The Multigenerational Effects of Legal Access to the Pill on Infant Health[*]

Hamid Noghanibehambari[†]

David Slusky[‡]

Erdal Tekin[§]

Hoa Vu[**]

September 1, 2026

Abstract

We examine whether expanded legal access to the contraceptive pill for minors generated health benefits for subsequent generations. We use data from the restricted-use Natality files from 1970-2020, linking mothers to the level of access to the pill in their state and year of birth and the extent to which their mothers could access the pill as minors. Our results show that children whose mothers were born after expanded access to the pill as minors had better birth outcomes, with gains concentrated among Black families. Among affected births, legalization increased birth weight by 12-16 grams and reduced incidence of low birth weight by 2.7-3.4 percent. These magnitudes have economic significance, corresponding to approximately $113.8–$115.3 million in medical cost savings, measured in 2025 dollars, for the 2000 Black birth cohort alone. Our mechanism analysis points to a postponement of early childbearing by the generation directly affected by legal access to the pill as minors whose daughters subsequently attained higher levels of education, used more prenatal care, and had longer birth spacing. Taken together, our results suggest that reproductive health policy can generate multigenerational benefits and contribute to reductions in racial disparities in early-life health.

Keywords: Contraceptive Pill; Infant Health; Multigenerational Health; Fertility; Birth Weight

JEL Codes: H75, I12, I18, J13, J16, J18

[*] We acknowledge financial support from the Center for Demography of Health and Aging (CDHA) at the University of Wisconsin-Madison under NIA core grant P30 AG17266. Any errors are our own.
[†] College of Business, Austin Peay State University and IZA@LISER. Email: noghanih@apsu.edu
[‡] Department of Economics, University of Kansas, NBER, and IZA@LISER. Email: david.slusky@ku.edu
[§] School of Public Affairs, American University, NBER, and IZA@LISER. Email: tekin@american.edu
[**] School of Education and Social Policy, Northwestern University. Email: hoa.vu@northwestern.edu

1. Introduction

Few twentieth-century policy changes reshaped the life cycles of American women as profoundly as the introduction and legalization of oral contraception ("the pill"). By giving women unilateral control over the timing of fertility, the pill enabled them to delay childbearing, complete schooling, enter careers, and form families under more favorable circumstances (Ananat & Hungerman, 2012; Bailey, 2006, 2010, 2013; Goldin & Katz, 2002). Yet an important question remains unanswered: did these changes also improve the health of the next generation? If access to contraception changed who became a young mother, when women entered motherhood, and the resources available to families during pregnancy, the pill may have shaped the health endowments of children born decades later.[1]

To our knowledge, this paper provides the first evidence of multigenerational effects of legal access to the pill on infant health. We exploit state-level variation in the timing of laws granting legal access to the pill by minors to examine whether exposure to these laws influenced the birth outcomes of their children decades later. The staggered timing of legalization across states generates plausibly exogenous variation in women's early-life access to contraception, providing an ideal policy setting for studying this question.

In our framework, the zeroth generation consists of women whose legal access to oral contraception as minors varied across states and over time. The first generation comprises their daughters, born between 1955 and 1977, whose prenatal environments were differentially affected by state-level changes in minors' legal access to oral contraception, while the second generation comprises their children observed in the restricted-access U.S. Natality Detail Files covering births from 1970 to 2020. Our empirical design compares second-generation birth outcomes across children whose first-generation mothers were born in the same state but in different cohorts relative to the year that state legalized pill access for minors. In this way, the design links zeroth-generation

[1] A large body of evidence shows that health at birth predicts later-life human capital, earnings, and health, with effects that persist across the life cycle (Almond et al., 2018; Almond & Currie, 2011). Birth outcomes also transmit across generations: low birth weight in one generation is associated with low birth weight in the next, so policies that improve neonatal health among today's mothers can compound into measurable gains for their children (Currie & Moretti, 2007). This multigenerational channel requires two causal links: pill access improved maternal birth outcomes (A→B), and maternal birth outcomes causally transmit to children's birth outcomes (B→C). The existing evidence on B→C is largely descriptive. One exception is Royer (2009), who provides causal estimates but finds positive effects that are not statistically significant. As a result, prior evidence does not establish that improvements in maternal birth outcomes necessarily translate into reduced-form effects on children's birth outcomes (A→C). One contribution of this paper is to provide reduced-form causal evidence on how access to the pill in one generation affects birth outcomes in a subsequent generation.

access to reproductive health policy to prenatal environment of the first-generation, and ultimately, to second-generation infant health.

Our paper contributes to several strands of literature. First, it contributes to the literature on contraception and women's outcomes by providing evidence on intergenerational effects of legal access to the pill. Existing studies document that legal access to the pill affected fertility timing, marriage, and childbearing behavior (Bailey, 2010, 2013; Goldin & Katz, 2002; Guldi, 2008; Lang & Weinstein, 2015). Most directly, Guldi (2008) shows that minors' access to the pill reduced fertility among young women, indicating that the policy changed early childbearing behavior. Ananat & Hungerman (2012) show that pill access affected the timing and composition of births, altering both maternal characteristics and the early-life circumstances of children. Together, these studies show that increased access to contraceptives affected both the timing of childbearing and the characteristics of the families into which children were born.

Beyond these effects on fertility and family formation, a related literature finds that expanded contraceptive access had longer-term effects on women's labor supply, education, and career investment (Bailey, 2006; Bailey et al., 2012; Fletcher & Wolfe, 2009; Goldin & Katz, 2002; Heissel, 2017; Hock, 2009; Kane et al., 2013; Lang & Weinstein, 2015). Bailey (2010) shows that the diffusion of legal contraceptive access after *Griswold v. Connecticut* contributed to declines in marital fertility, highlighting the pill's role in changing childbearing behavior within marriage. Bailey (2013) further shows that expanded access to contraception and family planning reduced fertility and generated longer-run improvements in the adult outcomes of children born to affected cohorts. More recently, Herbst & Tekin (2025) document that expanded access to contraception and abortion affected the composition and quality of the U.S. child-care workforce, suggesting that reproductive health policies may have consequences beyond individual fertility decisions. Collectively, this literature establishes the pill as a transformative policy for women's life-cycle outcomes. However, relatively little is known about whether legal access to the pill produced measurable improvements in the health of the subsequent generation. We fill this gap by showing that expanding contraceptive access for minors improved infant health decades later among the children of exposed women. Because health at birth predicts later-life health, human capital, earnings, and mortality (Behrman & Rosenzweig, 2004; Bharadwaj et al., 2018; Black et al., 2007; Currie & Moretti, 2007; Fletcher, 2011; Maruyama & Heinesen, 2020; Royer, 2009;

Samaras et al., 2003), our findings imply that the benefits of legal access to contraception extended well beyond the women directly affected by the policy.

Second, it contributes to the growing literature showing that maternal early-life conditions and policy environments can shape the birth outcomes of the next generation. Maternal early-life exposure to health insurance, pollution, disease environments, childhood vaccination, maternal education, famine and nutritional deprivation, and abortion legalization can affect the next generation (Almond et al., 2012; Colmer & Voorheis, 2020; Currie & Moretti, 2003; East et al., 2023; Noghanibehambari et al., 2025; Painter et al., 2008). East et al. (2023) show that maternal childhood access to Medicaid improved the birth outcomes of the next generation. Almond et al. (2012) show that mothers exposed to higher infant mortality early in life have heavier children with worse birth outcomes. Currie & Moretti (2007) document intergenerational transmission of low birth weight. Painter et al. (2008) and Almond et al. (2010) document second-generation effects of the Dutch and Chinese famines, respectively. Most closely related, Noghanibehambari et al. (2025) document multigenerational birth-outcome improvements from abortion legalization. We contribute to this literature by examining a distinct policy intervention: legal access to oral contraception for minors. Whereas prior work has focused primarily on shocks to health, nutrition, or abortion access, we show that expanding minors' reproductive autonomy before conception can generate measurable improvements in the health endowments of children born decades later.

Third, the paper contributes to the literature on racial disparities in infant health (Currie, 2011; M. Lu & Halfon, 2003; Rosenthal & Lobel, 2011; Vu et al., 2024). Our results suggest that reproductive health policy can affect the multigenerational transmission of health inequality, complementing prior evidence that public policies and institutional changes have shaped racial gaps in early-life health. For example, hospital desegregation and disease prevention reduced Black infant mortality and narrowed Black-White infant health gaps (Almond et al., 2006) while safety-net programs and public transfers improved the health and economic outcomes for disadvantaged children, with implications for racial inequality (Almond et al., 2011; Hoynes et al., 2016). Research on reproductive health policy also shows that abortion legalization affected selection into birth and improved infant health among more disadvantaged groups (Farin et al., 2024; Gruber et al., 1999). More recent work shows that clinic closures and defunding reduced access to reproductive and preventive healthcare, with especially large effects for disadvantaged women (Aslim et al., 2024, 2025; Lindo et al., 2020; Y. Lu & Slusky, 2016, 2019; Slusky, 2017;

Venator & Fletcher, 2021). We add to this literature by showing that minors' access to contraception generated multigenerational infant-health gains that were concentrated among Black families.

Finally, our findings speak to current debates about restrictions on reproductive health access. In the wake of the 2022 Dobbs v. Jackson Women's Health Organization decision, several states have moved to restrict aspects of contraceptive access for minors. Most existing assessments of these restrictions focus on contemporaneous effects on women's reproductive choices, mental health, and infant outcomes (Brindis et al., 2024; Caraher, 2024; Gemmill et al., 2024; Myers, 2024, 2025; Nobles et al., 2024; Rubin, 2022). Emerging evidence suggests that restrictions on reproductive healthcare may also generate broader downstream consequences for families and children, including economic hardship, housing insecurity, financially motivated crime, and child maltreatment (Aslim et al., 2024, 2025). Our findings highlight an additional, longer-horizon dimension: policies that affect young women's ability to control fertility can shape not only their own life trajectories but also the health endowments of children born decades later.

Our results indicate that expanded legal access to the pill for minors led to multigenerational improvements in infant health, with the effects concentrated among Black families. Among children of Black mothers, we find improvements in birth weight, low birth weight, fetal growth, and birth-weight rank, while effects among White infants are much smaller and generally not statistically significant. Furthermore, the effects are largest in states with higher pre-policy teenage pregnancy and infant mortality, where the policy had greater scope to affect fertility and early-life conditions. To provide an approximate sense of the magnitude among affected pregnancies, we scale the reduced-form estimates using plausible measures of policy exposure. This exercise implies increases in birth weight of approximately 12–16 grams and reductions in low birth weight of 2.7–3.4 percent among affected births. The implied reduction in low birth weight corresponds to about $113.8-115.3 million in medical cost savings for the 2000 Black birth cohort alone, measured in 2025 dollars. Our mechanism analysis further demonstrates that expanded pill access delayed early childbearing among the women directly affected by the policy, improved the birth outcomes of their daughters, and was subsequently associated with greater maternal education, more prenatal care, and longer birth spacing when those daughters became mothers themselves. Together, these findings suggest that the benefits of reproductive health policy can extend across multiple generations.

The remainder of the paper is organized as follows. Section 2 provides historical background on pill access for minors. Section 3 describes the empirical strategy. Section 4 discusses the data. Section 5 presents the results. Section 6 examines mechanisms. Section 7 discusses magnitudes. Section 8 concludes.

2. Background

The modern contraceptive pill entered the U.S. market in 1960, when the Food and Drug Administration approved Enovid for contraceptive use. The technology spread rapidly among married women, but access was constrained for unmarried women and minors. These restrictions reflected both the legacy of Comstock-era statutes and state laws governing the age of majority, parental consent, and minors' ability to consent to medical treatment (Bailey, 2010). In the early 1960s, many states required women to be married, pregnant, already mothers, or at least 21 years old to obtain prescription contraception without parental involvement (Ananat & Hungerman, 2012).

Legal access expanded over the following decade. Supreme Court decisions in Griswold v. Connecticut (1965) and Eisenstadt v. Baird (1972) helped establish constitutional protections for contraceptive access among married and unmarried adults (Bailey, 2006). At the same time, states lowered the age of majority or enacted broader medical-consent rules that allowed minors to obtain contraception. These legal changes generated meaningful cross-state and cross-cohort variation in whether young women could legally access the pill before age 21. Prior work argues that the timing of these reforms was not simply a reflection of contemporaneous fertility trends, supporting their use as policy variation in empirical designs (Bailey, 2006, 2013; Goldin & Katz, 2002; Guldi, 2008).

The reforms occurred during a period of rapid change in contraceptive behavior. Survey evidence from the 1970s shows that pill use among never-married women ages 18–19 increased sharply, from 36 percent in 1971 to 73 percent in 1976 (Zelnik & Kantner, 1977). Because the reforms specifically affected young women near the beginning of their reproductive years, they had the potential to alter fertility timing, completed family size, and the characteristics of mothers giving birth (Bumpass et al., 1978; Tomkinson, 2019). This setting allows us to examine whether expanded pill access shaped not only women's fertility choices but also next-generation health.

3. Econometric Method

Our empirical strategy compares second-generation birth outcomes for children whose mothers were born in different years relative to the state-specific year in which minors obtained legal access to the contraceptive pill. The identifying variation comes from the interaction of maternal birth state and maternal birth cohort, net of fixed effects and controls. The event-study specification can be written as:

$$y_{cbrg} = \alpha + \sum_{i=\underline{T}}^{-3} \gamma_i I(c - T_b^* = i) + \sum_{j=-1}^{\overline{T}} \lambda_j I(c - T_b^* = j) + \beta Z_{cbr} + \Theta_g + \Gamma_{\mathrm{rc}} + \Lambda_b + \varepsilon_{cbrg,} \quad (1)$$

where y is the average birth outcome of second-generation children whose mothers belong to birth cohort c, were born in state b, and in Census region r. The outcome is measured within cells defined by sociodemographic group g. These groups are based on maternal race, maternal education, maternal age, birth order, and child sex. The function $I(.)$ is an indicator function equal to one when the condition inside the parentheses is satisfied. The parameter T_b^* represents the year in which minors obtained legal access to the contraceptive pill in maternal birth state b.

The indices i and j denote event time relative to legalization, measured in years. Specifically, i indexes cohorts born in years prior to legalization, while j indexes cohorts born in years after legalization. The event-time coefficients (γ and λ) therefore measure differences in second-generation birth outcomes across maternal cohorts born in different years relative to the pill-access legalization year in their birth state. The omitted category is the cohort that was plausibly in utero one year before legalization, corresponding to $i = -2$.

The vector Z includes time-varying maternal birth-state controls, including per capita income, hospital beds per capita, hospitals per capita, and reported disease rates. It also includes the 12-year average pre-vaccine measles rate in the maternal birth state, measured over 1952 to 1963, interacted with maternal birth-year fixed effects. This control accounts for differential cohort trends associated with baseline measles exposure before the 1963 measles vaccine rollout, which prior work shows affected later-life maternal characteristics, human capital development, health, as well as next-generation birth outcomes (Altindag et al., 2025; Atwood, 2022; Noghanibehambari, 2024). The vector Θ includes maternal race, maternal education, maternal age,

birth order, and child sex controls. The Λ terms are maternal birth-state fixed effects, and Γ are maternal birth-year-by-birth-region fixed effects.

To summarize the event-study coefficients, we also estimate a difference-in-differences specification:

$$y_{cbrg} = \alpha + \beta Exp_{cbr} + \beta Z_{cbr} + \Theta_g + \Gamma_{\mathrm{rc}} + \Lambda_b + \varepsilon_{cbrg}. \quad (2)$$

The coefficient β is the reduced-form effect of being born in a maternal cohort exposed to legal pill access for minors. All regressions are weighted by the number of births in the collapsed cell. Standard errors are clustered by maternal birth state and maternal birth year to allow for spatial and serial correlation. Because treatment timing is staggered across states, the main estimates use the interaction-weighted difference-in-differences estimator of Sun & Abraham (2021), which uses not-yet-treated cohorts as the comparison group at each event time. Appendix D also reports robustness to the imputation estimator of Borusyak et al. (2024) and Wooldridge (2025).

Identification rests on the assumption that, conditional on the fixed effects and covariates above, residual variation in pill access exposure is uncorrelated with other unobserved drivers of next-generation birth outcomes. Prior work has documented that the timing of state-level pill legalization for minors was driven primarily by state-level developments in the age of majority and minor-consent doctrines and was largely unrelated to contemporaneous changes in attitudes toward sexuality, fertility, or maternal and infant health (Bailey, 2006, 2013; Goldin & Katz, 2002). Our event-study estimates allow us to test for differential pre-trends in birth outcomes across early- and late-legalizing states. The robustness analysis in Appendix D additionally conditions on state-cohort exposure to Medicaid, AFDC, the Food Stamp Program, abortion legalization, the Equal Pay Act, the Fair Employment Practices Act, and the Equal Rights Amendment.

4. Data

The primary data source is the restricted-access Natality Detail Files maintained by the National Center for Health Statistics (NCHS, 2024). A key advantage of these files is that they identify the mother's state of birth, which allows us to assign exposure using the legal environment in the state where the mother was born rather than the state where she later gave birth. The files also report maternal age, race, educational attainment, birth order, child sex, birth weight, and gestational age. For a subset of years, they also report the number of prenatal visits, the month in

which prenatal care began, the Kessner index of prenatal care adequacy, and birth spacing from the previous pregnancy. We use these latter variables in the mechanism analysis.

We use births from 1970 to 2020 and focus on maternal cohorts born around the staggered legalization window, primarily 1955–1977. We drop records with missing values for variables required by the empirical design (less than 1 percent of observations) and exclude mothers born outside the United States (approximately 11 percent of observations). The microdata are collapsed by mother's birth state, mother's birth year, child birth state, child birth year, maternal race, maternal education, maternal age group, birth order, and child sex. The underlying sample contains 73,422,112 birth records before aggregation.

Our primary outcomes are birth weight, low birth weight, gestational age, preterm birth, fetal growth, and birth-weight rank. Birth weight is measured in grams. Low birth weight is an indicator for birth weight below 2,500 grams. Gestational age is measured in weeks, and preterm birth is an indicator for gestation shorter than 37 weeks. Following standard practice (Bharadwaj et al., 2018; Currie & Moretti, 2007), we construct fetal growth as birth weight divided by gestational age, measured in grams per gestational week, and birth-weight rank as the decile of the infant's birth weight in the gestational-age-specific birth weight distribution. The small-for-gestational-age indicator equals one if the infant's birth weight falls in the lowest decile of the birth weight distribution for that gestational week.

