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ABSTRACT

Unlike most advanced countries, the United States does not have a federal paid sick leave (PSL) policy; however, multiple states have adopted PSL mandates. PSL can facilitate healthcare use among women of child bearing ages, including use of family planning services. We combine administrative 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 contraception services post mandate. Overall, our findings imply that PSL policies may help women balance family and work responsibilities, and facilitate their reproductive choices.

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1 Introduction

The United States is one of two 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 2018, nearly 40% of civilian employees in the U.S. reported that they did not have access to PSL through their employer (Asfaw et al., 2019), with substantial heterogeneity across types of employees (Bartel et al., 2019; DeSilver, 2020; Maclean et al., 2020). Generally, employees in jobs with high wages and generous benefit packages have access to PSL while those in other jobs do not. Given that the median employee earned approximately $200 per day in 2020 (National Equity Atlas, ND), 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: industry surveys show 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. As of October 2023, 14 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). These mandates require employers to provide, on average, seven days of PSL per year (Kaiser Family Foundation, 2021), granting access to PSL benefits to 21 million employees (National Partnership for Women & Families, 2023),[1] 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 critiques of requiring PSL benefits (Copland, 2013), these mandates are also not overly costly to employers.

[1]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.

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 have been declining for decades (Buckles et al., 2022; 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)[2] and nearly 13% of reproductive age women receive fertility treatment (Carson and Kallen, 2021). Finally, despite nearly universal support from healthcare professionals, 2.1% of pregnant mothers received no prenatal care in 2021 and 12.5% received inadequate care (Martin and Osterman, 2023).[3]

Policies increasing the access of women[4] to healthcare (including family planning services) and that allow improved pregnancy timing may have benefits both for women and for their children. 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., 2022; Miller et al., 2023). For children, being born as a result of an unintended pregnancy can lead to both immediate (e.g., low birthweight and birth complications) and longer−term (e.g., worse health and poverty later in life) consequences (Mohllajee et al., 2007; Ananat and Hungerman, 2012; Bailey, 2013; Kost and Lindberg, 2015; Lin et al., 2020). Prenatal care[5] is beneficial for mothers and children (U.S. Department of Health and Human Services, 2021), but requires time investments including regular healthcare professional appointments which typically occur during the workday.

PSL may be important for healthcare use among women of child−bearing age. In economic models of consumer choice (Becker, 1960; Michael and Willis, 1976), birth rates and fertility treatment use are determined by supply and demand factors. PSL relaxes the full cost of receiving care as women are not required to lose pay to attend healthcare appointments available during the workday, which should increase the quantity of healthcare, including family planning. For example, PSL may facilitate the receipt of

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

[3]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).

[4]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.

[5]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.

family planning and birth control treatments, particularly those requiring prescriptions or medical visits, such as intrauterine devices and implants, that are more reliable than other forms of contraception (Bailey and Lindo, 2017). This reduction in the overall costs of care raises the possibility that expanding access to PSL could *reduce* birth rates, a pattern that may be reinforced if PSL increases the receipt of abortion services. Conversely, PSL may allow women of child−bearing age to receive fertility treatment, which would *increase* birth rates. The prospect of having access to PSL after birth to care for the infant could increase birth rates and the ability to use PSL to receive prenatal care could better allow a woman to carry the birth to term. People may take time off just before the birth, which may enhance the propensity for the mother to carry the birth to term, thereby increasing the birth rate. PSL could also influence fertility rates through employment outcomes. If workers 'pay' for PSL through reduced wages, the income effect might reduce fertility, assuming that children are normal goods. This effect may be exacerbated if the wage reductions are concentrated among women (because employers view them as more likely to use PSL) and they respond by increasing work hours, which would raise the time costs of fertility. Conversely, wages might increase in the longer−run if PSL facilitates employment continuity and, unless offset by substantially longer work hours, the income effect might then be expected to increase fertility. In either case, the employment effects would need to be substantial to explain a significant portion of changes in fertility.[6]

In this study, we combine difference−in−differences and event−study methods with survey and administrative data from 2007 to 2019, a period where PSL mandates were implemented in multiple states. We obtain four principal findings. First, PSL mandates raise access to and use of PSL in a national survey of establishments. Second, the use of contraception among women of child−bearing age and interest in fertility treatments appear to increase post−mandate, with no evidence of changes in abortion rates. Third, we find suggestive evidence that employment and wages increase modestly among women of child−bearing age following adoption of a state PSL mandate. Fourth, birth rates decline post−PSL mandate, with some heterogeneity in effect size across mothers with different demographic characteristics. Thus, while the impact of PSL on birth rates is ex ante ambiguous as described above, the net effect of PSL in our sample is negative.

[6]Numerous studies demonstrate that the changes to the price (financial or hassle) of family planning services influence fertility outcomes, thus offering empirical premise for our work. For example, increasing insurance coverage for contraception (Kearney and Levine, 2009; Carlin et al., 2016) and providing free or reduced cost family planning services (Lindo and Packham, 2017) encourages women to use contraception, reduces fertility rates, and enhances the use of prenatal care (Daw and Sommers, 2018). Conversely, restricting access by closing family planning clinics can have the opposite effect on these outcomes (Packham, 2017). See Bailey and Lindo (2017) for an excellent review of this literature.

Overall, our findings suggest that mandated PSL benefits facilitate the reproductive choices of women, with the overall effect of reducing birth rates. These results are robust to numerous sensitivity checks, including the use of methods that account for potential bias in estimates of the average treatment effect with a staggered policy roll−out.

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 unpaid 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, unpaid 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 prenatal care (Nowak, 2022). FMLA benefits can be used for 'medically necessary' fertility treatments (Smith-Garcia, 2022), for example, surgery to treat endometriosis but not for procedures to become pregnant.

While the 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.[7] More generally, large portions of the U.S. workforce are unable to take time off for healthcare needs without losing earnings. In 2018 nearly 40% of employees in the U.S. reported that they lack access to paid sick leave (Asfaw et al., 2019), although government data suggest that coverage rates are higher with 79% of civilian workers working in a job that the employer reports offering PSL (Bureau of Labor Statistics, 2022).[8] The inability to take time off without losing earnings may prevent some individuals from seeking treatment for themselves or dependents.

Despite the lack of a federal provision, paid sick leave is popular, with 84% of Ameri-

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

[8] Possible explanations for the apparent discordance between the benefits reported by employees in survey settings and employers in establishment surveys include employees not being aware of their benefits and employers reporting overly generous benefit packages.

cans supporting policies that would mandate PSL (Global Strategy Group & Paid Leave for All Action, 2021). U.S. Senators Rosa DeLauro and Bernie Sanders 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 policies. Table 1 presents data on the 14 states with PSL mandates in place or announced as of October 2023, using legal data prepared by the National Partnership for Women & Families (2023). 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 provided 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 1 displays the geographic distribution of these mandates across states as of October 2023.

Most commonly, 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 (adopted or announced) 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 A1 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 mandates prohibit employer retaliation against employees who use (mandated) PSL.

In addition to states, some cities and counties have adopted PSL mandates (e.g., San Francisco, CA adopted a PSL policy in 2007). When a state and a sub−state jurisdiction 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 PSL mandates and these differences could result in heterogeneous impacts. For example, some mandates compel employers

[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.

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 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.

Four states (Illinois, Michigan, 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 PTO to employees 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. 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 documentations or requirements needed to be granted paid leave.[11] In our main analyses, we focus on PSL policies 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.[12]

During the COVID−19 pandemic, the U.S. federal government adopted a temporary PSL policy under the Families First Coronavirus Response Act (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 also show results with that year included. Doing so does not appreciably affect our findings.

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

Studies of U.S. PSL mandates 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., 2020; Callison and Pesko, 2022).[13] Using detailed

[11] This paragraph is based on the authors' personal conversations with a senior policy analyst at the National Partnership for Women & Families. Full details available on request.

[12] There can be differences across policy databases in which localities have a policy in place. For example, A Better Balance (2023) classifies Michigan has having a PSL mandate.

[13] 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

national establishment data, Maclean et al. (2020) find a 13 percentage point increase in employer provision of PSL, from 66% to 79%, after the implementation of a mandate. The authors suggest that employer non−compliance, legal issues,[14] and lack of benefit knowledge as potential reasons for less than full compliance. Similarly, using survey data, Callison and Pesko (2022) and Ahn and Yelowitz (2016) document that employee reports of access to PSL increase following mandate adoption.

Maclean et al. (2020) estimate that employees who gain access to PSL use approximately two additional days of paid leave per year. The authors also show that these mandates are not overly costly to employers, post−mandate employer costs associated with PSL use increase by an average of 3.3 cents per hour per employee. As described in Section 2.1, some state PSL mandates provide unpaid leave to employees (National Partnership for Women & Families, 2023) and Maclean et al. (2020) show that employee use of unpaid sick leave increases post-mandate: by 0.45 hours per year, which reflects a near doubling relative to the baseline mean (0.54).

Understanding the distribution of PSL mandate impacts across different groups of workers is important for describing policy equity. While their data − which are based on an establishment survey − lack information on employee characteristics, Maclean et al. (2020) show that the effects of state PSL mandates, in terms of access to PSL and benefit utilization, are experienced by a relatively broad range of jobs. For example, increases in access to and use of PSL are observed for unionized and non−unionized jobs, jobs in large and small establishments, part−time and full−time jobs, and jobs in different occupation and industry groups.

Employers do not curtail provision of other benefits (e.g., health insurance or vacation days) nor do they cut wages following mandate adoption (Pichler and Ziebarth, 2020a; Maclean et al., 2020). Indeed, there is some evidence that wages increase post−mandate, perhaps due to improved productivity attributable to less disease spread at the workplace (Maclean et al., 2020; Callison and Pesko, 2022). In a particularly relevant study, 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 poverty rates for women decline by 6.7%. The absence of reductions in other benefits may reflect the low cost to employers of offering PSL (Maclean et al., 2020).

Examining early PSL mandates in Connecticut and Washington DC, Stearns and White (2018) provide evidence of declines in reported sickness absence post−mandate.

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and contrasts PSL policies across advanced economies.

[14] Several major employers 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.

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), while reducing unnecessary service use (Ma et al., 2022), and improving health status (Callison and Pesko, 2022; Slopen, 2023). Finally, 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.

While we are the first study to examine the effect of PSL mandates on birth rates, previous work has explored the extent to which paid medical and family leave is associated with fertility−related outcomes. There are key differences between these leave types (see Pichler and Ziebarth [2020b] for a discussion of various types of paid leave in the U.S.), and their potential impacts on fertility. PSL offers workers the ability to take up to seven days per year away from work for their own health needs and the needs of their dependents, but this leave is not designed for lengthy separations from work. Paid medical and family leave is of much longer duration than PSL, ranging from four to 52 weeks, this leave can support a substantial amount of time away from work post−childbirth while PSL can only offer a short spell of time for recovery. Thus, the impact of these two types of leave policy on fertility are potentially quite different. Nonetheless, a comparison of findings from this literature is useful for our work. While reviewing this entire branch of literature is beyond the scope of this paper, we briefly discuss two studies that examine the experience of paid leave in California and refer interested readers to an excellent review by Rossin-Slater (2017). California was the first state in the U.S. to adopt paid medical and family leave in 2004. Bana et al. (2020) apply a regression−kink approach to California Employment Development Department administrative data and provide suggestive evidence that the California paid family and medical leave policy may increase child−bearing (which the authors proxy using subsequent family leave claims) over time among relatively high income women.[15] Child−bearing effects appear approximately three years following the implementation of the California policy, suggesting that if paid leave impacts fertility, these effects may take time to develop. On the other hand, Bailey et al. (2019) find no evidence that this policy leads to changes in child−bearing among women using a regression discontinuity framework and Internal Revenue Service tax record data. The differing findings suggest that paid medical and family leave may have heterogeneous effects across women with different characteristics.

[15] The authors' design implies that they identify effects for women at approximately the 92nd percentile of the earnings distribution.

3 Data and methods

3.1 Birth records

Our primary data comes from the restricted use National Center for Health Statistics (NCHS) administrative birth records database. Our sample includes a near universe of recorded U.S. births occurring between 2007 and 2019. The 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 health 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.[16] Aggregating in this manner leaves us with 663 observations.

Our outcome is the annual state birth rate per 1,000 women ages 16−44 years. Using data from other sources, described below, we test for first stage effects of mechanisms, selecting women in this age group 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 an annually updated database of all PSL mandates in the U.S. States adopt PSL mandates in different months of the year, we code the first partial year in which the mandate is in place as the effective year.

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 2009−2019 to study the effect of PSL mandates on PSL coverage and use. The NCS are maintained by the Bureau of Labor Statistics (BLS) and provide a nationally−representative sample of establishments.[17] The NCS are used to produce official government statistics on compensation and labor costs in the U.S., and to adjust government employee wages.

[16] The micro−data (i.e., measured at the birth−year−level) over our study period includes 55,573,601 observations. The substantial computing time required to analyze data at this level leads us to use aggregated data as described here.

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

The NCS provide information on access to and use of PSL. We follow Maclean et al. (2020) and construct annual measures using Q1 in year *t* and Q2, Q3, and Q4 for years *t-1* for the year *t* PSL utilization. We construct the variable in this manner to capture paid leave use across the full year rather than a specific quarter. For example, Q1 (January through March) will cover the influenza season when leave−taking may be elevated. Information is collected by trained BLS economists who interview human resources administrators at each establishment each quarter.[18] 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.

Information on contraceptive use among 18−44 year old women is obtained from the Behavioral Risk Factor Surveillance Survey (BRFSS), 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 year. Respondents are queried about health, health behaviors, and healthcare outcomes. In some years (2006, 2010, 2011, 2017, and 2019), the CDC includes an optional 'Preconception Health/Family Planning' module, which states may or may not choose to participate in. Table A1 lists the states that provide information on contraception for each of these years.[19] The contraception question asks if the respondent or their partner are 'doing anything now to keep from getting pregnant?'[20] Female respondents 18−44 years are coded one if they indicate yes to this question and zero otherwise. We use BRFSS−provided survey weights.

To study interest in fertility treatment, we use Google Insights data for 2007−2019 on searches (captured by the Google Index) for the term in−vitro fertilisation ('IVF'). IVF is a common and effective fertility treatment (Mayo Clinic, 2021) and internet searches for this term potentially capture changes in demand for this service as women are better able to take time off work for healthcare. Previous economic studies have used Google Insights data to study interest in specific outcomes or factors such as depression/anxiety, allergy

[18] 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.

[19] Two PSL−adopting states (Arizona and Oregon) are not observed pre−policy, thus we cannot test parallel trends for these states.

[20] There are some changes to the question and response wording over time. Full details available on request. A caveat to our BRFSS analysis is that our contraception metric is an overall measure of contraception use. Due to small sample sizes and changes in the contraception questions across module years we do not isolate specific forms of contraception. Consultations with healthcare professionals post−mandate could induce women to use pharmacological forms of birth control (pill or implant), but they may also prompt women to have their partner use condoms or undergo a male sterilization procedure, or for women themselves to use over−the−counter female contraception such as female condoms. Thus, our use of an overall contraception metric does not place restrictions on the types of contraception that a healthcare professional may encourage during a consult.

levels, office−based mental healthcare, employment growth, and awareness of naloxone availability (Tefft, 2011; Chalfin et al., 2019; Deza et al., 2022; Borup and Schütte, 2022; Doleac and Mukherjee, 2022). The Google Index ranges from zero to 100 and captures the relative popularity of search terms.[21]

We use data on the rate of legal abortions per women ages 15−44 by state of occurrence (including services received by state residents and non-residents) for 2009−2019 collected in the Centers for Disease Control and Prevention Abortion Surveillance annual reports (Kortsmit et al., 2022). This information is reported by healthcare providers to states or local health departments. No data are reported for 2017 and three states (California, Maryland, and New Hampshire), including two with PSL mandates, did not provide information or their data do not meet CDC reporting quality standards.[22]

Finally, we utilize data from the 2007−2019 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 information on various topics throughout the year. We use the basic monthly data to construct indicators of any and full−time (35 hours per week or more) employment, usual hours worked per week, the logarithm of hourly wages (both conditional and unconditional),[23] and being married or co−habitating at the time of the survey for women ages 16−44 years old. We use the Annual Social and Economic Supplement (ASEC−CPS), fielded once per year during February through April, to measure cross−state migration during the past year among these women 16−44 years old. Throughout, we utilize CPS data harmonized by the University of Minnesota IPUMS database and weight data by CPS−provided survey weights (Flood et al., 2022).[24]

[21]The Index is computed by Google data scientists as the quotient of the number of searches for any term divided by the total searches with a specified geographic area and time period (state and year in our context). To increase the validity of the data, Google data scientists conduct extensive data cleaning. For example, low volume searches are assigned a zero value, duplicate searches are removed, and searches that involve special characters are eliminated. For full details see: https://support.google.com/trends/answer/4365533?hl=en; last accessed May 28, 2023.

