leave use among women of child-bearing ages in the ATUS, but not the NCS as the NCS includes information on jobs and not the people who hold these jobs.

We utilize the MerativeTM MarketScan® Research Database ('MarketScan') to study the impact of state PSL mandates on contraception- and fertility-related healthcare service use. The MarketScan database is a longitudinal panel of employer-sponsored insurance claims over the period 2016-2022, and links paid claims and encounter records with monthly enrollment files and demographics. More specifically, these data include aggregated beneficiary-level healthcare use, expenditures, and enrollment across inpatient, outpatient, prescription-drug, and carve-out services. The data include approximately 350 large self-insured employers, and capture adjudicated paid claims and capitated encounters for each service rendered. The sample includes approximately 75 million beneficiaries – 53 million are planholders (i.e., employees) and 17 million are child and adult dependents. We consider several different healthcare measures to study mechanisms through which PSL mandates may impact birth rates. More specifically, we construct the following annual measures among beneficiaries who are female and ages 16 to 44 years of age: i) oral contraception, ii) long-acting reversible contraception and sterilization (LARC), iii) abortions, and iv) in vitro fertilization (IVF).[19] We would expect that use of oral contraception, LARCs, and abortion services would reduce birth rates while use of IVF would increase such rates. We use National Drug Codes (NDCs), procedure codes, and diagnosis codes to isolate these services. Table A2 reports example codes that are used to isolate encounters. A full list of codes is available on request from the corresponding authors. We convert the annual service counts to the rate per 1,000 benefactresses who are female and ages 16-44 years. We use enrollment counts in 2016 as there is some evidence that employers may increase health insurance offerings in response to state PSL mandates by offering health insurance (Maclean et al., 2025), using data from a base year may minimize concerns related to an endogenous denominator.

To supplement our analysis of MarketScan, which focuses exclusively on the commercially insured, we turn to survey data on contraceptive use among 18-44 year old women from the Behavioral Risk Factor Surveillance Survey (BRFSS). The BRFSS is a large telephone survey, administered by the Centers for Disease Control and Prevention (CDC), of approximately 400,000 non-institutionalized U.S. adults (18 years and older) each year. The unit of observation is a respondent in a state and month-year. Respondents are queried about health, health behavior, and healthcare outcomes. In some years (2006, 2010, 2011, 2017, 2019, and 2022), the CDC includes an optional 'Preconception Health/Family Planning' module, which states may or may not choose to administer. We include only treatment

[19]MarketScan data are not available in Vermont or Delaware for 2022. Thus, we drop those two states from the analysis.

states that we observe before and after the PSL policy change and we include all untreated states 2010-2022.[20] The contraception question asks if the respondent or their partner are 'doing anything now to keep from getting pregnant.'[21] For respondents reporting engaging in some form of contraceptive use, a follow-up question is asked about the specific behaviors. The possible responses include: oral contraceptives, LARC, condoms (male and female), rhythm method, and other. We consider all forms of contraception to create an indicator for any self-reported contraception and zero otherwise. Given the limited data we have in the BRFSS, we are not able to estimate an event-study. Thus, we view the BRFSS analysis as secondary to our analysis of the MarketScan data.

Finally, we utilize data from the Current Population Survey (CPS), conducted by the U.S. Census Bureau on behalf of the BLS, on approximately 150,000 U.S. residents each month. Respondents are queried about basic demographic information monthly and the BLS adds supplements to the basic monthly survey to collect data on various topics throughout the year. We use information from the the Annual Social and Economic Supplement, basic monthly CPS, and the Outgoing Rotation/Earners Study on women ages 16-44 years. We draw data on past-year across-state migration, family income below the Federal Poverty Level, and any work in the past year from the 2011-2020 and 2022-2023 Annual Social and Economic Supplement, fielded once per year during February through April. Information in this supplement over this time period refers to calendar years 2010-2019 and 2021-2022. We match state-level data based on calendar year in analysis of this supplement.

We use the basic monthly survey 2010-2019 and 2021-2022 to construct indicators of any and full-time (usually working 35 hours per week or more at the primary job) employment, and the usual hours worked per week at the primary job at the time of the survey. The Outgoing Rotation/Earners Study supplement includes information on the hourly wage (for employees paid hourly) and weekly earnings (for employees not paid hourly), we take the logarithm of conditional wages and earnings (thus we do not include those with zero wages or earnings) using information from the 2010-2019 and 2021-2022 surveys.[22] We use information on marital status at the time of the basic monthly survey for women ages 16-44 years old (married or living as married; divorced, separated, or widowed; and never married). We utilize CPS data harmonized by the University of Minnesota IPUMS database and weight data by CPS-provided survey weights (Flood et al., 2022).

[20] The treatment states include Arizona, Maryland, and New Jersey.

[21] There are minor changes to the question and response wording over time. Full details are available on request from the corresponding author.

[22] We inflate wages and earnings to 2022 values using the Consumer Price Index-Urban Consumers.

3.4 Summary statistics and trends

Table A3 reports summary statistics for our birth record sample. The mean annual birth rate is 61.5 per 1,000 women 16-44 years of age and birth rates are similar for states that adopt PSL mandates (61.7 per 1,000 women prior to policy implementation) and those that do not (62.9 per 1,000). PSL mandates are relatively new policies in the U.S. and just 12% of observations have a PSL mandate in place. While not identical, the two groups of states are broadly similar in terms of policies and demographics included in our regressions.

Trends in annual birth rates for states that adopt/announce a PSL mandate by July 2025 and states that do not are reported in Figure A3. The two time series are trending downward for most of the study period as has been established in earlier work (Buckles et al., 2025). Beginning in 2012 (when Connecticut adopted a PSL mandate), there is a moderate divergence between the two groups, with states that adopt a PSL mandate experiencing a somewhat sharper birth rate reduction than other states. We also report trends in birth rates for each of the treated states and the average of all other states in Figure A4.

3.5 Methods

To study the effect of state PSL mandates on birth rates and associated outcomes, we utilize difference-in-differences methods. In our main analyses, we use a two-stage procedure developed by Gardner (2022). The Gardner (2022) method is an imputation-style estimator that imputes 'missing' counterfactuals for the treated observations using information from untreated observations (not-yet-treated and never-treated). This approach is robust to bias associated with treatment effect dynamics and heterogeneity across treated units when the treatment regime is staggered (e.g., states adopt PSL mandates at different points in time). Unlike conventional two-way fixed effects (TWFE) regression, the Gardner (2022) prohibits 'forbidden comparisons,' where earlier treated units are used as comparison units for later treated units, and does not 'variance weight' the data.[23]

There are several 'new difference-in-differences' methods used within the literature. We choose the Gardner (2022) approach as this estimator offers several attractive properties for our setting. First, the approach avoids bias attributable to correlations between the treatment effect heterogeneity and the included time-varying covariates (Powell, 2021; Caetano et al., 2022). Second, the Gardner (2022) approach performs well against alternative difference-in-differences estimators in terms of inference (Gardner et al., 2024). Third, this approach can account for time-varying covariates, which we believe are important in our

[23]Conversely, TWFE disproportionately up-weights units for which treatment 'turns on' in the middle of the panel for efficiency.

setting. Finally, the Gardner (2022) procedure is based on regression, which is a concept familiar to most micro-economists. However, we will show in Section 5 that our results are not sensitive to using alternative estimators common within the economics literature.

In the first-stage, we estimate the relationships between birth rates, time-varying covariates, and fixed effects using only the untreated observations (the never-treated and the not-yet-treated observations). We then use these estimated parameters to residualize the outcomes for both the treated and untreated observations. In the second stage of the procedure, we regress the residualized outcomes on the treatment variable (i.e., state PSL mandate) for both treated and untreated observations. Thus, the specification takes the form:

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

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

$B_{s,t}$ is the annual birth rate per 1,000 women of child-bearing age in state s in birth year t. $PSL_{s,t-1}$ is an indicator for a PSL mandate lagged one year to allow for the typical nine-month gestation period and for women to gain access to PSL through the mandates we study and access family planning services. (As described in Section 3, workers must accrue benefits over time.) As shown in Section 5, our findings are not sensitive to using alternative lag structures. $X_{s,t}$ is a vector of time-varying state-level covariates that are potentially determinants of our outcomes and the propensity for a state to adopt a PSL mandate. These include: paid family and medical leave policies (National Partnership for Women & Families, 2022), PTO mandates (National Partnership for Women & Families, 2023), Medicaid income eligibility standards for pregnant women (Kaiser Family Foundation, 2023a), Temporary Assistance for Needy Families (TANF) monthly benefits for a family of four (University of Kentucky Center for Poverty Research, 2023), and demographics from the basic monthly CPS (Flood et al., 2022).[24] In robustness checking (Section 5) we report results that include a longer set of covariates and the coefficient estimates are not appreciably changed. We also control for mother's state of residence (α_s) and birth year (α_t) fixed effects. Residence state fixed effects control for time invariant state characteristics while birth year fixed effects account for changes that impact the nation as a whole in a given year. We convert nominal variables to 2022 terms using the Consumer Price Index-Urban Consumers.

We cluster standard errors by mother's residence state (Bertrand et al., 2004) and weight the data by the state female population of child-bearing age (16-44 years) using information

[24] We include the following state-level demographics: age, sex (male and female, female omitted), race (White, Black, and other race, White omitted), Hispanic ethnicity, and educational attainment (less than college versus a college degree or higher, less than college omitted).

from the U.S. Census (University of Kentucky Center for Poverty Research, 2023), and age and sex shares from the CPS (Flood et al., 2022).[25]

Our main analyses focuses on state mandates, even though some localities have also enacted rights to PSL. The rationale is that in our birth record data (see Section 3.1) we have information on location of residence but PSL mandates depend on the job location. Analysis of the American Community Survey (ACS) 2010-2019 and 2021-2022 (Ruggles et al., 2023) indicates that 19% of female employees 16-44 live in one county and work in another county, but just 2% live and work in different states.[26] As a result, errors in assigning geography will be rare at the state-level but relatively frequent when focusing on counties.[27] Adding to this concern, 35% of counties lack access to adequate maternity care which is defined as having at least one birthing facility or obstetric clinician (Simpson, 2023).[28]

For these reasons, our main analysis focuses on the mother's state of residence. However, as shown in Section 5, the results are robust to i) using the county of birth, ii) excluding states with substantial sub-state PSL mandates (e.g., California), iii) excluding groups of states with substantial cross-state commuting (e.g., the Connecticut/New Jersey/New York areas), and iv) incorporating sub-state mandates into our definition of a PSL mandate using data from the National Partnership for Women & Families (2023).

A key assumption of difference-in-differences methods is that treated and untreated units would have followed the same trends in outcomes had the policies not been implemented.[29] Therefore, we estimate a series of event-studies to explore the extent to which states that adopt and do not adopt a PSL mandate followed common trends pre-mandate, and to examine dynamics in treatment effects in the post-policy period.[30] We include five policy leads and five policy lags in our event-study, and we trim the end points, that is the $t-5$ and $t+5$ indicators are homogeneous in time-to-event.[31] Five states (Alaska, Michigan, Minnesota, Missouri, and Nebraska) adopt a PSL mandate after 2022 (see Table A1). In our main analysis, we code these states as being in the pre-treatment period. For example, in 2022 we code Minnesota as $t-2$. However, we report various alternative event-study

[25] We first construct the share of the population that is female and age 16-44 years in the basic monthly CPS in each state and year (aggregating the monthly data to the year-level). Then, we multiply that share by the state population to project the number of women ages 16-44 years in each state in each year.

[26] We use ACS-provided survey weights.

[27] To the best of our knowledge, no national data exist on the correlation between county of work and county of conception, or county where family planning services are received.

[28] The birth record data also include the location of birth occurrence. We show that results are robust to matching the PSL mandates to the birth record data on state of occurrence in Section 5.

[29] Thus, the parallel trends assumption requires restrictions on untreated potential outcome paths.

[30] We also assume no anticipation which allows us to test for common trends. Without this assumption, this analysis is a joint test of parallel trend and no anticipation.

[31] There is no omitted category in the Gardner (2022) event-study, instead coefficient estimates are implicitly normalized to the pre-treatment period mean.

specifications on different samples in Section 4 and demonstrate that our findings are not driven by the selection of a particular specification or sample period.

4 Results

4.1 Access to and use of paid sick leave, and healthcare use

Before analyzing annual birth rates, we examine the extent to which PSL mandates lead to increases in access to (proxied by employer offers) and use of (in hours per quarter) PSL (Table 1). We follow Maclean et al. (2025) in using the NCS to examine these relationships.[32] Here, and in later tables, we 'build-up' the regression specification in the following way: column (1) includes only the PSL policy variable (lagged one year) and vectors of state and year fixed effects; column (2) adds state-level policy variables (e.g., paid family leave mandates, TANF benefit levels, and Medicaid income eligibility thresholds) and state-level demographic covariates. Throughout, we emphasize the specification and sample shown in column (2), but the results are similar across specifications and samples.

Following a state PSL mandate adoption, the probability that an establishment offers PSL to employees increases by an estimated 11.3 to 12.2 percentage points (ppts), a 15.6 to 16.9% increase relative to the pre-treatment mean in PSL adopting states. The adoption of a PSL mandate increases the use of PSL by 2.1 to 2.3 hours (9.3 to 9.8%) per quarter, or just over one additional day per year.[33] Event-studies for these outcomes (using the specification and sample in column [2]) are reported in Figure A5. There is no evidence of differential pre-trends and, post-implementation, the mandate effects on access to and use of PSL appear to grow over time, likely as more employers comply with the mandates and employees learn about their benefits and accrue PSL.

To complement our analysis of the NCS, we utilize data from the ATUS measuring the extent to which adoption of a state paid sick leave mandate impacts reported access to paid leave and use of leave among women ages 16-44 years (Table 2). Following mandate adoption, the probability of reporting access to paid leave for own health increases by 14 to 16% and use of leave - measured by any use and hours of use in the past seven days - roughly doubles.

[32] Unfortunately, the NCS does not include information on the people who hold jobs, thus we cannot examine jobs held by women. However, Maclean et al. (2025) show that PSL mandates increase access to and use of PSL across a wide range of jobs including jobs that women may be disproportional likely to hold such as part-time jobs and jobs in the service industry.

[33] These results are qualitatively similar to those previously obtained by Maclean et al. (2025), although since we use different years, specifications, and establishments, they are not identical. In particular, our estimated effects are somewhat smaller than those of Maclean et al, which we attribute mainly to their exclusive focus on private establishments, which are less likely than public establishments to offer PSL when they are not mandated to do so.

We note that changes on the extensive margin (any use) do not rise to the level of statistical significance, likely due to the limited time period available (i.e., 2011, 2017, and 2018).

Next, we turn to the MarketScan data to examine the impact of state PSL mandates on use of contraception- and fertility-related healthcare service use among commercially insured women ages 16-44 years of age (Table 3). Here, we find that, following adoption of a state PSL mandate, women of child-bearing age increase their use of both contraception and fertility treatments. More specifically, oral contraceptives or LARCs, and abortion increase by 15-16% and 27-30%, respectively. We also separate oral contraceptives from LARCs and observe increases in both forms of contraceptives, though relative effect sizes are somewhat larger for oral contraceptives. We find large increases in the use of IVF services, rates nearly double post-mandate, but the baseline mean for this service is quite low which can lead to large relative effect sizes (Cotti et al., 2022). These findings suggest that PSL mandates allow women of child-bearing age to use healthcare services that may both increase and decrease birth rates, and our findings for birth rates will reflect the net effect of PSL mandates on women. Event-studies are reported in Figure A6 and display no evidence of differential pre-treands between PSL-adopting and non-PSL-adopting states. Becuase the MarketScan data are not available before 2016, we include four leads rather than five leads in the event-studies as we do in our analyses of other data sources.