We also construct several additional indicators for adverse and upper-tail birth outcomes. Very low birth weight equals one if birth weight is below 1,500 grams, and extremely low birth weight equals one if birth weight is below 1,000 grams. Very preterm birth equals one if gestational age is below 28 weeks. Low Apgar score equals one if the infant's five-minute Apgar score is below 8. We also define macrosomia-related indicators for birth weight above 4,000, 4,500, and 5,000 grams. Appendix C reports estimates for these additional outcomes.

We supplement the natality data with state-by-year covariates for the maternal birth state. These include per capita income, hospitals per capita, and hospital beds per capita from Goodman-Bacon (2018), as well as reported disease rates from the Tycho Project (Tycho, 2021). State-cohort exposure to Medicaid is computed using state-specific Medicaid implementation years from Goodman-Bacon (2018). State-cohort exposure to the Equal Pay Act, the Fair Employment Practices Act, and the Equal Rights Amendment, as well as state-specific years of contraceptive pill legalization for minors, our primary policy variation, are drawn from Farin et al. (2024).

Our conceptual framework spans three generational cohorts. The zeroth generation consists of women who gave birth between 1955 and 1977—the grandmothers of the infants in our analytic sample. For this group, we draw on publicly available NCHS natality records beginning in 1968 to construct race-specific fertility rates, the share of teenage births, the share of births to mothers with less than 12 years of education, and the share of married mothers at the time of childbirth.

The first generation consists of women born between 1955 and 1977 who later appear as mothers in the NCHS dataset. Because individual-level natality microdata are unavailable for these cohorts prior to 1968, we use state-level vital statistics from Goodman-Bacon (2018) to approximate the early-life health environment they experienced, including race-specific shares of low and very low birth weight and infant mortality. The second generation consists of the infants in our main analytic dataset—children born between 1970 and 2020 to first-generation mothers. To address residual cohort overlap, Appendix Table D-14 replicates our main results restricting the first-generation birth cohorts to 1955–1970; the estimates remain stable. Figure 1 provides a visual summary of the three generational cohorts and the timing of key policy variation used in our analysis.

4.1. Summary Statistics

Table 1 reports summary statistics separately for observations in which the mother was born after versus before pill legalization in her birth state. Mothers in the post-legalization group are, by construction, from younger birth cohorts and their children are born later on average. The post group has a slightly lower share of White mothers (71 versus 74 percent) and a similar share of Black mothers (21 versus 21 percent). Educational composition is also similar across the two subsamples, with college- or higher-educated mothers slightly more represented in the post group (48.2 versus 45.8 percent).

The raw differences in birth outcome across the two groups largely reflect well-known secular trends rather than the causal effect of pill legalization. Average birth weight is 3,293.9 grams in the post group and 3,314.7 grams in the pre group; the low-birth-weight share is 8.3 versus 8.0 percent; gestational age is 38.8 versus 39.1 weeks; and the preterm share is 12.3 versus 11.3 percent.

Figure 2 illustrates the geographic distribution of the year of contraceptive pill legalization for minors across U.S. states, showing substantial cross-state variation in the timing of legalization that is the source of identifying variation. Figure 3 plots the time-series evolution of the preterm-

birth share (top panel) and the low-birth-weight share (bottom panel) by maternal birth cohort. The preterm-birth series track one another closely for earlier maternal cohorts and begin to diverge among later cohorts. The low-birth-weight series remain closely aligned throughout, with relatively little visible divergence between early- and late-legalizing states. These descriptive patterns motivate the event-study analysis below, which more formally examines changes in birth outcomes relative to the timing of legalization.

5. Results

5.1. Second-Generation Infant Health

The event-study estimates from Equation (1) are reported in Figure 4 through Figure 6. Across the figures, the pre-reform coefficients are generally close to zero, providing little evidence of differential pre-trends. After legalization, the estimates suggest improvements in second-generation birth outcomes, especially among children of Black mothers. The gains are most visible for birth weight, low birth weight, and fetal growth, while the effects on gestational age and preterm birth are less consistent.

Table 2 presents the main difference-in-differences estimates of Equation (2) for second-generation birth outcomes separately for children of Black mothers (Panel A) and White mothers (Panel B). For Black mothers, exposure to pill access for minors significantly improved birth outcomes. The birth-weight coefficient is 4.6 grams, statistically significant at the 1 percent level (column 1). The low-birth-weight coefficient is −0.13 percentage points, or roughly a 1.0 percent decline relative to the mean of 14 percent (column 2). Fetal growth rises by 0.13 grams per week, a 0.16 percent increase from the mean (column 5), and birth-weight rank increases by 0.02 points, or a 0.4 percent change from the mean (column 6). The estimates for gestational age and preterm birth are small and statistically insignificant (columns 3 and 4). The pattern indicates that for Black mothers the improvements operate primarily through fetal growth rather than longer gestation, signaling a healthier in utero environment rather than additional weeks in utero.

Panel B shows little evidence of comparable improvements among children of White mothers. Most coefficients are small and statistically insignificant, indicating that the measurable second-generation health gains are concentrated among children of Black mothers.

5.2. Heterogeneity by Baseline Fertility and Health Conditions

Table 3 examines whether the effects vary with the maternal birth state's baseline share of births to young mothers. This is a direct test of whether the policy mattered more where minors'

access had greater scope to change fertility timing. Among Black mothers born in high-teen-pregnancy states (Panel A), exposure increases birth weight by 6.6 grams, reduces low birth weight by 0.21 percentage points, increases fetal growth by 0.20 grams per week, and raises birth-weight rank by 0.03 points. These estimates are statistically significant at conventional levels.

Among Black mothers in lower-teen-pregnancy states (Panel B), the estimates are smaller and statistically insignificant. For White mothers (Panels C and D), the pattern is more mixed: birth weight, fetal growth, and birth rank fall significantly in high-teen-pregnancy states, while in lower-teen-pregnancy states the estimates indicate small but precisely estimated improvements in birth weight (+1.7 g), gestational age (+0.01 weeks), low birth weight (−0.07 pp), and preterm birth (−0.14 pp). These White-mother effects in lower-teen-pregnancy states are an order of magnitude smaller than the Black-mother effects in high-teen-pregnancy states and likely reflect the marginal influence of pill access on a population whose pre-policy teenage pregnancy rates were already low.

Table 4 shows that the effects are concentrated among Black mothers from high-infant-mortality states. In these states, exposure increases birth weight by 8.0 grams, reduces low birth weight by about 1.6 percent relative to the mean, and raises fetal growth by about 0.27 percent relative to the mean. In lower-infant-mortality states, the corresponding Black estimates are smaller and do not show the same pattern of improvement. For White mothers, the estimates are generally weaker, with limited significant effects. Reading Table 3 and Table 4 together, the policy's multigenerational benefits are largest in the maternal birth states where pre-policy teenage childbearing was most common and where the underlying health environment for newborns was most adverse.

5.3. Additional Heterogeneity and Alternative Outcomes

Appendix Table B-1 examines heterogeneity by maternal education. Among children of Black mothers, the effects are positive in both education groups but somewhat larger for mothers with less than 12 years of education. For White mothers, the corresponding estimates are smaller and less systematic.

Appendix Table B-2 examines heterogeneity by child sex. Among children of Black mothers, the benefits are present for both boys and girls but are larger for boys. Among White infants, statistically significant effects are limited mainly to gestational age for girls and birth weight and fetal growth for boys. The Black-maternal-line effects are present for both boys and

girls, but the low-birth-weight reduction is more pronounced for boys, consistent with the "fragile male" hypothesis that male fetuses are more sensitive to changes in maternal conditions (Kraemer, 2000; Sanders & Stoecker, 2015; Trivers & Willard, 1973).

Appendix Table C-1 examines alternative measures of health at birth. For children of Black mothers, the results show improvements across the lower tail of the birth-weight distribution. In contrast, the estimates for Apgar score and macrosomia-related outcomes are small and statistically insignificant. For children of White mothers, the estimates are generally smaller and less consistent.

5.4. Correlates of Early Contraceptive Pill Adoption

An important question is whether states that granted minors earlier access to the contraceptive pill differed systematically from later-adopting states. To examine this issue, we construct a cross-state dataset of pre-policy characteristics measured in 1962, before most minors' pill-access reforms occurred. These characteristics include log per-capita income, hospital beds per capita, disease rates, early adult pill access, early abortion legalization, and the gender wage gap. To construct the gender wage gap, we use individual wage reports from the 1962 Annual Social and Economic Supplement of the Current Population Survey, extracted from Flood et al. (2025). The ASEC-CPS begins in 1962, which makes this the earliest year in which we can measure state-level wage gaps before the relevant reforms. We calculate both a raw state-level gender wage gap and a residual gender wage gap that partials out age, education, race, occupation, and industry. A caveat is that the 1962 CPS identifies only 40 states, so the correlates analysis is restricted to this subset. Further, we define early adoption as legalization of minors' access to the contraceptive pill before 1971, the median legalization year in the final sample.

Appendix Table A-1 reports correlations between these state characteristics and early pill adoption. Across specifications, including those with region fixed effects, none of the observable state characteristics is statistically significantly associated with early adoption. Overall, the table provides little evidence that early minors' pill-access adoption was systematically predicted by the pre-policy state characteristics considered here.

5.5. Robustness and Placebo Evidence

Appendix D evaluates robustness across alternative estimators, additional controls, fixed effects, samples, and weights. The estimates are comparable when we use the Borusyak et al. (2024) and Wooldridge (2021)'s imputation estimator (Appendix Table D-1 and Appendix Table

D-2). The estimates are similar when we add child birth-state and child birth-year fixed effects, which account for time-invariant differences across states where children are born and nationwide changes in birth outcomes over time (Appendix Table D-3). The results remain robust when we control for maternal state-cohort exposure to Medicaid, which accounts for the possibility that expansions in public health insurance during the same period also improved early-life health environments and later birth outcomes (Appendix Table D-4). The results are similar when we control for maternal birth-state age-specific child mortality rates, which capture broader early-life health conditions that may have varied across states and cohorts (Appendix Table D-5).

The results are also robust to controlling for the maternal birth-state share of AFDC recipients by race, which accounts for changes in cash-assistance exposure that may have affected childhood resources and later birth outcomes (Appendix Table D-6). The results remain similar when we interact maternal birth-state polio rates from 1950 to 1955 with birth-year fixed effects, which accounts for differential cohort trends related to baseline polio exposure before the 1955 polio vaccine rollout (Appendix Table D-7). The results are robust to controlling for maternal birth-state exposure to the Equal Rights Amendment, Fair Employment Practices Acts, and Equal Pay Laws, which accounts for other contemporaneous gender-equity policies that may have affected women's opportunities, family resources, and later birth outcomes (Appendix Table D-8).

The results are robust to controlling for related public health and safety-net conditions in the maternal birth state, including the number of health departments, income maintenance, disability and retirement benefits, Medicare benefits, and per-capita Food Stamp expenditures, which account for contemporaneous public investments that may have affected early-life health and household resources (Appendix Table D-9 – Appendix Table D-12). The estimates are little changed when we control for maternal state-cohort exposure to abortion legalization, which accounts for the possibility that contemporaneous changes in reproductive health access also shaped fertility timing and later birth outcomes (Appendix Table D-13).

The findings are not driven by the later maternal cohorts when we restrict the sample to mothers born between 1955 and 1970, which reduces reliance on cohorts farther from the original pill-access reforms (Appendix Table D-14). The results continue to hold when we control for adult contraceptive pill legalization, which separates the effects of minors' access from broader changes in contraceptive access for adults (Appendix Table D-15). The pattern remains stable when we add maternal birth-state-by-child birth-state fixed effects, which account for fixed differences in

migration patterns between mothers' birth states and the states where their children are born (Appendix Table D-16).

The results are unchanged when we allow maternal birth-state fixed effects to vary by maternal education and age groups, which accounts for the possibility that state-specific differences in maternal human capital or age composition drive the estimates (Appendix Table D-17). The results are similar when we weight regressions by both birth counts and the maternal birth-state share of teenage pregnancies, which gives greater weight to cells from states where minors' pill access had more scope to affect fertility timing (Appendix Table D-18). The findings are also robust to adding child birth-region-by-birth-year fixed effects, which account for region-specific trends in birth outcomes where children are born, and to estimating the model using individual-level rather than collapsed data (Appendix Table D-19 – Appendix Table D-20).

Appendix E reports a series of placebo tests. First, shifting the policy dates ten years earlier (Appendix Table E-1) does not reproduce the main pattern of improvements among Black children in birth weight, low birth weight, fetal growth, or birth-weight rank. Several White outcomes are statistically significant, but they do not reproduce the coherent Black-outcome pattern in the main analysis. Second, the Monte Carlo placebo tests (Appendix Table E-2 – Appendix Table E-7), which randomly draw fake legalization years from a distribution matched to the actual policy distribution, also produce isolated significant estimates, which is expected given the number of outcomes and draws. However, these estimates do not produce the systematic pattern observed in the main results for Black children.

Appendix Figure F-1 through Appendix Figure F-12 present leave-one-state-out estimates across 51 sequential exclusions. Across all specifications, the estimated effects on Black birth-weight, low-birth-weight, fetal-growth, and birth-rank estimates remain qualitatively unchanged, indicating that no single state drives the results. Importantly, the estimates remain comparable when we exclude California, which has been documented to disproportionately drive estimated effects of contemporaneous reproductive policy reforms (Hoehn-Velasco et al., 2024).

6. Mechanisms

We posit that there are two broad channels through which minors' access to the pill could affect the health of the next generation. The first is selection into birth and motherhood. By reducing unintended or mistimed pregnancies among young women, pill access may change who becomes a mother at young ages and under what circumstances children are born. This channel is

related to the marginal-child framework in Gruber et al. (1999), although the pill operates by preventing pregnancies before conception rather than by affecting decisions after conception. The second channel is improvement in the early-life health and socioeconomic environment of the first generation. By delaying or preventing births that would have occurred under less favorable circumstances, pill access may improve the conditions into which first-generation girls are conceived and raised, including their fetal environment, infant health, family resources, and other early-life investments (Almond et al., 2011; Almond & Currie, 2011; Currie & Moretti, 2007; Hoynes et al., 2015, 2016; Lindo, 2011). These improvements may persist as they grow up and eventually become mothers themselves, thereby affecting the birth outcomes of their children.

Because these channels operate at different stages of the multigenerational process, they are not mutually exclusive and may reinforce one another. Although our empirical design does not allow us to formally decompose the relative contributions of selection effects and improvements in early-life conditions, we can examine evidence at multiple points along the causal chain. We begin by examining first-generation mothers' characteristics and behaviors when they give birth to the second generation, including socioeconomic status, prenatal care utilization, and fertility timing (Section 6.1). We then turn to what we view as the central multigenerational mechanism: whether minors' access to the pill improved health at birth for the first generation itself (Section 6.2). If early-life health improved, these gains could persist into adulthood and be transmitted to the next generation, consistent with the intergenerational health literature. Next, we turn to the zeroth generation and assess whether pill access altered fertility behavior and the timing of childbearing among women directly exposed to the policy (Section 6.3). Finally, Section 6.4 reconciles our findings with the existing literature on fertility responses to minors' pill access, showing why differences in fertility effects across racial groups do not imply similar differences in second-generation birth outcomes. Taken together, the pattern of evidence across generations helps illuminate where each mechanism is likely to operate most strongly, even though the two channels cannot be fully disentangled within the framework of the current study.

6.1. First-Generation Mothers' Characteristics and Behaviors at the Time of Second-Generation Births (First Generation as Adults)

Table 5 examines potential mechanisms operating through first-generation mothers' characteristics, prenatal care utilization, and fertility timing, measured at the time they give birth to the second generation. For Blacks, the estimates provide little evidence of changes in

compositional margins of births, including maternal age at delivery, parity (first-birth status), or child sex. Instead, the results point to improvements in first-generation mothers' socioeconomic characteristics, prenatal care behavior, and birth spacing. Exposure reduces the share of Black first-generation mothers with 12 or fewer years of education by 0.91 percentage points, or 1.4 percent relative to the mean. It also reduces no prenatal care by 0.24 percentage points, or 8.0 percent relative to the mean, moves the start of prenatal care earlier by 0.02 months, increases adequate prenatal care by 0.34 percentage points, or 0.6 percent relative to the mean, and reduces inadequate care by 0.31 percentage points, or 2.4 percent relative to the mean. Birth spacing from the previous pregnancy increases by 1.2 months, or 2.8 percent relative to the mean, while births spaced less than two years apart fall by 0.51 percentage points, or 1.3 percent relative to the mean.

For White first-generation mothers, the estimates show some statistically significant effects on first-birth status and child sex, but no systematic pattern across maternal characteristics, prenatal care, or fertility timing. Exposure is associated with small increases in first-birth status and child female share, a modest reduction in no prenatal care, and longer birth spacing of 0.45 months, or 1.1 percent relative to the mean. However, these effects do not form a coherent pattern across the broader set of outcomes, with no systematic changes in maternal education, prenatal care adequacy, or closely spaced births. Overall, the results indicate that the strongest and most consistent changes in first-generation mothers' socioeconomic characteristics, prenatal care utilization, and fertility timing are concentrated among Black mothers.

6.2. First-Generation Infant Health (First generation at birth)

Table 6 examines whether minors' access to the pill improved the health of the first generation at birth, using state-level rates measured around the period of legalization. The strongest estimate is for Black low birth weight: exposure reduces the Black low-birth-weight rate by 3.6 per 1,000 births off a mean of 128.5 (1 percent significance), a 2.8 percent decline. The White low-birth-weight estimate is −0.58 per 1,000 births off a mean of 67.2 and is marginally significant. The estimates for very low birth weight and infant mortality are negative for Black infants but not statistically significant, though sign-consistent with improvements.

We can use these first-generation estimates to perform a back-of-the-envelope benchmark for the multigenerational patterns documented in Table 2. Currie & Moretti (2007) use grandmother fixed effects to estimate the cross-generational correlation in low birth weight and report a correlation of approximately 0.48. Applying this estimate to the Black low-birth-weight

estimate in Table 6 implies an expected reduction in second-generation low birth weight of approximately 1.3 percent operating solely through the direct intergenerational transmission channel alone. The actual second-generation estimate in Table 2 is approximately 1.0 percent—comparable in magnitude to the prediction from the direct intergenerational transmission channel.