[22]We weight the state−level data sources (Google Insights searches and abortion rates) by the state population that is 16 to 44 years of age.

[23]We inflate wages to 2019 values using the Consumer Price Index. Conditional wages only include those with positive wages while unconditional wages include those with zero wages, e.g., the unemployed. Samples are smaller for wages than other outcomes as wage data are only collected from respondents in the out−going−rotation sample which correspondents to respondents in month four and eight.

[24]We exclude women working in the armed forces from the sample.

3.4 Summary statistics and trends

Table A2 reports summary statistics for our birth record sample. The mean annual birth rate is 63.8 per 1,000 women ages 16−44 years of age and birth rates are similar for states that adopt PSL mandates (63.2 per 1,000 women ages 16−44 prior to policy implementation) and those that do not (65.0 per 1,000 women ages 16−44). PSL mandates are relatively new and just 6.7% of observations have a PSL mandate in place. While not identical, the two groups are broadly similar.

Trends in annual birth rates for states that adopt/announce a PSL mandate by October 2023 and states that do not are reported in Figure 3. The two time series are trending downward for most of the study period as has been established in earlier work (Buckles et al., 2022). 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 reductions than other states.

3.5 Methods

We primarily use two−way fixed−effects (TWFE) difference−in−differences methods (DID) to study the impact of PSL mandates on state annual birth rates although, described in Section 4.3, our results are not sensitive to using methods robust to potential bias associated with a staggered policy roll−out (and diagnostics suggest our context is not one where this roll−out would be expected to lead to bias). Our primary regression specification takes the form:

$$B_{s,t} = \beta_0 + \beta_1 PSL_{s,t-1} + X_{s,t}\beta_2 + \alpha_s + \alpha_t + \epsilon_{s,t} \tag{1}$$

$B_{s,t}$ 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.) However, 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 mandates (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).[25] We include 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. We convert nominal variables to 2019 terms using the Consumer Price Index.

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 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).[26]

As mentioned above, both states and sub−state localities have adopted PSL mandates in the U.S. In our main analyses, we focus on state mandates. Our rationale is that in our birth record data (see Section 3.1) we have the location of residence. However, PSL mandates depend on where the job (not the residence) is located, which is not available in the birth record data.[27] Our analysis of the American Community Survey (ACS) 2007−2019 (Ruggles et al., 2023) suggests that 19% of female employees 16−44 years live in one county and work in another county, but just 2% work in a different state than their state of residence.[28] These statistics suggest that errors in assigning geography will be rare at the state−level but relatively frequent when focusing on counties.[29] Adding to this concern, in our analysis of the 2021 County Business Patterns dataset (U.S. Census Bureau, 2022), we find that 2,698 of 3,190 (85%) counties lack a general hospital, suggesting that many women likely give birth outside their county of residence.[30]

For the reasons just discussed, our analysis focuses on the state of mother's residence.

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

[26]We first construct the share of the population that is female and age 16−44 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 in each state in each year.

[27]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.

[28]We use ACS−provided survey weights.

[29]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.

[30]We use NAICS code 622110 (General Medical and Surgical Hospitals) to isolate general hospitals. The U.S. Census (2023) definition for this code is: 'This industry comprises establishments known and licensed as general medical and surgical hospitals primarily engaged in providing diagnostic and medical treatment (both surgical and nonsurgical) to inpatients with any of a wide variety of medical conditions. These establishments maintain inpatient beds and provide patients with food services that meet their nutritional requirements; have an organized staff of physicians and other medical staff to provide patient care services; and usually provide other services, such as outpatient services, anatomical pathology services, diagnostic X−ray services, clinical laboratory services, operating room services for a variety of procedures, and pharmacy services.'

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), and iii) excluding groups of states with substantial cross−state commuting (i.e., the Washington DC/Maryland/Virginia and the Connecticut/New Jersey/New York tri−state areas). While the direction of bias attributable to measurement error is hard to sign (see Bound et al. [2001]), we suspect that, all else equal, ignoring sub−state PSL policies will attenuate our coefficient estimates toward zero, relative to the true policy impacts.[31] We also report results adding 2020−2021 to the analysis sample, thus demonstrating that are results are robust to including the COVID−19 pandemic period.

A key assumption of DID methods is that treated and untreated units (states in our setting) would have followed the same trends in outcomes had the policies not been implemented.[32] Therefore, we estimate a series of event−studies to explore the extent to which states that adopted and did not adopt a PSL mandate followed common trends pre−mandate, and to examine dynamics in treatment effects in the post−policy period.[33]

We include four policy leads and four policy lags in our event−study, and we trim the end points, that is the $t-4$ and $t+4$ indicators are homogeneous in time−to−event, with $t-1$ as the omitted reference year. Four states (Colorado, Minnesota, New Mexico, and New York) adopted a PSL mandate after 2019. In our main analysis, we code these states as being in the pre−treatment period. For example, in 2019 and 2020 we code New Mexico as $t-3$ and $t-2$ respectively. However, we report various alternative event−study specifications on different samples in Section 4 and demonstrate that our findings are not driven by the selection of a specific specification or sample period.

4 Results

4.1 Access to and use of paid sick leave, contraception use, interest in fertility treatment, and abortion rates

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 2). We follow Maclean et al. (2020) in using the NCS (2009−2019) to examine

[31] In particular, attenuation can potentially occur because, by ignoring the sub−state policies, we code some births as untreated when they are in fact treated.

[32] Thus, the DID assumption requires restrictions on untreated potential outcome paths.

[33] 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.

these relationships.[34] 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); column (3) also includes state−level demographic covariates; and column (4) is identical to column (3) except that observations from the 2020−2021 (pandemic) period are included. Throughout, we emphasize the specification and sanple shown in column (3), but the results are similar across models.

Following a state PSL mandate adoption, our coefficient estimates indicate that the probability that an establishment offers PSL to employees increases by 9.9 percentage points (ppts), a 13.9% increase relative to the pre−treatment mean in PSL adopting states. The adoption of a PSL mandate increases the use of PSL by 1.3 hours (5.2%) per quarter, or roughly one−half an additional day per year.[35] Event−studies for these outcomes (using the specification and sample in column [3]) are reported in Figure A2. There is no observable evidence of differential pre−trends and, post−implementation, the mandate effects on access to and use of PSL appear to grow over time (at least initially in the four years post−treatment period that we consider).

Next we examine whether PSL mandates are associated with three potential determinants of birth rates among among women of child−bearing age: use of contraception; interest in fertility treatment − proxied by internet searches for IVF; and abortion rates. These results are reported in Panels A, B, and C of Table 3. The set of covariates included in each specification and the sample corresponds to those in Table 2.

Following the adoption of a PSL mandate, there is a 2.9 to 4.4 ppt increase in the probability that a woman 16−44 reports using contraception at the time of the BRFSS survey, with larger estimates for the regression specifications that include a more comprehensive set of controls.[36] Comparing these coefficient estimates to pre−policy PSL mandate state mean (66.1%), the probability of contraception use increases by 4.4% to

[34]Unfortunately, the NCS does not include information on the people who hold jobs, thus we cannot examine jobs held by women.

[35]These results are qualitatively similar to those previously obtained by Maclean et al. (2020), although since we use different years, specifications, and establishments, they are not identical. For example, Maclean and colleagues use data on private establishments only from 2009 to 2017, while we use data on both public and private establishments from 2009 to 2019. In particular, our findings are modestly smaller than the findings of Maclean et al, we attribute that difference mainly to be due to Maclean et al's focus on private establishments which are less likely to offer PSL when they are not mandated to do so relative to government establishments. 95% confidence intervals for coefficient estimates overlap across in the two studies making us reluctant to draw strong conclusions about differential impacts.

[36]We replace year fixed−effects with year−by−quarter fixed−effects in the BRFSS data as we have finer time units in those data. We do not show results for this outcome in column (4) because information on contraception is not included in the BRFSS in 2020 or 2021.

6.7%. Event−study estimates corresponding to column (3) are reported in Figure A3. Because we have very few years of data and not all states are observed in all years, we consolidate the event−time indicators in the BRFSS event−study. The results while noisy, as expected since the BRFSS only has information on contraception for a quite limited set of states and years (see Table A1), suggest an absence of pre−trends and PSL−induced increases in contraception use.

For internet searches for IVF treatment, our proxy for interest in fertility treatment, we obtain a positive coefficient estimate in all specifications over the main study period, with estimated increases of 3.00 to 4.96 units in the Google Index, corresponding to growth of 4.2% to 7.0%. The findings are imprecise when the 2020−2021 period is included in the sample, although the coefficient estimate retains the positive sign and implies a 2.6% increase. However, event−study results (shown in A4) are inconclusive, so we interpret these findings with some caution. Multiple births are more common when IVF is used than when not used, suggesting that multiple births could be used as a proxy for IVF. If multiple births are a reasonable proxy for IVF, and if IVF use (here we measure interest) increases post−mandate, then we would expect the multiple birth rate to rise. In analyses reported later in the manuscript, we examine the extent to which single− and multiple−birth rates change post−mandate. We observe declines in both measures of birth rates and the relative effect size is larger for multiple (4.5%) than single (2.1%) births, although 95% confidence intervals overlap. These findings, which rely on strong assumptions, do not suggest that successful IVF use increases substantially.

We observe no statistically significant change in abortion rates following adoption of a PSL mandate: the coefficient estimates are never statistically significant, small in magnitude, and change sign across specifications. Results from the corresponding event−study (Figure A5) also provide no indication of a change in abortion rates.

To summarize, adoption of a state PSL mandate is associated with increased access to and use of PSL, higher contraception use, possible growth in IVF internet searches (which proxy interest in this service), and no change in abortions. With this evidence in hand, we turn to estimating the effects of state PSL mandates on birth rates.

4.2 Annual state birth rates

Estimated effects of state PSL mandates on annual birth rates are summarized in Table 4. The covariates accounted for again become more extensive when moving from left to right, with pandemic year observations included in the final column. The coefficient estimates suggest 2.43, 2.26, and 1.37 fewer births per 1,000 women of child−bearing age following mandate adoption in columns (1), (2), and (3). Compared to mean birth rates in PSL−adopting states pre−policy (63), these coefficient estimates imply 3.9%, 3.6%, and 2.2% declines in the annual birth rate. Including the pandemic years (2020−2021) does not change our results to any meaningful degree: births per 1,000 women ages 16−44 years decline by 1.49 per 1,000 or 2.4% post−mandate. Unless otherwise noted, the results reported below will be based on regression specifications and the sample analogous to those reported in column (3), that include state and year fixed−effects, state−level policies, and state−level demographics and analyze data from 2007 to 2019.

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 1.2 hours (5.2%) per quarter, or just under five hours per year. We observe increases in contraceptive use and interest in IVF treatment post−mandate (but not in multiple births which are, imperfect, proxies for IVF use), while the findings suggest no change in abortion rates. For brevity, we focus here on contraceptive use, but note that IVF interest might work to increase birth rates. Assuming that the time required for an office visit a physician is 2.5 hours,[37] then one−half additional day of PSL per year would provide sufficient time to obtain a prescription for oral contraception or to have a long acting reversible contraceptive (LARC) inserted.[38] Post−PSL mandate, contraceptive use among women of child−bearing age is predicted to increase by 6.7%. This estimated increase is more than three times greater than the estimated (2.2%) reduction in annual birth rates just described. This finding suggests that changes in contraceptive 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).

Event−study estimates for our preferred birth rate specifications are reported in Figure 4. The results indicate that adopting and non−adopting states followed similar pre−trends and that PSL effects emerge approximately two years after mandate adoption

[37]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 assumed substantially longer physician, total office, or travel times, one day of PSL would allow for an office visit.

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

and increase over time. The gradual emergence of effects is expected 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 take time (e.g., if patients receive birth control during their annual physical examination, births occur approximately nine months after contraception). 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 A6, we estimate event−studies using different specifications and samples to assess robustness. We also report our main specification and sample for comparison. First, we use −4 rather than −1 as the omitted period, recall that we trim the data in event time and thus −4 is homogeneous in time−to−event. Second, we remove the time−varying state−level covariates from the regression. Third, we drop states that adopt a PSL mandate after the end of our primary study period (2019). Fourth, we code post−2019 adopting states as untreated for all policy leads and lags (i.e., treating these states as though they never adopt a PSL mandate). Fifth, we include the pandemic years (2020−2021). The results are similar across all specifications and samples.

To further explore the importance of differential pre−trends between PSL adopting and non−adopting state , we have de−trended the birth rate data.[40] Results are reported in Table 5 column (2) and are very similar to our main findings, suggesting a 2.1% reduction in the birth rate following adoption of a PSL mandate. We also examine the logarithm of the birth rate to assess the importance of skewness in the birth rate outcome. These results, shown in column (3) of the table are not appreciably different from our main coefficient estimate (−2.2%).

We next examine heterogeneity in the effect of PSL mandates on birth rates across mothers' demographics. We first estimate separate regressions by mother's education (less than college education and a college degree or higher), race (White and non−White), ethnicity (Hispanic and non−Hispanic), and age (≤20 years, 21−29 years, 30−39 years, and 40 or more years). When doing so, we adjust the population used as the denominator in the birth rate calculation (i.e., education, race, ethnicity, and age).[41] Since the

[39]Eight states have a partially treated effective year (see Table 1).

[40]To this end, for each state that adopts a PSL mandate by 2019, we estimate the trend in birth rate rate in the pre−mandate period, while for each state that does not adopt a mandate by 2019, we estimate the trend over the full study period (2007−2019). We then remove the estimated trend for each state from the birth rate variable and estimate Equation 1.

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

baseline means will 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 from our heterogeneity analysis by education and race/ethnicity are reported in Figure 5 and those by age in Figure 6. Corresponding event−studies are displayed in Figure A7. The most notable difference is the large reduction in birth rates for women with less than a college education (−7.6%), versus no change for those with a college degree. There are substantial decreases for White mothers (−3.1%), versus no change or slight growth among non−White mothers. Births among Hispanic mothers decline by −2.1% while births among non−Hispanic mothers do not appear to decline. The data also suggest that the reductions in birth rates decrease with age for women 21 and older, with no change or increases in births for those younger than 21, with the largest reduction in birth rates observed among mothers ages 21 to 29 years. As discussed in Section 1, ex ante there are reasons to expect that PSL mandates could increase or decrease birth rates. While overall we observe a decrease in birth rates post−mandate, our heterogeneity analysis here hints that − for distinct sub−populations of women − the net effect of the offsetting channels may differ.[42]

Mandated PSL can also allow pregnant women to potentially plan a Cesarean birth and to receive prenatal care. To explore these possibilities, we estimate the effect of PSL mandates on annual birth rates for which the mother did and did not i) have a Cesarian birth and ii) receive prenatal care. DID estimates are reported in Figure 7 and event−studies are reported in Figure A8. Our estimated effects are driven by Cesarean births and births for which prenatal care is received: post−mandate Cesarean births and births with prenatal care decline by 4.7% and 5.4% respectively, with no change in other birth rates. We note that births for which no prenatal care is received are quite rare, reflecting 2% of births, and we may be under−powered to detect effects.[43]

[42]We have bootstrapped the difference in the coefficients using a parametric bootstrap procedure with 500 repetitions. The difference in the coefficient estimates for education, race, and age groups is statistically different from zero at the 5% level or better, but the coefficient estimate difference for the ethnicity groups is not statistically different from zero.