A caveat to our MarketScan findings is that some women with commercial coverage may choose not to use insurance to pay for contraception- and fertility-related services, instead self-paying or relying on free care. To explore this possibility to some extent, we turn to the BRFSS. These data should include all contraception thus do not focus on healthcare services financed by a single type of payer. Table 4 presents results on the use of contraception. Following the adoption of a PSL mandate, there is a 3.0 ppt increase in the probability of any contraception use.[34] Comparing these coefficient estimates to pre-policy PSL mandate state mean (55%),[35] the probability of contraception use increases by approximately 5%.

To summarize our findings thus far, adoption of a state PSL mandate is associated with increased access to and use of PSL, and higher contraception- and fertility-related healthcare service use. With this evidence in hand, we turn to our primary objective: estimating the effects of state PSL mandates on birth rates.

[34] We replace year fixed effects with year-by-month fixed effects in the BRFSS data as we have finer time units in these data.

[35] Our baseline means for the share of women 16-44 years of age is similar to other sources using the BRFSS, see, for example, Daniels and Abma (2020).

4.2 Annual state birth rates

Estimated effects of state PSL mandates on annual birth rates are summarized in Table 5. Column (1) includes state and year fixed effects, column (2) adds state-level policy and demographic variables, with pandemic year observations added to the analysis sample in the third column. The coefficient estimates suggest 1.69 and 1.55 fewer births per 1,000 women of child-bearing age annually following mandate adoption in columns (1) and (2). Compared to the 63 per 1,000 mean birth rate in PSL-adopting states pre-policy, these coefficient estimates imply 2.7 and 2.5% declines in the annual birth rate respectively. Including the pandemic year (2020) does not change our results to any meaningful degree: births per 1,000 women 16-44 years decline by 1.58 or 2.5% post-mandate. Unless otherwise noted, the results reported below will be based on regression specifications and the sample analogous to those reported in column (2), that include state and year fixed effects, state-level policies, and state-level demographics and analyze data from 2010 to 2019 and 2021 to 2022.

Comparing the relative magnitudes of effect sizes for annual birth rates, PSL use, and use of family planning services (see Section 4.1) is informative in assessing whether our main findings are of reasonable magnitude. Using the preferred specification, PSL use increases by 2.3 hours (9.8%) per quarter, or roughly one additional day per year. We observe increases in contraceptives and abortion services, and in vitro fertilization use. We first focus on contraceptive use. Assuming that the time required for an office visit with a physician is 2.5 hours,[36] then one additional day of PSL per year would provide sufficient time to obtain a prescription for oral contraception or to have a LARC inserted, or to discuss contraception more broadly in an office visit with a clinician.[37] Notably, patients can have multiple different health issues addressed in a consultation with a healthcare professional.[38] Thus, obtaining a prescription for oral contraceptives or scheduling a LARC insertion appointment could occur in a visit when a patient receives other treatments (e.g., annual physical exam or a vaccination). Post-PSL mandate, contraceptive use among women of child-bearing age is predicted to increase by 16%, more than six times greater than the estimated (2.5%) reduction in annual birth rates just described, suggesting that changes in contraceptive

[36]The physician's office is the setting where a prescription for an oral contraceptive would most likely be written or a LARC inserted. In the National Ambulatory Medical Care Survey, the mean time spent with a physician in 2019 was 23.5 minutes (Santo and Kang, 2023). Assuming that the total time spent in the office was three times the amount of direct physician time, the overall visit time would be 70.5 minutes. Further assuming that the travel time is equal to the time spent in the office implies that the total time for the visit was 141 minutes or 2.35 hours, which we round to 2.5 hours. Our calculation involves many assumptions, but even if we assume substantially longer physician, total office, or travel times, one day of PSL would provide sufficient time for a contraception-related office visit.

[37]For reference, insertion of an IUD takes approximately five to ten minutes (`https://shorturl.at/gpIOR`, last accessed August 12th, 2023).

[38]Tai-Seale et al. (2007) show that the median number of tasks per primary care visit is six.

use may provide a primary mechanism for the declining birth rates, especially since the effectiveness of birth control methods requiring prescriptions or physician visits, such as oral contraception or LARCs, are particularly high - 90% or higher (Bailey and Lindo, 2017; Teal and Edelman, 2021). Furthermore, we observe that abortion rates increase by 27 to 31%, the time required to receive an in-clinic abortion (without complications) is typically less than one day, with medical abortions requiring less time (The Women's Center, 2025). We note that IVF use should work against the decline in birth rates, suggesting that changes in healthcare service use that prevent pregnancy outweigh those that increase this outcome. However, the success rate of IVF ranges from 45 to 55% (Moragianni and Penzias, 2010), suggesting that the estimated effect sizes for this procedure represent an upper bound on the associated increase in birth rates. Given the leave conferred by PSL mandates and the time required for these services, our main findings for birth rates appear reasonable.

Event-study estimates for our preferred birth rate specifications, reported in Figure 1, indicate that adopting and non-adopting states followed similar pre-trends, with PSL effects emerging at the implementation date and increasing over time. Stronger effects as time passes are reasonable because: 1) the mandate is often in place for only a portion of the first 'effective' year;[39] 2) employees must initially 'earn' rights to PSL by working for the employer for a pre-defined period; and 3) healthcare services are hypothesized to be a key channel linking PSL to birth rates but accessing the healthcare system may occur with a delay (e.g., if patients receive birth control during their annual physical examination, births occur approximately nine months after contraception). Notably, the fact that previous research linking California's paid medical and family leave to fertility also shows effects that emerge several years after policy adoption (Bana et al., 2020) is in line with our findings for PSL.

In Figure A7, we estimate event-studies using different specifications and samples to assess robustness. We also report our main specification and sample for comparison. First, we remove the time-varying state-level covariates from the regression. Second, we add the pandemic year (2020). Third, we treat states that adopt a PSL mandate after the end of our primary study period (2022) as untreated for all policy leads and lags (i.e., treating these states as though they never adopt a PSL mandate). Fourth, we exclude the five states that adopted a PSL mandate after 2022 (Alaska, Michigan, Minnesota, Missouri, and Nebraska). Fifth, we do not trim the data in event-time for mandate adopting states. Sixth, we begin the study period in 2000 to observe a longer pre-treatment period (we include the District of Columbia in this analysis). Finally, we focus on a balanced sample of treatment states in event-time and exclude treatment states that are not observed at least three years prior to the mandate and three years after adoption (in this latter specification, we include three

[39]Eight states have a partially treated effective year (see Table A1).

leads and lags so that the same states are used to identify coefficient estimates). The results are similar across all specifications and samples: prior to mandate adoption treatment and comparison states follow similar trends in annual birth rates and, beginning in the year in which the mandate is adopted, the trends diverge with PSL mandate-adopting states experiencing declining trends in annual birth rates relative to other states.

We report results using commonly used alternative difference-in-differences procedures proposed by Callaway and Sant'Anna (2021), Borusyak et al. (2024), and Wooldridge (2023), a stacked difference-in-differences estimator (Cengiz et al., 2019), and two-way fixed effects. Specifics of each procedure are provided in the table notes. We report results using these alternative estimators as, to the best of our knowledge, at the time of writing the literature has not yet reached consensus on the most appropriate difference-in-differences estimator. Results, which are very similar to our main findings, are reported in Table A5.

To assess the extent to which treatment effect heterogeneity and dynamics may lead to bias in TWFE coefficient estimates, we next estimate the decomposition proposed by Goodman-Bacon (2021). This decomposition breaks a TWFE estimate of the ATT into all possible two-by-two comparisons, and calculates the contribution of both 'reasonable' and 'forbidden' comparisons to the overall ATT estimates. ATT estimates for which forbidden comparisons are common reflect settings in which TWFE is likely to provide a biased estimate of the ATT in the presence of treatment effect dynamics. The decomposition results, reported in Table A4, show that our overall ATT is composed of 7% comparisons of early vs. late treated states, 90% comparisons of treated vs. never treated states, and 3% of late vs. early treated states. Thus, 97% of the comparisons that contribute to our overall TWFE ATT appear reasonable. Given our large comparison group (just 12% of state-period pairs are treated, see Table A3), these results are not surprising. Further, the estimates of the ATT are relatively homogeneous: they all carry a negative sign and range from -0.40 (early treated vs. late treated) to -1.36 (late treated vs. early treated).

4.3 Heterogeneity

We next examine heterogeneity in the effect of state PSL mandates on birth rates across mothers' demographics. This analysis is motivated by potential differential access to PSL, demand for and access to healthcare services, and other considerations. We first estimate separate regressions by mother's age at birth (<35 years and 35 or more years), age at first birth (<35 years and 35 or more years), race (White and non-White), ethnicity (Hispanic and non-Hispanic), education (less than college degree and a college degree or higher),[40]

[40]Education is missing for approximately 90% of birth records over the period 2010-2023. Thus, for our analysis of heterogeneity by education, we drop years 2010-2013 and states that adopt a PSL mandate prior

delivery model (Caesarism and vaginal), prenatal care (adequate and inadequate),[41] birth weight (low birth weight [less than 2,500 grams] and not low birth weight), and premature (pre-mature [less than 37 weeks] and not pre-mature). We adjust the population used as the denominator in the birth rate calculation for age, race, ethnicity, and education groups.[42] Since the baseline means differ substantially across these outcomes, we convert the coefficient estimates and associated 95% confidence intervals to percent changes (comparing each to the sample-specific pre-mandate mean). Results are reported in Figure 2.[43]

The effects of state PSL mandates on birth outcomes are relatively homogeneous across age at birth, ethnicity, delivery mode, birth weight, and maturity. However, there are notable differences by mother's age at first birth, race, education, and adequacy of prenatal care. Birth rates among mothers whose age at first birth was less than 35 years decline by 0.9% while births among mothers who have their first birth at age 35 years or older increase by 0.8%, which is in line with the hypothesis that younger women may use PSL to obtain contraception while older women utilize these benefits for fertility treatments that increase the probability of becoming pregnant. Birth rates among White mothers decline by 3.8% post-mandate while birth rates increase by 3.3% among non-White mothers. Our overall finding that birth rates decline appears to be driven by mothers with less than a college degree: post-mandate, birth rates among this group of mothers decline by 5.8%, with no observable change among mothers with a college degree. Finally, birth rates fall by 4.8% following adoption of a state PSL mandate among mothers who receive adequate prenatal care and increase by 1.0% among mothers who do not receive such care.[44]

4.4 Analysis of additional mechanisms: Migration, labor markets, and marriage markets

Some women may migrate towards states with PSL mandates to take advantage of the newly offered benefits, and these women may be particularly likely to use paid sick leave,

to 2014 (as these states are always treated), and use years prior to treatment (2013-2019, and 2021-2022) to define the pre-treatment period.

[41] Inadequate prenatal care is defined as fewer than seven prenatal care visits or prenatal care that began after 4th month of the pregnancy (Martin and Osterman, 2023).

[42] We construct population shares using data from the basic monthly CPS (Flood et al., 2022).

[43] We have also examined heterogeneity by insurance status - Medicaid, private, and other. Results, available on request, are inconclusive.

[44] We have bootstrapped the difference in the coefficient estimates using a nonparametric bootstrap procedure with 500 repetitions. The p-values are as follows: age: 0.149, age at first birth: 0.023, race: 0.001, ethnicity: 0.910, education: 0.327, delivery mode: 0.698, adequacy of prenatal care: 0.062, birth weight: 0.250, and maturity: 0.023.

general medical care, and family planning services.[45] Similarly, if PSL mandates encourage some women to take-up employment or increase hours worked, there may be changes in the composition of employees who are impacted by PSL mandates. If PSL mandates alter wages through employer response, improvements in productivity, or changes in benefit packages, these could be channels linking PSL mandates to birth rates. Finally, variations in birth rates could alter rates of marriage or cohabitation (e.g., pregnancy may induce some couples to enter marriage/cohabitation when they would not otherwise do so).

To investigate these possibilities, we use data from the CPS on past-year migration, poverty, any work in the past year, current employment, current full-time employment, usual hours worked per week at the main job, hourly wage (logged), and weekly earnings (logged). Difference-in-differences and event-study results are reported in Table 6 and Figure A8. We observe no change in migration propensity post PSL mandate, but we observe an increase in the probability of any work in the past year (1.4%), full-time work (1.2%), usual hours worked per week at the main job (0.8%), hourly wages (2.8%), and weekly wages (4.0%).[46] Though not statistically significant at conventional levels, the results hint that the probability of family income below the Federal Poverty Level declines while the probability of reporting current employment at the time of the basic monthly CPS increases following adoption of a state PSL mandate. Event-studies do not reveal any evidence of differential pre-treatment trends between adopting and non-adopting states. Collectively, these findings raise the possibility that state PSL mandates may influence birth rates through improved labor market outcomes (i.e., higher employment, hourly wages, and earnings) which can impact birth rates through both income and substitution effects.

In Table 7 and Figure A9 we report difference-in-differences and event-study findings for marital status in the basic monthly CPS. The coefficient estimates suggest that, post-PSL mandate, the probability of marriage declines by 1.2 ppt (2.9%) while the probability of never being married increases by 0.9 ppt (1.8%). Mandated PSL may delay or deter marriage among younger women, which may also contribute to the reduction in birth rates. Notably, all state PSL mandates include a provision for 'safe time,' which can be used to facilitate leaving a domestic violence situation (e.g., attending court proceedings). Thus, mandated PSL may allow women to leave unsafe relationships. Recent work suggests that state PSL

[45] Another possibility is that women near borders increasingly take jobs in states mandating PSL. We address this possibility below (see Section 5).

[46] While a Summers (1989) model of mandated benefits might suggest that employers would reduce wages to offset benefit costs and discrimination against women in terms of promotions would predict lower wages, previous analyses of state PSL mandates also show that wages rise post-mandate (Pichler and Ziebarth, 2020; Maclean et al., 2025) with improved productivity being a possible mechanism. Thus, our wage findings are in line with the broader U.S. PSL mandate literature and suggest that wage gains extend to women of child-bearing age.

mandates reduec child maltreatment reports and domestic violence (Deza et al., 2025).

5 Robustness checks

We conduct a series of robustness checks to test whether covariates are balanced across treated and untreated states, and to assess the stability of our main outcome - annual birth rates - across alternative specifications, samples, and time periods.

Figure A10 reports balance tests where each time-varying control variable is regressed (in separate specifications) on the lagged PSL mandate variable and state and year fixed effects. If PSL mandates are associated with these variables (indicating a lack of balance across states that do and do not adopt PSL mandates), their inclusion in the regressions could lead to bias. The covariates are generally balanced across the two groups of states. While achieving full balance is ideal, we are reassured that the results described earlier in this section are not very sensitive to excluding time-varying covariates from the regression.