6.3. Effects on Zeroth-Generation Outcomes

Table 7 examines whether minors' access to the contraceptive pill affected fertility timing and maternal characteristics among the zeroth generation, defined as women who gave birth between 1955 and 1977. Panel A uses state-level birth statistics digitized from the NCHS historical reports, which extend back to 1955. The advantage of these reports is their longer historical coverage, allowing us to examine fertility patterns over a broader portion of the policy-expansion period. Their limitation, however, is that births are reported only for broad age-by-race groups, which restricts the set of outcomes we can examine. Panel B instead uses the NCHS Natality Detail Files aggregated to the state-year level. These files contain richer individual-level information on maternal characteristics, allowing us to examine additional outcomes. The disadvantage is that the detailed natality files begin only in 1968, which shortens the pre-policy window and limits our ability to examine earlier changes in zeroth-generation fertility and maternal characteristics. In both panels, we restrict the sample through 1977 so that the zeroth-generation analysis aligns with the birth-cohort range of the first-generation mothers in the main analysis.

The results indicate that pill access delayed childbearing, particularly among Black women. In Panel A, exposure is associated with a modest decline in the share of births to Black mothers ages 10-19, although the estimate is only marginally significant. The corresponding estimate for White teen births is small and statistically insignificant.

In Panel B, the detailed natality records show clearer evidence of delayed childbearing. For Black mothers, exposure increased average maternal age by 0.09 years (0.4 percent relative to the mean), and reduces the share of births to mothers under age 20 by 0.49 percentage points (1.7 percent relative to the mean). For White mothers, average maternal age rose by 0.03 years (0.1 percent relative to the mean), while the share of births to mothers under age 20 fell by 0.18 percentage points (1.1 percent relative to the mean). The estimates for the share of married mothers, maternal education, and the Black-to-White birth-share ratio are small and statistically insignificant. The clearest zeroth-generation evidence is therefore delayed early childbearing, with a larger effect among Black women.

6.4. Zeroth-Generation Fertility Effects and Reconciliation with Prior Evidence

Guldi (2008) shows that minors' access to the pill reduced fertility among young women in the zeroth generation, with somewhat larger effects for White women than for non-White women in age-targeted specifications. However, based on the estimates reported in Guldi (2008), we cannot reject the null hypothesis that the effects are the same across racial groups. At first glance, this pattern may appear inconsistent with our finding that second-generation infant-health gains are concentrated among children of Black mothers. There is, however, no contradiction. The two papers examine different outcomes. Guldi (2008) estimates the effect of pill access on fertility, whereas our paper estimates its effect on birthweight in the next generation. The second-generation birthweight effect depends on two components: the share of births affected by pill access and the improvement in birthweight among those affected births. Consequently, a somewhat larger fertility response among White women does not necessarily imply larger second-generation birthweight gains for White infants.

Appendix Table G-1 replicates the age-targeted design of Guldi (2008), comparing women ages 15–21, who were directly affected by minors' pill access, with women ages 23–35 in the same states, while controlling for state-by-year and age fixed effects. In this design, pill access reduces log birth rates by 5 percent overall, with declines of 5 percent for White women and 4 percent for non-White women.[2] Similar patterns emerge for first births. These estimates confirm the central finding of the pill-access fertility literature: when treatment is defined around the ages directly affected by the law, fertility reductions are sizable, precisely estimated, and somewhat larger for White women than for non-White women. Because the natality files do not contain information on grandmothers' ages, we cannot implement an age-targeted design in the second-generation analysis and instead rely on cohort-level exposure measures.

Appendix Table G-2 then estimates fertility effects using a specification that more closely resembles the difference-in-differences framework employed in the current paper. Although the estimated effects on birth rates remain negative, they become smaller and less precise. The total log birth-rate estimate changes from approximately -5 percent in Appendix Table G-1 to -3 percent in Appendix Table G-2. The White estimate is -3 percent and marginally significant, while the non-White estimate is -4 percent and statistically insignificant. Estimates for first births and

[2] Throughout the main analysis, we use the term Black to refer to Black mothers and children. In the replication of Guldi (2008), however, we use non-White to match the racial classification and comparison used in that study.

fertility counts are close to zero. These results indicate that the fertility response becomes more attenuated when the design moves away from the tightly age-targeted comparison and toward the broader cohort-level framework used in the main analysis.

Two distinct explanations reconcile the racial pattern in our birth-outcome results with the fertility evidence. The first is a design-based explanation related to attenuation. The second-generation estimates reported in the main analysis are intent-to-treat effects for all children born to exposed maternal cohorts and therefore average over many births that were never directly affected by the policy. As a result, racial differences in the prevalence of teenage childbearing can lead to different degrees of attenuation. As shown in Table 7, before legalization, 29 percent of Black births, compared with 16 percent of White births, were to mothers younger than age 20. Combining these prevalence rates with the age-targeted fertility responses in Appendix Table G-1 (a 5 percent reduction for White women and a 4 percent reduction for Black women) implies that approximately 0.8 percent of White births (0.16×0.05) and 1.2 percent of Black births (0.29×0.04) were prevented by minors' pill access. Thus, the treated share is somewhat larger among Black births. However, we may not be able to reject the hypothesis that the two treated shares are similar. Therefore, differences in exposure alone are unlikely to fully account for the substantially larger birthweight gains among Black infants. A second, substantive explanation is that, conditional on being affected by the policy, births to Black mothers experienced larger improvements because baseline infant health was considerably worse, leaving greater scope for improvement. These two mechanisms are complementary: differences in treatment exposure, together with larger treatment effects among affected Black births, can jointly explain why the second-generation birthweight gains are concentrated among children of Black mothers.

7. Discussion on the Magnitude of the Results

The estimates should be interpreted as intent-to-treat (ITT) effects. Legal access to the pill for minors did not mean that every affected woman used the pill, nor did it mean that every birth in an exposed maternal cohort was directly affected by the reform. To provide a sense of the magnitude among affected pregnancies, we present two illustrative scaling exercises. The first approach uses the change in pill use among never-married women ages 18 to 19 reported by Zelnik & Kantner (1977). Pill use in this group increased by 37 percentage points, from 36 percent in 1971 to 73 percent in 1976. Dividing the Black birth-weight ITT estimate of 4.6 grams by this 0.37 increase in pill use yields an illustrative implied effect of approximately 12.4 grams among

affected pregnancies. Applying the same scaling to low birth weight implies a 2.7 percent reduction relative to the baseline, and the fetal-growth estimate implies an increase of about 0.35 grams per gestational week. The second approach uses the pre-policy share of births to mothers under age 20 among Black mothers, 29 percent, as a proxy for the share of births most likely to have been affected by minors' pill access. This approach yields somewhat larger implied effects: a 15.9-gram increase in birth weight, a 3.4 percent reduction in low birth weight, and a 0.45 gram-per-week increase in fetal growth.

Even the ITT estimates are economically meaningful. The estimated 4.6-gram increase in birth weight among Black infants is nearly identical to the 4.7-gram multigenerational birth-weight effect of early-life Medicaid access estimated by East et al. (2023), although East et al. (2023) report effects for the overall population rather than a specific subgroup. It is smaller than the 13.3-gram effect of early-life abortion legalization estimated by Noghanibehambari et al. (2025), roughly one-third as large, which is consistent with minors' pill access representing a more targeted reproductive policy intervention.

The illustrative scaling estimates also suggest meaningful reductions in low birth weight of roughly 2.7 to 3.4 percent among treated Black births. To illustrate the magnitude, in 2000 there were 79,927 low-birth-weight births among Black infants. Applying the illustrative effect among affected pregnancies yields approximately 798 avoided low-birth-weight births using the pill-use benchmark ($79{,}927 \times 0.37 \times 0.027 = 798$) and 788 avoided births using the teenage-birth benchmark ($79{,}927 \times 0.29 \times 0.034 = 788$). Beam et al. (2020) report average six-month medical spending of $114,437 for low-birth-weight infants compared with $6,743 for normal-birth-weight infants, measured in 2016 dollars. The resulting incremental medical spending of $107,694 is equivalent to approximately $144,460 in 2025 dollars. Multiplying this inflation-adjusted incremental cost by the estimated number of avoided low-birth-weight births implies approximately $113.8 million to $115.3 million in reduced medical costs for the 2000 Black birth cohort alone. These estimates should be viewed as conservative measures of the broader economic benefits of reducing low birth weight because they capture only short-term medical spending and do not account for other potential consequences of low birth weight, including infant mortality, disability, poorer long-term health, reduced adult earnings, and long-term mortality (Bharadwaj et al., 2018; Black et al., 2007; Currie et al., 2026; Royer, 2009).

The magnitudes are also relevant for racial disparities in birth outcomes. The baseline Black-White birth-weight gap is approximately 288 grams, calculated as the difference between average birth weight among White infants (3,379 grams) and Black infants (3,091 grams). The illustrative scaling estimates suggest birth-weight gains of 12.4 to 15.9 grams among affected Black births, which would close approximately 4.3 to 5.5 percent of the existing Black-White birth-weight gap. For low birth weight, the baseline Black-White gap is 8 percentage points, calculated as 14 percent for Black infants minus 6 percent for White infants. The illustrative scaling reductions of 2.7 to 3.4 percent relative to the Black baseline correspond to reductions of approximately 0.38 to 0.48 percentage points, closing about 4.7 to 6.0 percent of the baseline Black-White low-birth-weight gap. Although these magnitudes are not large enough to eliminate racial gaps in birth outcomes, they represent meaningful improvements for a historical policy change whose direct exposure was limited to only a subset of each birth cohort.

8. Conclusion

This paper examines whether legal access to the contraceptive pill among one generation of young women improved the birth outcomes of the children born two generations later. Using restricted natality records from 1970 to 2020 and state-by-cohort variation in minors' pill access, we find that exposure improved birth outcomes among second-generation children born to Black mothers whose own mothers had legal access to the pill as minors. These children experienced higher birth weight, lower rates of low birth weight, improved fetal growth, and higher birth-weight rank. Effects among White infants were smaller and generally not statistically significant. The gains were most pronounced in states where the policy had greater scope to matter: those with higher baseline teenage pregnancy and higher infant mortality.

To shed light on the underlying mechanisms, we examine how the effects unfold across different generations in our framework. We first study *the zeroth generation*, the young women directly exposed to expanded pill access, and find that the policy delayed early childbearing. We next consider *the first generation***,** the daughters of these women, at birth and document that pill access improved infant health among Black children. We then examine first generation women when they become mothers themselves, and find improvements in maternal education, prenatal care utilization, and birth spacing. Together, these results suggest that expanded access to the pill shaped the health of subsequent generations through two complementary pathways: by altering the

timing and circumstances of childbearing and by improving early-life conditions that may have lasting effects into adulthood and parenthood.

Our findings show that reproductive health policy could affect population health beyond the generation directly exposed to the policy. By altering fertility decisions and shaping early-life environments, expanded access to contraception generated health improvements among children born decades later. The findings extend prior work on reproductive health policy and contribute to the growing literature showing that early-life interventions and safety-net policy expansions can produce multigenerational effects (East et al., 2023; Noghanibehambari et al., 2025).

The findings also speak to current debates about contraceptive access. In the wake of the 2022 Dobbs v. Jackson Women's Health Organization decision and subsequent state-level proposals to restrict contraceptive access for minors, our findings highlight a long-horizon dimension of these policies. Most existing assessments focus on contemporaneous effects on women's reproductive choices, mental health, and infant outcomes (Brindis et al., 2024; Caraher, 2024; Gemmill et al., 2024; Myers, 2024, 2025; Nobles et al., 2024; Rubin, 2022). Our results suggest that contemporaneous assessments may understate the full consequences of restrictive policy because effects on the next generation's health may operate on a timescale measured in decades. The larger gains in states with higher pre-policy teenage pregnancy and infant mortality, and the concentration of effects among Black families, suggest that restrictions on reproductive health access may have implications for racial disparities in infant health. Evaluations of reproductive health policy should therefore account not only for immediate effects on individuals but also for benefits and costs that extend to future generations.

References

Almond, D., Chay, K., & Greenstone, M. (2006). Civil Rights, the War on Poverty, and Black-White Convergence in Infant Mortality in the Rural South and Mississippi. *SSRN Electronic Journal*.

Almond, D., & Currie, J. (2011). Killing Me Softly: The Fetal Origins Hypothesis. *Journal of Economic Perspectives*, *25*(3), 153–172.

Almond, D., Currie, J., & Herrmann, M. (2012). From infant to mother: Early disease environment and future maternal health. *Labour Economics*, *19*(4), 475–483.

Almond, D., Edlund, L., Li, H., & Zhang, J. (2010). Long-term effects of early-life development: Evidence from the 1959 to 1961 China famine. In *The economic consequences of demographic change in East Asia* (pp. 321–345). University of Chicago Press.

Almond, D., Hoynes, H., & Schanzenbach, D. (2011). Inside the war on poverty: The impact of food stamps on birth outcomes. *Review of Economics and Statistics*, *93*(2), 387–403.

Altindag, O., Greve, J., & Tekin, E. (2025). Impact of Measles, Mumps, and Rubella Vaccination on Hospitalizations and Human Capital: Evidence from Copenhagen School Health Records. *Vaccines 2025, Vol. 13, Page 302*, *13*(3), 302.

Ananat, E., & Hungerman, D. (2012). The Power of the Pill for the Next Generation: Oral Contraception's Effects on Fertility, Abortion, and Maternal and Child Characteristics. *The Review of Economics and Statistics*, *94*(1), 37–51.

Aslim, E., Fu, W., Myers, C., Tekin, E., & Xue, B. (2025). *Abortion, Economic Hardship, and Crime*.

Aslim, E., Fu, W., & Tekin, E. (2024). *Abortion Access and Child Maltreatment*.

Atwood, A. (2022). The Long-Term Effects of Measles Vaccination on Earnings and Employment. *American Economic Journal: Economic Policy*, *14*(2), 34–60.

Bailey, M. (2006). More Power to the Pill: The Impact of Contraceptive Freedom on Women's Life Cycle Labor Supply. *The Quarterly Journal of Economics*, *121*(1), 289–320.

Bailey, M. (2010). Momma Got the Pill;: How Anthony Comstock and Griswold v. Connecticut Shaped US Childbearing. *American Economic Review*, *100*(1), 98–129.

Bailey, M. (2013). Fifty Years of Family Planning: New Evidence on the Long-Run Effects of Increasing Access to Contraception. *Brookings Papers on Economic Activity*, *2013*(1), 341–409.

Bailey, M., Hershbein, B., & Miller, A. (2012). The Opt-In Revolution? Contraception and the Gender Gap in Wages. *American Economic Journal: Applied Economics*, *4*(3), 225–254.

Beam, A., Fried, I., Palmer, N., Agniel, D., Brat, G., Fox, K., Kohane, I., Sinaiko, A., Zupancic, J., & Armstrong, J. (2020). Estimates of healthcare spending for preterm and low-birthweight infants in a commercially insured population: 2008-2016. *Journal of Perinatology*, *40*(7), 1091–1099.

Behrman, J., & Rosenzweig, M. (2004). Returns to birthweight. In *Review of Economics and Statistics* (Vol. 86, Number 2, pp. 586–601).

Bharadwaj, P., Lundborg, P., & Rooth, D. (2018). Birth weight in the long run. *Journal of Human Resources*, *53*(1), 189–231.

Black, S., Devereux, P., & Salvanes, K. (2007). From the cradle to the labor market? The effect of birth weight on adult outcomes. *The Quarterly Journal of Economics*, *122*(1), 409–439.

Borusyak, K., Jaravel, X., & Spiess, J. (2024). Revisiting Event-Study Designs: Robust and Efficient Estimation. *Review of Economic Studies*, *91*(6), 3253–3285.

Brindis, C., Laitner, M., Clayton, E., Scrimshaw, S., Grosz, B., Simpson, L., Rosenbaum, S., Brierley, C., Simon, M., Roubideaux, Y., Calonge, B., Johnson, P., DeStefano, L., Bear, A., Arora, K., & Dzau, V. (2024). Societal implications of the Dobbs v Jackson Women's Health Organization decision. *The Lancet*, *403*(10445), 2751–2754.

Bumpass, L., Rindfuss, R., & Jamosik, R. (1978). Age and marital status at first birth and the pace of subsequent fertility. *Demography 1978 15:1*, *15*(1), 75–86.

Caraher, R. (2024). Do Abortion Bans Affect Reproductive and Infant Health? Evidence from Texas's 2021 Ban and its Impact on Health Disparities. *Political Economy Research Institute Working Paper No*, *606*.

Colmer, J., & Voorheis, J. (2020). The grandkids aren't alright: the intergenerational effects of prenatal pollution exposure. *London School of Economics and Political Science. Centre for Economic Performance*.

Currie, J. (2011). Inequality at birth: Some causes and consequences. *American Economic Review*, *101*(3), 1–22.

Currie, J., Dursun, B., Hatch, M., & Tekin, E. (2026). The Hidden Cost of Firearm Violence on Infants in Utero. *Review of Economics and Statistics*, 1–49.

Currie, J., & Moretti, E. (2003). Mother's Education and the Intergenerational Transmission of Human Capital: Evidence from College Openings. *The Quarterly Journal of Economics*, *118*(4), 1495–1532.

Currie, J., & Moretti, E. (2007). Biology as destiny? Short- and long-run determinants of intergenerational transmission of birth weight. *Journal of Labor Economics*, *25*(2), 231–264.

East, C., Miller, S., Page, M., & Wherry, L. (2023). Multigenerational Impacts of Childhood Access to the Safety Net: Early Life Exposure to Medicaid and the Next Generation's Health. *American Economic Review*, *113*(1), 98–135.

Farin, S., Hoehn-Velasco, L., & Pesko, M. (2024). The Impact of Legal Abortion on Maternal Mortality. *American Economic Journal: Economic Policy*, *16*(3), 174–216.

Fletcher, J. (2011). The medium term schooling and health effects of low birth weight: Evidence from siblings. *Economics of Education Review*, *30*(3), 517–527.

Fletcher, J., & Wolfe, B. (2009). Education and labor market consequences of teenage childbearing evidence using the timing of pregnancy outcomes and community fixed effects. *Journal of Human Resources*, *44*(2), 303–325.

Flood, S., King, M., Rodgers, R., Ruggles, S., Warren, J. R., Backman, D., Breton, E., Cooper, G., Rivera Drew, J. A., Richards, S., Van Riper, D., & Williams, K. C. W. (2025). *IPUMS CPS: Version 13.0*. IPUMS.

Gemmill, A., Margerison, C., Stuart, E., & Bell, S. (2024). Infant Deaths After Texas' 2021 Ban on Abortion in Early Pregnancy. *JAMA Pediatrics*, *178*(8), 784.