[43]We also investigated whether PSL mandates are associated with birth outcomes such as low birth-weight, premature births, and APGAR scores. Given reductions in birth rates, this analysis is potentially vulnerable to compositional shifts in the births that occur and thus findings are difficult to interpret causally. With this caveat in mind, we do not observe any consistent pattern of results and the event−studies, in several cases, suggest pre−trends or inconsistent patterns of post−treatment effects. Thus, we do not feel that our analysis provides credible evidence of changes in these birth outcomes due to PSL policy adoption. These results are available on request.

4.3 Bias from dynamics and heterogeneity in treatment effects

Recent econometric work shows that using TWFE regression to estimate DID with staggered policy roll−out (as is the case for the state PSL mandates we study) can be vulnerable to bias from dynamic and heterogeneous treatment effects (Goodman-Bacon, 2021), resulting in a poor quality estimate of the ATT. The TWFE ATT is essentially a weighted average of all possible two−by−two DID comparisons available in the data. Some of these comparisons contrast treated states to never treated states and others compare early treated states to those treated later. These comparisons are 'reasonable.' Other comparisons will be 'unreasonable' or 'forbidden,' specifically those where later treated units are compared to those treated earlier (Borusyak et al., 2021).[44] If there are dynamics in treatment effects (e.g., PSL mandate effects 'grow' over time as employees learn about their new benefits and increasingly use them, as suggested by our event−study results), then the forbidden comparisons will provide biased estimates of the ATT. TWFE, all else equal, also upweights treated states that experience the policy change in the middle of the panel ('variance weighting') which, in our setting, includes California, Connecticut, and Massachusetts (see Figure 2).[45] Heterogeneity in treatment effects across treated units, combined with TWFE variance weighting, could therefore provide another reason why TWFE may return a poor estimate of the ATT.

To assess whether our results are vulnerable to biases from dynamic treatment effects, we estimate the decomposition proposed by Goodman-Bacon (2021). The results, reported in Table 6, and show that our overall ATT is composed of 2.9% comparisons of early vs. late treated states, 89.1% comparisons of treated vs never treated states, and 8.0% of late vs. early treated states. Thus, 92% of the comparisons that contribute to our overall TWFE ATT appear reasonable. Given our large comparison group (just 6.7% of state−period pairs are treated, see Table A2), these results are not surprising. Further, the estimates of the ATT are relatively homogeneous: they all carry a negative sign and range from −0.35 (early treated vs. late treated) to −1.04 (treated vs. never treated). Finally, excluding the earliest treated unit (DC), and thus removing the largest contributor to forbidden comparisons (Section 5), does not alter our findings.

[44] Comparisons using the 'always treated' group are possible in some settings, however we do not have such a group since the earliest state PSL policy was implemented two years after the start of our study period; thus we do not emphasize this comparison here.

[45] We can summarize the treatment variable (lagged PSL mandate) for each treated state and determine which of those states has values closest to 0.50, these states will be upweighted in our TWFE regression. We report states and the share of the year the lagged PSL mandate variable is in effect in parentheses: CO (0), MN (0), NM (0), NY (0), MD (0.08), NJ (0.08), RI (0.08), WA (0.08), AZ (0.15), VT (0.15), OR (0.23), CA (0.31), MA (0.31), CT (0.54), and DC (0.85). This analysis suggests that CA, MA, and CT will be upweighted in TWFE regressions.

We confirm the suggestive evidence from the Goodman−Bacon decomposition, that bias in TWFE regressions is likely to be modest, using the two−step DID (TSDID) approach proposed by Gardner (2022).[46] In the first step, parameter estimates for time-varying state−level covariates, state fixed−effects, and year fixed−effects are estimated using untreated data only. These parameter estimates are then used to residualize outcomes for both the treated and untreated observations. In the second step, the residualized outcomes are regressed on the treatment variable (here lagged PSL mandates) using the sample of all (treated and untreated) observations. TSDID is developed in a generalized method of moments framework. Standard errors account for the two−step estimation and within−state clustering (Hansen, 1982).

The TSDID results are reported in Table 7. PSL mandate adoption is estimated to result in 1.49 to 2.38 fewer births per 1,000 women of child−bearing age, in columns (1) through (3), a 2.4% to 3.8% decline over the period 2007 to 2019. This reduction is similar, although for each specification of slightly larger magnitude than for the corresponding TWFE coefficient estimate.[47] Including the pandemic period (column [4]) barely changes the coefficient estimate: birth rates decline by 1.62 per 1,000 women 16−44 years or 2.6%. The Gardner (2022) estimator allows the researcher to estimate a variant of an event−study to examine violations of the parallel trends assumption and to assess dynamics.[48] These results, reported in Figure 8, again reveal no substantial evidence of a violation of the parallel trends assumption.

We also report results using a DID procedure proposed by Callaway and Sant'Anna (2021) and a stacked difference−in−differences estimator. 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 estimator for DID with a staggered policy roll−out. Results, which are similar to those generated using TWFE and TSDID, are reported in Table A3.

Given the results of the Goodman−Bacon decomposition and the similarity between the TSDID, alternative estimators, and TWFE regressions, for brevity we report only TWFE regression results for the remainder of the manuscript.

[46] We implement this estimator using the −did2s− procedure by Butts and Gardner (2021).

[47] Prohibiting forbidden comparisons is predicted to increase the size of the coefficient estimates in most cases as forbidden comparisons generally lead to attenuated estimates of the ATT.

[48] There is no omitted category in this specification. See Gardner (2022) for a discussion.

4.4 Additional threats to the identification strategy

One potential concern is that 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, general medical care, and family planning services.[49] 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 or alter wages through employer response or improvements in productivity, these could be similar channels linking PSL mandates to birth rates. Finally, changes in birth rates could alter rates of marriage or cohabitation (for example, pregnancy may induce some couples to enter marriage or cohabitation when they would not otherwise do so).[50]

To address these possibilities, we collect data from the ASEC−CPS on past−year migration and from the basic monthly CPS on any and full−time employment, usual hours worked, the logarithm of wages (conditional and unconditional), and marriage or cohabitation (Flood et al., 2022). We again focus on women 16−44 years and regress these indicators on the lagged PSL mandate, state−level (policies and demographics) controls, and state− and year− fixed−effects. Results, reported in Tables 8 and 9, provide no indication that PSL mandates induce women of child−bearing age to migrate towards (or away from) states where PSL is mandated, or lead to substantial changes in full-time employment, usual hours, or marriage/cohabitation. However, PSL mandates may result in very small (<1%) increases in employment probabilities and, among women who are employed, a slight (0.8%) increase in wages, though this finding may partially reflect modest selection effects. 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 PSL also show that wages rise post−mandate (Pichler and Ziebarth, 2020a; Maclean et al., 2020) 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. Figures A9 and A10, which report event−studies for these outcomes, supply no indication of differential pre−trends, or of policy effects on migration or marriage/cohabitation, while raising the possibility of longer−term gains in any or full−time employment.

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

[50] We note that some readers may view these behaviors as additional channels linking PSL to birth rates.

As a further check, Figure 9 reports balance tests where each time-varying control variable included in Equation 1 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; however, those adopting PSL mandates have a very slightly higher share of non−White residents ($\widehat{\beta} = 0.01$, which corresponds to a 4.8% difference).[51] 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.

5 Robustness checks

We conduct a series of robustness checks to assess the stability of our main outcome − annual birth rates − across alternative specifications, samples, and time periods. These are reported graphically in Figure A11, with our primary specification and sample also included for ease of comparison.

First, we construct a measure of either PTO or PSL mandate as our treatment variable, as PTO may offer similar benefits to women as PSL (though with less protections for workers). Second, we use alternative lag structures for PSL mandates (no lag and a two−year lag). Third, we estimate unweighted regressions. Fourth, we exclude, sequentially, i) Washington DC, 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. Fourth, we use California only as the treatment group, given the size of this state, and exclude all other PSL adopting states. Fifth, some studies (Pichler et al., 2021) code the effective date for DC as 2014 and not 2009, hence we include DC in the sample and code the mandate as effective in 2014 onward. Sixth, we match the PSL mandate data to the birth records on occurrence state and year, residence county and year (and using sub−state PSL policies), and residence state, year, and month. Seventh, we use the data at the county−year level and incorporate sub−state PSL mandates (here we replace state fixed−effects with county fixed−effects, and use the county population as the weight). Eight, we include an extended set of controls (whether the state has legislation around IVF coverage in private health insur-

[51] This finding is likely due to some states adopting PSL mandates (e.g. Arizona, California, Maryland, New York, and Washington DC are some of the more racially and ethnically diverse states in the U.S.) (Kaiser Family Foundation, ND).

ance plans that the state can regulate (The National Infertility Association, 2023), the number of family planning clinics in each state (U.S. Census Bureau, 2022),[52] whether the state expanded Medicaid with the Affordable Care Act (Kaiser Family Foundation, 2023b), and whether the state borders another state with a PSL mandate in place.[53] Finally, we control political preferences as proxied by the political part of the Governor (University of Kentucky Center for Poverty Research, 2023).[54] Our results are stable to these changes, with negative and similarly sized coefficient estimates in all cases.

We also implement a 'leave−one−out' analysis, where each treated state was sequentially 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 estimates become considerably less precise when California is excluded.[55]

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 find that PSL mandates raise the use of contraceptives which, *ceteris paribus*, would be expected to lower birth rates, and without any corresponding change in abortion rates. Conversely, some women may have difficulty getting pregnant. If rights to PSL facilitate fertility treatments, this policy may have the offsetting effect of increasing birth rates. We do not have direct evidence on the use of such treatments but are able to examine internet searches for in−vitro fertilization as a proxy. Our results hint at the possibility that PSL mandates increase these searches; however, the effects are small in magnitude and generally statistically insignificant. The combined result of these factors is that PSL policy adoption reduces birth rates by 2.2% in our preferred specification. The results do not appear to be driven by common threats to identification, such as a violation of parallel trends or bias from staggered treatment adoption. PSL

[52] 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.' Please see `https://shorturl.at/vABDH`, last accessed October 5th, 2023.

[53] We have also estimated contraception, IVF, and abortion regressions including these controls and results are not appreciably different than those reported in the manuscript.

[54] We treat the Mayor of DC as the de−facto Governor of that jurisdiction following Maclean and Saloner (2018).

[55] In unreported analyses available on request from the corresponding author, we have estimated Equation 1 for each cohort of treatment states (i.e., states that adopt a PSL policy in the same year). The comparison group in each specification is the set of states that do not adopt PSL mandates. Results are relatively similar across the cohorts.

mandates also have modest employment effects, slightly increasing employment rates and wages conditional on working, but these seem unlikely to explain the increases in fertility. Specifically, the resulting income effects would be expected to increase fertility, if children are a normal good, which could be offset by the substitution effect associated with higher wages. However, with relatively small labor supply elasticities, both effects should be modest.

Studies of other types of policy interventions also frequently find associated 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. Similarly, 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) uncovered an 8.6% reduction.[56] We find a reduction of 2.2% in the birth rate, which is smaller than most estimates of ACA impacts.

The birth rate effects are potentially heterogeneous across population groups, with suggestions of larger relative reductions for non−college than college−educated women, White vs. non−White women, Hispanic vs. non-Hispanic women, and 21−25 year olds compared to 35−44 year olds. Some of these patterns are expected while others are not. Given disparities in access to PSL voluntarily provided by employers (Cook, 2011; DeSilver, 2020; Maclean et al., 2020), ex ante we anticipated that less educated mothers would be more strongly affected by PSL mandates than their more educated counterparts, and this was observed in the data. Stronger birth rate reductions for younger than older women also make sense if the former group is relatively more interested in avoiding pregnancy (at the given age) while their older peers might more often wish to conceive, but have difficulty doing so. Younger women may also be less likely to work in jobs that offer PSL, as compensation (including benefits) tends to increase with work experience.[57] On the other hand, we also expected stronger PSL mandate effects for non−White women than White women, given that the former potentially have less access to PSL voluntarily provided by their employers and so would be more likely to gain coverage when a state mandate is adopted. Assessing why we do not observe this pattern of results is beyond the scope of the current analysis but several explanations seem possible. Other barriers (e.g., differences in insurance coverage, the ability to pay for healthcare services, and discrimination within the healthcare system) may mute effects for non−White women, who

[56] 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.

[57] The lack of an effect for women under 20 is likely to reflect the low employment rates (and so potential for PSL coverage) for this age group.

may also be more likely to work in exempt (e.g. part-time) jobs or those where employers are less likely to comply with state PSL mandates. Hegland and Berdahl (2022) note the importance of 'flexible' work arrangements, independent of PSL, for healthcare use, and such arrangements are particularly important for non−White workers. We acknowledge that these hypotheses are not fully developed and encourage additional research on these heterogeneous effects of PSL across sub−populations of the workforce.

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 the national birth rate. In 2021, there were 3,664,292 registered births in the U.S. (Martin et al., 2022), 2,468,155 occurring in states without a PSL mandate. Assuming the federal mandate was similar in generosity to the state mandates we examine, our preferred model predicts a 2.2% reduction in these births, corresponding to a decline of around 54 thousand births nationally (2,468,155 $\times$ $-0.022 = 54{,}299$).[58]

Our study is subject to caveats. First, the extent to which the results obtained using early adopting PSL states would generalize to other states that (may) adopt PSL in the future is unclear. Second, our data are somewhat limited along key dimensions: our proxy for fertility treatment (internet searches for IVF) is not ideal; the abortion data are incomplete; and the information contraception is available for only some states and years. Third, we do not rigorously examine downstream effects on children nor study other effects of PSL mandates.

Not withstanding these limitations, 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 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.[59] There are potential broader questions about the effects of any resulting decline

[58] 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.

[59] 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

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

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).