Results using alternative specifications, samples, and time periods are summarized graphically in Figure A11, with our primary specification and sample also included for ease of comparison. First, we de-trend the birth rate data to guard against the possibility that adopting and non-adopting states may have followed different trend absent PSL mandate adoption. More specifically, for each treatment state we estimate a linear time trend using data prior to mandate adoption, as well as separately for each untreated states using all years of data in the sample. We subtract the estimated trends from our birth rate outcome and use the 'de-trended' birth rate variable as our outcome. Second, we construct a measure of either PTO or PSL mandate as our treatment variable. PTO is considered because, as discussed above, this mandate may offer similar benefits to women as PSL (though with less protections for workers). Third, we use alternative lag structures for PSL mandates (no lag and a two-year lag). Fourth, we estimate unweighted regression. Fifth, we exclude, sequentially, i) Maryland and Virginia and ii) the Connecticut, New Jersey, and New York tri-state area from the sample, given that many people in these areas live in one state and work in another. Sixth, we use California only as the treatment group, given the size of this state, and exclude all other PSL adopting states. Seventh, we match the PSL mandate data to the birth records on occurrence state and year; residence county and year; and residence state, year, and month. Using data at the county-year level, we then incorporate sub-state PSL mandates (here we replace state fixed effects with county fixed effects, and use the county population of child-bearing age women as the weight and the denominator in the birth rate). Eight, we include an extended set of controls (whether the state has legislation around in vitro fertilization coverage in private health insurance plans that the state can regulate (The National Infertility Association, 2023), the number of family planning clinics and offices of general physicians in each state (U.S. Census Bureau, 2022),[47] whether the state expanded Medicaid with the Affordable Care Act (Kaiser Family Foundation, 2023b), the state unemployment rate (University of Kentucky Center for Poverty Research, 2023), and whether the state borders another state with a PSL mandate in place. These additional controls may allow us to better account for confounding factors, but may be less exogenous or less balanced across treated and untreated states. Our results are robust to all of these changes, although with weaker predicted effects when we do not weight the data by the state population that is female and age 16-44 years, possibly suggesting smaller birth rate declines in less populous than large states post-mandate.

We also implement a 'leave-one-out' analysis, where each treated state is excluded from the analysis. The results, summarized in Figure A12, indicate that our findings are not driven by a particular state(s) although, not surprisingly, the coefficient estimates become considerably less precise when California is excluded. California is the largest state in the U.S. in terms of population and has a relatively generous PSL mandate (National Partnership for Women & Families, 2023). In addition, we estimate regressions where we sequentially begin the study period in each year from 2000 to 2009, and again obtain robust results. As discussed in Section 3, we chose 2010 as the year in which to begin our main analysis sample to avoid confounding from the Great Recession of 2007-2009 which likely impacted birth rates and access to healthcare, and because key aspects of the ACA, including those improving access to contraceptives where put in place in 2010. Results are reported in Figure A13. In Figure A14, we re-estimate our regressions after dropping single years of data from the analysis. The results are not sensitive to doing so. Finally, we conduct a placebo analysis in which we we randomly re-assign PSL mandates across states and re-estimate our main specification 100 times (Figure A15). Our coefficient estimate, presented farthest to the left of the figure, is an outlier relative to the placebo sample estimates.

[47] We use NAICS code 621410 to classify family planning clinics. The NAICS definition of this code is as follows: 'This industry comprises establishments with medical staff primarily engaged in providing a range of family planning services on an outpatient basis, such as contraceptive services, genetic and prenatal counseling, voluntary sterilization, and therapeutic and medically induced termination of pregnancy.' We use NAICS code 621111 to classify offices of general physicians. The NAICS definition of this code is as follows: 'This U.S. industry comprises establishments of health practitioners having the degree of M.D. (Doctor of Medicine) or D.O. (Doctor of Osteopathy) primarily engaged in the independent practice of general or specialized medicine (except psychiatry or psychoanalysis) or surgery. These practitioners operate private or group practices in their own offices (e.g., centers, clinics) or in the facilities of others, such as hospitals or HMO medical centers.' Please see `https://www.census.gov/naics/`, last accessed December 28, 2024.

6 Discussion

In this analysis, we study whether entitlements to paid sick leave allow women of child-bearing age to better facilitate their reproductive choices and how these changes affect birth rates. We use robust difference-in-differences methods and several administrative and survey data sources. We find that PSL mandates raise time spent on healthcare and the use of contraceptives among women of child-bearing age which, ceteris paribus, would be expected to lower birth rates and also increase the use of fertility treatments, which would be expected to increase birth rates. The main result is that PSL mandate adoption reduces birth rates by 2.5% in our preferred specification. PSL mandates also decrease state-to-state mobility among women of child-bearing age, potentially reflecting women's preferences towards jobs that (post-mandate) offer PSL. An analysis of labor market effects offers further insight on mechanisms: post-PSL mandate, women of child-bearing age are more likely to work full-time and earn higher wages. These findings are in line with earlier work on state PSL mandate impacts (Callison and Pesko, 2022; Maclean et al., 2025; Slopen, 2024). Alongside these findings, women are less likely to be married/live as married and more likely to remain single. The main results do not appear to be driven by common threats to identification when using difference-in-differences methods, such as a violation of parallel trends or bias from staggered treatment adoption.

Studies of other types of policy interventions also frequently find reductions in birth rates. For instance, Kearney and Levine (2009) estimate that Medicaid expansions in the late 1990s and 2000s, that extended income eligibility above traditional, federally mandated, income levels led to 2% reduction in birth rates. Notably, the use of highly effective long-acting reversible contraception was much more common during our study period than during the 1990s and 2000s.[48] Eliason et al. (2022) find a 5.3% decrease in the birth rates among low-income women of child-bearing age when the ACA expanded Medicaid, and Leguizamon (2023) estimate an 8.6% reduction.[49] The 2.5% decrease in the birth rate we obtain is therefore smaller than most estimates of ACA impacts, and as described above, PSL mandates appear to impact several potentially important determinants of birth rates.[50]

We observe some evidence of heterogeneity in PSL mandate effects across women of

[48]For example, in 1995 less than 2% of women 15-44 years of age reported use of a long-acting reversible contraception (Branum and Jones, 2015) while this rate was over 10% in 2017-2019 (Daniels and Abma, 2020). We note that the age group considered in Daniels and Abma (2020) is 15-49 years.

[49]We acknowledge that the results of this literature are somewhat mixed, with other studies (e.g. (Palmer, 2020)) finding less conclusive evidence of an effect of Medicaid expansions on birth rates.

[50]A useful approach to contextualize the impact of public policies is to conduct a Marginal Value of Public Funds (MVPF) analysis. The policy we study is an employer mandate and most employers in the U.S. are private. Thus, a MVPF analysis is not clearly appropriate in our setting.

different racial and educational backgrounds. In particular, birth rates are predicted to decline by 3.8% for White women following a PSL mandate but to increase by 3.3% among non-White women. These findings suggest that women of different racial backgrounds may use PSL for different purposes, with White women (on average) using PSL for contraception and non-White women (on average) using these benefits for activities that may increase birth rates (e.g., potentially improved economic standing documented by Slopen (2024) post-mandate may lead to an increase in demand for children among non-White women). PSL mandates lead to 4.8% reductions in birth rates among women with less than a college degree but little or no effect for women with a college degree. One possible reason is that college-educated women are more likely to work in jobs that offer PSL absent a state mandate. Consistent with this hypothesis, using data from the 2011 American Time Use Survey Leave Module (Flood et al., 2023), we estimate that 69.4% of 16-44 year old women with a college degree report PSL access versus just 47.0% of women with less than a college degree.

Some lawmakers are advocating for a national PSL mandate and there is considerable public support Americans for such a policy (Global Strategy Group & Paid Leave for All Action, 2021; Sanders, 2023). We can use our findings to provide a rough estimate of the potential impact of such a federal policy on the national birth rate. In 2022, there were 3,667,758 registered births in the U.S. (Martin et al., 2022), 2,468,155 occurring in states without a PSL mandate. Assuming a federal mandate of similar generosity to the state mandates we examine, our preferred model predicts a reduction of around 61 thousand births nationally (=2,468,155 × -0.0246 = 60,717).[51]

Previous research indicates that PSL mandates increase the ability of employees to take leaves when needed, raise access to valuable medical care while decreasing the use of unnecessary emergency care, and improve health outcomes. Our findings add to this growing literature by showing that these policies may also help in facilitating the reproductive choices of American women and families. Understanding how PSL policies influence pregnancy outcomes has become even more salient given recent policy changes in the U.S. that limit women's access to family planning services.[52] There are potential broader questions about

[51]We likely modestly overstate the effects as we do not account for women already covered by sub-state policies in untreated states. Based on legal analysis conducted by National Partnership for Women & Families (2023), there were five such sub-state policies in 2021: Chicago/Cook County, Illinois; Duluth, Minnesota; Minneapolis/Saint Paul, Minnesota; Philadelphia, Pennsylvania; and Pittsburgh, Pennsylvania. These mandates are estimated by National Partnership for Women & Families (2023) to provide coverage to 1,337,300 employees, or around 0.4% of the population.

[52]In particular, in 2023, the Supreme Court determined that the U.S. Constitution does not confer the right to have an abortion (Dobbs vs. Jackson Women's Health Organization), thus overturning landmark cases that provided this protection to pregnant women (Roe v Wade [1973] and Planned Parenthood v. Casey [1992]). In turn, this decision gave states the greater ability to regulate abortion and, by the time of writing, 15 states have banned abortion (McCann et al., 2023).

the effects of any resulting decline in fertility on national welfare (e.g., through the indirect effects on the financing of government programs for seniors). However, even if increasing birth rates at the national level was viewed as desirable, it is difficult to imagine that the best way of accomplishing this is by restricting the reproductive choices of women.

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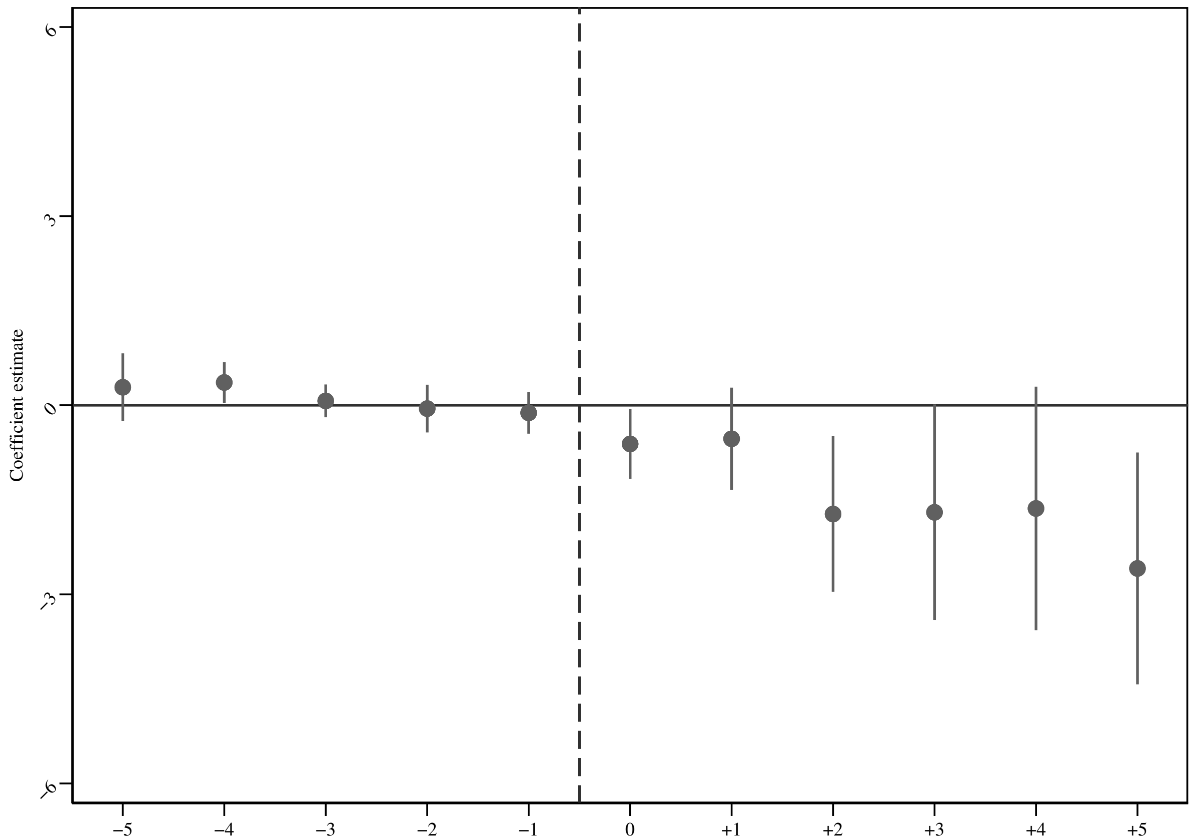
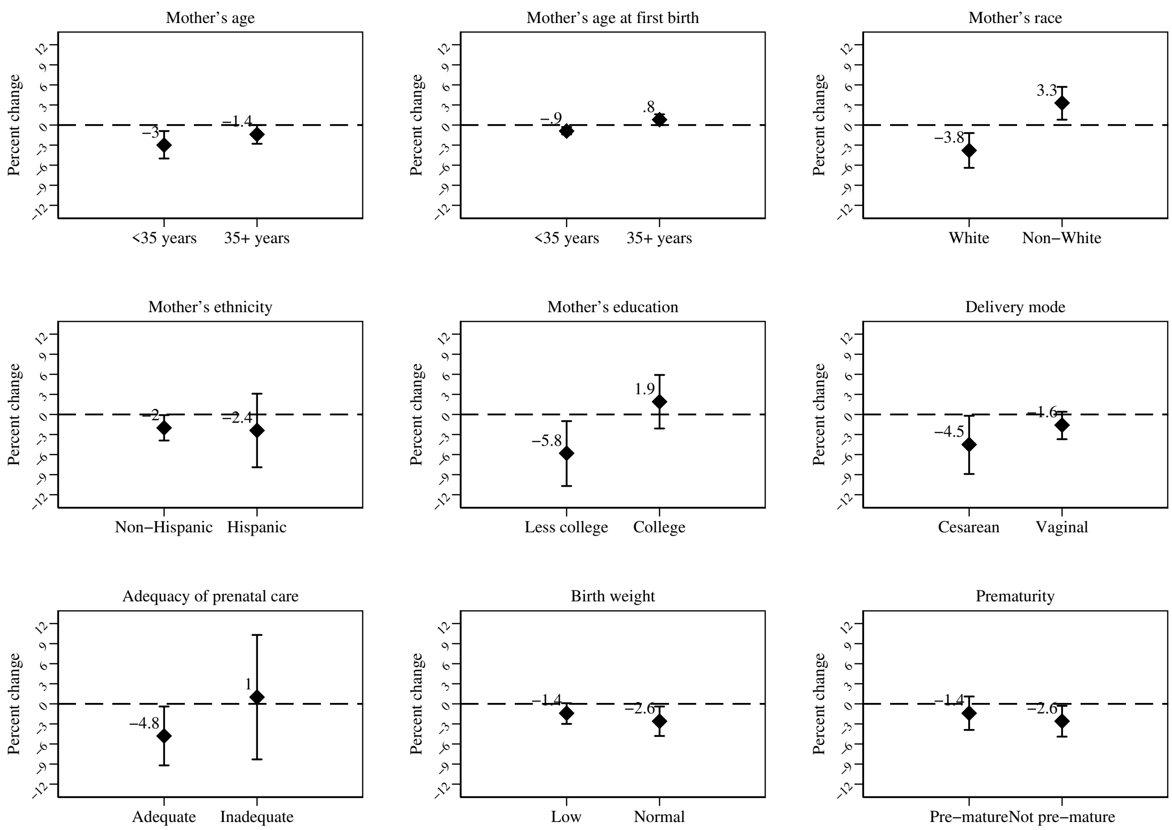
7 Tables and figures

Figure 1: Effect of a state paid sick leave mandate on birth rates per 1,000 women 16-44 years using an event-study: National Center for Health Statistics 2010 to 2019 and 2021 to 2022



Notes: The outcome in the regression is the annual state-level birth rate among women ages 16-44 years. The unit of observation is a state in a year. We utilize a two-stage difference-in-differences procedure proposed by Gardner (2022). The regression is estimated with OLS and includes state policies and demographics, and state and year fixed effects. The leads and lags represent single-year bins corresponding to five years pre-mandate through five years post-mandate. There is no omitted category in the Gardner (2022) procedure. The sample excludes observations more than five years prior to the mandate and more than five years after the event for mandate-adopting states. Data are weighted by the state-year population of women 16-44 years. The District of Columbia is excluded from the sample as this locality is always treated during our study period. Coefficient estimates are reported with circles. 95% confidence intervals are reported with vertical lines and account for within state clustering.