Goldin, C., & Katz, L. (2002). The Power of the Pill: Oral Contraceptives and Women's Career and Marriage Decisions. *Journal of Political Economy*, *110*(4), 730–770.

Goodman-Bacon, A. (2018). Public Insurance and Mortality: Evidence from Medicaid Implementation. *Journal of Political Economy*, *126*(1), 216–262.

Gruber, J., Levine, P., & Staiger, D. (1999). Abortion Legalization and Child Living Circumstances: Who is the "Marginal Child"? *The Quarterly Journal of Economics*, *114*(1), 263–291.

Guldi, M. (2008). Fertility effects of abortion and birth control pill access for minors. *Demography*, *45*(4), 817–827.

Heissel, J. (2017). Teenage Motherhood and Sibling Outcomes. *American Economic Review*,

107(5), 633–637.
Herbst, C., & Tekin, E. (2025). *Reproductive Technology and the Child Care Sector: How Access to Oral Contraception and Abortion Shaped Workforce Composition and Quality*.
Hock, H. (2009). The Pill and the College Attainment of American Women and Men. *Working Papers*.
Hoehn-Velasco, L., Penglase, J., Pesko, M., & Shahid, H. (2024). The California Effect: The Challenges of Identifying the Impact of Social Policies during an Era of Social Change. *Available at SSRN 4802701*.
Hoynes, H., Miller, D., & Simon, D. (2015). Income, the earned income tax credit, and infant health. *American Economic Journal: Economic Policy*, *7*(1), 172–211.
Hoynes, H., Schanzenbach, D., & Almond, D. (2016). Long-run impacts of childhood access to the safety net. *American Economic Review*, *106*(4), 903–934.
Kane, J., Morgan, P., Harris, K., & Guilkey, D. (2013). The Educational Consequences of Teen Childbearing. *Demography*, *50*(6), 2129–2150.
Kraemer, S. (2000). The fragile male. *BMJ : British Medical Journal*, *321*(7276), 1609–1612.
Lang, K., & Weinstein, R. (2015). The consequences of teenage childbearing before roe v. wade. *American Economic Journal: Applied Economics*, *7*(4), 169–197.
Lindo, J. (2011). Parental job loss and infant health. *Journal of Health Economics*, *30*(5), 869–879.
Lindo, J., Myers, C., Schlosser, A., & Cunningham, S. (2020). How Far Is Too Far? *Journal of Human Resources*, *55*(4), 1137–1160.
Lu, M., & Halfon, N. (2003). Racial and ethnic disparities in birth outcomes: a life-course perspective. *Maternal and Child Health Journal*, *7*(1), 13–30.
Lu, Y., & Slusky, D. (2016). The Impact of Women's Health Clinic Closures on Preventive Care. *American Economic Journal: Applied Economics*, *8*(3), 100–124.
Lu, Y., & Slusky, D. (2019). The Impact of Women's Health Clinic Closures on Fertility. , *5*(3), 334–359.
Maruyama, S., & Heinesen, E. (2020). Another look at returns to birthweight. *Journal of Health Economics*, *70*, 102269.
Myers, C. (2024). Forecasts for a post-Roe America: The effects of increased travel distance on abortions and births. *Journal of Policy Analysis and Management*, *43*(1), 39–62.
Myers, C. (2025). From Roe to Dobbs: 50 Years of Cause and Effect of US State Abortion Regulations. *Annual Review of Public Health*, *46*(1), 433–446.
NCHS. (2024). Natality Detailed Files. *National Center for Health Statistics, US Vital Statistics Birth Records*.
Nobles, J., Hwang, S., Bennett, E., & Jacques, L. (2024). Abortion Restrictions Threaten Miscarriage Management In The United States: Article examines abortion restrictions and miscarriage management. *Health Affairs*, *43*(9), 1219–1224.
Noghanibehambari, H. (2024). Intergenerational Benefits of Childhood Health Intervention: Evidence from Measles Vaccination. *American Journal of Health Economics*, *10*(4), 670–698.
Noghanibehambari, H., Slusky, D., & Vu, H. (2025). *From Access to Wellness: Early Life Exposure to Abortion Legalization and the Next Generation's Health*. National Bureau of Economic Research.
Painter, R., Osmond, C., Gluckman, P., Hanson, M., Phillips, D., & Roseboom, T. (2008). Transgenerational effects of prenatal exposure to the Dutch famine on neonatal adiposity and

health in later life. *BJOG: An International Journal of Obstetrics & Gynaecology*, *115*(10), 1243–1249.
Rosenthal, L., & Lobel, M. (2011). Explaining racial disparities in adverse birth outcomes: Unique sources of stress for Black American women. *Social Science & Medicine*, *72*(6), 977–983.
Royer, H. (2009). Separated at girth: US twin estimates of the effects of birth weight. *American Economic Journal: Applied Economics*, *1*(1), 49–85.
Rubin, R. (2022). How Abortion Bans Could Affect Care for Miscarriage and Infertility. *JAMA*, *328*(4), 318.
Samaras, T., Elrick, H., & Storms, L. (2003). Birthweight, rapid growth, cancer, and longevity: a review. *Journal of the National Medical Association*, *95*(12), 1170.
Sanders, N., & Stoecker, C. (2015). Where have all the young men gone? Using sex ratios to measure fetal death rates. *Journal of Health Economics*, *41*, 30–45.
Slusky, D. (2017). Defunding women's health clinics exacerbates Hispanic disparity in preventive care. *Economics Letters*, *156*, 61–64.
Sun, L., & Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, *225*(2), 175–199.
Tomkinson, J. (2019). Age at first birth and subsequent fertility: The case of adolescent mothers in France and England and Wales. *Demographic Research*, *40*, 761–798.
Trivers, R., & Willard, D. (1973). Natural selection of parental ability to vary the sex ratio of offspring. *Science*, *179*(4068), 90–92.
Tycho. (2021). *Project Tycho*.
Venator, J., & Fletcher, J. (2021). Undue Burden Beyond Texas: An Analysis of Abortion Clinic Closures, Births, and Abortions in Wisconsin. *Journal of Policy Analysis and Management*, *40*(3), 774–813.
Vu, H., Green, T., & Swan, L. (2024). Born on the wrong side of the tracks: Exploring the causal effects of segregation on infant health. *Journal of Health Economics*, *95*, 102876.
Wooldridge, J. (2025). Two-way fixed effects, the two-way mundlak regression, and difference-in-differences estimators. *Empirical Economics*, *69*(5), 2545–2587.
Zelnik, M., & Kantner, J. (1977). Sexual and Contraceptive Experience of Young Unmarried Women in the United States, 1976 and 1971. *Family Planning Perspectives*, *9*(2), 55.

Figures

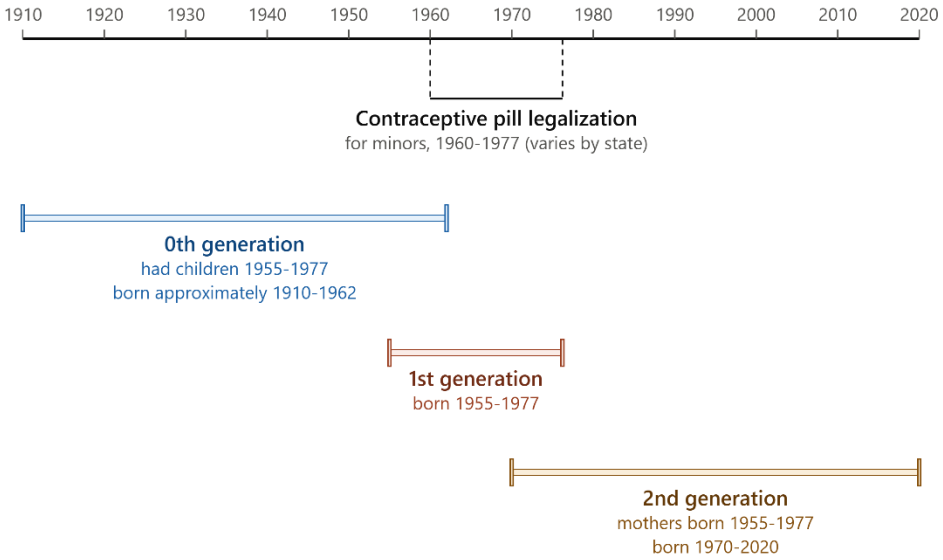


Figure 1 - Timeline of Cohorts

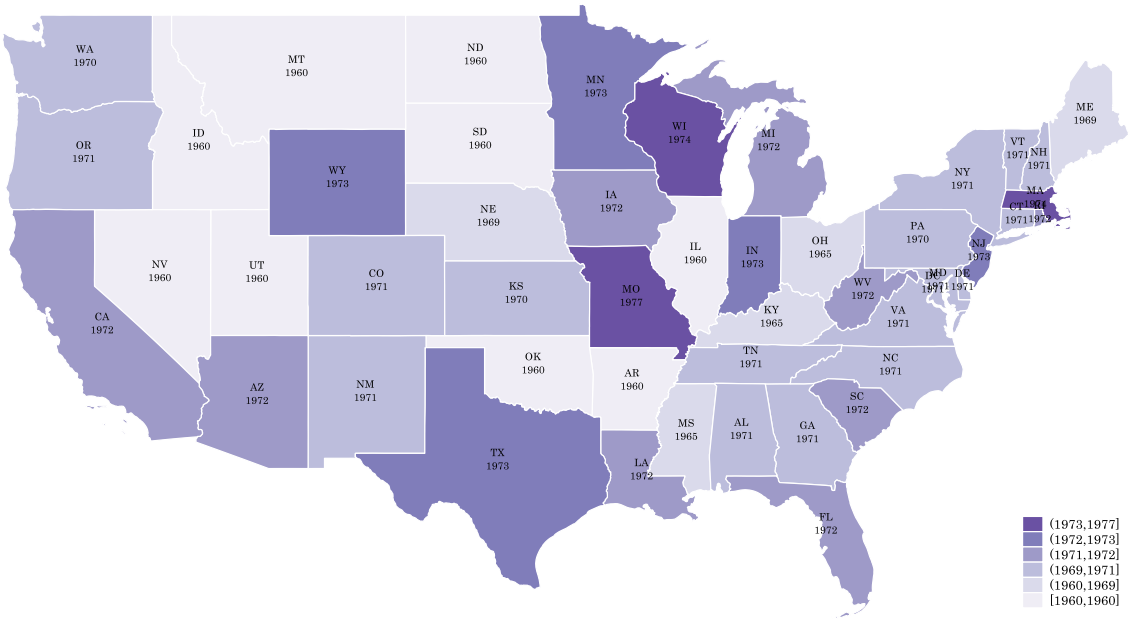


Figure 2 - Timing of Contraceptive Pill Legalization for Minors across States

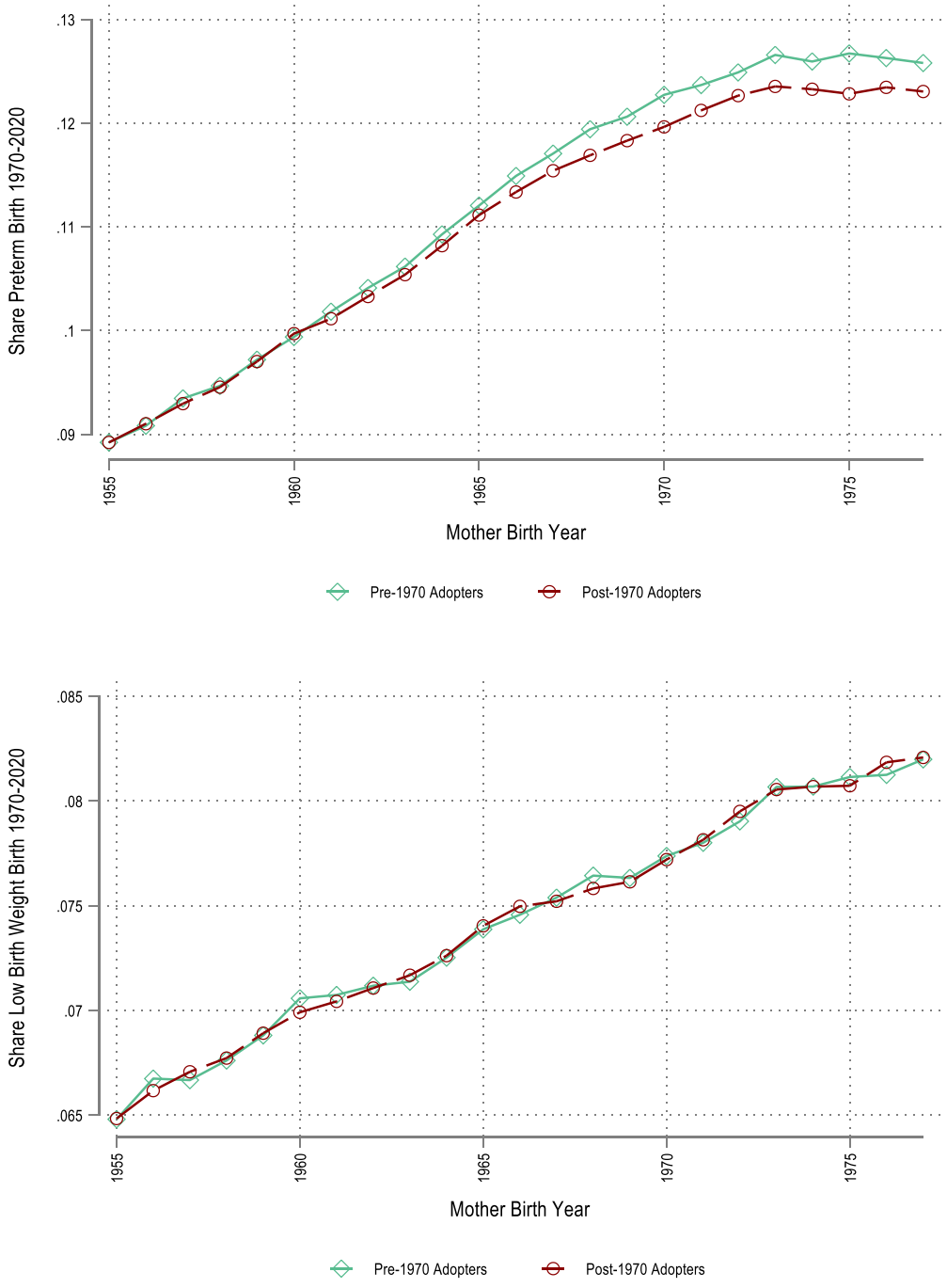


Figure 3 - Time series Evolution of Preterm Birth Share and Low Birth Weight Share by Maternal Birth Cohort

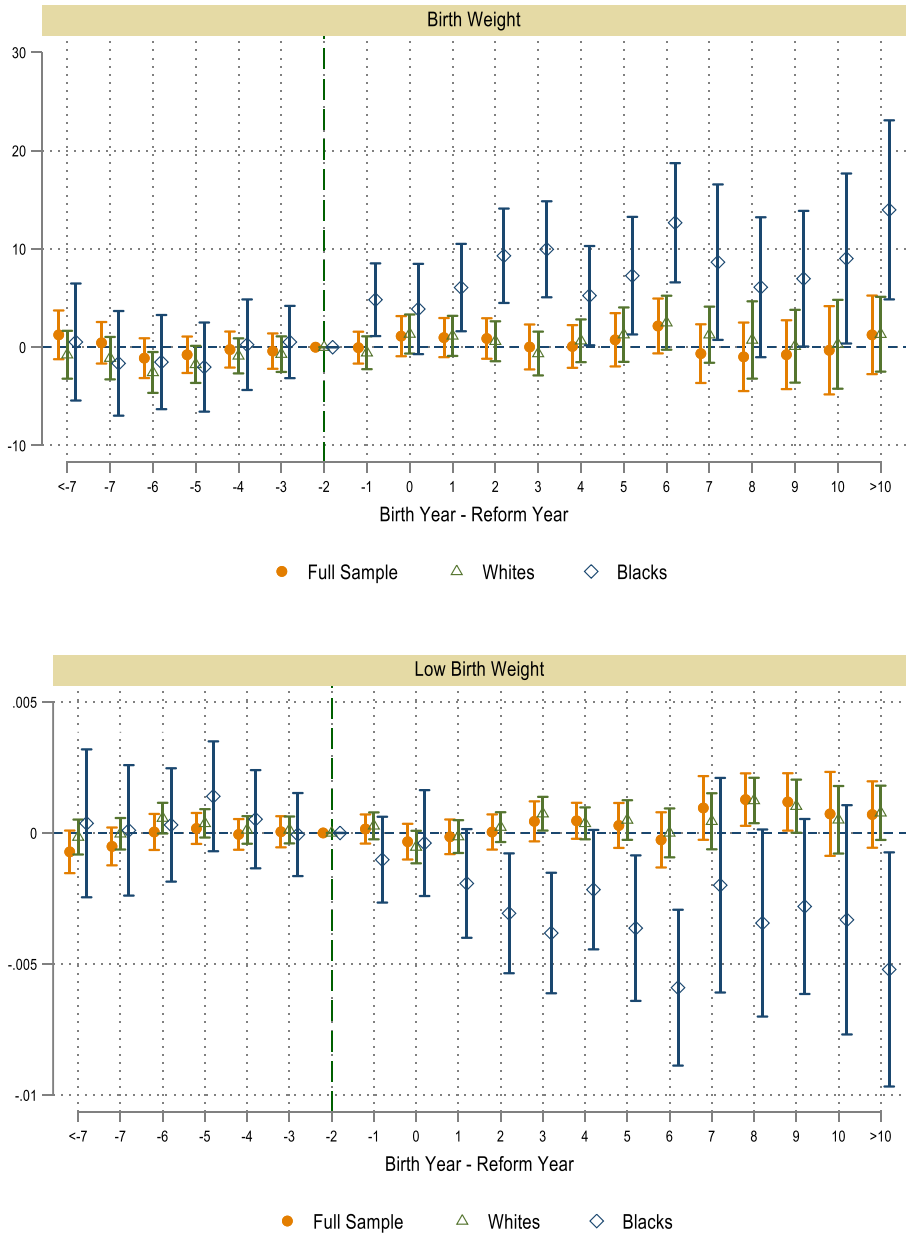


Figure 4 - Event Study Analysis to Show the Evolution of Second-Generation Birth Outcomes in Different Years Relative to the State-Specific Contraceptive Pill Legalization Year