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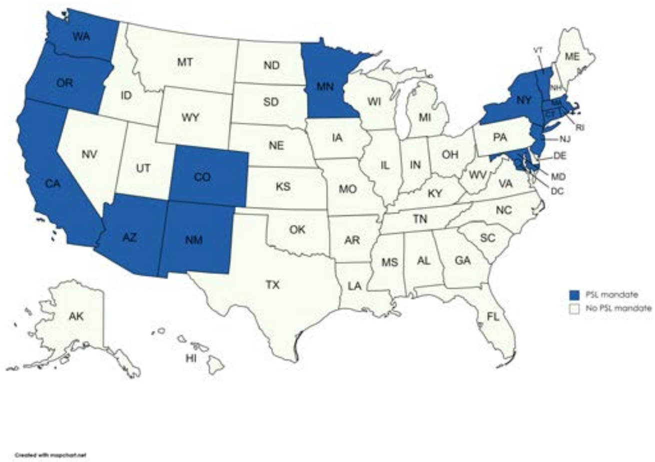
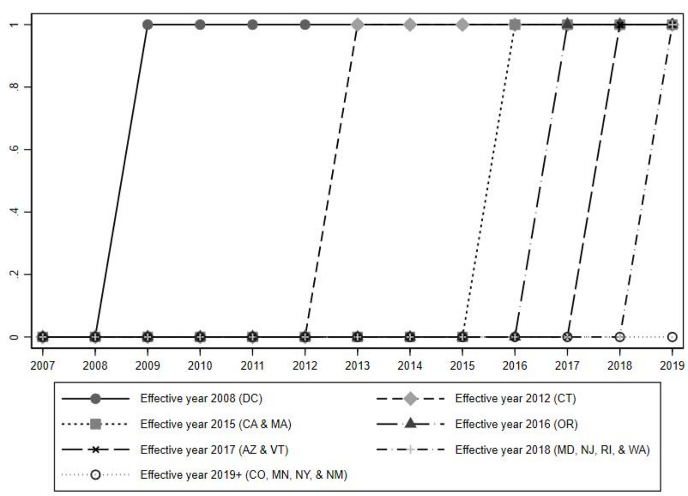
7 Tables and figures

Figure 1: Geographic distribution of state PSL mandates in the U.S. as of 2023 Q3



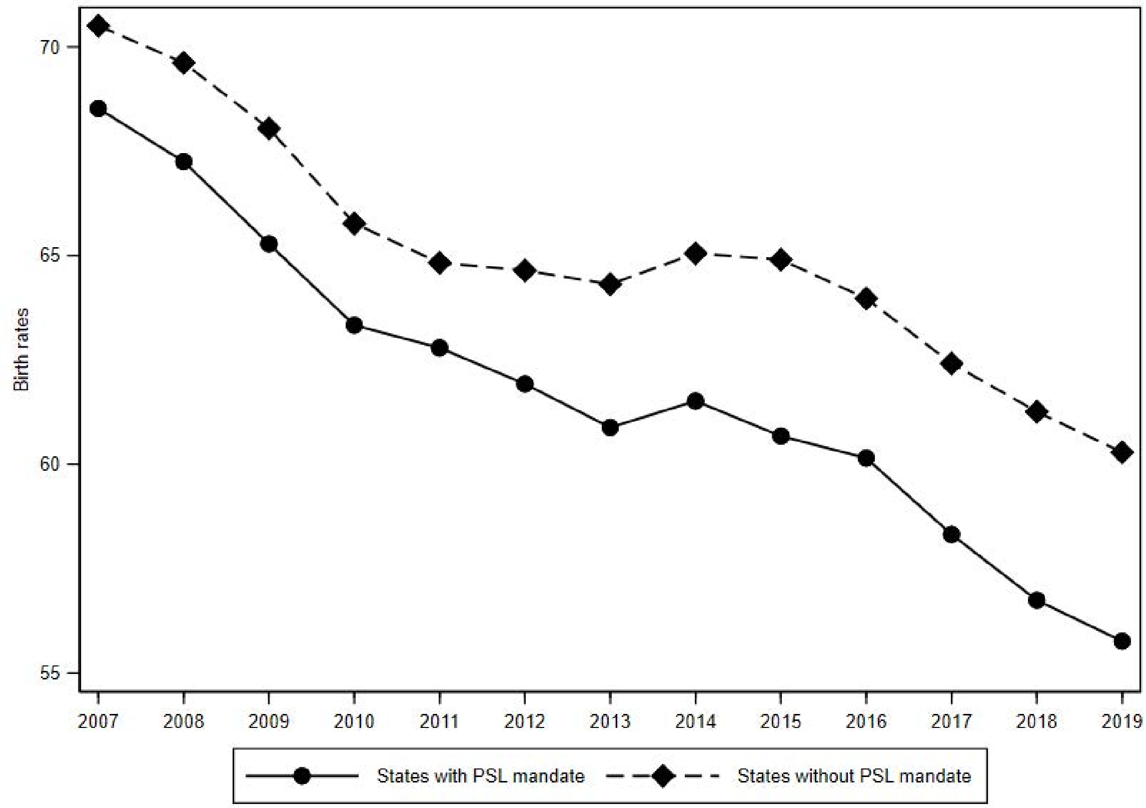
Notes: Data source is National Partnership for Women & Families (2023). State PSL mandates effective or announced by 2023 Q3.

Figure 2: Temporal distribution of state PSL mandates in the U.S. as of October 2023



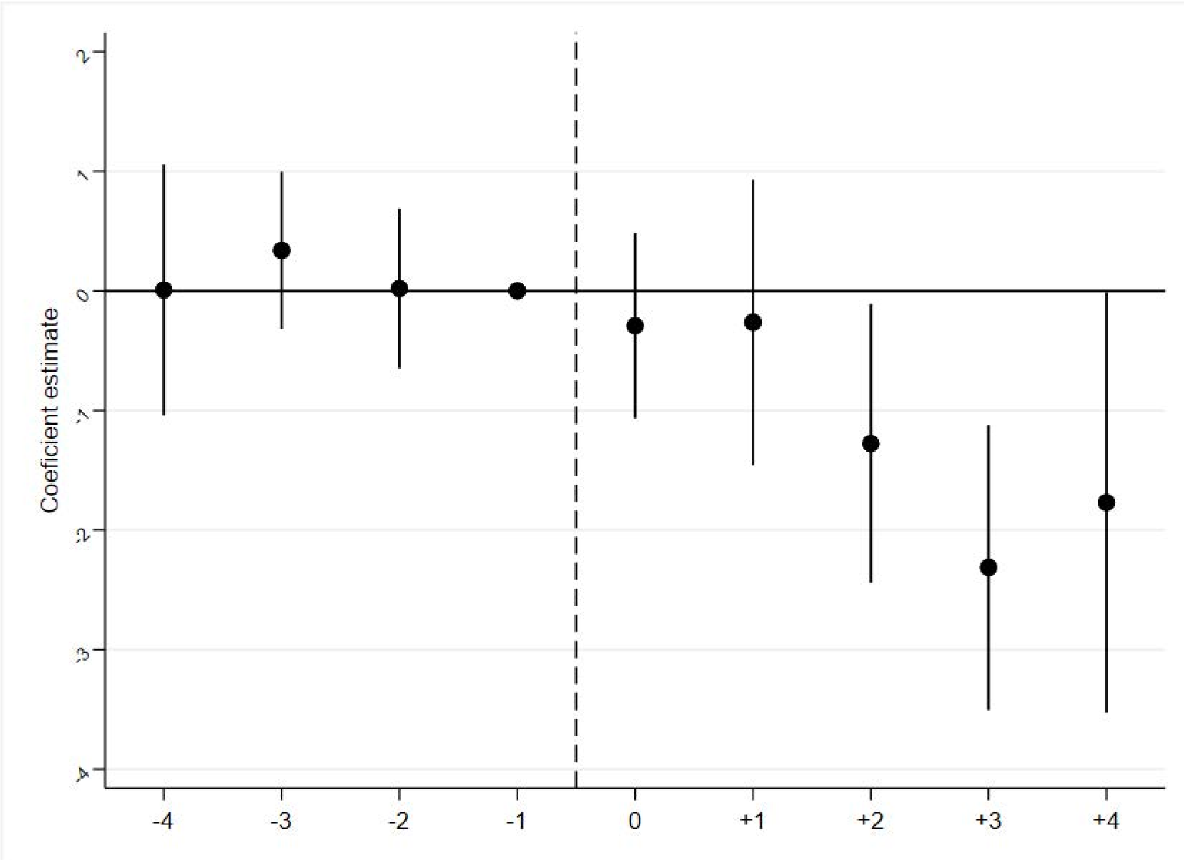
Data source is National Partnership for Women & Families (2023). State PSL mandates effective or announced by October 2023.

Figure 3: Trends in birth rates: NCHS 2007 to 2019



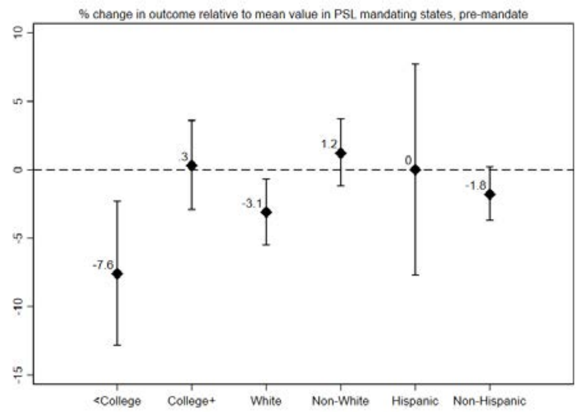
Notes: NCHS = National Center for Health Statistics. Data are weighted by the state/year population of women aged 16−44 and aggregated to the state−year−treatment level.

Figure 4: Effect of a state PSL mandate on annual birth rates using a two−way fixed−effect event−study: NCHS 2007−2019



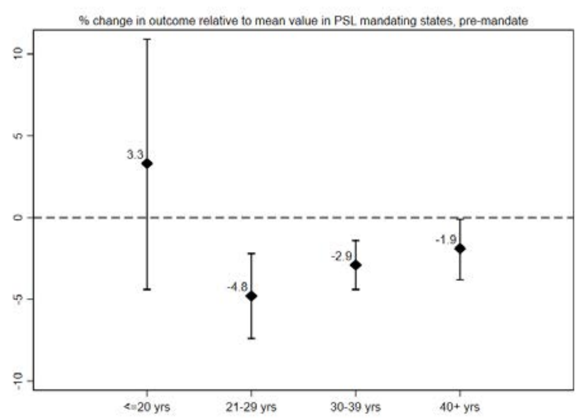
Notes: NCHS = National Center for Health Statistics. The dependent variable is the annual birth rate (per 1,000 state population of women aged 16−44). The unit of observation is a state in a year. Regressions are estimated with OLS and control for state policies and demographics, state and year fixed−effects. The leads and lags represent single-year bins corresponding to four years pre−law through four years post−law. The sample excludes observations more than four years prior to the event and more than four years after the event. 95% confidence intervals are reported with vertical lines and account for within state clustering. The omitted period is −1. Data are weighted by the state/year population of women aged 16−44.

Figure 5: Heterogeneity in the effect of a state PSL mandate on annual birth rates by mother's education, race, and ethnicity: NCHS 2007−2019



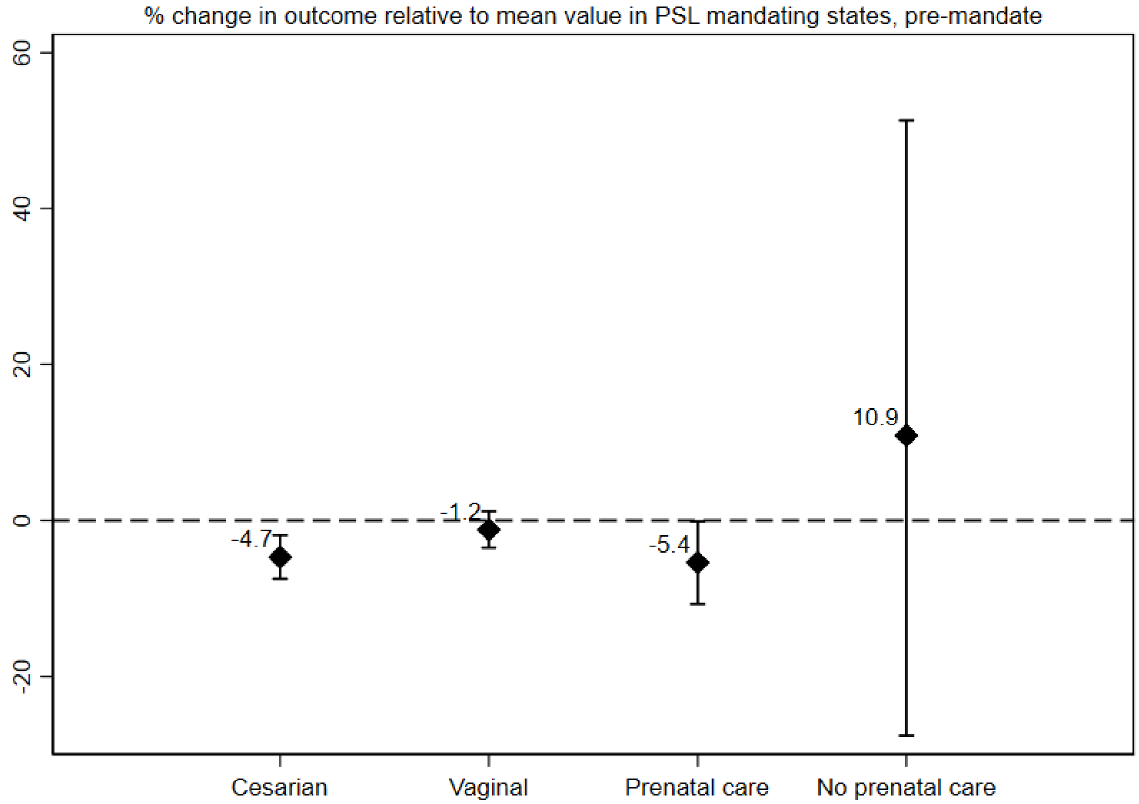
Notes: NCHS = National Center for Health Statistics. The dependent variable is the annual birth rate (per 1,000 state population of women aged 16−44). Regressions estimated with OLS and include lagged PSL mandate, state level policy variables and demographics, and state and year fixed−effects. The unit of observation is a state in a year. Data are weighted by the state/year population of women ages 16−44. 95% confidence intervals are reported with vertical lines and account for within state clustering.

Figure 6: Heterogeneity in the effect of a state PSL mandate on annual birth rates by mother's age: NCHS 2007−2019



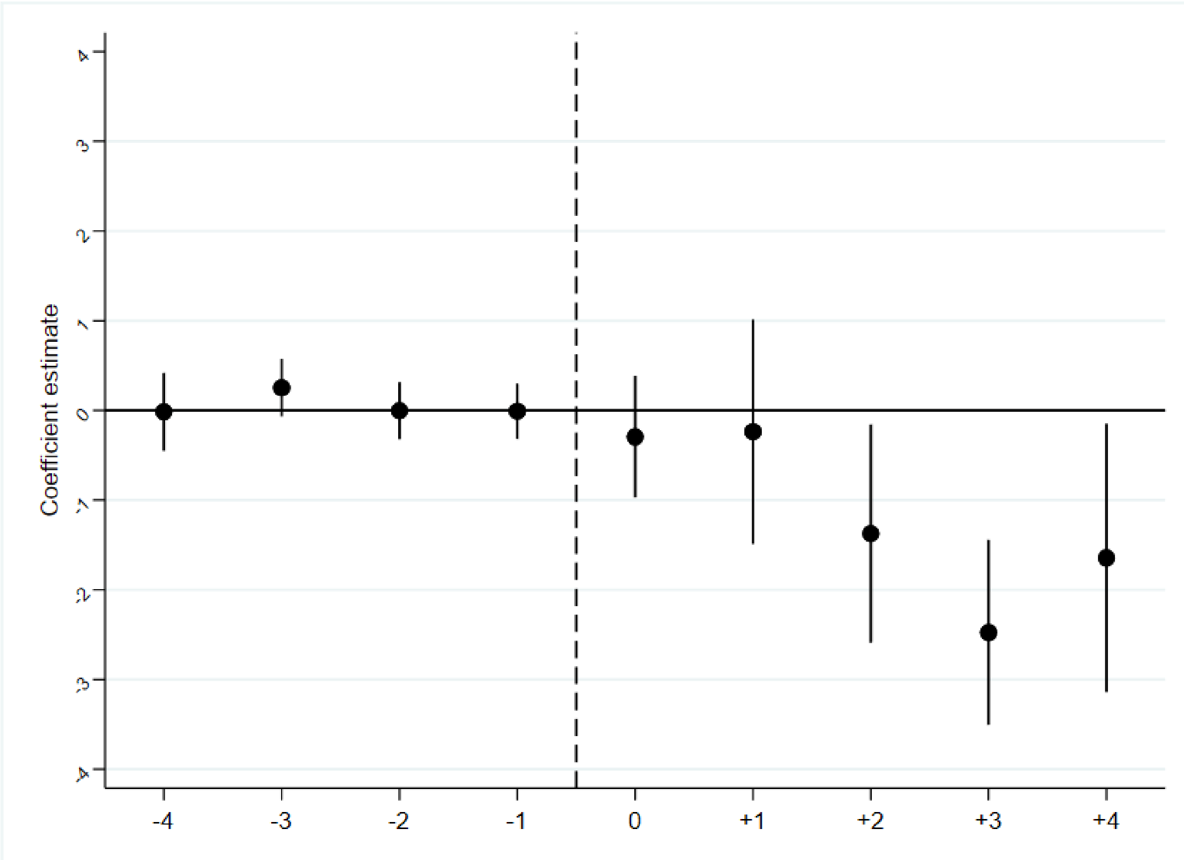
Notes: NCHS = National Center for Health Statistics. The dependent variable is the annual birth rate (per 1,000 state population of women aged 16−44). Regressions estimated with OLS and include lagged PSL mandate, state level policy variables and demographics, and state and year fixed−effects. The unit of observation is a state in a year. Data are weighted by the state/year population of women ages 16−44. 95% confidence intervals are reported with vertical lines and account for within state clustering.