Figure 2: Heterogeneity in the effect of a state paid sick leave mandate on annual birth rates per 1,000 women ages 16-44 years by mother's demographics and birth characteristics: National Center for Health Statistics 2010 to 2019 and 2021 to 2022



Notes: Coefficient estimates and 95% confidence intervals are converted to percent changes relative to the mean value in treatment states prior to treatment. The outcome in the regressions is the annual state-level birth rate among women ages 16-44 years. The unit of observation is a state in a year. The regressions are estimated with a two-stage difference-in-differences procedure proposed by Gardner (2022). The regressions are estimated with OLS and include state policies and demographics, and state and year fixed effects. Data are weighted by the state-year population of women 16-44 years. The District of Columbia is excluded from the sample as this locality is always treated during our study period. Coefficient estimates are reported with circles. 95% confidence intervals are reported with vertical lines and account for within state clustering.

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

Specification:	(1)	(2)
Panel A: Access		
Paid sick leave mandate	0.113***	0.122***
	(0.024)	(0.028)
Percent change	15.66%	16.91%
Pre-treatment mean, treatment states	0.7214	0.7214
Panel B: Use (hours)		
Paid sick leave mandate	2.145***	2.268***
	(0.688)	(0.656)
Percent change	9.27%	9.81%
Pre-treatment mean, treatment states	23.1269	23.1269
Observations	691388	691388
State policies	N	Y
State demographics	N	Y

Notes: The outcomes in the regressions are an indicator for access to paid sick leave (Panel A) and hours of paid sick leave use per quarter (Panel B). The unit of observation is a job in an establishment in state in a year. The pre-treatment period is defined as 2009-2011, the period before any states in the sample adopt a paid sick leave mandate. All regressions are estimated with a two-stage difference-in-differences procedure proposed by Gardner (2022). The state paid sick leave mandate is lagged one year. All regressions are estimated with OLS and include state and year fixed effects. Specification 2 also includes state-level policy and demographic variables. Data are weighted by National Compensation Survey sample weights. The District of Columbia is excluded from the analysis as this locality is always treated during our study period. Standard errors are clustered at the state-level and are reported in parentheses.
***, **, and * = statistically different from zero at the 1%, 5%, and 10% level.

Table 2: Effect of a state paid sick leave mandate on leave access and use among women 16-44 years: American Time Use Survey Leave Module 2011, 2017, & 2018

Outcome:	Any paid leave - health	Use leave	Hours of leave used
Panel A: No time-varying covariates	0.08	0.12***	1.40*
	(0.09)	(0.03)	(0.76)
Percent change	16.33%	80.00%	100.00%
Panel B: Time -varying covariates	0.07	0.13***	1.55*
	(0.08)	(0.04)	(0.84)
Percent change	14.29%	86.67%	110.71%
2011 mean, treatment states	0.49	0.15	1.40
Observations	96092	97730	97638

Notes: The outcomes are i) reporting paid leave at job, ii) any leave used in the past five days, and iii) hours of leave used in the past five days. The unit of observation is a respondent in state in a year. The pre-treatment period is defined as 2011, the period before any states in the sample adopt a paid sick leave mandate. All regressions are estimated with a two-stage difference-in-differences procedure proposed by Gardner (2022). The state paid sick leave mandate is lagged one year. All regressions are estimated with OLS and include a fixed effect for a weekday interview, and state and year fixed effects. Specification 2 also includes respondent level demographics and state policy variables. Data are weighted by American Time Use Survey Leave Module sample weights. The District of Columbia is excluded from the analysis as this locality is always treated during our study period. Standard errors are clustered at the state-level and are reported in parentheses.

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

Table 3: Effect of a state paid sick leave mandate on contraception, abortion, and fertility treatment use among women 16-44 years: MarketScan commercial claims data 2016-2019 & 2021-2022

Specification:	No time-varying covariates	Time-varying covariates
Oral or long-acting reversible contraceptives	23.44**	22.04**
	(10.89)	(9.65)
Percent change	15.98%	15.02%
2010-2011 mean, treatment states	147	147
Oral contraceptives	17.67**	18.21**
	(8.93)	(7.91)
Percent change	17.24%	17.76%
2010-2011 mean, treatment states	102	102
Long-acting reversible contraceptives	5.78**	3.83
	(2.35)	(2.93)
Percent change	13.07%	8.67%
2010-2011 mean, treatment states	44	44
Abortion services	1.92**	1.67***
	(0.84)	(0.55)
Percent change	30.82 %	26.78%
2010-2011 mean, treatment states	6	6
In vitro fertilization services	1.31**	1.41**
	(0.65)	(0.56)
Percent change	76.54%	82.33%
2010-2011 mean, treatment states	2	2
Observations	288	288

Notes: The outcomes are claim counts per 1,000 beneficiaries who are female and 16-66 years of age i) oral or long-acting reversible contraceptives, ii) oral contraceptives, iii) long-acting reversible contraceptives, iv) abortions, and v) in vitro fertilization. The unit of observation is a beneficiary in state in a year. The pre-treatment period is defined as 2011, the period before any states in the sample adopt a paid sick leave mandate. All regressions are estimated with a two-stage difference-in-differences procedure proposed by Gardner (2022). The state paid sick leave mandate is lagged one year. All regressions are estimated with OLS and state policy variables, state fixed effects, and year fixed effects. Data are weighted by the state number of beneficiaries who are female and 16-66 years of age in 2016. Connecticut and the District of Columbia is excluded from the analysis as these localities are always treated during our study period. Standard errors are clustered at the state-level and are reported in parentheses.
***, **, and * = statistically different from zero at the 1%, 5%, and 10% level.

Table 4: Effect of a state paid sick leave mandate on contraception use among women 18-44 years: Behavioral Health Risk Surveillance Survey 2010 to 2019 and 2022

Specification:	(1)	(2)
Paid sick leave mandate	0.03*	0.03**
	(0.02)	(0.01)
Percent change	5.38%	5.95%
2010-2011 mean, treatment states	0.55	0.55
Observations	54434	54434
State policies	N	Y
Demographics	N	Y

Notes: The outcomes in the regressions are binary indicator variables for any contraception use. The unit of observation is a respondent in state in a year. The pre-treatment period is defined as 2010-2011, the period before any states in the sample adopt a paid sick leave mandate. We utilize a two-stage difference-in-differences procedure proposed by Gardner (2022). The state paid sick leave mandate is lagged one year. All regressions are estimated with OLS and include state and year fixed effects. Specification 2 also includes state-level policy and respondent demographic variables. Data are weighted by Behavioral Risk Factor Surveillance Survey sample weights. The District of Columbia is excluded from the analysis as this locality is always treated during our study period. Standard errors are clustered at the state-level and are reported in parentheses.
***, **, and * = statistically different from zero at the 1%, 5%, and 10% level.

Table 5: Effect of a state paid sick leave mandate on state annual birth rates per 1,000 women 16-44 years: National Center for Health Statistics 2010 to 2019 and 2021 to 2022

Specification:	(1)	(2)	(3)
Paid sick leave mandate	-1.69*	-1.55**	-1.58**
	(0.91)	(0.66)	(0.73)
Percent change	-2.67%	-2.45%	-2.50%
State policies	N	Y	Y
State demographics	N	Y	Y
Includes 2020	N	N	Y
2010-2011 mean, treatment states	63	63	63
Observations	600	600	650

Notes: The outcome in the regressions is the annual state-level birth rate per 1,000 women 16-44 years. The unit of observation is a state in a year. The pre-treatment period is defined as 2010-2011, the period before any states in the sample adopt a paid sick leave mandate. We utilize a two-stage difference-in-differences procedure proposed by Gardner (2022). The state paid sick leave mandate is lagged one year. All regressions are estimated with OLS and include state and year fixed effects. Specification 2 also includes state-level policy and demographic variables. Data are weighted by the female population ages 18-44 years of age. The District of Columbia is excluded from the analysis as this locality is always treated during our study period. Standard errors are clustered at the state-level and are reported in parentheses.
***,**,* = statistically different from zero at the 1%,5%,10% level.

Table 6: Effect of a state paid sick leave mandates on migration and employment outcomes: Current Population Survey 2010 to 2019 and 2021 to 2022

Outcome:	Coefficient estimate (Standard error)
Past year across-state migration	-0.001
	(0.002)
Percent change	-4.000%
2010-2011 mean, treatment states	0.025
Observations	380121
Any work in the past year	0.009**
	(0.004)
Percent change	1.362%
2010-2011 mean, treatment states	0.661
Observations	382013
Family income < Federal Poverty Level	-0.006
	(0.004)
Percent change	-3.390%
2010-2011 mean, treatment states	0.177
Observations	382013
Usual hours per week at main job	0.275**
	(0.131)
Percent change	0.831%
2010-2011 mean, treatment states	33.095
Observations	2147300
Any employment	0.003
	(0.004)
Percent change	0.334%
2010-2011 mean, treatment states	0.898
Observations	2329039
Full-time employment	0.009*
	(0.005)
Percent change	1.240%
2010-2011 mean, treatment states	0.726
Observations	2183422
Hourly wage among those with positive wages†	0.028***
(logarithm)	(0.007)
Percent change	2.84%
2010-2011 mean, treatment states ($)	14.180
Observations	320843
Weekly earnings among those with positive earnings††	0.039***
(logarithm)	(0.011)
Percent change	3.98%
2010-2011 mean, treatment states ($)	18.958
Observations	327563

Notes: The unit of observation is a respondent in state in a year. The pre-treatment period is defined as 2010-2011, the period before any states in the sample adopt a paid sick leave mandate. All regressions are estimated with a two-stage difference-in-differences procedure proposed by Gardner (2022). The state paid sick leave mandate is lagged one year. All regressions are estimated with OLS and include respondent demographics, state characteristics, and state and year fixed effects. Data are weighted by Annual Social and Economic Supplement to the Current Population Survey sample weights. The District of Columbia is excluded from the analysis as this locality is always treated during our study period. Standard errors are clustered at the state-level and are reported in parentheses.
***,**,* = statistically different from zero at the 1%,5%,10% level.

†This question is included in the Current Population Survey Outgoing Rotation/Earner Study and is reported by workers who are paid hourly. Data are weighted by the Current Population Survey earnings weight. Percent change is calculated as follows: $(exp^{\hat{\beta}} - 1) * 100$.

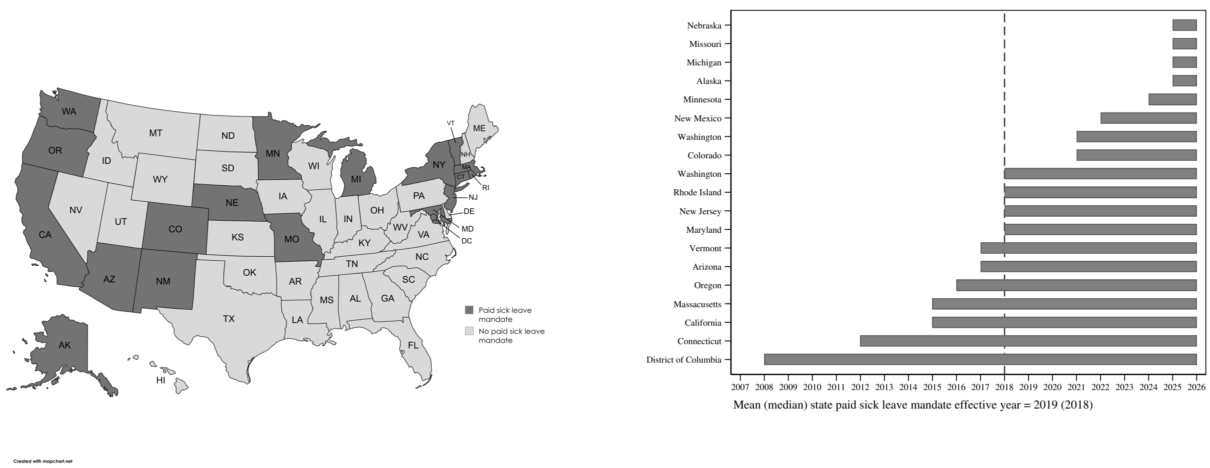
††This question is included in the Current Population Survey Outgoing Rotation/Earner Study and is reported by workers who are not paid hourly. Data are weighted by the Current Population Survey earnings weight. Percent change is calculated as follows: $(exp^{\hat{\beta}} - 1) * 100$.

Table 7: Effect of a state paid sick leave mandates on marriage outcomes: Current Population Survey 2010 to 2019 and 2021 to 2022

Outcome:	Coefficient estimate (Standard error)
Married or living as married	-0.012***
	(0.003)
Percent change	-2.906%
2010-2011 mean, treatment states	0.413
Observations	3363072
Divorced and separated	0.001
	(0.002)
Percent change	0.877%
2010-2011 mean, treatment states	0.114
Observations	3363072
Widowed	-0.001
	(0.000)
Percent change	-20.000%
2010-2011 mean, treatment states	0.005
Observations	3363072
Never married	0.009***
	(0.003)
Percent change	1.811%
2010-2011 mean, treatment states	0.497
Observations	3363072

Notes: The unit of observation is a respondent in state in a year. The pre-treatment period is defined as 2010-2011, the period before any states in the sample adopt a paid sick leave mandate. All regressions are estimated with a two-stage difference-in-differences procedure proposed by Gardner (2022). The state paid sick leave mandate is lagged one year. All regressions are estimated with OLS and include respondent demographics, state characteristics, and state and year fixed effects. Data are weighted by basic monthly Current Population Survey sample weights. The District of Columbia is excluded from the analysis as this locality is always treated during our study period. Standard errors are clustered at the state-level and are reported in parentheses.
***,**,* = statistically different from zero at the 1%,5%,10% level.

Figure A1: Geographic & temporal distribution of all effective or announced state paid sick leave mandates in the U.S. as of July 2025



Notes: This figure reports the states and districts that have adopted or announced a paid sick leave mandate by July 2025. Data sources are National Partnership for Women & Families (2023), A Better Balance (2025), and Lieb (2025). The District of Columbia is excluded from the analysis sample as this locality is always treated during our study period. See Table A1 for exact mandate effective dates.

Figure A2: Massachusetts notice of paid sick leave benefits to employees

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?