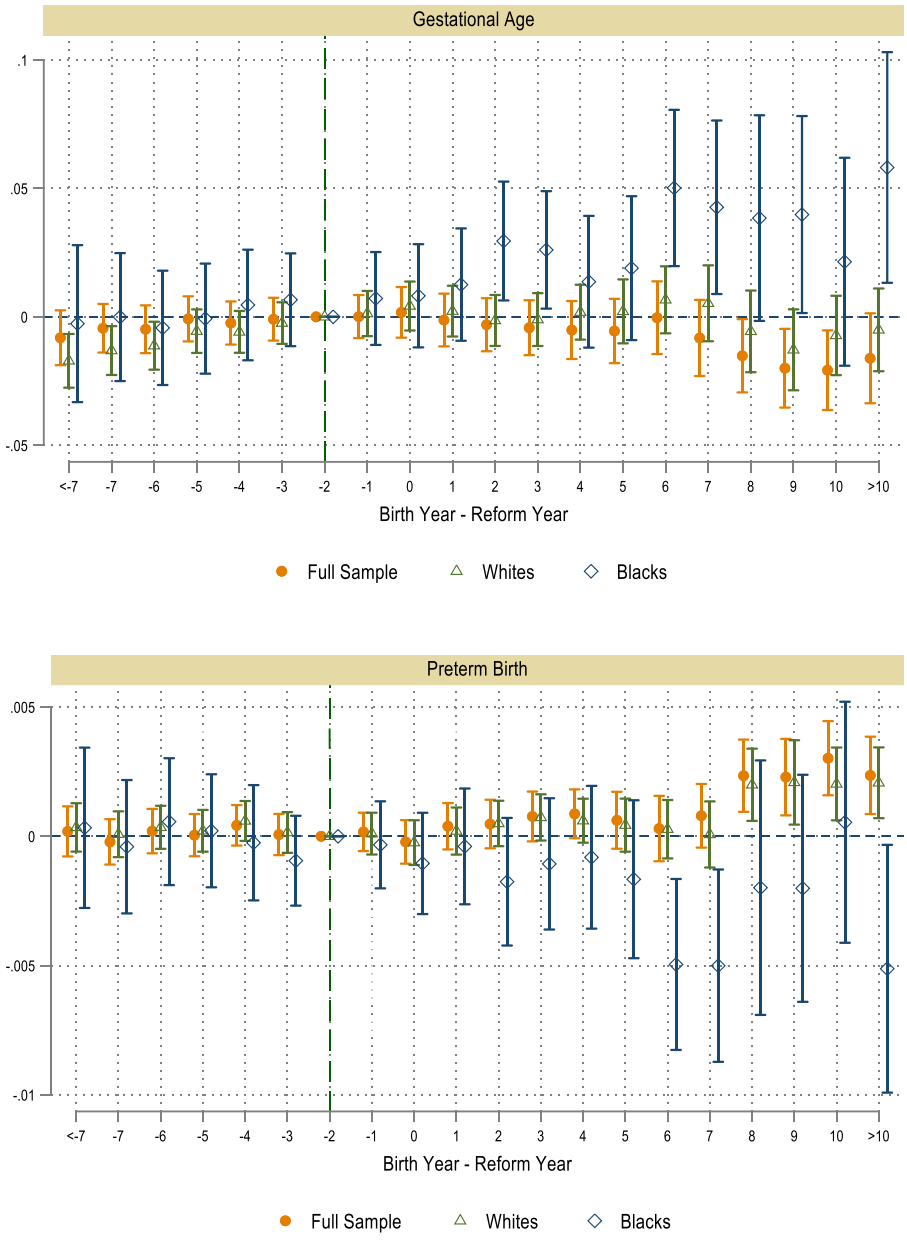


Figure 5 - Event Study Analysis to Show the Evolution of Second-Generation Birth Outcomes in Different Years Relative to the State-Specific Contraceptive Pill Legalization Year

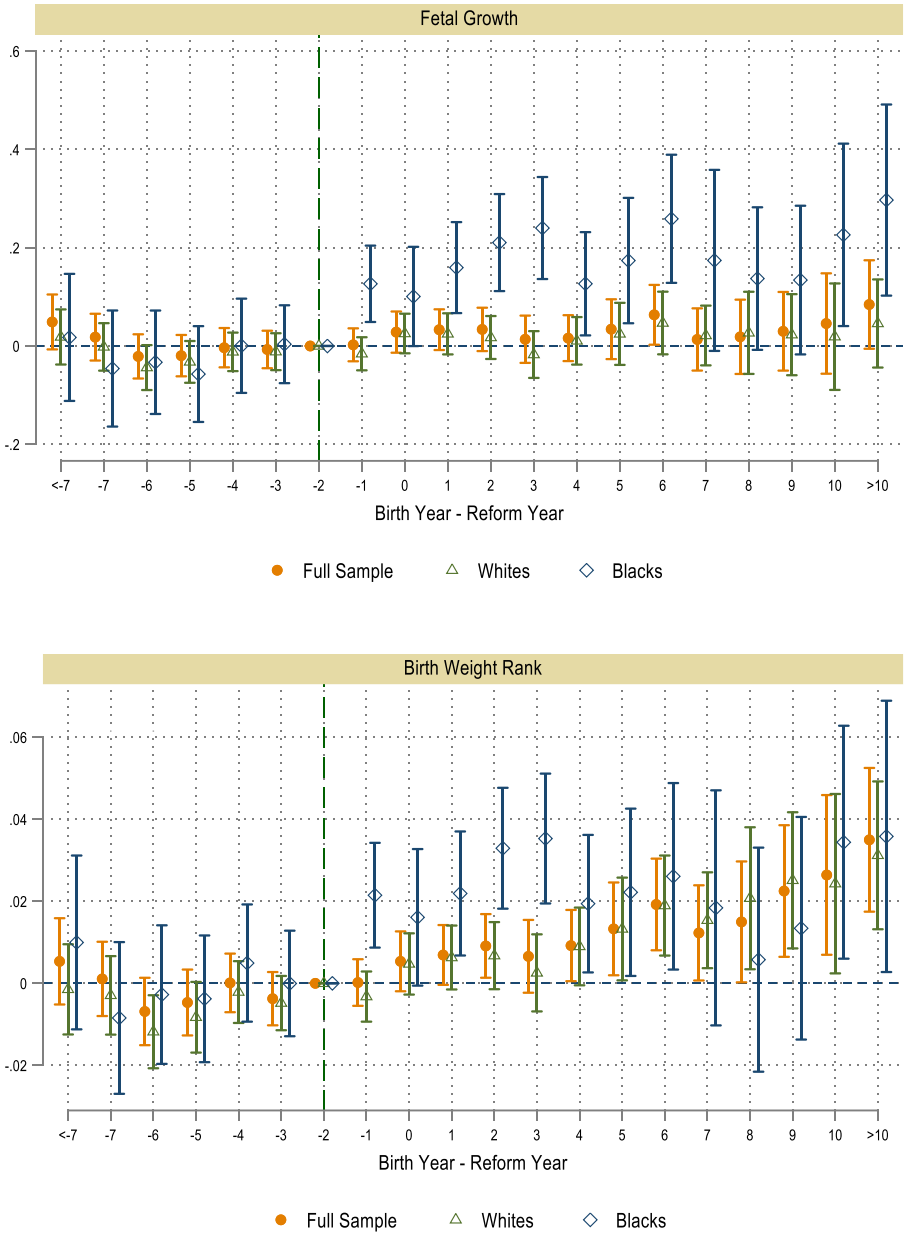


Figure 6 - Event Study Analysis to Show the Evolution of Second-Generation Birth Outcomes in Different Years Relative to the State-Specific Contraceptive Pill Legalization Year

Tables

Table 1 - Summary Statistics

	Post-Pill-Access Cohorts		Pre-Pill-Access Cohorts	
	Mean	**SD**	**Mean**	**SD**
NCHS Data:				
Birth Year of Child	1999.5	8.4	1990.6	8.3
Birth Year of Mother	1971.6	4.6	1962.3	4.7
White	0.71	0.45	0.74	0.44
Black	0.21	0.40	0.21	0.41
Mother's Education: 0-8 Years	0.06	0.23	0.06	0.23
Mother's Education: High School	0.39	0.49	0.41	0.49
Mother's Education: College-More	0.48	0.50	0.46	0.50
Mother's Age 12-18	0.09	0.28	0.07	0.25
Mother's Age 19-39	0.85	0.35	0.86	0.34
Mother's Age: 40-54	0.06	0.24	0.07	0.25
Firstborn Child	0.38	0.49	0.38	0.49
Birth Count	9.1	53.4	10.5	65.3
Child Female	0.49	0.50	0.50	0.50
Birth Weight	3293.9	502.6	3314.7	498.4
Low Birth Weight	0.08	0.22	0.08	0.22
Gestational Age	38.8	2.2	39.1	2.3
Preterm Birth	0.12	0.27	0.11	0.25
Fetal Growth	84.6	11.8	84.6	11.7
Birth Weight Rank	5.5	2.3	5.5	2.3
Birth-State State Covariates:				
Per-Capita Income ($1,000)	4.8	1.7	2.6	0.83
Hospital Beds per Capita (1,000)	5.1	0.92	4.8	0.86
Hospitals per Capita (1,000)	0.04	0.02	0.04	0.02
Total Disease Rate (per 100K)	287.5	274.4	288.5	300.8
Infant Mortality Rate, Whites (per 1,000)	16.6	3.4	21.9	2.8
Infant Mortality Rate, Blacks (per 1,000)	27.4	9.1	39.3	9.4
Observations	3,144,774		4,254,849	

Notes. "Post" and "pre" cohorts are defined relative to the timing of legal access to contraceptive pill for minors in each individual's birth state. A cohort is considered post-treatment if it was exposed in utero to the pill access in its birth state, and pre-treatment otherwise. Due to staggered legalization across states, a given birth year may contribute to both pre- and post-treatment groups depending on the birth state. The NCHS sample is collapsed at the maternal birth state, maternal birth year, maternal race (White, Black), child sex, birth order (first-time vs. higher-order), maternal age group (teenage, middle-age, older), and maternal education group (less than high school, high school, college or more). The number of pre-collapse birth observations is 73,422,112. The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977.

Table 2 – Effects on Second Generations' Birth Outcomes

	Outcomes:					
	Birth Weight	Low Birth Weight	Gestational Age	Preterm Birth	Fetal Growth	Birth Weight Rank
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Blacks						
Exposure	4.6***	-0.0013**	0.0015	0.0005	0.13***	0.02***
	(1.4)	(0.0007)	(0.0068)	(0.0007)	(0.03)	(0.0050)
Observations	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351
R-squared	0.09	0.03	0.04	0.04	0.10	0.16
Mean DV	3091.1	0.14	38.3	0.19	80.3	4.7
Panel B. Whites						
Exposure	1.1	-0.0002	0.0064**	-0.0004	0.01	0.0016
	(0.66)	(0.0002)	(0.0031)	(0.0003)	(0.01)	(0.0029)
Observations	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172
R-squared	0.19	0.03	0.15	0.04	0.24	0.29
Mean DV	3379.1	0.06	39.2	0.10	86.1	5.7

Notes. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age, birth order, and child sex. Exposure equals one for mothers born in or after the year in which their birth state extended legal access to the contraceptive pill to minors, and zero otherwise. Standard errors, two-way clustered by maternal birth state and maternal birth year, are in parentheses. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year by birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state (measured over the years preceding the 1963 introduction of the measles vaccine) interacted with maternal birth-year fixed effects to control for exposure to measles vaccination. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977.
Second generation: Children whose mothers, born between 1955 and 1977, were exposed to the legalization of the contraceptive pill for minors.
*** p<0.01, ** p<0.05, * p<0.1

Table 3 – Heterogeneity in the Effects on Second Generations' Birth Outcomes by Maternal Birth-State Share of Births to Mothers Age 20 or Younger

	Outcomes:					
	Birth Weight	Low Birth Weight	Gestational Age	Preterm Birth	Fetal Growth	Birth Weight Rank
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Blacks, Higher Birth-State Pregnancy Age ≤ 20						
Exposure	6.6***	-0.0021**	-0.0072	0.0003	0.20***	0.03***
	(1.8)	(0.0009)	(0.0094)	(0.0010)	(0.04)	(0.0068)
Observations	938,202	938,202	938,202	938,202	938,202	938,202
R-squared	0.08	0.02	0.04	0.04	0.10	0.16
Mean DV	3089.5	0.13	38.3	0.19	80.3	4.7
Panel B. Blacks, Lower Birth-State Pregnancy Age ≤ 20						
Exposure	2.6	-0.0001	0.0067	0.0006	0.04	0.01
	(2.4)	(0.0012)	(0.0117)	(0.0011)	(0.05)	(0.0082)
Observations	581,149	581,149	581,149	581,149	581,149	581,149
R-squared	0.09	0.03	0.03	0.04	0.10	0.16
Mean DV	3094.0	0.14	38.4	0.18	80.3	4.7
Panel C. Whites, Higher Birth-State Pregnancy Age ≤ 20						
Exposure	-2.8**	0.0005	-0.0028	-0.0000	-0.07***	-0.01**
	(1.1)	(0.0004)	(0.0044)	(0.0004)	(0.03)	(0.0055)
Observations	2,428,858	2,428,858	2,428,858	2,428,858	2,428,858	2,428,858
R-squared	0.17	0.02	0.15	0.04	0.21	0.27
Mean DV	3364.9	0.07	39.1	0.10	85.8	5.7
Panel D. Whites, Lower Birth-State Pregnancy Age ≤ 20						
Exposure	1.7**	-0.0007**	0.01***	-0.0014***	0.02	-0.0013
	(0.84)	(0.0003)	(0.0041)	(0.0004)	(0.02)	(0.0034)
Observations	2,777,314	2,777,314	2,777,314	2,777,314	2,777,314	2,777,314
R-squared	0.20	0.03	0.15	0.04	0.25	0.31
Mean DV	3389.1	0.06	39.2	0.09	86.3	5.8

Notes. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age, birth order, and child sex. Exposure equals one for mothers born in or after the year in which their birth state extended legal access to the contraceptive pill to minors, and zero otherwise. Standard errors, two-way clustered by maternal birth state and maternal birth year, are in parentheses. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year by birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state (measured over the years preceding the 1963 introduction of the measles vaccine) interacted with maternal birth-year fixed effects to control for exposure to measles vaccination. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977. This table reports the baseline specification estimated separately for subsamples defined by whether the maternal birth state was above or below the median share of teenage-young pregnancy in the pre-legalization period.

Second generation: Children whose mothers, born between 1955 and 1977, were exposed to the legalization of the contraceptive pill for minors.

*** p<0.01, ** p<0.05, * p<0.1

Table 4 – Heterogeneity in the Effects on Second Generations' Birth Outcomes by Maternal Birth-State Infant Mortality Rate

	Outcomes:					
	Birth Weight	Low Birth Weight	Gestational Age	Preterm Birth	Fetal Growth	Birth Weight Rank
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Blacks, Higher IMR						
Exposure	8.0***	-0.0022**	0.0064	0.0007	0.22***	0.03***
	(1.8)	(0.0010)	(0.0088)	(0.0009)	(0.04)	(0.0075)
Observations	846,013	846,013	846,013	846,013	846,013	846,013
R-squared	0.09	0.03	0.04	0.04	0.10	0.16
Mean DV	3089.2	0.14	38.4	0.19	80.2	4.7
Panel B. Blacks, Lower IMR						
Exposure	-1.1	-0.0001	-0.02**	0.0027***	0.02	0.01
	(1.8)	(0.0008)	(0.0095)	(0.0009)	(0.04)	(0.0078)
Observations	673,338	673,338	673,338	673,338	673,338	673,338
R-squared	0.08	0.02	0.03	0.04	0.09	0.15
Mean DV	3093.7	0.13	38.3	0.18	80.4	4.7
Panel C. Whites, Higher IMR						
Exposure	1.5	-0.0000	-0.0061	0.0003	0.05**	0.01***
	(1.1)	(0.0004)	(0.0050)	(0.0004)	(0.02)	(0.0047)
Observations	2,569,655	2,569,655	2,569,655	2,569,655	2,569,655	2,569,655
R-squared	0.19	0.03	0.11	0.04	0.24	0.30
Mean DV	3391.9	0.06	39.3	0.09	86.1	5.7
Panel D. Whites, Lower IMR						
Exposure	0.73	-0.0006**	0.0051	-0.0002	0.01	-0.0015
	(0.71)	(0.0002)	(0.0034)	(0.0003)	(0.01)	(0.0032)
Observations	2,636,517	2,636,517	2,636,517	2,636,517	2,636,517	2,636,517
R-squared	0.18	0.02	0.13	0.03	0.23	0.29
Mean DV	3366.6	0.07	39.0	0.10	86.1	5.8

Notes. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age, birth order, and child sex. Exposure equals one for mothers born in or after the year in which their birth state extended legal access to the contraceptive pill to minors, and zero otherwise. Standard errors, two-way clustered by maternal birth state and maternal birth year, are in parentheses. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year by birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state (measured over the years preceding the 1963 introduction of the measles vaccine) interacted with maternal birth-year fixed effects to control for exposure to measles vaccination. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977. This table reports the baseline specification estimated separately for subsamples defined by whether the maternal birth state was above or below the median race-specific infant mortality rate in the pre-legalization period.

Second generation: Children whose mothers, born between 1955 and 1977, were exposed to the legalization of the contraceptive pill for minors.