Figure 7: Heterogeneity in the effect of a state PSL mandate on annual birth rates by birth characteristics: NCHS 2007−2019



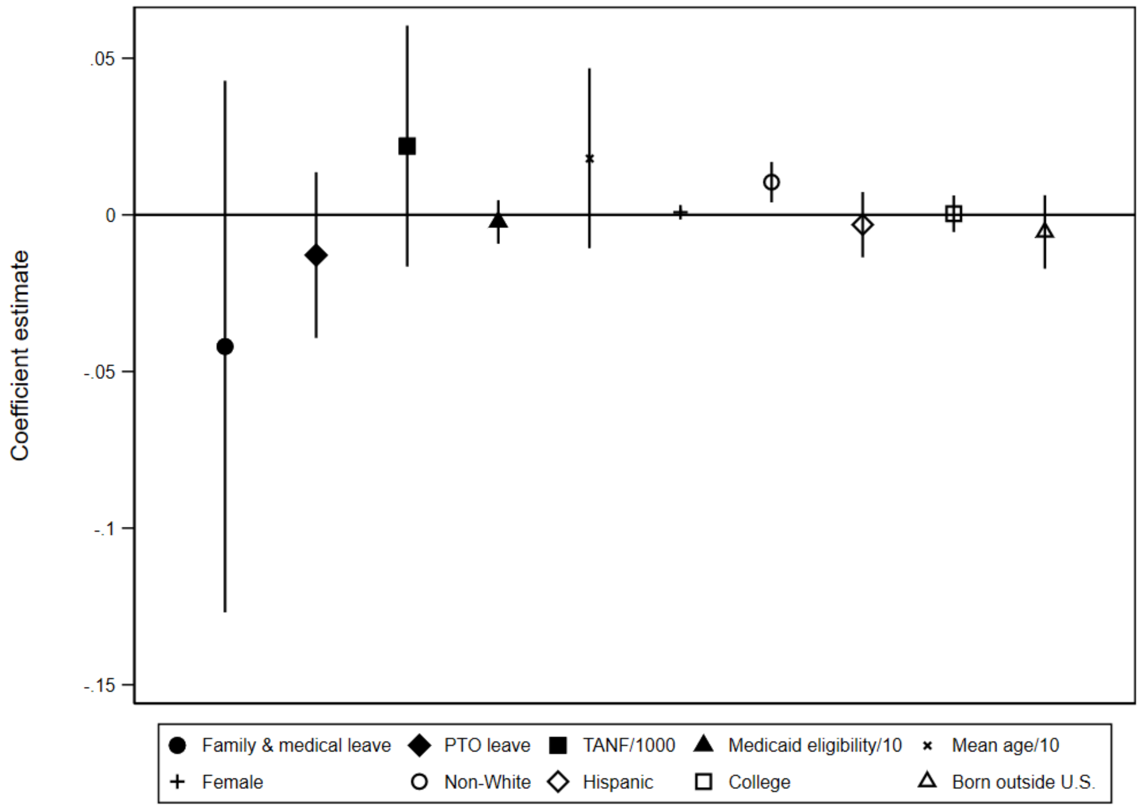
Notes: NCHS = National Center for Health Statistics. The dependent variable is the annual birth rate (per 1,000 state population of women aged 16−44). Regressions estimated with OLS and include lagged PSL mandate, state level policy variables and demographics, and state and year fixed−effects. The unit of observation is a state in a year. Data are weighted by the state/year population of women ages 16−44. 95% confidence intervals are reported with vertical lines and account for within state clustering.

Figure 8: Effect of a state PSL mandate on birth rates using a two−step event−study method proposed by Gardner (2022): NCHS 2007−2019



Notes: NCHS = National Center for Health Statistics. The dependent variable is the birth rate (per 1,000 state population of women aged 16−44). The unit of observation is a state in a year. Regressions control for state level policy variables and demographics, and state and year fixed−effects. The leads and lags represent single−year bins corresponding to four years pre−law through four years post−law. The sample excludes observations more than four years prior to the event and more than four years after the event. 95% confidence intervals are reported with vertical lines and account for within state clustering. Data are weighted by the state/year population of women aged 16−44.

Figure 9: Covariate balance: 2007−2019



Notes: The dependent variable is indicated on the x axis. The unit of observation is a state in a year. Regressions are estimated with OLS and include lagged PSL mandate, and state and year fixed−effects. 95% confidence intervals are reported with vertical lines and account for within state clustering. Data are weighted by the state/year population of women aged 16−44.

Table 1: State PSL mandate effective dates: National Partnership of Women and Families

State	Effective date	Employees gaining coverage for the first time
Arizona	7/2017	934,000
California	7/2015	6,900,000
Colorado	1/2021	813,000
Connecticut	1/2012	200,000
District of Columbia	5/2008	220,000
Massachusetts	7/2015	900,000
Maryland	2/2018	750,000
Minnesota	1/2024	N/A†
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: State PSL mandates adopted or announced as of October 2023. Estimates of employees gaining PSL coverage for the first time based on National Partnership for Women & Families (2023) 'Law/Bill Number and Impact.'

†The NPWF has not released data on the number of employees gaining PSL through the MN policy change.

Table 2: Effect of a state PSL mandate on access to and use of PSL: NCS 2009−2019

Specification:	(1)	(2)	(3)	(4)
Panel A: Access				
Paid sick leave mandate	0.101***	0.100***	0.099***	0.114***
(lagged one year)	(0.023)	(0.023)	(0.023)	(0.020)
Percent change	14.2%	14.1%	13.9%	16.1%
Pre−treatment mean,	0.710	0.710	0.710	0.710
treatment states				
Panel B: Use (Hours)				
Paid sick leave mandate	1.101**	1.162**	1.129*	2.071***
(lagged one year)	(0.538)	(0.564)	(0.570)	(0.565)
Percent change	5.1%	5.4%	5.2%	9.6%
Pre−treatment mean,	21.594	21.594	21.594	21.594
treatment states				
Observations	553,389	553,389	553,389	651,840
State policies	N	Y	Y	Y
State demographics	N	N	Y	Y
Includes pandemic years	N	N	N	Y

Notes: NCS = National Compensation Survey. Regressions estimated with OLS. All regressions include state and year fixed−effects. Specification 2 also includes state level policy variables. Specification 3 includes state level policy variables and demographics. Specification 4 is the same as specification 3 but includes the pandemic years (2020−2021). Data are weighted by NCS−provided weights. 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 PSL mandate on contraception use, interest in fertility treatment, and abortion rates: BRFSS, Google Insights, and CDC

Specification:	(1)	(2)	(3)	(4)
Panel A: BRFSS				
Any contraception				
Paid sick leave mandate	0.029*	0.040**	0.044**	–
(lagged one year)	(0.016)	(0.020)	(0.021)	–
Percent change	4.4%	6.1%	6.7%	–
Pre−treatment mean, treatment states	0.661	0.661	0.661	–
Panel B: Google Insights				
IVF searches				
Paid sick leave mandate	4.96	4.60**	3.00**	1.82
(lagged one year)	(3.04)	(2.22)	(1.35)	(1.47)
Percent change	7.0%	6.5%	4.2%	2.6%
Pre−treatment mean, treatment states	71.13	71.13	71.13	71.13
Panel C: CDC				
Abortion rate per 1,000 women 15−44				
Paid sick leave mandate	-0.19	-0.41	-0.07	0.42
(lagged one year)	(0.58)	(0.48)	(0.60)	(0.75)
Percent change	-1.2%	-2.5%	-0.4%	2.6%
Pre−treatment mean, treatment states	16.46	16.46	16.46	16.46
State policies	N	Y	Y	Y
State demographics	N	N	Y	Y
Includes pandemic years	N	N	N	Y

Notes: BRFSS = Behavioral Risk Factor Surveillance Survey. The BRFSS sample includes women ages 18−44 and years 2006, 2010, 2011, 2017, and 2019. The Google Insights sample includes years 2007−2019. The CDC sample includes years 2009−2019. There are 41 states and 67,432 observations in the BRFSS sample; 51 states and 661 and 763 observations in the 2007−2019 and 2007−2021 Google Insights samples (there are two states that do not have data in one year [Montana in 2007 and Wyoming in 2008] in the Google Insights data); and 47 states and 469 and 565 observations in the 2009−2019 and 2009−2021 CDC samples. Regressions estimated with OLS. All BRFSS regressions include state and time (quarter−year) fixed−effects. All Google Insights and CDC regressions include state and year fixed−effects. Specification 2 also includes state level policy variables. Specification 3 includes state level policy variables and demographics. Specification 4 is the same as specification 3 but includes the pandemic period (2020−2021). Specification 4 is not estimated in the BRFSS sample as there is no data beyond 2019. Data are weighted by BRFSS−provided weights in the BRFSS and by the state population that is female and 16−44 years of age in the Google Insights and CDC samples. 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 PSL mandate on annual birth rates using two−way fixed−effects regression: NCHS 2007−2019

Specification:	(1)	(2)	(3)	(4)
Paid sick leave mandate	-2.43**	-2.26**	-1.37**	-1.49**
(lagged one year)	(0.94)	(1.01)	(0.63)	(0.73)
Percent change	-3.9%	-3.6%	-2.2%	-2.4%
State policies	N	Y	Y	Y
State demographics	N	N	Y	Y
Includes pandemic years	N	N	N	Y
Pre−treatment mean, treatment states	63	63	63	63
Observations	663	663	663	765

Notes: NCHS = National Center for Health Statistics. All regressions include state and year fixed−effects. Specification 2 also includes state level policy variables. Specification 3 includes state level policy variables and demographics. Specification 4 is the same as specification 3 but includes the pandemic period (2020−2021). The dependent variable is the annual birth rate per 1,000 women 16−44 years. The unit of observation is a state in a year. Data are weighted by the state/year population of women ages 16−44. Regressions estimated with OLS. Standard errors are clustered at the state level and are reported in parentheses.
***,**,* = statistically different from zero at the 1%,5%,10% level.

Table 5: Effect of a state PSL mandate on annual birth rates using two−way fixed−effects regression with de−trended and logged outcomes: NVSS 2007-2019

Outcome:	Birth rates	De−trended birth rates	Logged birth rates
Paid sick leave mandate	-1.37**	-1.30**	-0.029***
(lagged one year)	(0.63)	(0.63)	(0.009)
Percent change	-2.2%	-2.1%	2.9%
Pre−treatment mean, treatment states	63	63	63
Observations	663	650	663

Notes: NCHS = National Center for Health Statistics. All regressions include state level policy variables and demographics, and state and year fixed−effects. The dependent variable is the annual birth rate per 1,000 women 16−44 years. The unit of observation is a state in a year. Data are weighted by the state/year population of women ages 16−44. Regressions estimated with OLS. 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: Goodman−Bacon (2021) decomposition: NCHS 2007−2019

Two−by−two comparison:	ATT	Weight
Early treated vs. late treated	-0.35	0.029
Treated vs. never treated	-1.04	0.891
Late treated vs. early treated	-0.44	0.080
Re−weighted ATT	-1.03	–
Pre−treatment mean	63	–
treatment states		–
Observations	663	–

Notes: NCHS = National Center for Health Statistics. The dependent variable is the annual birth rate per 1,000 women 16−44 years. The unit of observation is a state in a year. Data are unweighted. No time−varying covariates are included to isolate the two−by−two comparisons. ATT = average treatment on the treated.

Table 7: Effect of a state PSL mandate on annual birth rates using a two−step DID method proposed by Gardner (2022): NCHS 2007−2019

Specification:	(1)	(2)	(3)	(4)
Paid sick leave mandate	-2.38**	-2.25**	-1.49**	-1.62**
(lagged one year)	(0.97)	(1.04)	(0.60)	(0.71)
Percent change	-3.8%	-3.6%	-2.4%	-2.6%
State policies	N	Y	Y	Y
State demographics	N	N	Y	Y
Includes pandemic years	N	N	N	Y
Pre−treatment mean, treatment states	63	63	63	63
Observations	663	663	663	765

Notes: NCHS = National Center for Health Statistics. All regressions include state and year fixed−effects. Specification 2 also includes state level policy variables. Specification 3 includes state level policy variables and demographics. Specification 4 is the same as specification 3 but excludes the pandemic period (2020−2021). The dependent variable is the annual birth rate per 1,000 women 16−44 years. The unit of observation is a state in a year. Data are weighted by the state/year population of women ages 16−44. Regressions estimated a two−step DID procedure proposed by Gardner (2022). Standard errors are clustered at the state level and are reported in parentheses.
***,**,* = statistically different from zero at the 1%,5%,10% level.

Table 8: Effect of a state PSL mandate on past−year across−state moves using two−way fixed−effects regression: ASEC−CPS 2007−2019

Outcome:	Move across state lines in past year
Paid sick leave mandate	-0.002
(lagged one year)	(0.002)
Percent change	-9.5%
Pre−treatment mean, treatment states	0.021
Observations	512767

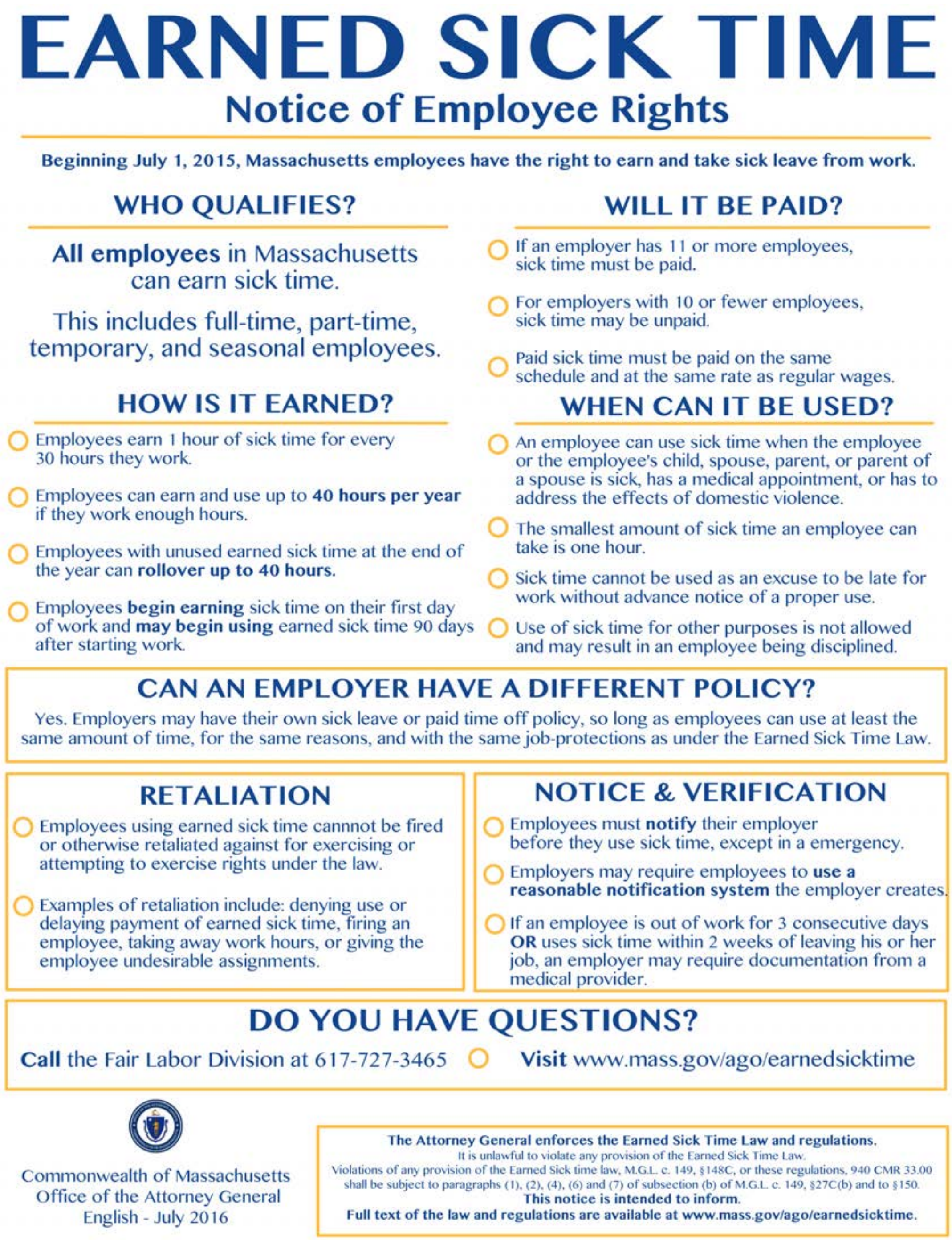
Notes: ASEC−CPS = Annual Social and Economic Supplement to the Current Population Survey. Data are weighted by ASEC−CPS−provided weights. Regressions estimated with OLS and control for state level policy variables and demographics, and state and year fixed−effects. Standard errors are clustered at the state level and are reported in parentheses.
***,**,* = statistically different from zero at the 1%,5%,10% level.