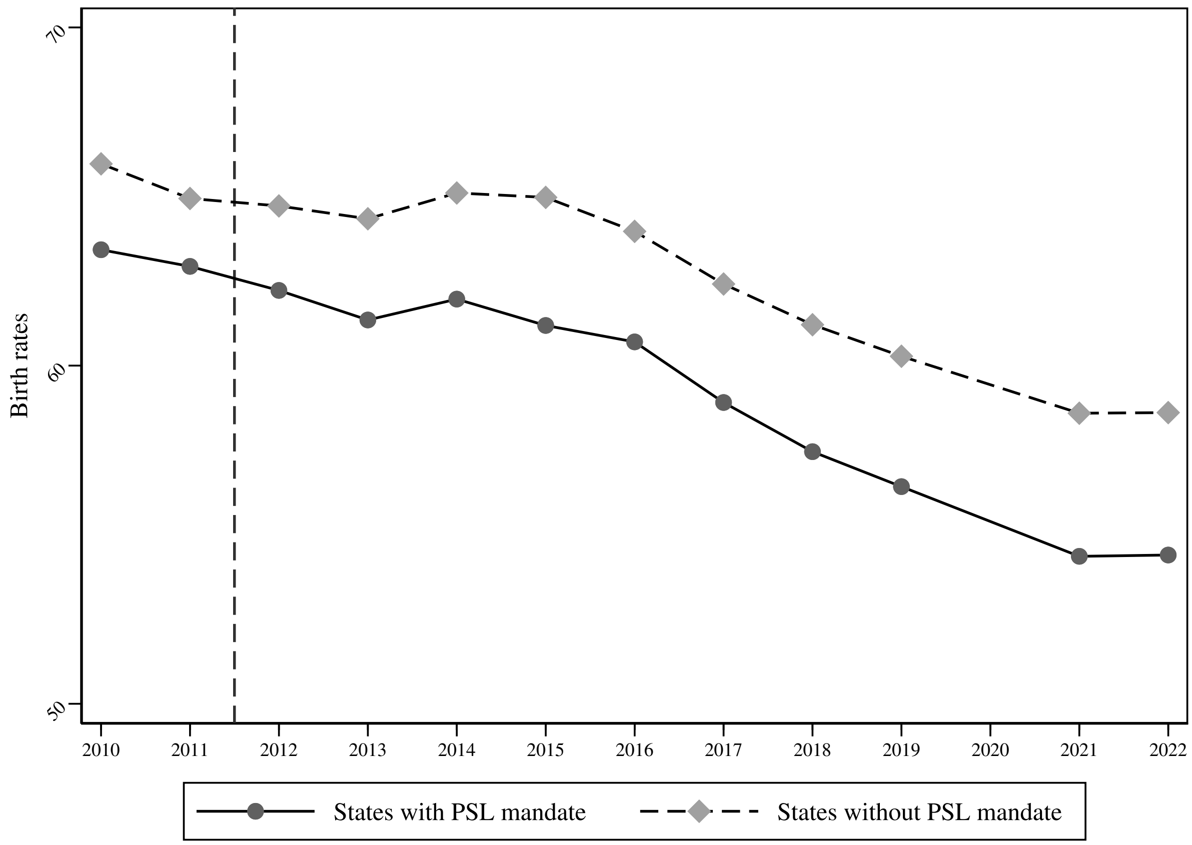
Call the Fair Labor Division at 617-727-3465 ○ **Visit** www.mass.gov/ago/earnedsicktime

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/earnedsicktime.

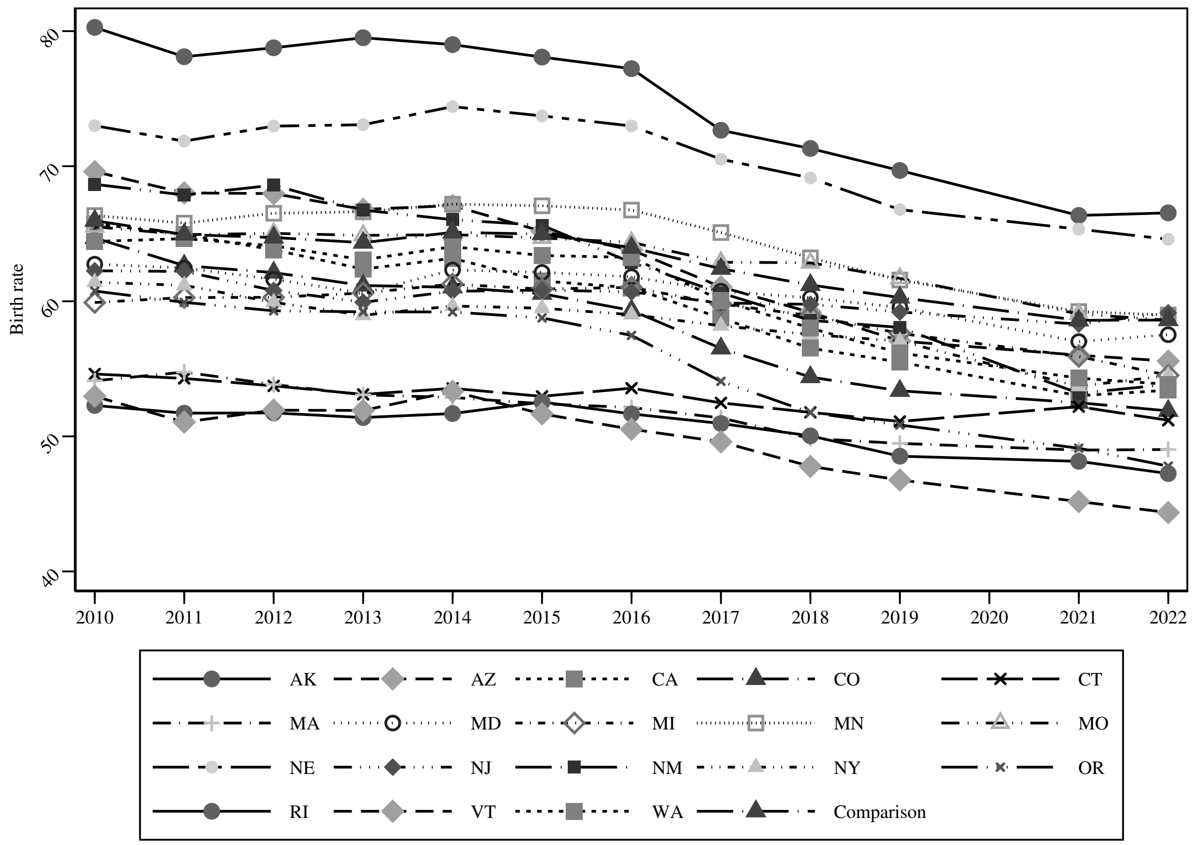
Notes: This figure reports the announcement that employers must post in the workplace to be compliant with the Massachusetts paid sick leave mandate. Source: Massachusetts Office of the Attorney General (https://www.mass.gov/info-details/earned-sick-time, last accessed May 26, 2023).

Figure A3: Trends in birth rates: National Center for Health Statistics 2010 to 2019 and 2021 to 2022



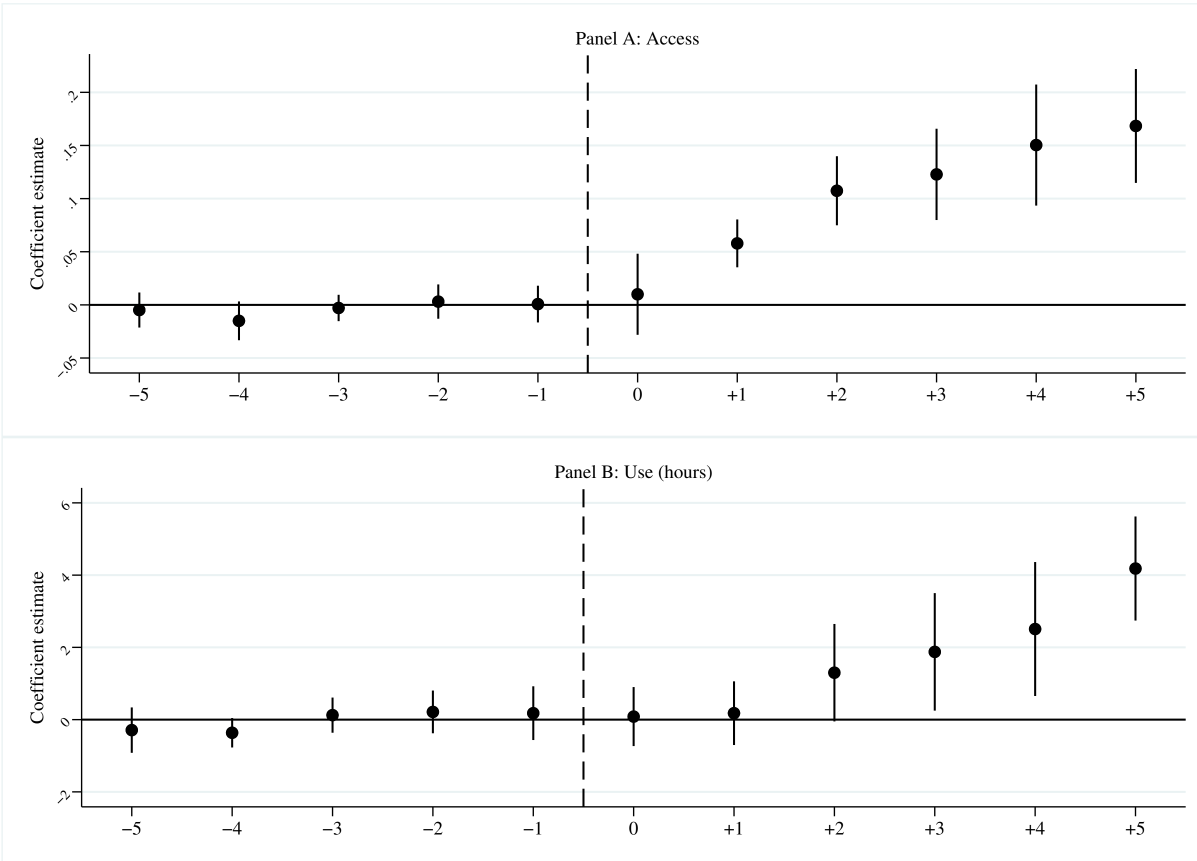
Notes: This figure reports trends in birth rates for states that do and do not adopt a paid sick leave mandate by July 2025. Data are weighted by the state-year population of women 16-44 years of age and aggregated to the state-year-treatment level. The dashed vertical line divides the sample into the time period before and after the first state (Connecticut) adopts a paid sick leave mandate. The District of Columbia is excluded from the analysis sample as this locality is always treated during our study period.

Figure A4: Trends in birth rates by treatment state: National Center for Health Statistics 2010 to 2019 and 2021 to 2022



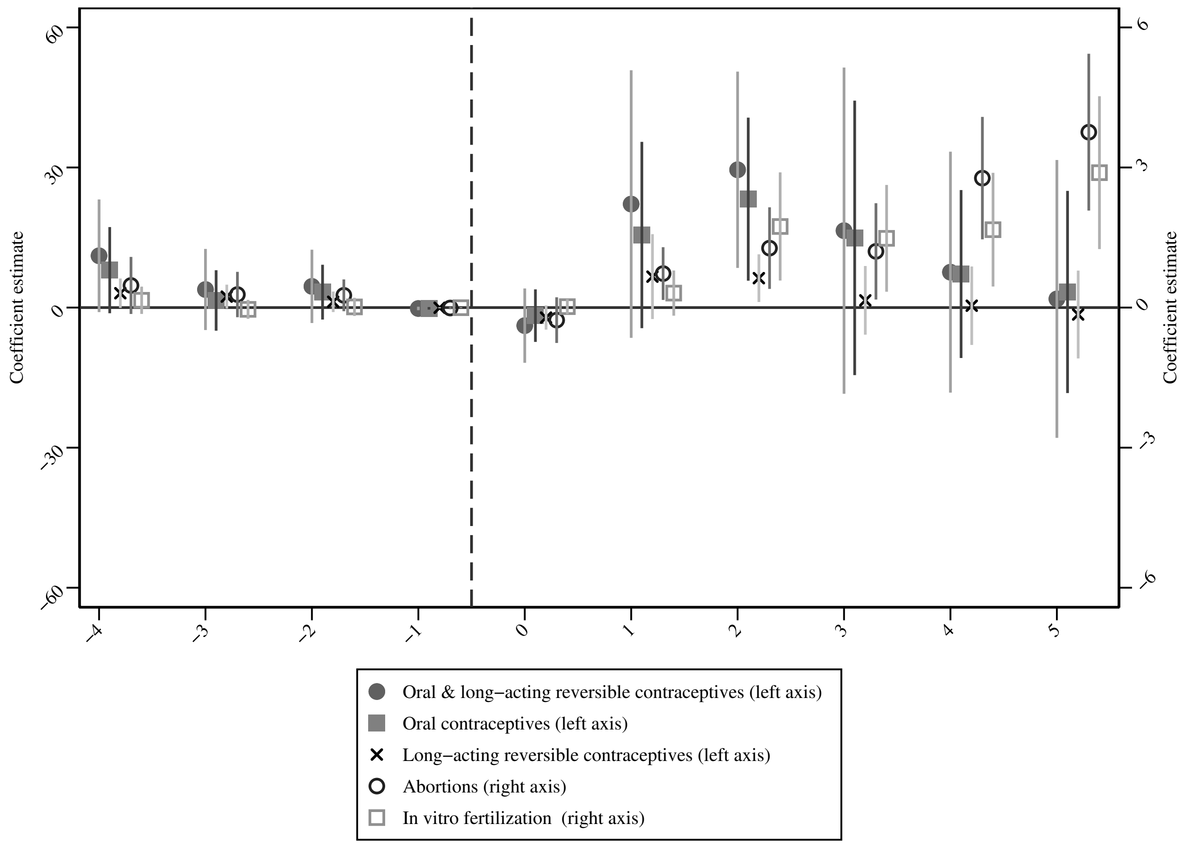
Notes: This figure reports trends in birth rates separately for each state that adopts a paid sick leave mandate by July 2025, and for all other states. Data are weighted by the state-year population of women 16-44 years of age and aggregated to the state-year-treatment level. Comparison = all states that do not adopt or announce a paid sick leave mandate by October 2023. The District of Columbia is excluded from the analysis sample as this locality is always treated during our study period.