*** p<0.01, ** p<0.05, * p<0.1

Table 5 - Exploring Mechanisms: Maternal Characteristics and Prenatal Behaviors of Mothers of Second-Generation Children

	Outcomes:					
	Child Firstborn	Child Female	Mother Age	Mother Education ≤ 12	Number Of Prenatal Visits	No Prenatal Visits
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Blacks						
Exposure	0.0003	-0.0005	-0.02	-0.0091**	0.0079	-0.0024***
	(0.0018)	(0.0006)	(0.04)	(0.0046)	(0.0252)	(0.0006)
Observations	1,519,351	1,519,351	1,519,351	1,519,351	1,454,564	1,519,351
R-squared	0.0043	0.0000	0.03	0.04	0.08	0.03
Mean DV	0.29	0.49	24.9	0.64	9.9	0.03
Panel B. Whites						
Exposure	0.0026***	0.0006**	-0.04	0.0008	0.0056	-0.0002**
	(0.0010)	(0.0003)	(0.03)	(0.0057)	(0.0327)	(0.0001)
Observations	5,206,172	5,206,172	5,206,172	5,206,172	5,097,108	5,206,172
R-squared	0.0020	0.0001	0.02	0.03	0.11	0.01
Mean DV	0.34	0.49	27.4	0.46	11.4	0.0080
	Month Prenatal Care Began	Kessner Index: Adequate	Kessner Index: Inadequate	Birth Spacing from Previous Pregnancy	Birth Spacing from Previous Pregnancy: 0-1 Years	Birth Spacing from Previous Pregnancy: 0-2 Years
	(7)	(8)	(9)	(10)	(11)	(12)
Panel C. Blacks						
Exposure	-0.02**	0.0034**	-0.0031***	1.2***	-0.0016**	-0.0051**
	(0.0099)	(0.0016)	(0.0011)	(0.19)	(0.0008)	(0.0020)
Observations	1,464,155	758,911	758,911	474,734	474,734	474,734
R-squared	0.07	0.05	0.03	0.17	0.05	0.15
Mean DV	2.9	0.60	0.13	43.5	0.07	0.38
Panel D. Whites						
Exposure	0.0030	0.0005	-0.0008*	0.45***	0.0001	-0.0025*
	(0.0037)	(0.0012)	(0.0004)	(0.12)	(0.0004)	(0.0014)
Observations	5,080,640	2,629,554	2,629,554	1,446,703	1,446,703	1,446,703
R-squared	0.15	0.11	0.05	0.12	0.03	0.08
Mean DV	2.3	0.80	0.04	41.1	0.04	0.33

Notes. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age, birth order, and child sex. Exposure equals one for mothers born in or after the year in which their birth state extended legal access to the contraceptive pill to minors, and zero otherwise. Standard errors, two-way clustered by maternal birth state and maternal birth year, are in parentheses. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year by birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state (measured over the years preceding the 1963 introduction of the measles vaccine) interacted with maternal birth-year fixed effects to control for exposure to measles vaccination. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977. Outcomes in this table are maternal and prenatal-care characteristics drawn from the same NCHS cell-level data used in the main analysis.
Second generation: Children whose mothers, born between 1955 and 1977, were exposed to the legalization of the contraceptive pill for minors.
*** p<0.01, ** p<0.05, * p<0.1

Table 6 - Exploring Mechanisms: Effects on First-Generation Infant Health

	Outcomes:					
	Share Low Birth Weight Whites (per 1,000 Births)	Share Low Birth Weight Blacks (per 1,000 Births)	Share Very Low Birth Weight Whites (per 1,000 Births)	Share Very Low Birth Weight Blacks (per 1,000 Births)	Infant Mortality Rate Whites (per 1,000 Births)	Infant Mortality Rate Blacks (per 1,000 Births)
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure	-0.58*	-3.6***	0.04	-0.63	-0.10	-0.31
	(0.30)	(0.99)	(0.12)	(0.46)	(0.12)	(0.46)
Observations	967	967	967	967	967	967
R-squared	0.92	0.86	0.56	0.61	0.96	0.86
Mean DV	67.2	128.5	9.7	22.4	18.5	31.8

Notes. The unit of observation is a state-by-year cell. Standard errors, clustered by state, are in parentheses. Regressions are weighted by state population. All regressions include state fixed effects, region-by-year fixed effects, and state-by-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates. Exposure equals one for state-years in or after the year in which the state extended legal access to the contraceptive pill to minors, and zero otherwise.
First generation: Women born between 1955 and 1977 who were directly exposed to legal contraceptive pill access in early life.
*** p<0.01, ** p<0.05, * p<0.1

Table 7 - Exploring Mechanism Channels: Effects on Zeroth-Generation Outcomes

	Panel A. Fertility Timing			
	Teen Births: Share of Births to Mothers Aged 10-19		Prime-Age Births: Share of Births to Mothers Aged 20-35	
	Black	White	Black	White
	(1)	(2)	(3)	(4)
Exposure	-.00208*	.00069	.00161	-.00169
	(.00112)	(.00078)	(.00166)	(.00156)
Observations	1161	1161	1161	1161
R-squared	.97345	.97715	.98887	.98695
Mean DV	0.062	0.118	0.162	0.658

	Panel B. Maternal Characteristics at Birth								
	Black Mothers				White Mothers				Share Of Black to White Births
	Married	Age	Age < 20	Education < 12	Married	Age	Age < 20	Education < 12	
	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Exposure	-0.0038	0.09***	-0.0049**	-0.01	-0.0005	0.03*	-0.0018**	-0.0067	-0.0069
	(0.0189)	(0.04)	(0.0021)	(0.02)	(0.0036)	(0.01)	(0.0009)	(0.0063)	(0.0078)
Observations	497	500	500	449	500	500	500	450	500
R-squared	0.76	0.95	0.96	0.72	0.53	0.99	0.99	0.86	0.95
Mean DV	0.70	23.5	0.29	0.78	0.93	24.8	0.16	0.71	0.19

Notes. This table examines the effects of expanding legal access to the contraceptive pill for minors on fertility timing and maternal characteristics among the zeroth generation, defined as women who gave birth between 1955 and 1977. The unit of observation is a state-by-year cell. Standard errors, clustered by state, are in parentheses. Panel A uses state-level birth statistics digitized from the NCHS historical reports for 1955–1977. The advantage of these reports is their longer historical coverage, allowing us to examine fertility patterns over a broader portion of the policy-expansion period. Their limitation, however, is that births are reported only for broad age-by-race groups, which restricts the set of outcomes we can examine. Panel B instead uses the NCHS Natality Detail Files aggregated to the state-year level for 1968–1977. These files contain richer individual-level information on maternal characteristics, allowing us to examine additional outcomes. The disadvantage is that the detailed natality files begin only in 1968, which shortens the pre-policy window and limits our ability to examine earlier changes in zeroth-generation fertility and maternal characteristics. Regressions are weighted by state population. All regressions include state fixed effects, region-by-year fixed effects, and state-by-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates. Exposure equals one for state-years in or after the year in which the state extended legal access to the contraceptive pill to minors, and zero otherwise. The sample covers state-years from 1955 to 1977 in Panel A and from 1968 to 1977 in Panel B.

*** p<0.01, ** p<0.05, * p<0.1

Appendix A

Appendix Table A-1 - Correlation Between Early Contraceptive Pill Adoption and State Characteristics

	Outcome: Early Adoption (legalization year < 1971)			
	(1)	(2)	(3)	(4)
Gender Wage Gap 1962-1968 ($1,000 in 2020 dollars)	-.06236	-.05619		
	(.04491)	(.04723)		
Residual Gender Wage Gap 1962-1968 ($1,000 in 2020 dollars)			-.0837	-.08493
			(.06329)	(.06006)
Log Per-Capita Income	-.62833	-.24178	-.59314	-.28037
	(.79354)	(.83262)	(.74503)	(.77658)
Hospital Beds per Capita (1,000)	.05771	.08989	.05863	.0945
	(.10791)	(.1148)	(.10704)	(.11469)
Disease Rate	-.0331	-.05849	-.03161	-.05648
	(.06657)	(.06917)	(.06629)	(.06833)
Early Pill for Adults Adopter	.21329	.13268	.25088	.17128
	(.19131)	(.1983)	(.20069)	(.20592)
Early Abortion Legalization	.22169	.00224	.19676	-.01969
	(.29338)	(.32644)	(.28252)	(.31233)
Observations	40	40	40	40
R-squared	.10801	.21283	.1091	.22144
Mean DV	0.600	0.600	0.600	0.600
Region FE	No	Yes	No	Yes

Notes. The unit of observation is a U.S. state, with all variables measured in 1968. The dependent variable is an indicator equal to one if the state extended legal access to the contraceptive pill to minors during an early portion of the staggered legalization window. Standard errors are in parentheses. In columns 3 and 4, the residual gender wage gap is obtained by partialing out the effects of age, education, race, occupation (272 codes), and industry (836 codes) from the raw state-level gender wage gap. The 1962 CPS identifies only 40 states, so the correlates analysis is restricted to this subset. Further, we define early adoption as legalization of minors' access to the contraceptive pill before 1971, the median legalization year in the final sample.

*** p<0.01, ** p<0.05, * p<0.1

Appendix B

Appendix Table B-1 - Heterogeneity in the Effects on Second Generations' Birth Outcomes by Maternal Education

	Outcomes:					
	Birth Weight	Low Birth Weight	Gestational Age	Preterm Birth	Fetal Growth	Birth Weight Rank
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Blacks, Mother Education < 12 Years						
Exposure	5.4***	-0.0017**	0.0017	0.0005	0.15***	0.02***
	(1.8)	(0.0009)	(0.0084)	(0.0009)	(0.04)	(0.0061)
Observations	753,168	753,168	753,168	753,168	753,168	753,168
R-squared	0.09	0.03	0.04	0.03	0.10	0.16
Mean DV	3064.9	0.14	38.3	0.20	79.7	4.6
Panel B. Blacks, Mother Education ≥ 12 Years						
Exposure	4.5**	-0.0011	0.0057	0.0002	0.12***	0.02***
	(2.0)	(0.0009)	(0.0092)	(0.0011)	(0.04)	(0.0084)
Observations	766,183	766,183	766,183	766,183	766,183	766,183
R-squared	0.06	0.02	0.04	0.02	0.07	0.12
Mean DV	3137.9	0.12	38.4	0.17	81.3	4.9
Panel C. Whites, Mother Education < 12 Years						
Exposure	0.29	-0.0000	0.0059	0.0000	-0.0060	-0.0004
	(0.95)	(0.0003)	(0.0043)	(0.0004)	(0.0213)	(0.0042)
Observations	2,346,361	2,346,361	2,346,361	2,346,361	2,346,361	2,346,361
R-squared	0.15	0.02	0.12	0.04	0.18	0.24
Mean DV	3339.8	0.07	39.3	0.10	84.9	5.5
Panel D. Whites, Mother Education ≥ 12 Years						
Exposure	1.3*	-0.0003	0.0089***	-0.0007**	0.02	-0.0001
	(0.69)	(0.0003)	(0.0033)	(0.0003)	(0.02)	(0.0033)
Observations	2,859,811	2,859,811	2,859,811	2,859,811	2,859,811	2,859,811
R-squared	0.17	0.02	0.17	0.04	0.21	0.27
Mean DV	3412.6	0.06	39.1	0.09	87.1	6.0

Notes. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age, birth order, and child sex. Exposure equals one for mothers born in or after the year in which their birth state extended legal access to the contraceptive pill to minors, and zero otherwise. Standard errors, two-way clustered by maternal birth state and maternal birth year, are in parentheses. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year by birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state (measured over the years preceding the 1963 introduction of the measles vaccine) interacted with maternal birth-year fixed effects to control for exposure to measles vaccination. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977. This table reports the baseline specification estimated separately for subsamples defined by maternal education.
Second generation: Children whose mothers, born between 1955 and 1977, were exposed to the legalization of the contraceptive pill for minors.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table B-2 - Heterogeneity in the Effects on Second Generations' Birth Outcomes by Child's Gender

	Outcomes:					
	Birth Weight	Low Birth Weight	Gestational Age	Preterm Birth	Fetal Growth	Birth Weight Rank
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Blacks, Child Female						
Exposure	3.2*	-0.0009	-0.01	0.0017*	0.12***	0.02***
	(1.7)	(0.0009)	(0.0087)	(0.0009)	(0.04)	(0.0061)
Observations	753,640	753,640	753,640	753,640	753,640	753,640
R-squared	0.04	0.02	0.04	0.04	0.03	0.05
Mean DV	3033.7	0.15	38.4	0.18	78.7	4.3
Panel B. Blacks, Child Male						
Exposure	5.9***	-0.0017**	0.02*	-0.0007	0.14***	0.02***
	(1.7)	(0.0008)	(0.0081)	(0.0009)	(0.04)	(0.0064)
Observations	765,711	765,711	765,711	765,711	765,711	765,711
R-squared	0.04	0.02	0.04	0.04	0.04	0.06
Mean DV	3146.9	0.12	38.3	0.19	81.8	5.1
Panel C. Whites, Child Female						
Exposure	0.73	-0.0003	0.0098***	-0.0005	-0.0020	-0.0011
	(0.75)	(0.0003)	(0.0034)	(0.0003)	(0.0167)	(0.0034)
Observations	2,574,183	2,574,183	2,574,183	2,574,183	2,574,183	2,574,183
R-squared	0.10	0.02	0.15	0.04	0.12	0.14
Mean DV	3315.8	0.07	39.2	0.09	84.4	5.3
Panel D. Whites, Child Male						
Exposure	1.4*	-0.0001	0.0033	-0.0003	0.03*	0.0042
	(0.74)	(0.0002)	(0.0034)	(0.0003)	(0.02)	(0.0032)
Observations	2,631,989	2,631,989	2,631,989	2,631,989	2,631,989	2,631,989
R-squared	0.11	0.02	0.14	0.04	0.14	0.17
Mean DV	3439.2	0.06	39.1	0.10	87.8	6.2

Notes. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age, birth order, and child sex. Exposure equals one for mothers born in or after the year in which their birth state extended legal access to the contraceptive pill to minors, and zero otherwise. Standard errors, two-way clustered by maternal birth state and maternal birth year, are in parentheses. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year by birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state (measured over the years preceding the 1963 introduction of the measles vaccine) interacted with maternal birth-year fixed effects to control for exposure to measles vaccination. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977. This table reports the baseline specification estimated separately for subsamples defined by child sex.
Second generation: Children whose mothers, born between 1955 and 1977, were exposed to the legalization of the contraceptive pill for minors.
*** p<0.01, ** p<0.05, * p<0.1

Appendix C

Appendix Table C-1 - The Effects on Alternative Measures of Health at Birth

	Outcomes:									
	Full-Term Birth Weight	Very Low Birth Weight (BW < 1,500 G)	Extremely Low Birth Weight (BW < 1,000 G)	Small For Gestational Age	Very Preterm Birth (GW < 28 Weeks)	Apgar Score	Level Apgar Score (Apgar < 8)	Birth Weight > 4,000 G	Birth Weight > 4,500 G	Birth Weight > 5,000 G
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A. Blacks										
Exposure	2.6**	-0.0013***	-0.0008***	-0.0019***	-0.0006***	0.0029	-0.0005	0.0002	0.0001	0.0000
	(1.1)	(0.0003)	(0.0002)	(0.0007)	(0.0002)	(0.0066)	(0.0012)	(0.0004)	(0.0001)	(0.0000)
Observations	1,318,939	1,519,351	1,519,351	1,519,351	1,519,351	1,309,050	1,309,050	1,519,351	1,519,351	1,519,351
R-squared	0.16	0.0050	0.0035	0.07	0.0042	0.06	0.02	0.05	0.01	0.0016
Mean DV	3232.3	0.03	0.02	0.16	0.02	8.9	0.05	0.05	0.0070	0.0010
Panel B. Whites										
Exposure	0.75	-0.0001	-0.0000	0.0002	-0.0000	-0.0022	-0.0001	0.0006**	0.0001	-0.0000
	(0.54)	(0.0001)	(0.0001)	(0.0002)	(0.0001)	(0.0018)	(0.0004)	(0.0003)	(0.0001)	(0.0000)
Observations	4,835,164	5,206,172	5,206,172	5,206,172	5,206,172	4,694,385	4,694,385	5,206,172	5,206,172	5,206,172
R-squared	0.26	0.0045	0.0021	0.10	0.0042	0.09	0.02	0.13	0.04	0.0058
Mean DV	3458.1	0.01	0.0050	0.09	0.0050	9.0	0.03	0.12	0.02	0.0020

Notes. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age, birth order, and child sex. Exposure equals one for mothers born in or after the year in which their birth state extended legal access to the contraceptive pill to minors, and zero otherwise. Standard errors, two-way clustered by maternal birth state and maternal birth year, are in parentheses. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year by birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state (measured over the years preceding the 1963 introduction of the measles vaccine) interacted with maternal birth-year fixed effects to control for exposure to measles vaccination. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977. This table reports the baseline specification using alternative measures of infant health at birth as dependent variables.
Second generation: Children whose mothers, born between 1955 and 1977, were exposed to the legalization of the contraceptive pill for minors.
*** p<0.01, ** p<0.05, * p<0.1

Appendix D

Appendix Table D-1 - Robustness Checks: (Borusyak et al., 2024)'s DiD Estimator

	Outcomes:					
	Birth Weight	Low Birth Weight	Gestational Age	Preterm Birth	Fetal Growth	Birth Weight Rank
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Blacks						
Exposure	6.1**	-0.0025***	0.0061	-0.0005	0.16***	0.03***
	(2.9)	(0.0008)	(0.0080)	(0.0008)	(0.05)	(0.0078)
Observations	1,190,455	1,190,455	1,190,455	1,190,455	1,190,455	1,190,455
Mean DV	3106.9	0.13	38.4	0.18	80.5	4.7
Panel B. Whites						
Exposure	1.9	-0.0003	-0.0085***	0.0002	0.06***	0.02***
	(1.8)	(0.0002)	(0.0028)	(0.0003)	(0.02)	(0.0044)
Observations	4,239,723	4,239,723	4,239,723	4,239,723	4,239,723	4,239,723
Mean DV	3371.9	0.07	39.2	0.10	85.8	5.7

Notes. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age, birth order, and child sex. Exposure equals one for mothers born in or after the year in which their birth state extended legal access to the contraceptive pill to minors, and zero otherwise. Standard errors, two-way clustered by maternal birth state and maternal birth year, are in parentheses. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year by birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state (measured over the years preceding the 1963 introduction of the measles vaccine) interacted with maternal birth-year fixed effects to control for exposure to measles vaccination. The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977. Relative to the baseline, this specification replaces the Sun and Abraham (2021) estimator with the imputation estimator of Borusyak, Jaravel, and Spiess (2024).
Second generation: Children whose mothers, born between 1955 and 1977, were exposed to the legalization of the contraceptive pill for minors.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table D-2 - Robustness Checks: (Wooldridge, 2021)'s DiD Estimator

	Outcomes:					
	Birth Weight	Low Birth Weight	Gestational Age	Preterm Birth	Fetal Growth	Birth Weight Rank
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Blacks						
Exposure	7.4***	-0.0023***	0.0101	-0.0003	0.19***	0.03***
	(1.9)	(0.0008)	(0.0091)	(0.0009)	(0.04)	(0.0073)
Observations	1,007	1,007	1,007	1,007	1,007	1,007
Mean DV	3091.8	0.13	38.4	0.19	80.3	4.7
Panel B. Whites						
Exposure	0.70	-0.0001	0.0017	-0.0002	0.01	0.0030
	(1.0)	(0.0003)	(0.0041)	(0.0003)	(0.02)	(0.0044)
Observations	1,008	1,008	1,008	1,008	1,008	1,008
Mean DV	3383.2	0.06	39.2	0.09	86.2	5.7

Notes. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age, birth order, and child sex. Exposure equals one for mothers born in or after the year in which their birth state extended legal access to the contraceptive pill to minors, and zero otherwise. Standard errors, two-way clustered by maternal birth state and maternal birth year, are in parentheses. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year by birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state (measured over the years preceding the 1963 introduction of the measles vaccine) interacted with maternal birth-year fixed effects to control for exposure to measles vaccination. The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977. Relative to the baseline specification, estimates are obtained using the difference-in-differences estimator proposed by Wooldridge (2021).