Table 9: Effect of a state PSL mandate on past−year across−state moves, employment outcomes, and marriage outcomes using two−way fixed−effects regression: CPS 2007−2019

Outcome:	Employed	Full time	Usual hours	Conditional wages	Unconditional wages	Married
Paid sick leave mandate	0.006*	0.007	0.077	0.008*	0.007	-0.002
(lagged one year)	(0.003)	(0.005)	(0.116)	(0.004)	(0.013)	(0.003)
Percent change	0.7%	1.1%	0.2%	0.8%	0.7%	-0.5%
Pre−treatment mean, treatment states	0.922	0.660	35.551	$16.311	$5.911	0.414
Observations	2676655	2493893	2333406	383200	966214	3829679

Notes: CPS = Current Population Survey. Conditional wages = zero wage observations not included in the sample. Unconditional wages = zero wages included in the sample. Married = married or cohabitating. Data are weighted by CPS−provided weights. Regressions estimated with OLS and control for state level policy variables and demographics, and state and year fixed−effects. 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: Massachusetts notice of PSL 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 cannnot 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?

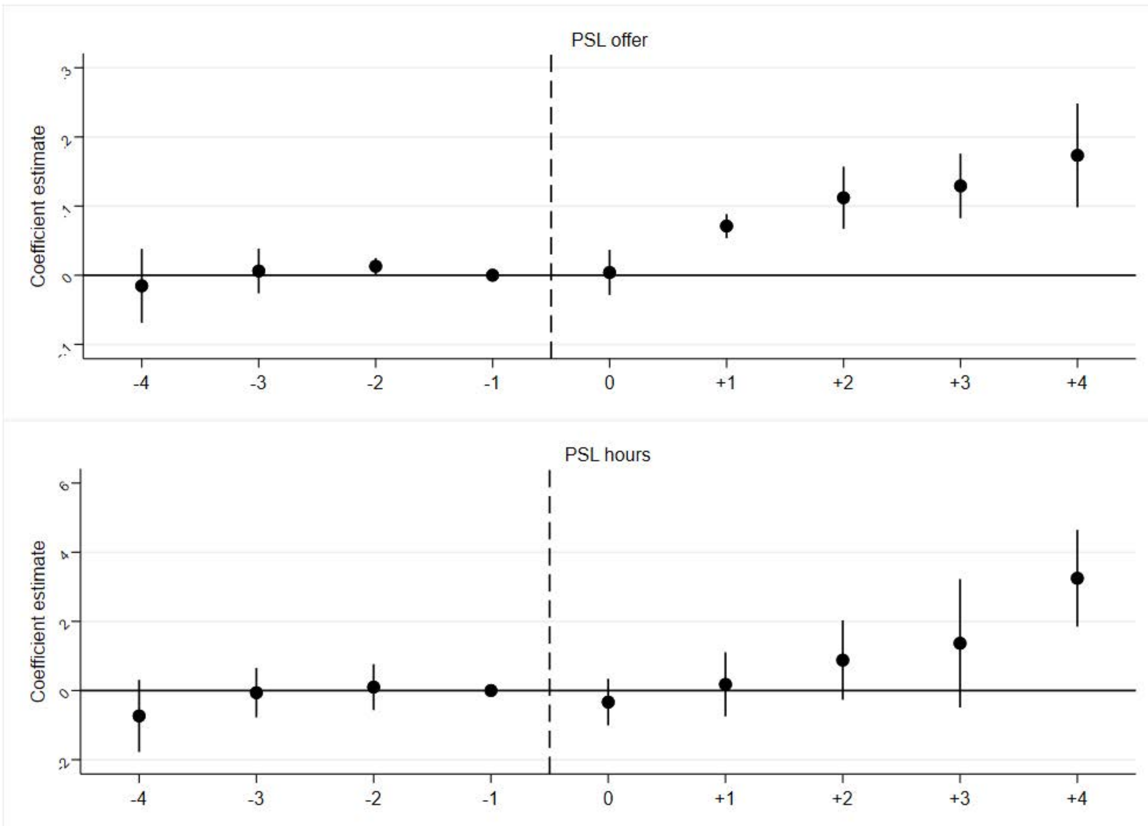
Call the Fair Labor Division at 617-727-3465 **Visit** www.mass.gov/ago/earnedsicktime

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.

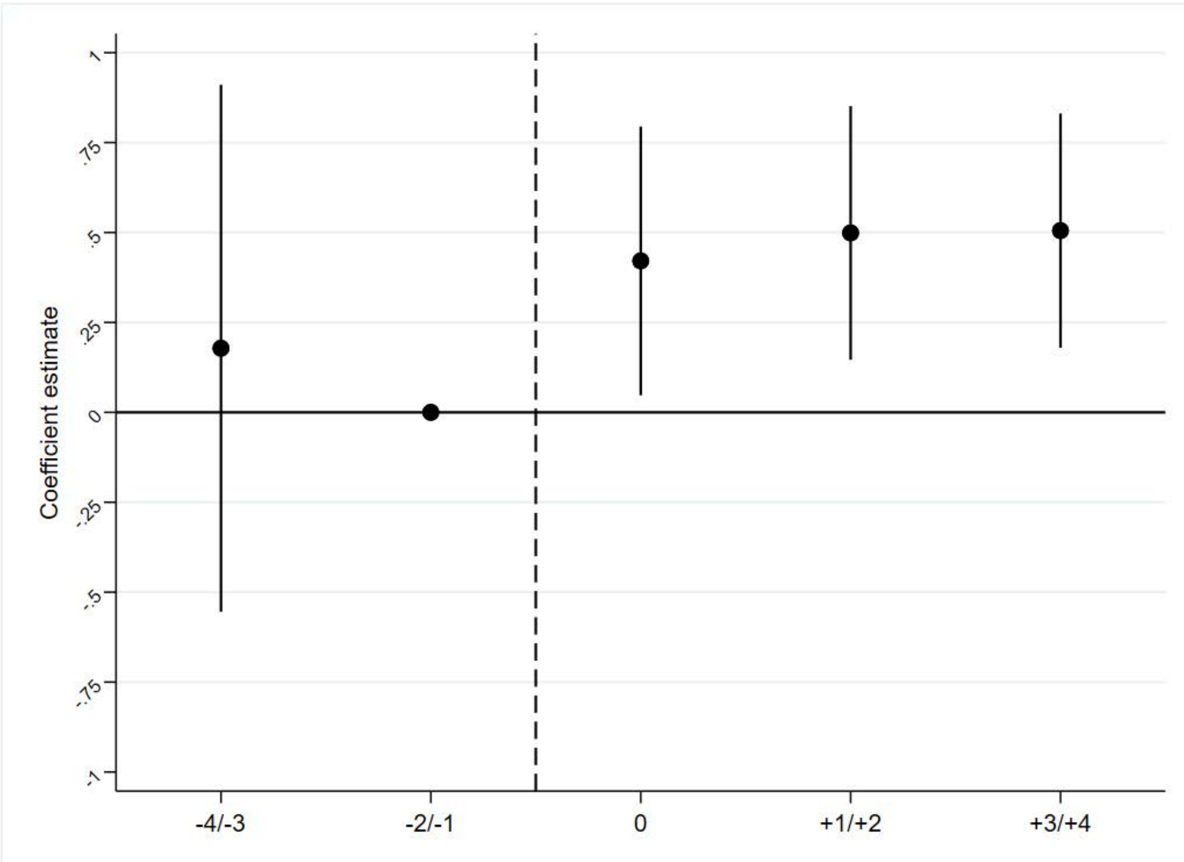
Notes: Source: Massachusetts Office of the Attorney General (https://www.mass.gov/info-details/earned-sick-time, last accessed May 26, 2023).

Figure A2: Effect of a state PSL mandate on access to and use of PSL using a two−way fixed−effect event−study: NCS 2009−2019



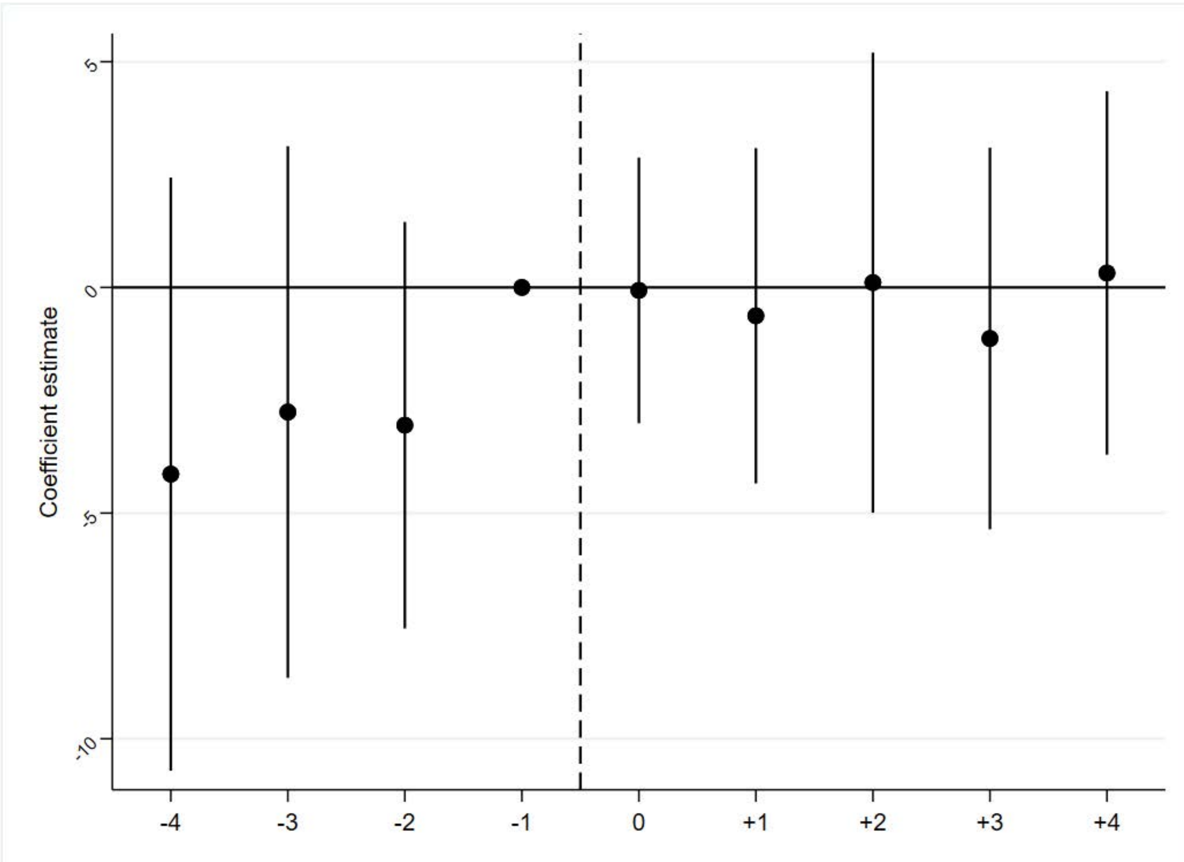
Notes: NCS = National Compensation Survey. The dependent variables are an offer of PSL and average quarterly use of PSL. The regressions is estimated with OLS and controls for state policy variables and demographics, and state and year fixed−effects. The leads and lags represent single−year bins corresponding to four years pre−law through four years post−law. The omitted period is −1. The sample excludes observations more than four years prior to the event and more than four years after the event. Data are weighted by NCS−provided weights. 95% confidence intervals are reported with vertical lines and account for within state clustering.

Figure A3: Effect of a state PSL mandate on any contraception use using a two−way fixed−effect event−study: BRFSS 2006, 2010, 2011, 2017, and 2019



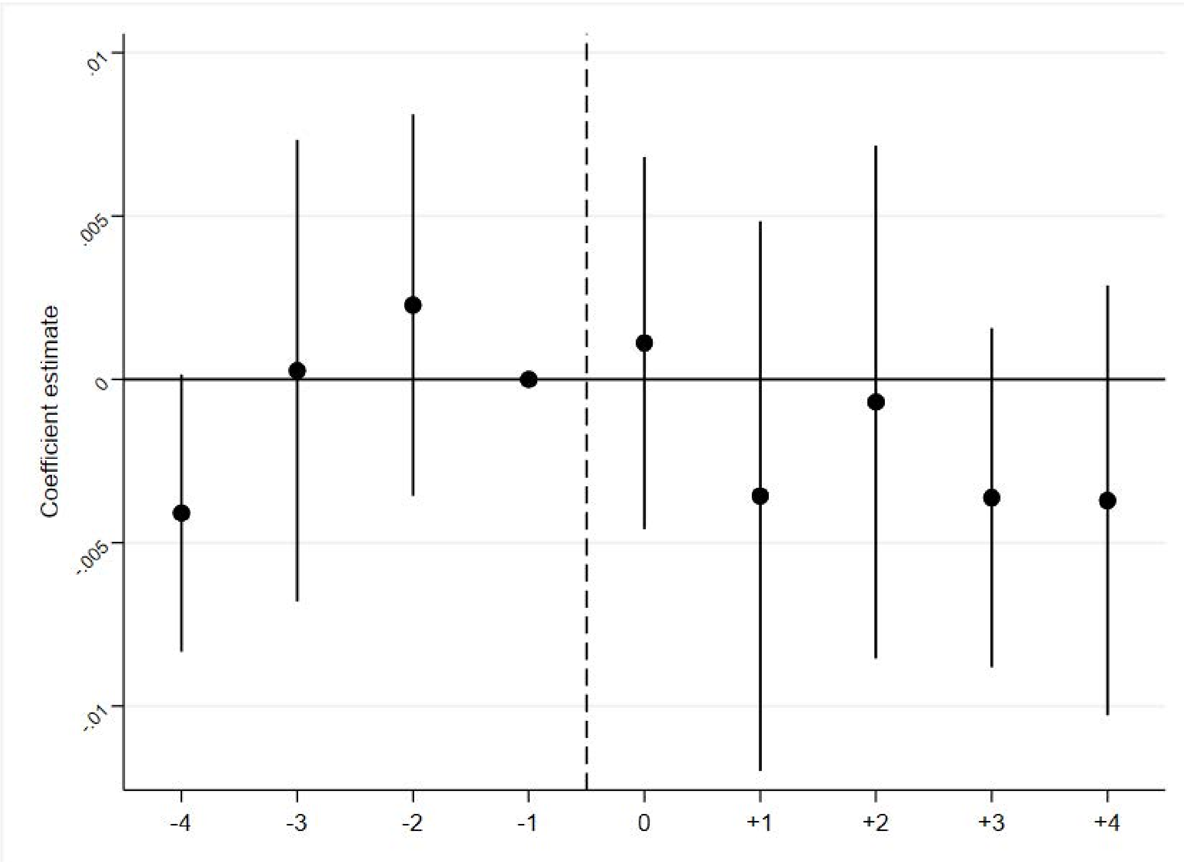
Notes: BRFSS = Behavioral Risk Factor Surveillance Survey. The dependent variable is contraception use in the past year among women ages 18−44. The regression is estimated with OLS and controls for state policy variables and demographics, and state and time (quarter−year) fixed−effects. The leads and lags represent single−year bins corresponding to four years pre−law through four years post−law. The omitted period is −1. The sample excludes observations more than four years prior to the event and more than four years after the event. Data are weighted by BRFSS−provided weights. 95% confidence intervals are reported with vertical lines and account for within state clustering.