Figure A5: Effect of a state paid sick leave mandate on access to and use of paid sick leave using event-study: National Compensation Survey 2009-2022



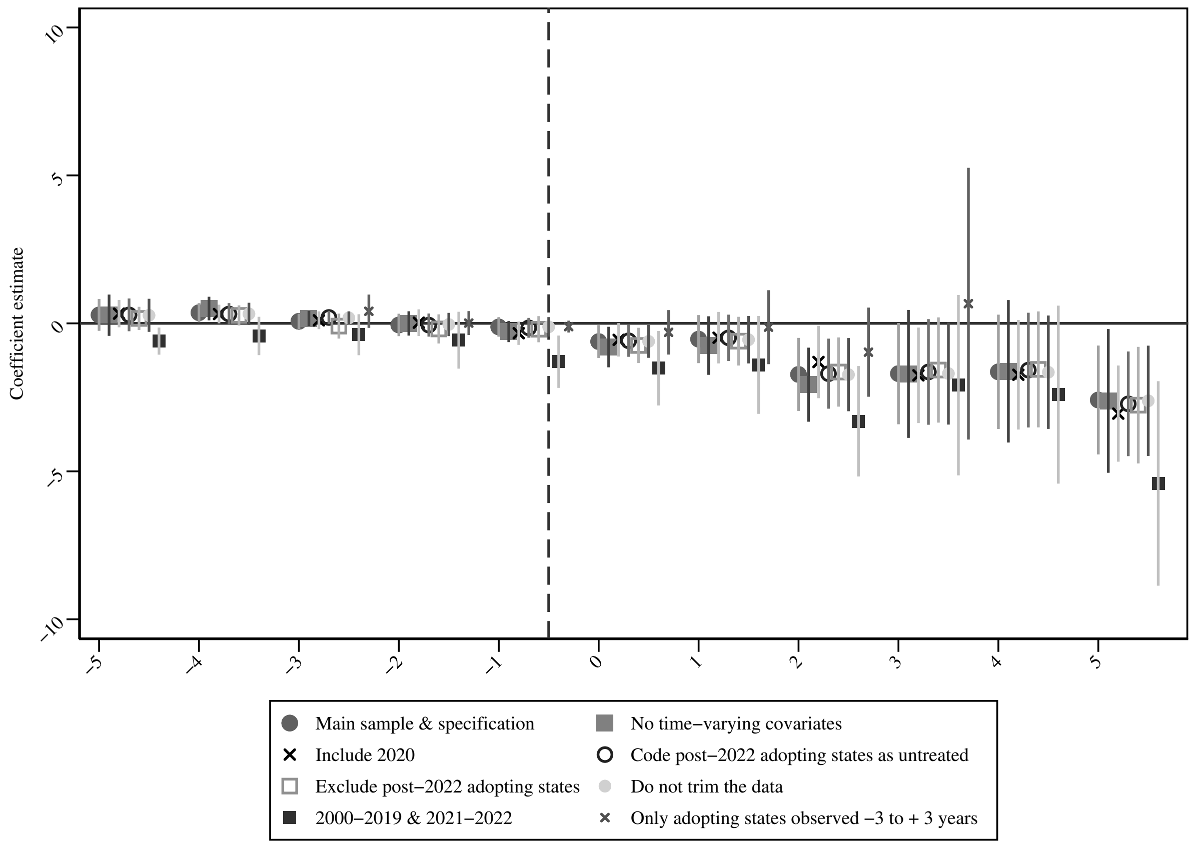
Notes: This figure reports results from regressing an indicator for access to paid sick leave (Panel A) and the average number of hours of paid sick leave use per quarter (Panel B) on indicators for time to paid sick leave mandate. The unit of observation is a job in an establishment in state in a year. We utilize a two-stage difference-in-differences procedure proposed by Gardner (2022). All regressions are estimated with OLS and include job characteristics, state characteristics, and state and year fixed effects. The leads and lags represent single-year bins corresponding to five years pre-mandate through five years post-mandate. There is no omitted category in the Gardner (2022) procedure. The sample excludes observations more than five years prior to the mandate and more than five years after the event for mandate-adopting states. Data are weighted by National Compensation Survey weights. The District of Columbia is excluded from the analysis as this locality is always treated during our study period. Coefficient estimates are reported with circles. 95% confidence intervals are reported with vertical lines and account for within state clustering.

Figure A6: Effect of a state paid sick leave mandate on contraception, abortion, and fertility treatments using event-study: MarketScan commercial claims 2016-2022



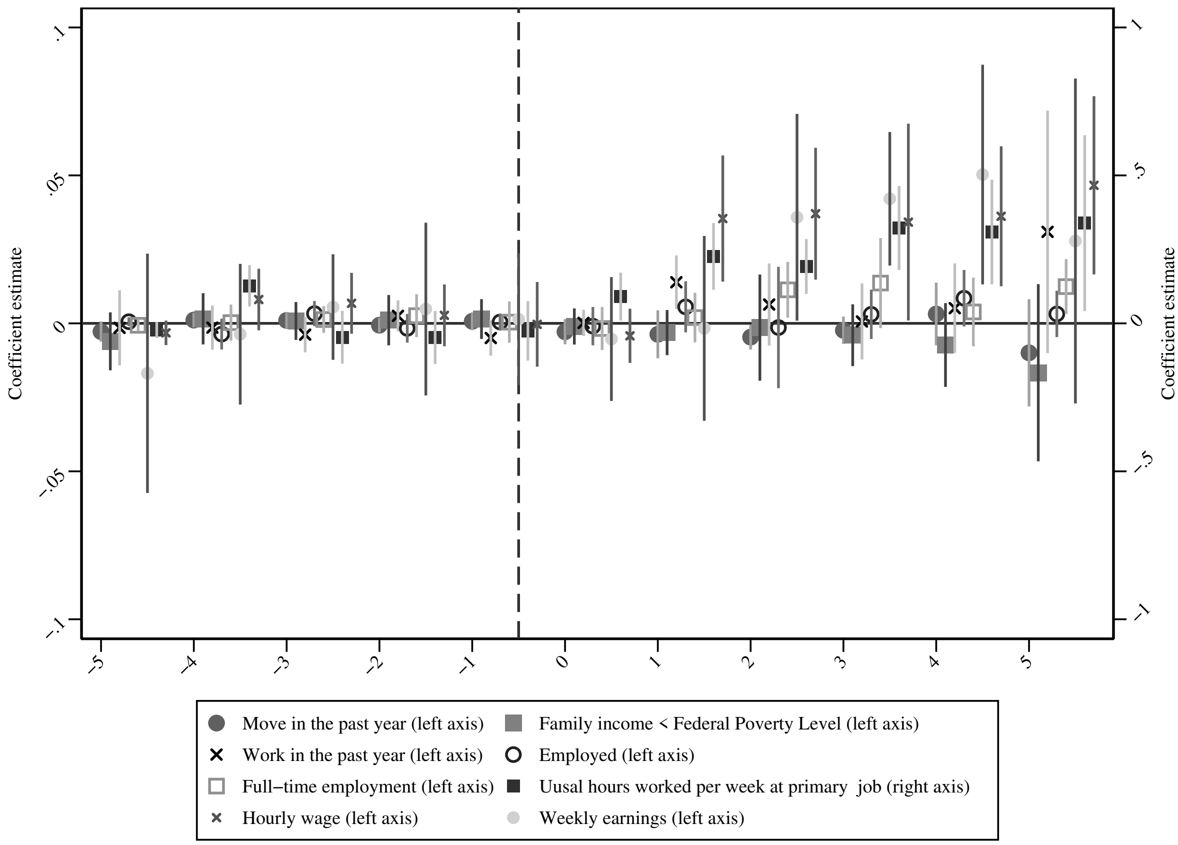
Notes: This figure reports results from regressing contraception, abortion, and fertility treatment rates per 1,000 female beneficiaries 16-44 years on indicators for time to paid sick leave mandate. The unit of observation is a state in a quarter-year. We utilize a two-stage difference-in-differences procedure proposed by Gardner (2022). All regressions are estimated with OLS and include state characteristics, and state and quarter-year fixed effects. The leads and lags represent single-year bins corresponding to five years pre-mandate through five years post-mandate. There is no omitted category in the Gardner (2022) procedure. The sample excludes observations more than five years prior to the mandate and more than five years after the event for mandate-adopting states. Data are weighted by the number of female beneficiaries 16-44 years of age in 2016. Connecticut and the District of Columbia are excluded from the analysis sample as these localities are always treated during our study period. Coefficient estimates are reported with shapes. 95% confidence intervals are reported with vertical lines and account for within state clustering.

Figure A7: Effect of a state paid sick leave mandate on annual birth rates per 1,000 women 16-44 years using different event-study specifications and samples: National Center for Health Statistics 2010 to 2019 and 2021 to 2022



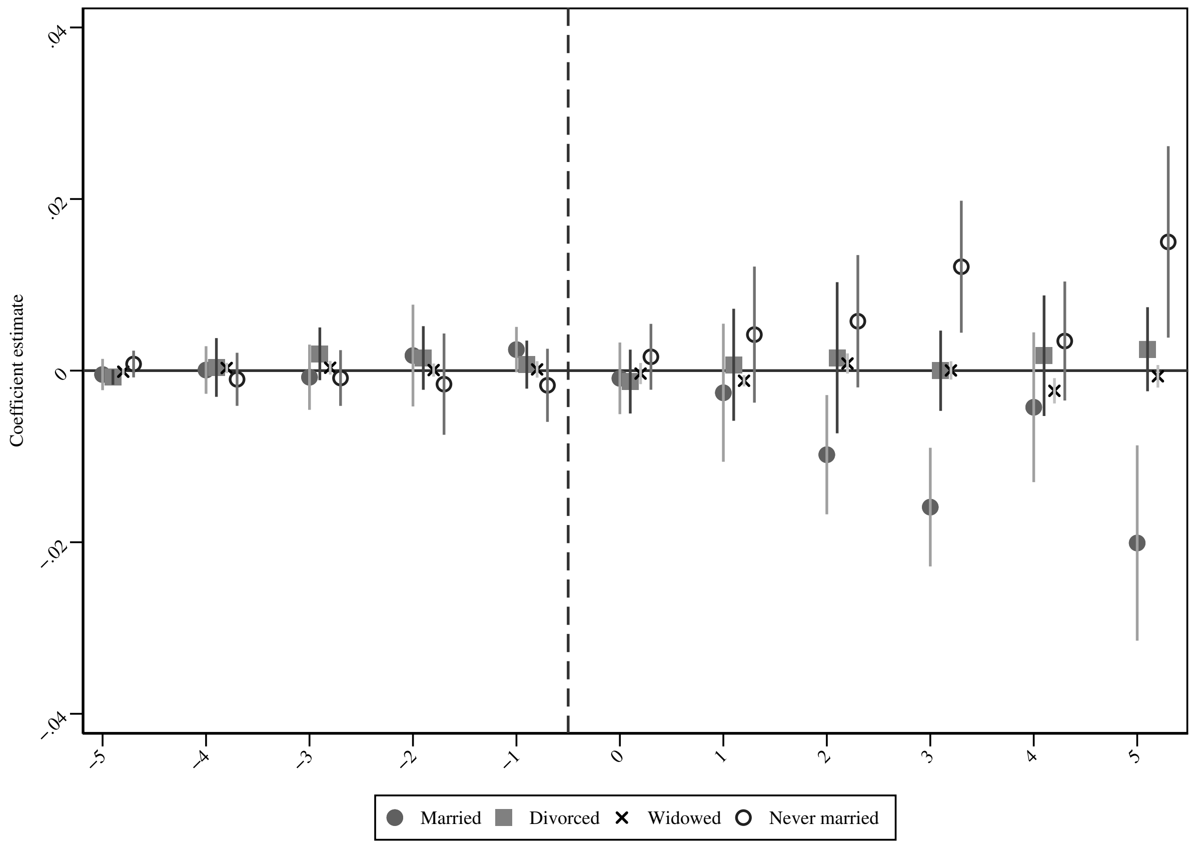
Notes: This figure reports results from regressing the annual state-level birth rate among women ages 16-44 years on indicators for time to paid sick leave mandate using different samples and specifications. The unit of observation is a state in a year. We utilize a two-stage difference-in-differences procedure proposed by Gardner (2022) unless otherwise noted. The regressions are estimated with OLS and includes state characteristics, and state and year fixed effects unless otherwise noted. The leads and lags represent single-year bins corresponding to five years pre-mandate through five years post-mandate. There is no omitted category in the Gardner (2022) procedure. In the two-way fixed effects specification, the omitted category is -1. The sample excludes observations more than five years prior to the mandate and more than five years after the event for mandate-adopting states. Data are weighted by the state-year population of women 16-44 years of age unless otherwise noted. The District of Columbia is excluded from the analysis sample as this locality is always treated during our study period. Coefficient estimates are reported with shapes. 95% confidence intervals are reported with vertical lines and account for within state clustering.

Figure A8: Effect of state paid sick leave mandate on migration and economic outcomes among women 16-44 years using an event-study: Current Population Survey 2010 to 2019 and 2021 to 2022



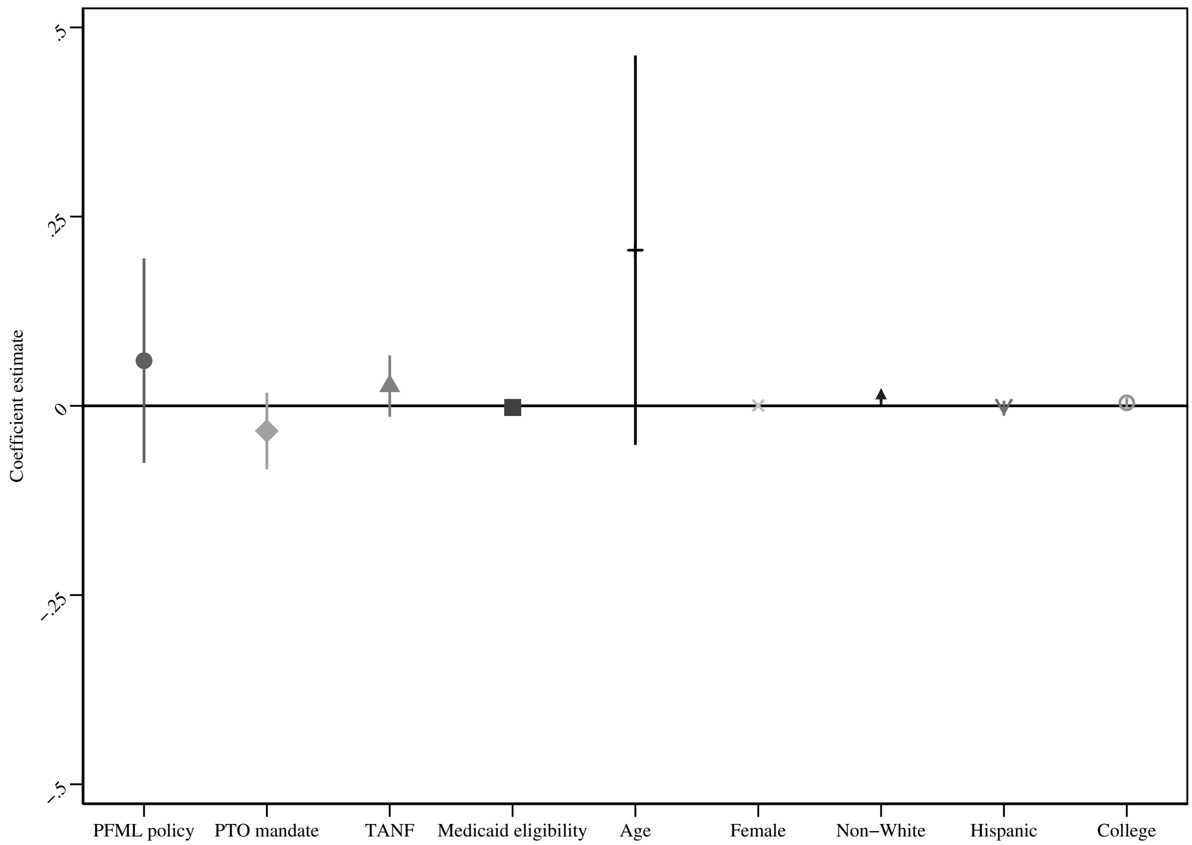
Notes: This figure reports results from regressing the past-year across state migration and economic outcomes among women ages 16-44 years on indicators for time to paid sick leave mandate. The unit of observation is a respondent in state in a month-year in the Annual Social and Economic Supplement to the basic monthly Current Population Survey (migration, family income below the Federal Poverty Level, and any work in the past year) and a respondent in a state in a year in the Current Population Survey (all other variables). We utilize a two-stage difference-in-differences procedure proposed by Gardner (2022). The regressions are estimated with OLS and include respondent demographics, state characteristics, and state fixed effects; in analyses of the Annual and Social Economic Supplement to the Current Population Survey we include quarter-year fixed effects and in analyses of the basic monthly Current Population Survey we include year fixed effects. The leads and lags represent single-year bins corresponding to five years pre-mandate through five years post-mandate. There is no omitted category in the Gardner (2022) procedure. The sample excludes observations more than five years prior to the mandate and more than five years after the event for mandate-adopting states. Data are weighted by Annual Social and Economic Supplement to the Current Population Survey, basic monthly Current Population, or Outgoing Rotation/Earner survey weights. The District of Columbia is excluded from the analysis sample as this locality is always treated during our study period. Coefficient estimates are reported with shapes. 95% confidence intervals are reported with vertical lines and account for within state clustering.