Second generation: Children whose mothers, born between 1955 and 1977, were exposed to the legalization of the contraceptive pill for minors.

*** p<0.01, ** p<0.05, * p<0.1

Appendix Table D-3 - Robustness Checks: Adding Child's Birth State and Birth Year Fixed Effects

	Outcomes:					
	Birth Weight	Low Birth Weight	Gestational Age	Preterm Birth	Fetal Growth	Birth Weight Rank
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Blacks						
Exposure	5.2***	-0.0014**	0.0022	0.0003	0.14***	0.02***
	(1.4)	(0.0006)	(0.0067)	(0.0007)	(0.03)	(0.0049)
Observations	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351
R-squared	0.10	0.04	0.07	0.05	0.10	0.17
Mean DV	3091.1	0.14	38.3	0.19	80.3	4.7
Panel B. Whites						
Exposure	0.56	-0.0001	0.0061**	-0.0003	0.0010	-0.0007
	(0.62)	(0.0002)	(0.0027)	(0.0003)	(0.0128)	(0.0025)
Observations	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172
R-squared	0.21	0.04	0.24	0.06	0.26	0.32
Mean DV	3379.1	0.06	39.2	0.10	86.1	5.7

Notes. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age, birth order, and child sex. Exposure equals one for mothers born in or after the year in which their birth state extended legal access to the contraceptive pill to minors, and zero otherwise. Standard errors, two-way clustered by maternal birth state and maternal birth year, are in parentheses. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year by birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state (measured over the years preceding the 1963 introduction of the measles vaccine) interacted with maternal birth-year fixed effects to control for exposure to measles vaccination. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977. Relative to the baseline, this specification additionally includes child birth-state fixed effects and child birth-year fixed effects.
Second generation: Children whose mothers, born between 1955 and 1977, were exposed to the legalization of the contraceptive pill for minors.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table D-4 - Robustness Checks: Controlling for Exposure to Medicaid

	Outcomes:					
	Birth Weight	Low Birth Weight	Gestational Age	Preterm Birth	Fetal Growth	Birth Weight Rank
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Blacks						
Exposure	4.1***	-0.0011*	-0.0019	0.0009	0.12***	0.02***
	(1.4)	(0.0006)	(0.0066)	(0.0007)	(0.03)	(0.0051)
Observations	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351
R-squared	0.09	0.03	0.04	0.04	0.10	0.16
Mean DV	3091.1	0.14	38.3	0.19	80.3	4.7
Panel B. Whites						
Exposure	1.1*	-0.0001	0.0065**	-0.0004	0.01	0.0021
	(0.67)	(0.0002)	(0.0029)	(0.0003)	(0.01)	(0.0030)
Observations	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172
R-squared	0.19	0.03	0.15	0.04	0.24	0.29
Mean DV	3379.1	0.06	39.2	0.10	86.1	5.7

Notes. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age, birth order, and child sex. Exposure equals one for mothers born in or after the year in which their birth state extended legal access to the contraceptive pill to minors, and zero otherwise. Standard errors, two-way clustered by maternal birth state and maternal birth year, are in parentheses. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year by birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state (measured over the years preceding the 1963 introduction of the measles vaccine) interacted with maternal birth-year fixed effects to control for exposure to measles vaccination. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977. Relative to the baseline, this specification additionally controls for state-cohort exposure to Medicaid.

Second generation: Children whose mothers, born between 1955 and 1977, were exposed to the legalization of the contraceptive pill for minors.

*** p<0.01, ** p<0.05, * p<0.1

Appendix Table D-5 - Robustness Checks: Controlling for Maternal Birth State Age-Specific Child Mortality Rates

	Outcomes:					
	Birth Weight	Low Birth Weight	Gestational Age	Preterm Birth	Fetal Growth	Birth Weight Rank
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Blacks						
Exposure	4.8***	-0.0013**	0.0028	0.0004	0.13***	0.02***
	(1.4)	(0.0007)	(0.0068)	(0.0008)	(0.03)	(0.0052)
Observations	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351
R-squared	0.09	0.03	0.04	0.04	0.10	0.16
Mean DV	3091.1	0.14	38.3	0.19	80.3	4.7
Panel B. Whites						
Exposure	1.0	-0.0002	0.0064**	-0.0004	0.01	0.0013
	(0.66)	(0.0002)	(0.0031)	(0.0003)	(0.01)	(0.0029)
Observations	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172
R-squared	0.19	0.03	0.15	0.04	0.24	0.29
Mean DV	3379.1	0.06	39.2	0.10	86.1	5.7

Notes. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age, birth order, and child sex. Exposure equals one for mothers born in or after the year in which their birth state extended legal access to the contraceptive pill to minors, and zero otherwise. Standard errors, two-way clustered by maternal birth state and maternal birth year, are in parentheses. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year by birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state (measured over the years preceding the 1963 introduction of the measles vaccine) interacted with maternal birth-year fixed effects to control for exposure to measles vaccination. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977. Relative to the baseline, this specification additionally controls for maternal birth-state age-specific child mortality rates.
Second generation: Children whose mothers, born between 1955 and 1977, were exposed to the legalization of the contraceptive pill for minors.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table D-6 - Robustness Checks: Controlling for Maternal Birth State Share of AFDC Recipients By Race

	Outcomes:					
	Birth Weight	Low Birth Weight	Gestational Age	Preterm Birth	Fetal Growth	Birth Weight Rank
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Blacks						
Exposure	4.0***	-0.0012*	0.0006	0.0008	0.12***	0.02***
	(1.4)	(0.0006)	(0.0067)	(0.0007)	(0.03)	(0.0050)
Observations	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351
R-squared	0.09	0.03	0.04	0.04	0.10	0.16
Mean DV	3091.1	0.14	38.3	0.19	80.3	4.7
Panel B. Whites						
Exposure	0.87	-0.0002	0.0061**	-0.0003	0.0087	0.0006
	(0.63)	(0.0002)	(0.0029)	(0.0003)	(0.0139)	(0.0028)
Observations	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172
R-squared	0.19	0.03	0.15	0.04	0.24	0.29
Mean DV	3379.1	0.06	39.2	0.10	86.1	5.7

Notes. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age, birth order, and child sex. Exposure equals one for mothers born in or after the year in which their birth state extended legal access to the contraceptive pill to minors, and zero otherwise. Standard errors, two-way clustered by maternal birth state and maternal birth year, are in parentheses. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year by birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state (measured over the years preceding the 1963 introduction of the measles vaccine) interacted with maternal birth-year fixed effects to control for exposure to measles vaccination. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977. Relative to the baseline, this specification additionally controls for the maternal birth-state share of AFDC recipients, by race.
Second generation: Children whose mothers, born between 1955 and 1977, were exposed to the legalization of the contraceptive pill for minors.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table D-7 - Robustness Checks: Adding Maternal Birth State Polio Rate (1950 – 1955) by Birth Year Fixed Effects

	Outcomes:					
	Birth Weight	Low Birth Weight	Gestational Age	Preterm Birth	Fetal Growth	Birth Weight Rank
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Blacks						
Exposure	3.2**	-0.0007	-0.0060	0.0012	0.11***	0.02***
	(1.4)	(0.0006)	(0.0065)	(0.0007)	(0.03)	(0.0052)
Observations	1,515,321	1,515,321	1,515,321	1,515,321	1,515,321	1,515,321
R-squared	0.09	0.03	0.04	0.04	0.10	0.16
Mean DV	3091.1	0.14	38.3	0.19	80.3	4.7
Panel B. Whites						
Exposure	0.69	-0.0001	0.0035	-0.0003	0.0086	0.0008
	(0.66)	(0.0002)	(0.0030)	(0.0003)	(0.0145)	(0.0029)
Observations	5,162,366	5,162,366	5,162,366	5,162,366	5,162,366	5,162,366
R-squared	0.19	0.03	0.15	0.04	0.24	0.30
Mean DV	3379.1	0.06	39.2	0.10	86.1	5.7

Notes. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age, birth order, and child sex. Exposure equals one for mothers born in or after the year in which their birth state extended legal access to the contraceptive pill to minors, and zero otherwise. Standard errors, two-way clustered by maternal birth state and maternal birth year, are in parentheses. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year by birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state (measured over the years preceding the 1963 introduction of the measles vaccine) interacted with maternal birth-year fixed effects to control for exposure to measles vaccination. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977. Relative to the baseline, this specification additionally includes maternal birth-state pre-vaccine polio rates (1950-1955) interacted with maternal birth-year fixed effects.
Second generation: Children whose mothers, born between 1955 and 1977, were exposed to the legalization of the contraceptive pill for minors.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table D-8 - Robustness Checks: Controlling for Maternal Birth State ERA, FEPA, And EPL Policy Exposures

	Outcomes:					
	Birth Weight	Low Birth Weight	Gestational Age	Preterm Birth	Fetal Growth	Birth Weight Rank
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Blacks						
Exposure	7.9***	-0.0024***	0.01*	-0.0006	0.18***	0.03***
	(1.3)	(0.0006)	(0.0067)	(0.0008)	(0.03)	(0.0059)
Observations	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351
R-squared	0.09	0.03	0.04	0.04	0.10	0.16
Mean DV	3091.1	0.14	38.3	0.19	80.3	4.7
Panel B. Whites						
Exposure	1.9***	-0.0003	0.0082***	-0.0004*	0.03**	0.0056*
	(0.63)	(0.0002)	(0.0032)	(0.0003)	(0.01)	(0.0031)
Observations	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172
R-squared	0.19	0.03	0.15	0.04	0.24	0.29
Mean DV	3379.1	0.06	39.2	0.10	86.1	5.7

Notes. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age, birth order, and child sex. Exposure equals one for mothers born in or after the year in which their birth state extended legal access to the contraceptive pill to minors, and zero otherwise. Standard errors, two-way clustered by maternal birth state and maternal birth year, are in parentheses. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year by birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state (measured over the years preceding the 1963 introduction of the measles vaccine) interacted with maternal birth-year fixed effects to control for exposure to measles vaccination. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977. Relative to the baseline, this specification additionally controls for state-cohort exposure to the Equal Rights Amendment (ERA), the Fair Employment Practices Act (FEPA), and Equal Pay Laws (EPL).

Second generation: Children whose mothers, born between 1955 and 1977, were exposed to the legalization of the contraceptive pill for minors.

*** p<0.01, ** p<0.05, * p<0.1

Appendix Table D-9 - Robustness Checks: Controlling for Maternal Birth State Number of Health Departments

	Outcomes:					
	Birth Weight	Low Birth Weight	Gestational Age	Preterm Birth	Fetal Growth	Birth Weight Rank
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Blacks						
Exposure	4.2***	-0.0012*	0.0003	0.0005	0.12***	0.02***
	(1.4)	(0.0006)	(0.0066)	(0.0007)	(0.03)	(0.0051)
Observations	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351
R-squared	0.09	0.03	0.04	0.04	0.10	0.16
Mean DV	3091.1	0.14	38.3	0.19	80.3	4.7
Panel B. Whites						
Exposure	1.2*	-0.0002	0.0064**	-0.0004	0.02	0.0021
	(0.65)	(0.0002)	(0.0030)	(0.0003)	(0.01)	(0.0029)
Observations	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172
R-squared	0.19	0.03	0.15	0.04	0.24	0.29
Mean DV	3379.1	0.06	39.2	0.10	86.1	5.7

Notes. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age, birth order, and child sex. Exposure equals one for mothers born in or after the year in which their birth state extended legal access to the contraceptive pill to minors, and zero otherwise. Standard errors, two-way clustered by maternal birth state and maternal birth year, are in parentheses. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year by birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state (measured over the years preceding the 1963 introduction of the measles vaccine) interacted with maternal birth-year fixed effects to control for exposure to measles vaccination. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977. Relative to the baseline, this specification additionally controls for the maternal birth-state number of health departments.
Second generation: Children whose mothers, born between 1955 and 1977, were exposed to the legalization of the contraceptive pill for minors.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table D-10 - Robustness Checks: Controlling for Maternal Birth State Per Capita Income Maintenance Benefits, Disability Insurance Benefits, and Retirement Benefits

	Outcomes:					
	Birth Weight	Low Birth Weight	Gestational Age	Preterm Birth	Fetal Growth	Birth Weight Rank
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Blacks						
Exposure	4.9***	-0.0013**	0.0021	0.0004	0.14***	0.02***
	(1.4)	(0.0007)	(0.0067)	(0.0007)	(0.03)	(0.0050)
Observations	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351
R-squared	0.09	0.03	0.04	0.04	0.10	0.16
Mean DV	3091.1	0.14	38.3	0.19	80.3	4.7
Panel B. Whites						
Exposure	1.3*	-0.0002	0.0064**	-0.0004	0.02	0.0027
	(0.66)	(0.0002)	(0.0031)	(0.0003)	(0.01)	(0.0029)
Observations	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172
R-squared	0.19	0.03	0.15	0.04	0.24	0.29
Mean DV	3379.1	0.06	39.2	0.10	86.1	5.7

Notes. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age, birth order, and child sex. Exposure equals one for mothers born in or after the year in which their birth state extended legal access to the contraceptive pill to minors, and zero otherwise. Standard errors, two-way clustered by maternal birth state and maternal birth year, are in parentheses. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year by birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state (measured over the years preceding the 1963 introduction of the measles vaccine) interacted with maternal birth-year fixed effects to control for exposure to measles vaccination. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977. Relative to the baseline, this specification additionally controls for maternal birth-state per capita income maintenance benefits, disability insurance benefits, and retirement benefits.
Second generation: Children whose mothers, born between 1955 and 1977, were exposed to the legalization of the contraceptive pill for minors.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table D-11 - Robustness Checks: Controlling for Medicare Benefits Per Capita

	Outcomes:					
	Birth Weight	Low Birth Weight	Gestational Age	Preterm Birth	Fetal Growth	Birth Weight Rank
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Blacks						
Exposure	5.0***	-0.0015**	0.0023	0.0004	0.14***	0.02***
	(1.4)	(0.0006)	(0.0067)	(0.0007)	(0.03)	(0.0050)
Observations	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351
R-squared	0.09	0.03	0.04	0.04	0.10	0.16
Mean DV	3091.1	0.14	38.3	0.19	80.3	4.7
Panel B. Whites						
Exposure	1.2*	-0.0002	0.0065**	-0.0004	0.02	0.0018
	(0.66)	(0.0002)	(0.0031)	(0.0003)	(0.01)	(0.0029)
Observations	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172
R-squared	0.19	0.03	0.15	0.04	0.24	0.29
Mean DV	3379.1	0.06	39.2	0.10	86.1	5.7

Notes. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age, birth order, and child sex. Exposure equals one for mothers born in or after the year in which their birth state extended legal access to the contraceptive pill to minors, and zero otherwise. Standard errors, two-way clustered by maternal birth state and maternal birth year, are in parentheses. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year by birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state (measured over the years preceding the 1963 introduction of the measles vaccine) interacted with maternal birth-year fixed effects to control for exposure to measles vaccination. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977. Relative to the baseline, this specification additionally controls for maternal birth-state Medicare benefits per capita.
Second generation: Children whose mothers, born between 1955 and 1977, were exposed to the legalization of the contraceptive pill for minors.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table D-12 - Robustness Checks: Controlling for Per Capita Food Stamp Expenditure

	Outcomes:					
	Birth Weight	Low Birth Weight	Gestational Age	Preterm Birth	Fetal Growth	Birth Weight Rank
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Blacks						
Exposure	4.6***	-0.0013**	0.0013	0.0005	0.13***	0.02***
	(1.4)	(0.0007)	(0.0068)	(0.0007)	(0.03)	(0.0050)
Observations	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351
R-squared	0.09	0.03	0.04	0.04	0.10	0.16
Mean DV	3091.1	0.14	38.3	0.19	80.3	4.7
Panel B. Whites						
Exposure	1.1*	-0.0002	0.0066**	-0.0004	0.01	0.0015
	(0.66)	(0.0002)	(0.0031)	(0.0003)	(0.01)	(0.0029)
Observations	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172
R-squared	0.19	0.03	0.15	0.04	0.24	0.29
Mean DV	3379.1	0.06	39.2	0.10	86.1	5.7

Notes. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age, birth order, and child sex. Exposure equals one for mothers born in or after the year in which their birth state extended legal access to the contraceptive pill to minors, and zero otherwise. Standard errors, two-way clustered by maternal birth state and maternal birth year, are in parentheses. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year by birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state (measured over the years preceding the 1963 introduction of the measles vaccine) interacted with maternal birth-year fixed effects to control for exposure to measles vaccination. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977. Relative to the baseline, this specification additionally controls for maternal birth-state per capita Food Stamp expenditure.
Second generation: Children whose mothers, born between 1955 and 1977, were exposed to the legalization of the contraceptive pill for minors.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table D-13 - Robustness Checks: Controlling for Exposure to Abortion Legalization

	Outcomes:					
	Birth Weight	Low Birth Weight	Gestational Age	Preterm Birth	Fetal Growth	Birth Weight Rank
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Blacks						
Exposure	4.4***	-0.0012*	0.0012	0.0005	0.13***	0.02***
	(1.4)	(0.0007)	(0.0067)	(0.0007)	(0.03)	(0.0050)
Observations	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351
R-squared	0.09	0.03	0.04	0.04	0.10	0.16
Mean DV	3091.1	0.14	38.3	0.19	80.3	4.7
Panel B. Whites						
Exposure	1.1*	-0.0002	0.0066**	-0.0004	0.01	0.0017
	(0.65)	(0.0002)	(0.0030)	(0.0003)	(0.01)	(0.0028)
Observations	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172
R-squared	0.19	0.03	0.15	0.04	0.24	0.29
Mean DV	3379.1	0.06	39.2	0.10	86.1	5.7