Figure A4: Effect of a state PSL mandate on annual Google searches for IVF using a two−way fixed−effect event−study: Google Insights 2007−2019



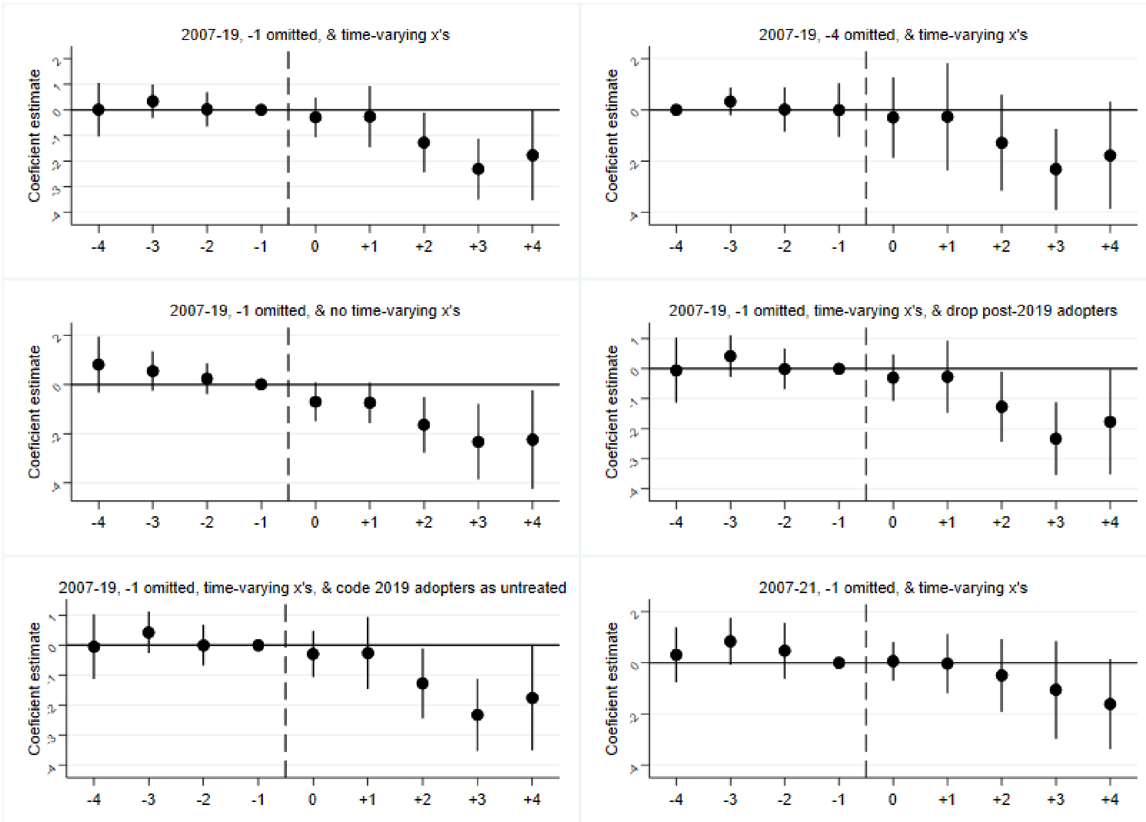
Notes: The dependent variable is the relative popularity of Google Searches for the term 'IVF' in a state. The unit of observation is a state in a year. The regression is estimated with OLS and controls for state policies and demographics, and state and year fixed−effects. The leads and lags represent single−year bins corresponding to four years pre−law through four years post−law. The omitted period is −1. The sample excludes observations more than four years prior to the event and more than four years after the event. 95% confidence intervals are reported with vertical lines and account for within state clustering. Data are weighted by the state/year population of women aged 16−44.

Figure A5: Effect of a state PSL mandate on annual abortion rates per 1,000 women 16−44 years using a two−way fixed−effect event−study: CDC 2009−2019



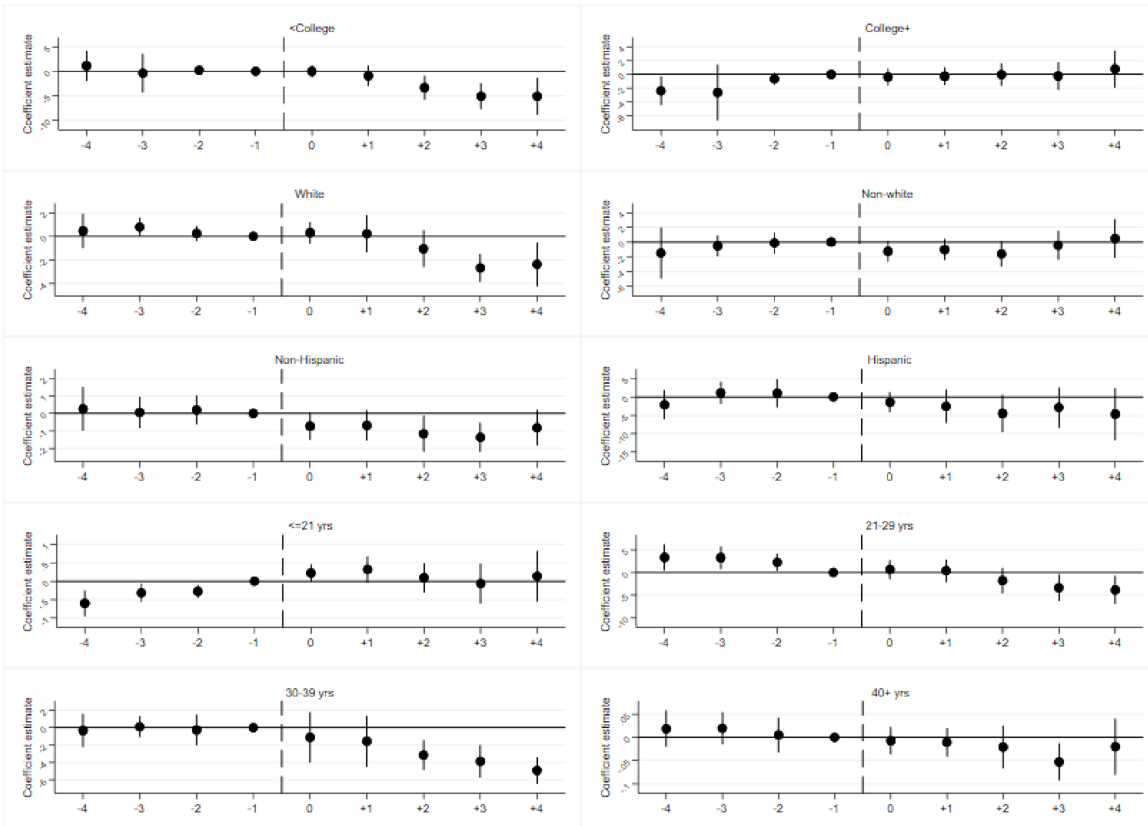
Notes: CDC = Centers for Disease Control and Prevention. The dependent variable is the annual abortion rate (per 1,000 state population of women aged 16−44). The unit of observation is a state in a year. The regression is estimated with OLS and controls for state policies and demographics, and state and year fixed−effects. The leads and lags represent single−year bins corresponding to four years pre−law through four years post−law. The omitted period is −1. The sample excludes observations more than four years prior to the event and more than four years after the event. 95% confidence intervals are reported with vertical lines and account for within state clustering. Data are weighted by the state/year population of women aged 16−44.

Figure A6: Effect of a PSL mandate on annual birth rates using a two−way fixed−effect event−study using different specifications and samples: NCHS 2007−2019



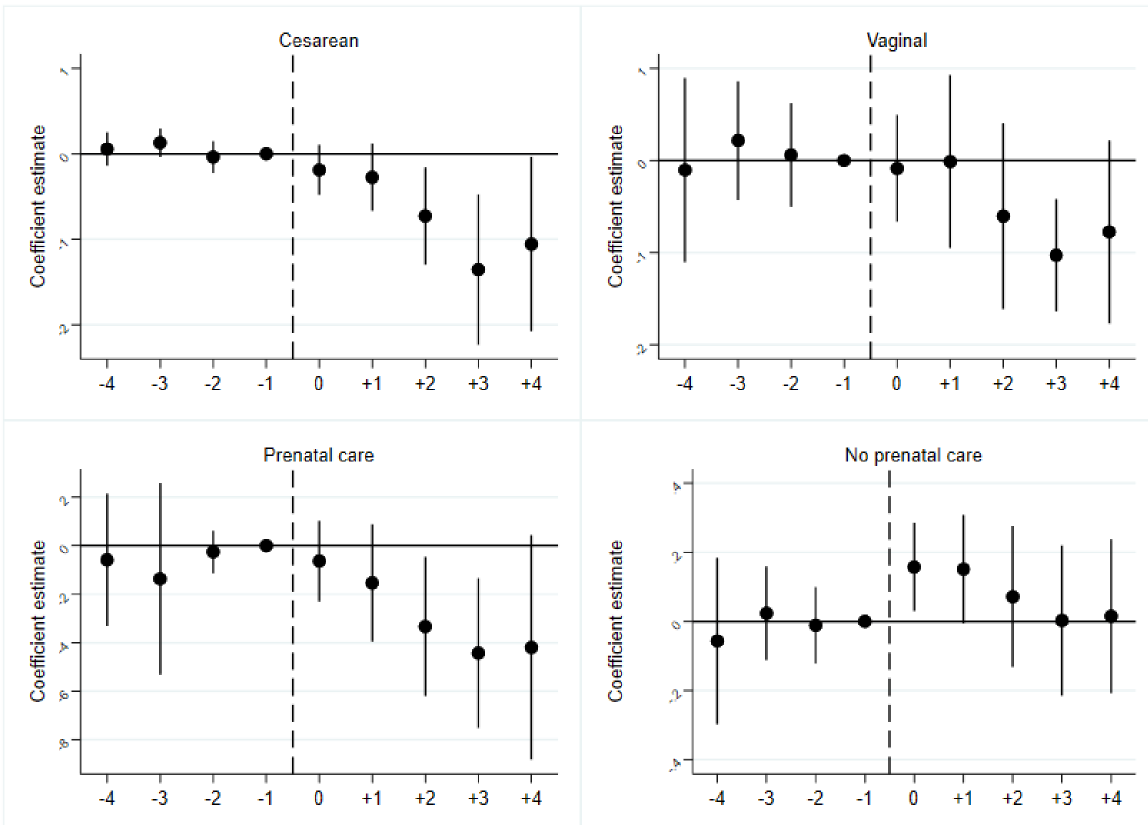
Notes: NCHS = National Center for Health Statistics. The dependent variable is the annual birth rate (per 1,000 state population of women aged 16−44). The unit of observation is a state in a year. Regressions are estimated with OLS and control for state policies and demographics, and state and year fixed−effects. The leads and lags represent single-year bins corresponding to four years pre−law through four years post−law. The sample excludes observations more than four years prior to the event and more than four years after the event. 95% confidence intervals are reported with vertical lines and account for within state clustering. Data are weighted by the state/year population of women aged 16−44.

Figure A7: Heterogeneity in the effect of a PSL mandate on annual birth rates by mother's demographics using a two−way fixed−effect event−study: NCHS 2007−2019



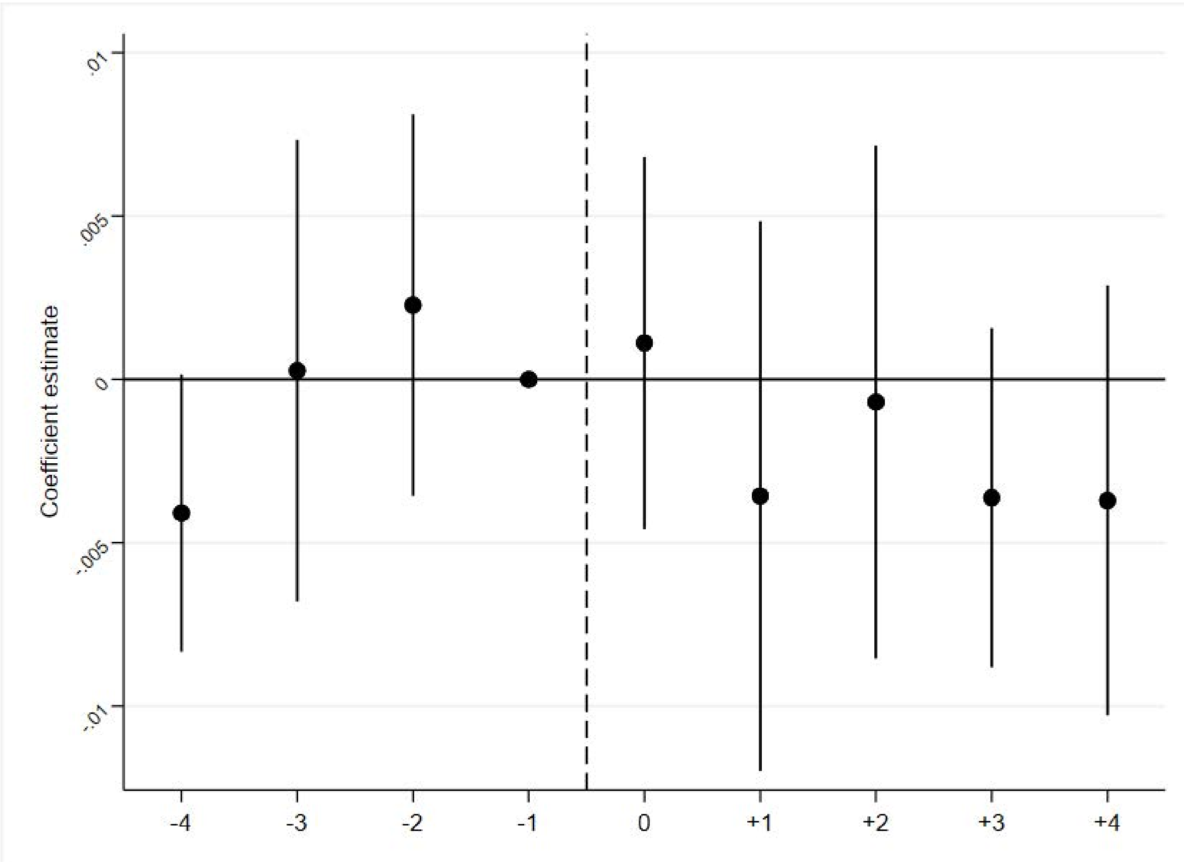
Notes: NCHS = National Center for Health Statistics. The dependent variable is the annual birth rate (per 1,000 state population of women aged 16−44). The unit of observation is a state in a year. Regressions are estimated with OLS and control for state policies and demographics, and state and year fixed−effects. The leads and lags represent single−year bins corresponding to five years pre−law through five years post−law. The sample excludes observations more than five years prior to the event and more than five years after the event. 95% confidence intervals are reported with vertical lines and account for within state clustering. Data are weighted by the state/year population of women aged 16−44.