Figure A9: Effect of state paid sick leave mandate on marital status outcomes among women 16-44 years using an event-study: Basic monthly Current Population Survey 2010 to 2019 and 2021 to 2022



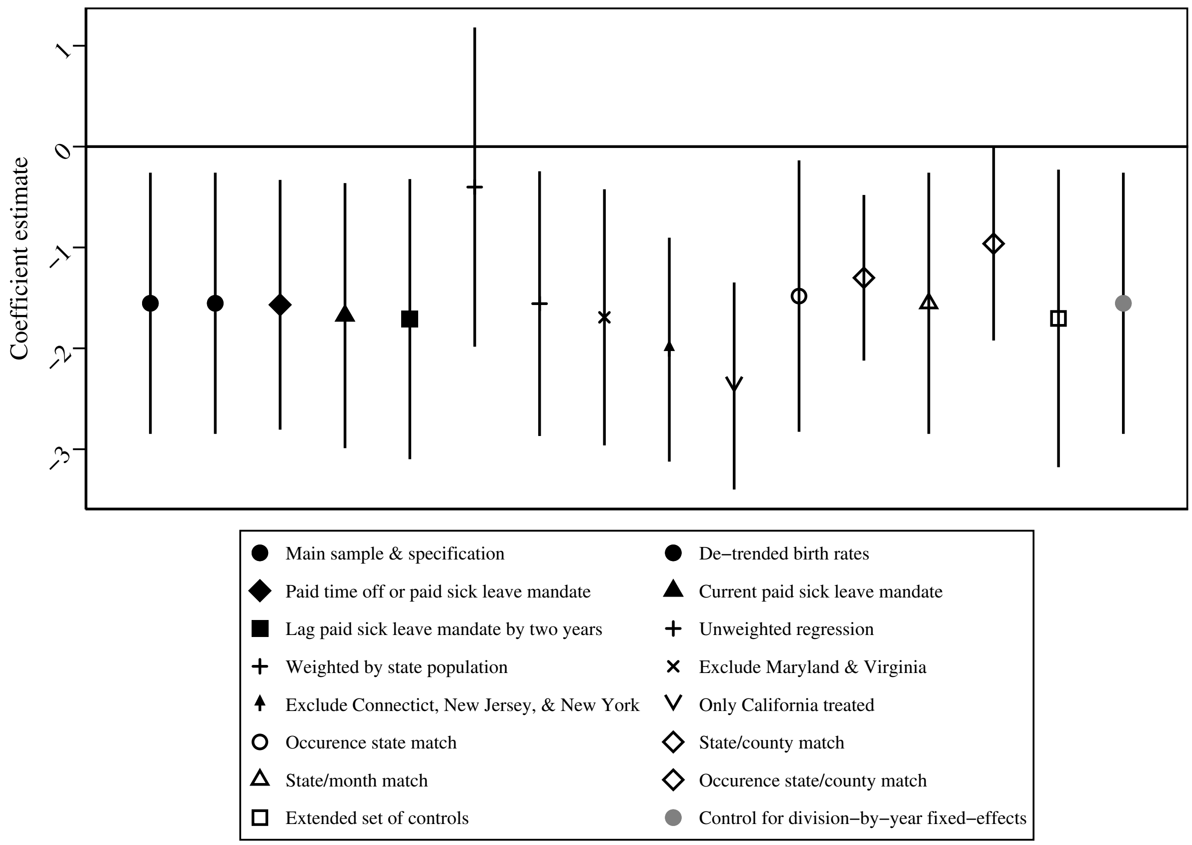
Notes: This figure reports results from regressing the past-year across state migration and economic outcomes among women ages 16-44 years on indicators for time to paid sick leave mandate. The unit of observation is a respondent in state in a quarter-year. We utilize a two-stage difference-in-differences procedure proposed by Gardner (2022). The regressions are estimated with OLS and include respondent demographics, state characteristics, and state and year fixed effects. The leads and lags represent single-year bins corresponding to five years pre-mandate through five years post-mandate. There is no omitted category in the Gardner (2022) procedure. The sample excludes observations more than five years prior to the mandate and more than five years after the event for mandate-adopting states. Data are weighted by basic monthly Current Population Survey weights. The District of Columbia is excluded from the analysis sample as this locality is always treated during our study period. Coefficient estimates are reported with shapes. 95% confidence intervals are reported with vertical lines and account for within state clustering.

Figure A10: Covariate balance: 2010 to 2019 and 2021 to 2022



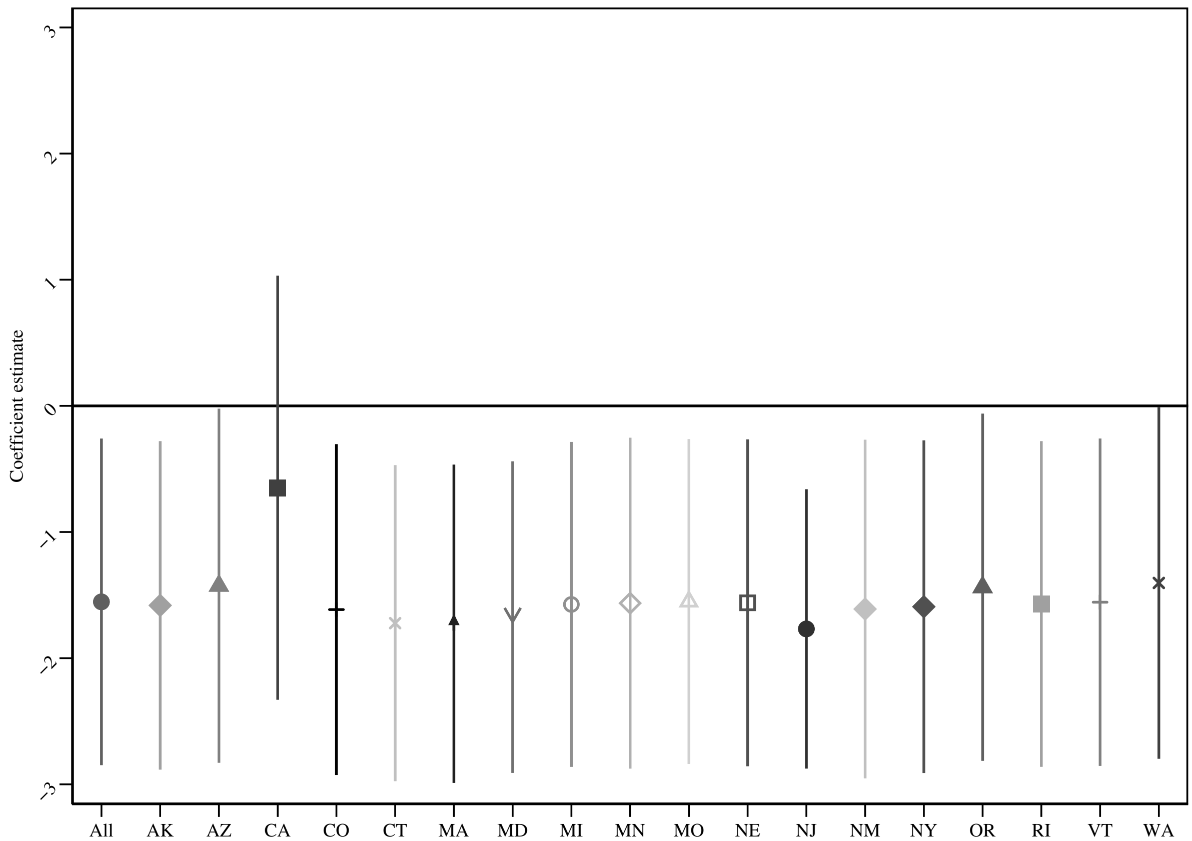
Notes: This figure reports results from regressing each state-level characteristic on a state paid sick leave mandate. The outcome in the regressions is indicated on the x axis. See Section 3 for details on outcome variables. The unit of observation is a state in a year. We utilize a two-stage difference-in-differences procedure proposed by Gardner (2022). The regressions are estimated with OLS and include state and year fixed effects. Data are weighted by the state-year population of women 16-44 years of age. The District of Columbia is excluded from the analysis sample as this locality is always treated during our study period. Coefficient estimates are reported with shapes. 95% confidence intervals are reported with vertical lines and account for within state clustering.

Figure A11: Effect of a state paid sick leave mandate on annual birth rates per 1,000 women 16-44 years using alternative specifications and samples: National Center for Health Statistics 2010 to 2019 and 2021 to 2022



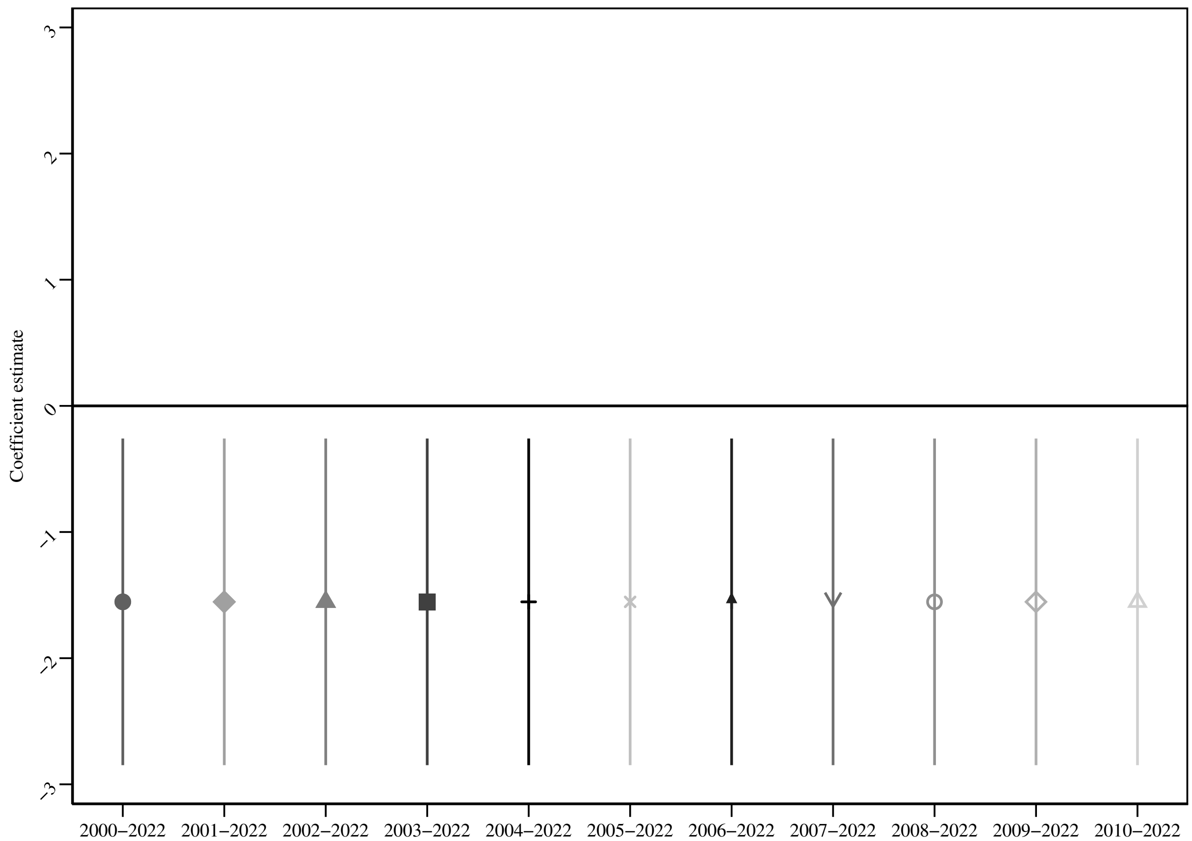
Notes: This figure reports results from regressing the annual state-level birth rate among women ages 16-44 years of age on a state paid sick leave mandate using different specifications and samples. The unit of observation is a state in a year. We utilize a two-stage difference-in-differences procedure proposed by Gardner (2022) unless otherwise noted. The regressions are estimated with OLS and include state characteristics, and state and year fixed effects unless otherwise noted. Data are weighted by the state-year population of women 16-44 years of age unless otherwise noted. The District of Columbia is excluded from the analysis sample as this locality is always treated during our study period. Coefficient estimates are reported with shapes. 95% confidence intervals are reported with vertical lines and account for within state clustering.