Figure A8: Heterogeneity in the effect of a PSL mandate on annual birth rates by delivery mode and mother's use of prenatal care using a two−way fixed−effect event−study: NCHS 2007−2019



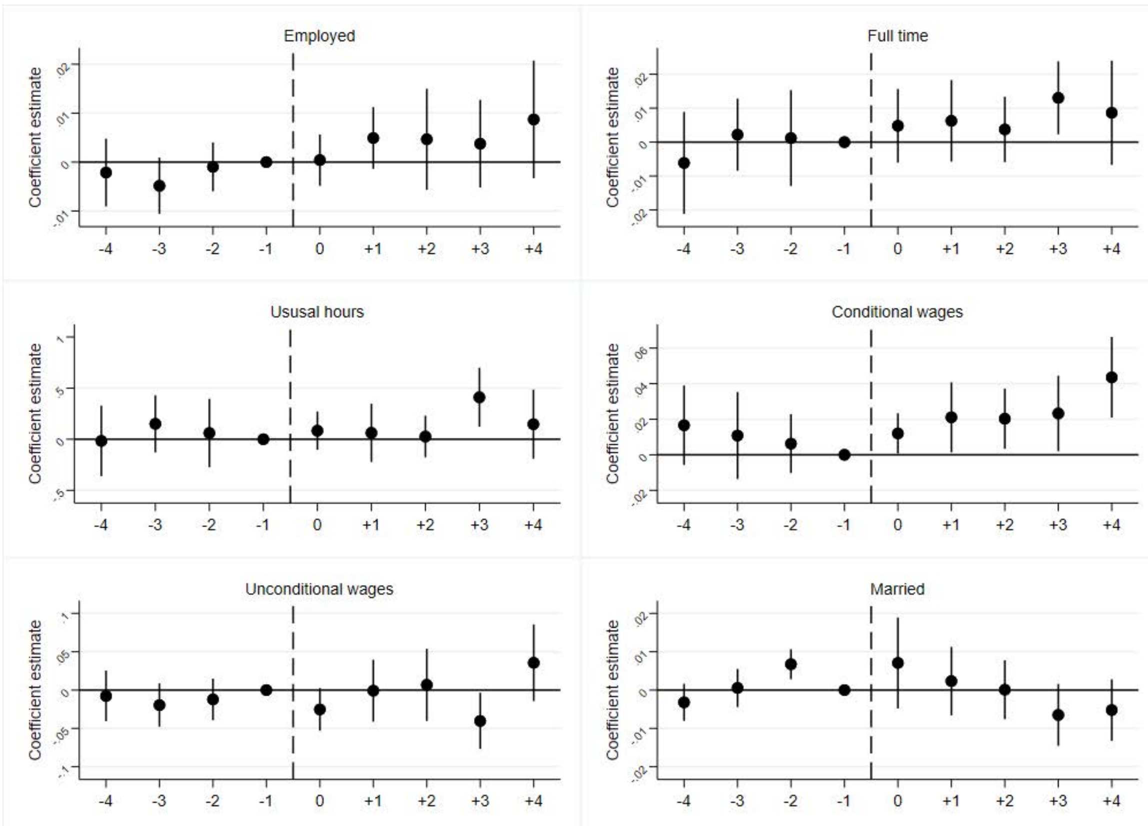
Notes: NCHS = National Center for Health Statistics. The dependent variable is the annual birth rate (per 1,000 state population of women aged 16−44). The unit of observation is a state in a year. Regressions are estimated with OLS and control for state policies and demographics, and state and year fixed−effects. The leads and lags represent single−year bins corresponding to four years pre−law through four years post−law. The sample excludes observations more than four years prior to the event and more than four years after the event. 95% confidence intervals are reported with vertical lines and account for within state clustering. Data are weighted by the state/year population of women aged 16−44.

Figure A9: Effect of a state PSL mandate on past−year across−state migration among women 16−44 years using two−way fixed−effects event−study: ASEC−CPS 2007−2019



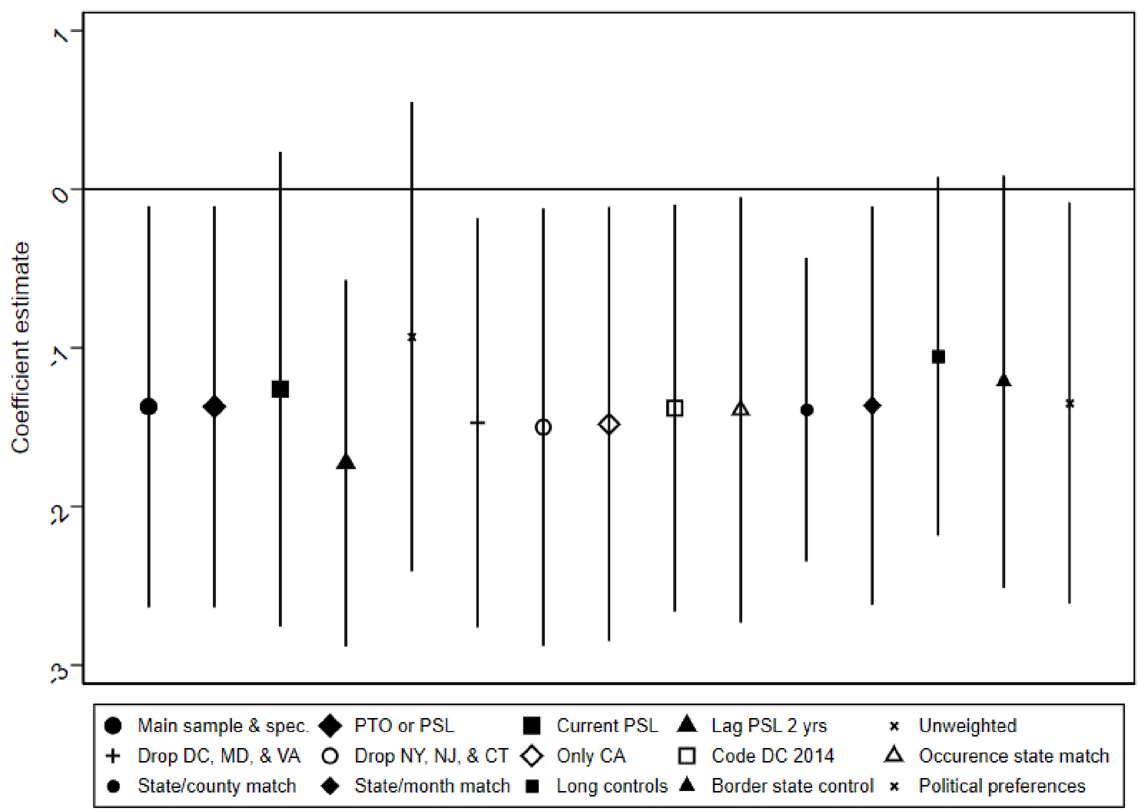
Notes: ASEC−CPS = Annual Social and Economic Supplement to the Current Population Survey. The dependent variable is an indicator for a past−year across−state move. The unit of observation is a respondent in a state in a year. Data are weighted by ASEC−CPS−provided weights. The regression is estimated with OLS and controls for respondent characteristics, state characteristics, and state and year fixed−effects. The leads and lags represent single−year bins corresponding to four years pre−law through four years post−law. The omitted period is −1. The sample excludes observations more than four years prior to the event and more than four years after the event. 95% confidence intervals are reported with vertical lines and account for within state clustering.

Figure A10: Effect of state PSL mandates on employment and marriage outcomes among women 16−44 years using a two−way fixed−effect event−study: CPS 2007−2019



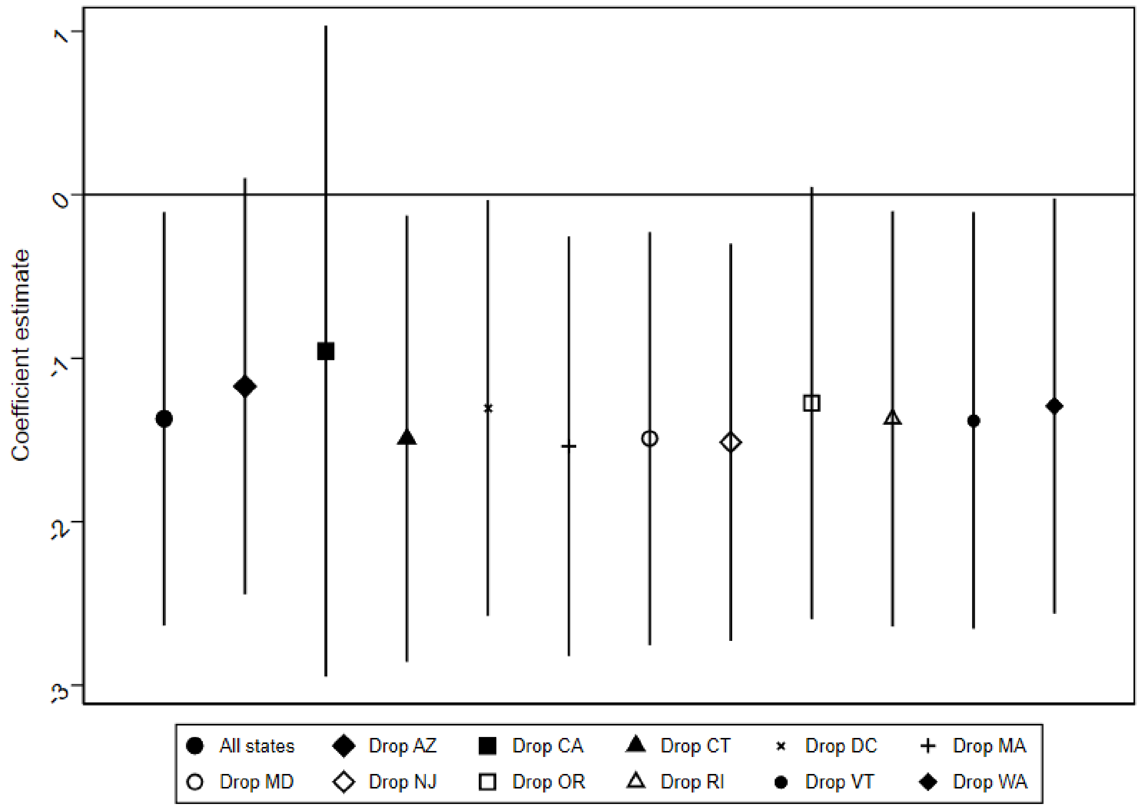
Notes: CPS = Current Population Survey. The dependent variable is an employment or marriage outcome. The unit of observation is a respondent in a state in a year. The regression is estimated with OLS and controls for state policies and demographics, and state and year fixed−effects. The leads and lags represent single−year bins corresponding to four years pre−law through four years post−law. The omitted period is −1. The sample excludes observations more than four years prior to the event and more than four years after the event. 95% confidence intervals are reported with vertical lines and account for within state clustering. Data are weighted by CPS−provided weights.

Figure A11: Effect of a state PSL mandate on annual birth rates in alternative specifications and samples: NCHS 2007−2019



Notes: NCHS = National Center for Health Statistics. The dependent variable is the annual birth rate (per 1,000 state population of women aged 16−44). The unit of observation is a state in a year. Regressions estimated with OLS and include state level policy variables and demographics, and state and year fixed−effects unless otherwise noted. Data are weighted by the state/year population of women ages 16−44 unless otherwise noted. 95% confidence intervals are reported with vertical lines and account for within state clustering.

Figure A12: Effect of a state PSL mandate on annual birth rates using two−way fixed−effects regression and sequentially excluding treated states ('leave−one−out' analysis): NCHS 2007−2019



Notes: NCHS = National Center for Health Statistics. The dependent variable is the annual birth rate (per 1,000 state population of women aged 16−44). The excluded treated state is reported on the x axis. The unit of observation is a state in a year. Regressions include state level policy variables and demographics, and state and year fixed−effects. Data are weighted by the state/year population of women ages 16−44. 95% confidence intervals are reported with vertical lines and account for within state clustering.

Table A1: States providing information on contraception in the 2006, 2010, 2011, 2017, and 2019 BRFSS

Survey year	States reporting	Number of states
2006	**AZ**, KY, MN, MO, MT, **OR**, WI	7
2010	DE, FL, KY, MS, MT	5
2011	**AZ**, SC, TN	3
2017	AL, AK, **AZ**, **CA**, **CT**, DE, **DC**, AL, AK, **AZ**, **CA**, **CT**, DE, **DC**, FL, GA, HI, ID, IN, IA, KS, KY, LA, **MA**, MN, MS, MO, NV, **NJ**, **NM**, **NY**, NC, OH, **OR**, PA, SC, SD, TX, UT, VI, VA, WV, WI, WY	37
2019	AL, **AZ**, AR, **CT**, DE, FL, GA, HI, ID, IL, IN, IA, KS, LA, **MD**, **MA**, MN, MS, MO, MT, **NM**, NC, **OR**, PA, **RI**, SC, SD, TN, UT, VA,	33

Notes: BRFSS = Behavioral Risk Factor Surveillance Survey. States in bold adopted or announced a PSL by October, 2023.

Table A2: Summary statistics: NCHS 2007−2019

Sample:	All	PSL states, pre-policy	Non-PSL states
Outcome			
Annual birth rate per 1,000	63.8	63.2	65.0
women ages 16-44 years	(5.98)	(5.03)	(5.98)
Paid sick leave			
Paid sick leave mandate	0.067	0	0
(lagged one year)	(0.25)	(0.00)	(0.00)
State policies			
Paid family and medical	0.16	0.37	0
leave mandate	(0.37)	(0.49)	(0.00)
Paid time off mandate	0.0023	0	0.0037
	(0.05)	(0.00)	(0.06)
TANF 4−person ($)	676.7	940.5	515.4
family ($)	(283.79)	(231.99)	(164.15)
Medicaid income	2.18	2.39	2.00
eligibility thresholds	(0.48)	(0.49)	(0.32)
State demographics			
Age	37.6	37.3	37.6
	(1.68)	(1.27)	(1.88)
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	0.78	0.78	0.79
	(0.09)	(0.07)	(0.09)
Non−White	0.22	0.22	0.21
	(0.09)	(0.07)	(0.09)
Hispanic	0.17	0.22	0.13
	(0.13)	(0.12)	(0.12)
No college	0.72	0.69	0.74
	(0.05)	(0.04)	(0.04)
College degree or higher	0.28	0.31	0.26
	(0.05)	(0.04)	(0.04)
Born outside the U.S.	0.15	0.20	0.11
	(0.08)	(0.07)	(0.06)
Observations	663	147	468

Notes: NCHS = National Center for Health Statistics. The unit of observation is a state/year. Data are weighted by the state/year population of women ages 16−44. Standard errors are reported in parentheses.

Table A3: Effect of a state PSL mandates on annual birth rates using an estimator proposed by Callaway and Sant'Anna (2021) and a stacked difference−in−differences estimator: NCHS 2007−2019

Specification:	(1)	(2)	(3)	(4)
Callaway and Sant'Anna (2021)				
Paid sick leave mandate	-1.32**	-1.43	-4.16**	-4.40***
(lagged one year)	(0.59)	(1.76)	(1.90)	(1.66)
Percent change	-2.1%	-2.3%	-6.6%	-7.0%
xx%				
Observations	663	663	663	765
Stacked DID				
Paid sick leave mandate	-2.43**	-2.28**	-1.45*	-1.61*
(lagged one year)	(1.05)	(1.07)	(0.76)	(0.91)
Percent change	-3.9%	-3.6%	-2.3%	-2.6%
xx%				
Observations	2,951	2,951	2,951	3,975
State policies	N	Y	Y	Y
State demographics	N	N	Y	Y
Includes pandemic years	N	N	N	Y
Pre-treatment mean, treatment states	63	63	63	63

Notes: NCHS = National Center for Health Statistics. All regressions include state and year fixed−effects, stacked difference−in−differences include cohort−specific state and year fixed−effects. Specification 2 also includes state level policy variables. Specification 3 includes state level policy variables and demographics. Specification 4 is the same as specification 3 but includes the pandemic period (2020−2021). The dependent variable is the annual birth rate per 1,000 women 16−44 years. The unit of observation is a state in a year. Data are weighted by the state/year population of women ages 16−44. Doubly robust regression is used for Callaway and Sant'Anna (2021) and OLS is used for stacked difference−in−differences. The never treated group is used as the comparison group. Standard errors are clustered at the state level and are reported in parentheses.
***,**,* = statistically different from zero at the 1%,5%,10% level.