Figure A12: Effect of a state paid sick leave mandate on annual birth rates per 1,000 women 16-44 years sequentially excluding each state that adopts a mandate ('leave-one-out' analysis): National Center for Health Statistics 2010 to 2019 and 2021 to 2022



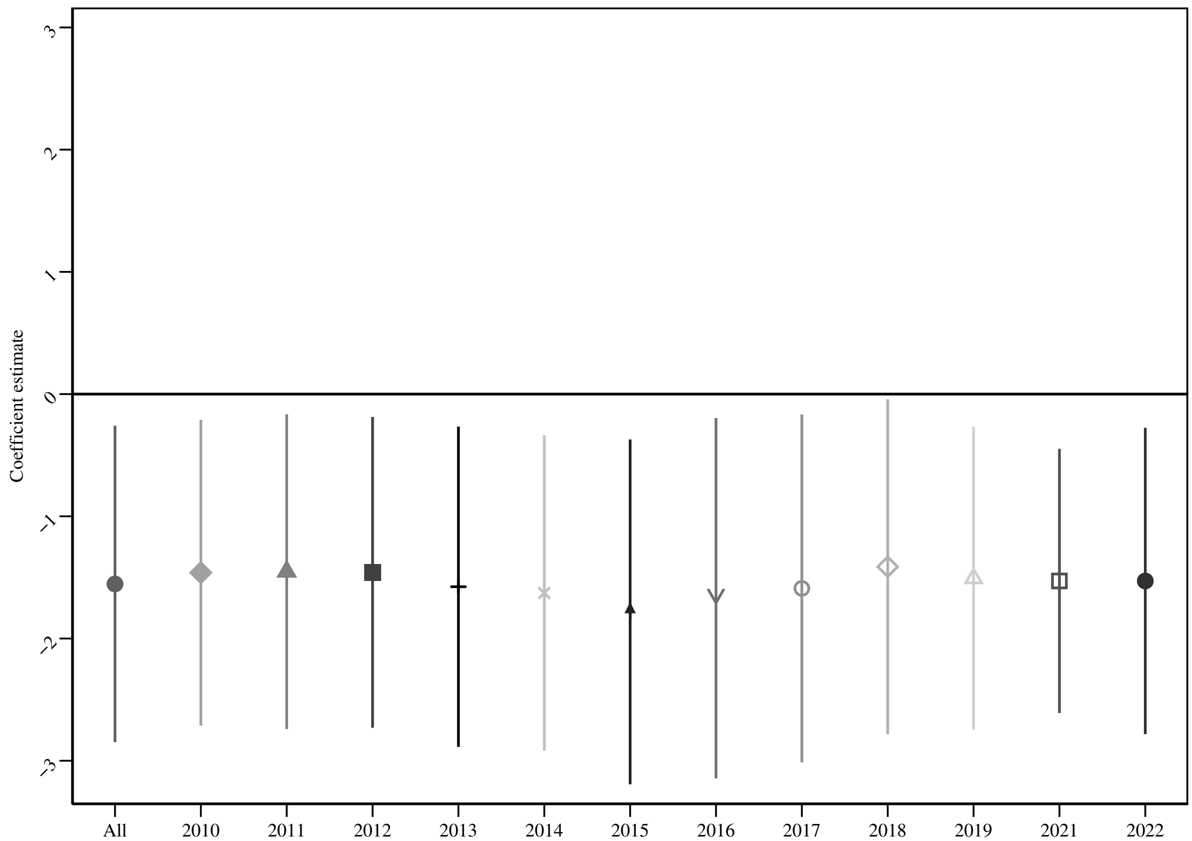
Notes: This figure reports results from regressing the annual state-level birth rate among women ages 16-44 years of age on a state paid sick leave mandate in samples that exclude each state that adopts or announces a paid sick leave mandate by July 2025. The excluded paid sick leave mandate-adopting state is listed in the x-axis. The unit of observation is a state in a year. We utilize a two-stage difference-in-differences procedure proposed by Gardner (2022). The regressions are estimated with OLS and include state policies and demographics, and state and year fixed effects unless otherwise. Data are weighted by the state-year population of women 16-44 years of age. The District of Columbia is excluded from the analysis sample as this locality is always treated during our study period. Coefficient estimates are reported with shapes. 95% confidence intervals are reported with vertical lines and account for within state clustering.

Figure A13: Effect of a state paid sick leave mandate on annual birth rates per 1,000 women 16-44 years sequentially adding earlier years: National Center for Health Statistics 2000 to 2022



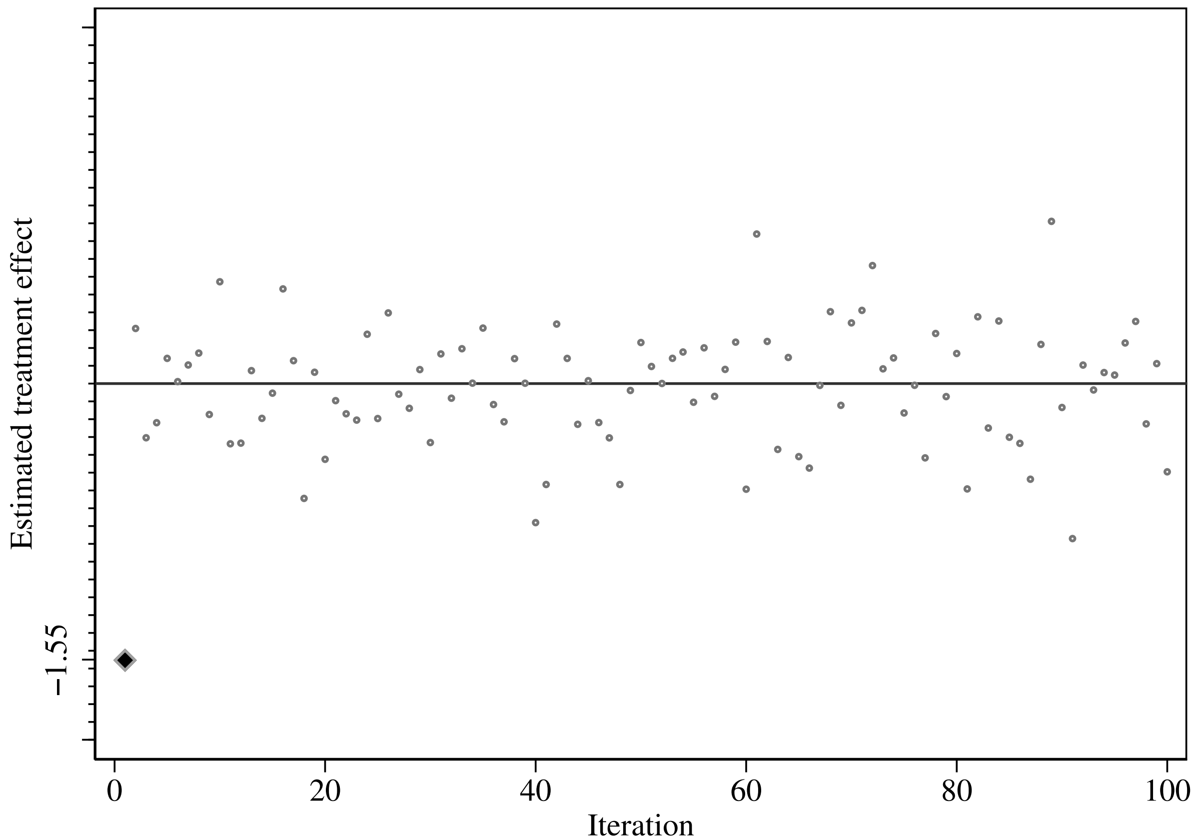
Notes: This figure reports results from regressing the annual state-level birth rate among women ages 16-44 years of age on a state paid sick leave mandate including additional years into the analysis sample. The year in which the study period begins is listed in the x-axis. The unit of observation is a state in a year. We utilize a two-stage difference-in-differences procedure proposed by Gardner (2022). The regressions are estimated with OLS and include state characteristics, and state and year fixed effects unless otherwise. Data are weighted by the state-year population of women 16-44 years of age. The District of Columbia is excluded from the analysis sample as this locality is always treated during our study period. Coefficient estimates are reported with shapes. 95% confidence intervals are reported with vertical lines and account for within state clustering.

Figure A14: Effect of a state paid sick leave mandate on annual birth rates per 1,000 women 16-44 years sequentially excluding each year included in the main study period: National Center for Health Statistics 2010-2022



Notes: This figure reports results from regressing the annual state-level birth rate among women ages 16-44 years of age on a state paid sick leave mandate excluding each year from the sample. The excluded year is listed on the x-axis. The unit of observation is a state in a year. We utilize a two-stage difference-in-differences procedure proposed by Gardner (2022). The regressions are estimated with OLS and include state policies and demographics, and state and year fixed effects unless otherwise. Data are weighted by the state-year population of women 16-44 years of age. The District of Columbia is excluded from the analysis sample as this locality is always treated during our study period. Coefficient estimates are reported with circles. 95% confidence intervals are reported with vertical lines and account for within state clustering.

Figure A15: Effect of a state paid sick leave mandate on annual birth rates per 1,000 women 16-44 years using placebo randomization inference: National Center for Health Statistics 2010 to 2019 and 2021 to 2022



Notes: This figure reports results from regressing the annual state-level birth rate among women ages 16-44 years of age on a state paid sick leave mandate randomly assigning paid sick leave mandates to states using 100 simulations. The unit of observation is a state in a year. We utilize a two-stage difference-in-differences procedure proposed by Gardner (2022) unless otherwise noted. The regressions are estimated with OLS and include state characteristics, and state and year fixed effects unless otherwise noted. Data are weighted by the state-year population of women 16-44 years of age unless otherwise noted. The District of Columbia is excluded from the analysis sample as this locality is always treated during our study period. Coefficient estimates are reported with shapes. 95% confidence intervals are reported with vertical lines and account for within state clustering.

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 (2025)

State	Effective date	Employees gaining coverage for the first time
Alaska	7/2025	N/A†
Arizona	7/2017	934,000
California	7/2015	6,900,000
Colorado	1/2021	813,000
Connecticut	1/2012	200,000
District of Columbia	5/2008	220,000
Massachusetts	7/2015	900,000
Maryland	2/2018	750,000
Michigan	2/2025	N/A†
Minnesota	1/2024	N/A†
Missouri††	5/2025	N/A†
Nebraska	10/2025	N/A†
New Mexico	7/2022	286,000
New York	1/2021	2,600,000
New Jersey	10/2018	1,200,000
Oregon	1/2016	473,000
Rhode Island	7/2018	100,000
Vermont	1/2017	60,000
Washington	1/2018	1,000,000

Notes: This table reports each state that has adopted or announced a paid sick leave mandate by July 2025, the effective month and year, and the estimated number of employees who gain access to paid sick leave for the first time due to the mandate. The data sources are National Partnership for Women & Families (2023) and A Better Balance (2025). Dates are in the format of month/year. State paid sick leave mandates adopted or announced as of 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 locality is always treated during our study period.

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

††Missouri is set to repeal this mandate on 8/2025 (Lieb, 2025).

Table A2: Example National Drug Codes, procedure codes, and diagnosis codes used to classify contraception, fertility, and abortion services in the MarketScan commercial claims data

Healthcare service category	Codes
Oral contraceptives	23586228, 52026108
(National Drug Codes)	52028306, 93214028
	254202991, 555900957
Long-acting reversible	58565 (CPT), A4264 (HCPCS)
contraceptives	11981 (CPT), 0JHG0HZ (ICD10)
	52027201 (National Drug Code)
Abortion	59840,59841,59850,59851 (CPT),
	O039, O034, O031, Z332 (ICD10)
In vitro fertilization	S4011, S4016 , 89261, 58970 58974 (CPT)

Notes: This table reports the codes used to identify healthcare service use in the MarketScan commercial claims data. The codes are obtained from contraceptive look up files maintained by U.S. Department of Health and Human Services (2025) at https://opa.hhs.gov/research-evaluation/title-x-services-research.

Table A3: Summary statistics: National Center for Health Statistics 2010 to 2019 and 2021 to 2022

Sample:	All states	Paid sick leave states, pre-policy	Non-paid sick leave states
Outcome			
Annual birth rate per 1,000	61.5	61.7	62.9
women ages 16-44 years	(5.28)	(3.99)	(5.19)
Paid sick leave			
Paid sick leave mandate	0.12	0	0
(lagged one year)	(0.33)	(0.00)	(0.00)
State policies			
Paid family and medical	0.18	0.30	0
leave policy	(0.39)	(0.46)	(0.00)
Paid time off mandate	0.010	0.03	0.004
	(0.10)	(0.16)	(0.06)
Temporary Aid to Needy Families y	661	857	490
4-person family monthly benefit ($)	(282)	(257)	(154)
Medicaid income eligibility threshold	2.22	2.34	2.04
(% of Federal Poverty Level)	(0.49)	(0.47)	(0.35)
State demographics			
Age (years)	38.1	37.9	38.1
	(1.69)	(1.22)	(1.95)
Female	0.51	0.51	0.51
	(0.01)	(0.01)	(0.01)
Male	0.49	0.49	0.49
	(0.01)	(0.01)	(0.01)
White	0.77	0.78	0.78
	(0.09)	(0.07)	(0.10)
Non-White	0.23	0.22	0.22
	(0.09)	(0.07)	(0.10)
Hispanic	0.18	0.19	0.15
	(0.13)	(0.13)	(0.13)
Non-Hispanic	0.82	0.81	0.85
	(0.13)	(0.13)	(0.13)
No college degree	0.71	0.69	0.73
	(0.05)	(0.04)	(0.04)
College degree	0.29	0.31	0.27
	(0.05)	(0.04)	(0.04)
Observations	600	155	384

Notes: This table reports summary statistics for the full sample, the sample of states that adopt or announce a paid sick leave mandate by July 2025 measured before mandate adoption, and the sample of states that do not adopt or announce a paid sick leave mandate by July 2024. The number of observations in column (1) is greater than the sum of the number of observations in columns (2) and (3) because column (2) excludes state-year observations post-mandate adoption. The unit of observation is a state in a year. Data are weighted by the state-year population of women 16-44 years of age. Standard errors are reported in parentheses. The District of Columbia is excluded from the analysis sample as this locality is always treated during our study period.

Table A4: Goodman-Bacon (2021) decomposition of the effect of a state paid sick leave mandate on annual birth rates per 1,000 women 16-44 years: National Center for Health Statistics 2010 to 2019 and 2021 to 2022

Two-by-two comparison:	ATT	Weight
Early treated versus late treated	-0.40	0.07
Treated versus never treated	-0.21	0.90
Late treated versus early treated	-1.36	0.03
Re-weighted ATT	-0.23	-
2010-2011 mean,	63	-
treatment states		-
Observations	600	-

Notes: ATT = average treatment on the treated. This table reports results from a Goodman-Bacon (2021) decomposition analysis of the effect of state paid sick leave mandates on the annual state birth rate among women 16-44 years. The unit of observation is a state in a year. The pre-treatment period is defined as 2010-2011, the period before any states in the sample adopt a paid sick leave mandate. The District of Columbia is excluded from the analysis sample as this locality is always treated during our study period. No time-varying covariates are included to isolate the two-by-two comparisons. Data are unweighted to isolate the two-by-two comparisons.

Table A5: Effect of a state paid sick lave mandate on annual birth rates per 1,000 women ages 16-44 years using alternative estimators: National Center for Health Statistics 2010 to 2019 and 2021 to 2022

Specification:	Coefficient estimate (Standard error)
Gardner (2022)	-1.55**
	(0.66)
Percent change	-2.45%
2010-2011 mean, treatment states	63
Observations	600
Callaway and Sant'Anna (2021)†	-1.37*
	(0.74)
Percent change	-2.17%
2010-2011 mean, treatment states	63
Observations	600
Borusyak et al. (2024)	-1.68***
	(0.51)
Percent change	-2.66%
2010-2011 mean, treatment states	63
Observations	600
Wooldridge (2023)	-1.68***
	(0.41)
Percent change	-2.66%
2010-2011 mean, treatment states	63
Observations	600
Stacked difference-in-differences††	-1.25*
	(0.68)
Percent change	-1.97%
2010-2011 mean, treatment states	63
Observations	1330
Two-way fixed effects	-1.44**
	(0.62)
Percent change	-2.28%
2010-2011 mean, treatment states	63
Observations	600

Notes: This figure reports results from regressing the annual state-level birth rate among women ages 16-44 years of age on a state paid sick leave mandate using alternative difference-in-differences estimators. The unit of observation is a respondent in state in a year. The pre-treatment period is defined as 2010-2011, the period before any states in the sample adopt a paid sick leave mandate. The state paid sick leave mandate is lagged one year. All regressions are estimated with OLS and include state policies and demographics, and state and year fixed effects. Data are weighted by the state-year population of women 16-44 years of age. The District of Columbia is excluded from the analysis sample as this locality is always treated during our study period. Standard errors are clustered at the state-level and are reported in parentheses.
***,**,* = statistically different from zero at the 1%,5%,10% level.
† The Callaway and Sant'Anna (2021) procedure does not include time-varying covariates.
††The stacked difference-in-differences analysis includes states treated in years 2015, 2016, 2017, and 2018. Observations more than four years pre-treatment and four years post-treatment are excluded from the sample. No observations are excluded from the comparison group of states. We include separate state and year fixed effects for each treated cohort.