Notes. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age, birth order, and child sex. Exposure equals one for mothers born in or after the year in which their birth state extended legal access to the contraceptive pill to minors, and zero otherwise. Standard errors, two-way clustered by maternal birth state and maternal birth year, are in parentheses. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year by birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state (measured over the years preceding the 1963 introduction of the measles vaccine) interacted with maternal birth-year fixed effects to control for exposure to measles vaccination. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977. Relative to the baseline, this specification additionally controls for state-cohort exposure to abortion legalization.
Second generation: Children whose mothers, born between 1955 and 1977, were exposed to the legalization of the contraceptive pill for minors.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table D-14 - Robustness Checks: Restricting the Sample to Maternal Birth Years of 1955 – 1970

	Outcomes:					
	Birth Weight	Low Birth Weight	Gestational Age	Preterm Birth	Fetal Growth	Birth Weight Rank
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Blacks						
Exposure	5.6***	-0.0014*	0.0071	0.0001	0.15***	0.02***
	(1.6)	(0.0008)	(0.0074)	(0.0008)	(0.04)	(0.0060)
Observations	1,035,989	1,035,989	1,035,989	1,035,989	1,035,989	1,035,989
R-squared	0.09	0.03	0.03	0.04	0.10	0.16
Mean DV	3091.5	0.14	38.4	0.19	80.2	4.7
Panel B. Whites						
Exposure	1.00	-0.0002	0.0056*	-0.0003	0.01	0.0026
	(0.69)	(0.0002)	(0.0032)	(0.0003)	(0.02)	(0.0030)
Observations	4,770,027	4,770,027	4,770,027	4,770,027	4,770,027	4,770,027
R-squared	0.18	0.03	0.14	0.04	0.24	0.30
Mean DV	3383.2	0.06	39.2	0.10	86.2	5.7

Notes. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age, birth order, and child sex. Exposure equals one for mothers born in or after the year in which their birth state extended legal access to the contraceptive pill to minors, and zero otherwise. Standard errors, two-way clustered by maternal birth state and maternal birth year, are in parentheses. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year by birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state (measured over the years preceding the 1963 introduction of the measles vaccine) interacted with maternal birth-year fixed effects to control for exposure to measles vaccination. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). Relative to the baseline, this specification restricts the maternal birth-cohort sample to 1955-1970.
Second generation: Children whose mothers, born between 1955 and 1970, were exposed to the legalization of the contraceptive pill for minors.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table D-15 - Robustness Checks: Controlling for Contraceptive Pill Legalization for Adults

	Outcomes:					
	Birth Weight	Low Birth Weight	Gestational Age	Preterm Birth	Fetal Growth	Birth Weight Rank
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Blacks						
Exposure	3.9***	-0.0011*	-0.0025	0.0006	0.12***	0.02***
	(1.4)	(0.0007)	(0.0070)	(0.0007)	(0.03)	(0.0053)
Observations	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351
R-squared	0.09	0.03	0.04	0.04	0.10	0.16
Mean DV	3091.1	0.14	38.3	0.19	80.3	4.7
Panel B. Whites						
Exposure	1.1*	-0.0001	0.0060*	-0.0003	0.01	0.0024
	(0.67)	(0.0002)	(0.0031)	(0.0003)	(0.01)	(0.0029)
Observations	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172
R-squared	0.19	0.03	0.15	0.04	0.24	0.29
Mean DV	3379.1	0.06	39.2	0.10	86.1	5.7

Notes. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age, birth order, and child sex. Exposure equals one for mothers born in or after the year in which their birth state extended legal access to the contraceptive pill to minors, and zero otherwise. Standard errors, two-way clustered by maternal birth state and maternal birth year, are in parentheses. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year by birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state (measured over the years preceding the 1963 introduction of the measles vaccine) interacted with maternal birth-year fixed effects to control for exposure to measles vaccination. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977. Relative to the baseline, this specification additionally controls for state-cohort exposure to contraceptive pill legalization for adults.
Second generation: Children whose mothers, born between 1955 and 1977, were exposed to the legalization of the contraceptive pill for minors.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table D-16 - Robustness Checks: Adding the Interaction of Maternal Birth State Fixed Effects with Child Birth State Fixed Effects

	Outcomes:					
	Birth Weight	Low Birth Weight	Gestational Age	Preterm Birth	Fetal Growth	Birth Weight Rank
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Blacks						
Exposure	4.9***	-0.0015**	0.0021	0.0004	0.14***	0.02***
	(1.4)	(0.0006)	(0.0069)	(0.0007)	(0.03)	(0.0051)
Observations	1,519,304	1,519,304	1,519,304	1,519,304	1,519,304	1,519,304
R-squared	0.09	0.03	0.04	0.04	0.10	0.16
Mean DV	3091.1	0.14	38.3	0.19	80.3	4.7
Panel B. Whites						
Exposure	0.74	-0.0001	0.0062*	-0.0003	0.0051	-0.0002
	(0.65)	(0.0002)	(0.0032)	(0.0003)	(0.0140)	(0.0028)
Observations	5,206,164	5,206,164	5,206,164	5,206,164	5,206,164	5,206,164
R-squared	0.20	0.03	0.15	0.04	0.25	0.31
Mean DV	3379.1	0.06	39.2	0.10	86.1	5.7

Notes. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age, birth order, and child sex. Exposure equals one for mothers born in or after the year in which their birth state extended legal access to the contraceptive pill to minors, and zero otherwise. Standard errors, two-way clustered by maternal birth state and maternal birth year, are in parentheses. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year by birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state (measured over the years preceding the 1963 introduction of the measles vaccine) interacted with maternal birth-year fixed effects to control for exposure to measles vaccination. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977. Relative to the baseline, this specification additionally includes the interaction of maternal birth-state fixed effects with child birth-state fixed effects.
Second generation: Children whose mothers, born between 1955 and 1977, were exposed to the legalization of the contraceptive pill for minors.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table D-17 - Robustness Checks: Adding the Interaction of Birth State Fixed Effects with Maternal Education and Age Dummies

	Outcomes:					
	Birth Weight	Low Birth Weight	Gestational Age	Preterm Birth	Fetal Growth	Birth Weight Rank
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Blacks						
Exposure	4.6***	-0.0013**	0.0015	0.0005	0.13***	0.02***
	(1.4)	(0.0007)	(0.0068)	(0.0007)	(0.03)	(0.0050)
Observations	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351
R-squared	0.09	0.03	0.04	0.04	0.10	0.16
Mean DV	3091.1	0.14	38.3	0.19	80.3	4.7
Panel B. Whites						
Exposure	1.1	-0.0002	0.0064**	-0.0004	0.01	0.0016
	(0.66)	(0.0002)	(0.0031)	(0.0003)	(0.01)	(0.0029)
Observations	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172
R-squared	0.19	0.03	0.15	0.04	0.24	0.29
Mean DV	3379.1	0.06	39.2	0.10	86.1	5.7

Notes. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age, birth order, and child sex. Exposure equals one for mothers born in or after the year in which their birth state extended legal access to the contraceptive pill to minors, and zero otherwise. Standard errors, two-way clustered by maternal birth state and maternal birth year, are in parentheses. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year by birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state (measured over the years preceding the 1963 introduction of the measles vaccine) interacted with maternal birth-year fixed effects to control for exposure to measles vaccination. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977. Relative to the baseline, this specification additionally includes the interaction of maternal birth-state fixed effects with maternal education and maternal age dummies.
Second generation: Children whose mothers, born between 1955 and 1977, were exposed to the legalization of the contraceptive pill for minors.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table D-18 - Robustness Checks: Weighting the Regressions by Birth Counts and Share of Teenage Pregnancy in Mother's Birth State

	Outcomes:					
	Birth Weight	Low Birth Weight	Gestational Age	Preterm Birth	Fetal Growth	Birth Weight Rank
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Blacks						
Exposure	4.7***	-0.0013**	-0.0010	0.0006	0.14***	0.02***
	(1.4)	(0.0006)	(0.0066)	(0.0007)	(0.03)	(0.0050)
Observations	1,511,661	1,511,661	1,511,661	1,511,661	1,511,661	1,511,661
R-squared	0.09	0.03	0.04	0.04	0.10	0.16
Mean DV	3090.6	0.13	38.3	0.19	80.3	4.7
Panel B. Whites						
Exposure	0.76	-0.0001	0.0049	-0.0003	0.0078	0.0008
	(0.65)	(0.0002)	(0.0030)	(0.0003)	(0.0145)	(0.0029)
Observations	5,191,372	5,191,372	5,191,372	5,191,372	5,191,372	5,191,372
R-squared	0.18	0.03	0.15	0.04	0.23	0.29
Mean DV	3376.6	0.06	39.2	0.10	86.1	5.7

Notes. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age, birth order, and child sex. Exposure equals one for mothers born in or after the year in which their birth state extended legal access to the contraceptive pill to minors, and zero otherwise. Standard errors, two-way clustered by maternal birth state and maternal birth year, are in parentheses. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year by birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state (measured over the years preceding the 1963 introduction of the measles vaccine) interacted with maternal birth-year fixed effects to control for exposure to measles vaccination. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977. Relative to the baseline, this specification weights regressions by the product of birth counts in the cell and the maternal birth-state share of teenage pregnancy.
Second generation: Children whose mothers, born between 1955 and 1977, were exposed to the legalization of the contraceptive pill for minors.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table D-19 - Robustness Checks: Adding Child's Birth Region by Birth Year Fixed Effects

	Outcomes:					
	Birth Weight	Low Birth Weight	Gestational Age	Preterm Birth	Fetal Growth	Birth Weight Rank
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Blacks						
Exposure	5.3***	-0.0015**	0.0037	0.0002	0.14***	0.02***
	(1.4)	(0.0006)	(0.0067)	(0.0007)	(0.03)	(0.0049)
Observations	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351
R-squared	0.10	0.04	0.07	0.05	0.10	0.17
Mean DV	3091.1	0.14	38.3	0.19	80.3	4.7
Panel B. Whites						
Exposure	0.54	-0.0001	0.0064**	-0.0003	-0.0005	-0.0012
	(0.61)	(0.0002)	(0.0027)	(0.0003)	(0.0127)	(0.0025)
Observations	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172
R-squared	0.21	0.04	0.25	0.06	0.26	0.32
Mean DV	3379.1	0.06	39.2	0.10	86.1	5.7

Notes. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age, birth order, and child sex. Exposure equals one for mothers born in or after the year in which their birth state extended legal access to the contraceptive pill to minors, and zero otherwise. Standard errors, two-way clustered by maternal birth state and maternal birth year, are in parentheses. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year by birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state (measured over the years preceding the 1963 introduction of the measles vaccine) interacted with maternal birth-year fixed effects to control for exposure to measles vaccination. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977. Relative to the baseline, this specification additionally includes child birth-region by child birth-year fixed effects.
Second generation: Children whose mothers, born between 1955 and 1977, were exposed to the legalization of the contraceptive pill for minors.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table D-20 - Robustness Checks: Using Individual Level Data

	Outcomes:					
	Birth Weight	Low Birth Weight	Gestational Age	Preterm Birth	Fetal Growth	Birth Weight Rank
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Blacks						
Exposure	4.6***	-0.0013**	0.0015	0.0005	0.13***	0.02***
	(1.4)	(0.0007)	(0.0068)	(0.0007)	(0.03)	(0.0050)
Observations	11,708,305	11,708,305	11,708,305	11,708,305	11,708,305	11,708,305
R-squared	0.01	0.0036	0.0050	0.0049	0.01	0.02
Mean DV	3091.1	0.14	38.3	0.19	80.3	4.7
Panel B. Whites						
Exposure	1.1	-0.0002	0.0064**	-0.0004	0.01	0.0016
	(0.66)	(0.0002)	(0.0031)	(0.0003)	(0.01)	(0.0029)
Observations	59,307,429	59,307,429	59,307,429	59,307,429	59,307,429	59,307,429
R-squared	0.02	0.0028	0.02	0.0041	0.03	0.04
Mean DV	3379.1	0.06	39.2	0.10	86.1	5.7

Notes. Exposure equals one for mothers born in or after the year in which their birth state extended legal access to the contraceptive pill to minors, and zero otherwise. Standard errors, two-way clustered by maternal birth state and maternal birth year, are in parentheses. All regressions include maternal birth-state fixed effects, maternal birth-year by birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state (measured over the years preceding the 1963 introduction of the measles vaccine) interacted with maternal birth-year fixed effects to control for exposure to measles vaccination. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977. Relative to the baseline, this specification estimates the regression on individual-level data without collapsing into cells. Second generation: Children whose mothers, born between 1955 and 1977, were exposed to the legalization of the contraceptive pill for minors.
*** p<0.01, ** p<0.05, * p<0.1

Appendix E

Appendix Table E-1 - Placebo Tests: Shifting the Policies 10 Years Earlier

	Outcomes:					
	Birth Weight	Low Birth Weight	Gestational Age	Preterm Birth	Fetal Growth	Birth Weight Rank
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Blacks						
Exposure	-0.50	0.0008	-0.01*	0.0008	0.0067	0.0032
	(1.16)	(0.0006)	(0.0064)	(0.0007)	(0.0259)	(0.0050)
Observations	1,548,865	1,548,865	1,548,865	1,548,865	1,548,865	1,548,865
R-squared	0.09	0.02	0.04	0.04	0.10	0.16
Mean DV	3078.8	0.14	38.2	0.18	80.2	4.7
Panel B. Whites						
Exposure	0.50	0.0001	-0.01***	0.0004*	0.04***	0.0088***
	(0.59)	(0.0002)	(0.0036)	(0.0003)	(0.01)	(0.0026)
Observations	4,892,025	4,892,025	4,892,025	4,892,025	4,892,025	4,892,025
R-squared	0.19	0.02	0.10	0.03	0.23	0.29
Mean DV	3342.1	0.07	38.9	0.11	85.8	5.7

Notes. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age, birth order, and child sex. Standard errors, two-way clustered by maternal birth state and maternal birth year, are in parentheses. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year by birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state (measured over the years preceding the 1963 introduction of the measles vaccine) interacted with maternal birth-year fixed effects to control for exposure to measles vaccination. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977. This table reports a placebo specification that shifts the state-specific legalization years 10 years earlier than the actual policy dates.
Second generation: Children whose mothers, born between 1955 and 1977, were exposed to the legalization of the contraceptive pill for minors.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table E-2 - Placebo Tests Using Fake Policy Sets Drawn from a Normal Distribution with the Same Mean and Standard Deviation as the Actual Policy Distribution

	Outcome: Birth Weight, Iteration:									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A. Blacks										
Exposure	-0.20	0.40	-0.79	0.78	-0.24	-1.6**	1.2**	-0.72	-0.15	1.2*
	(0.68)	(0.70)	(0.74)	(0.68)	(0.63)	(0.70)	(0.62)	(0.73)	(0.67)	(0.69)
Observations	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351
R-squared	0.09	0.09	0.09	0.09	0.09	0.09	0.09	0.09	0.09	0.09
Mean DV	3106.5	3106.1	3106.8	3106.1	3106.4	3106.5	3106.5	3106.0	3106.6	3105.9
Panel B. Whites										
Exposure	-0.09	0.09	-0.45	-0.39	-0.24	0.26	0.41	0.14	0.55	-0.0092
	(0.34)	(0.36)	(0.36)	(0.36)	(0.33)	(0.34)	(0.33)	(0.37)	(0.37)	(0.3456)
Observations	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172
R-squared	0.19	0.19	0.19	0.19	0.19	0.19	0.19	0.19	0.19	0.19
Mean DV	3372.8	3372.8	3372.8	3372.5	3372.8	3372.7	3372.7	3372.9	3372.8	3372.8

Notes. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age, birth order, and child sex. Standard errors, two-way clustered by maternal birth state and maternal birth year, are in parentheses. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year by birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state (measured over the years preceding the 1963 introduction of the measles vaccine) interacted with maternal birth-year fixed effects to control for exposure to measles vaccination. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977. This table reports a placebo specification in which the state-specific legalization years are drawn at random from a normal distribution with the same mean and standard deviation as the actual policy distribution. Different appendix tables in this group report independent draws.
Second generation: Children whose mothers, born between 1955 and 1977, were exposed to the legalization of the contraceptive pill for minors.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table E-3 - Placebo Tests Using Fake Policy Sets Drawn from a Normal Distribution with the Same Mean and Standard Deviation as the Actual Policy Distribution

	Outcome: Low Birth Weight, Iteration:									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A. Blacks										
Exposure	-0.0001	0.0004	-0.0003	-0.0000	0.0002	-0.0003	0.0005	-0.0000	0.0005	-0.0002
	(0.0003)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0003)	(0.0004)	(0.0003)	(0.0004)	(0.0004)
Observations	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351
R-squared	0.03	0.03	0.03	0.03	0.03	0.03	0.03	0.03	0.03	0.03
Mean DV	0.13	0.13	0.13	0.13	0.13	0.13	0.13	0.13	0.13	0.13
Panel B. Whites										
Exposure	0.0001	0.0001	0.0001	0.0002*	0.0000	0.0002	0.0001	-0.0001	0.0000	-0.0002
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Observations	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172
R-squared	0.03	0.03	0.03	0.03	0.03	0.03	0.03	0.03	0.03	0.03
Mean DV	0.07	0.07	0.07	0.07	0.07	0.07	0.07	0.07	0.07	0.07

Notes. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age, birth order, and child sex. Standard errors, two-way clustered by maternal birth state and maternal birth year, are in parentheses. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year by birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state (measured over the years preceding the 1963 introduction of the measles vaccine) interacted with maternal birth-year fixed effects to control for exposure to measles vaccination. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977. This table reports a placebo specification in which the state-specific legalization years are drawn at random from a normal distribution with the same mean and standard deviation as the actual policy distribution. Different appendix tables in this group report independent draws.
Second generation: Children whose mothers, born between 1955 and 1977, were exposed to the legalization of the contraceptive pill for minors.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table E-4 - Placebo Tests Using Fake Policy Sets Drawn from a Normal Distribution with the Same Mean and Standard Deviation as the Actual Policy Distribution

	Outcome: Gestational Age, Iteration:									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A. Blacks										
Exposure	-0.0017	0.0063*	-0.0044	-0.0012	0.0012	0.0022	-0.0030	-0.0014	0.0011	-0.0052
	(0.0039)	(0.0036)	(0.0037)	(0.0039)	(0.0036)	(0.0037)	(0.0037)	(0.0037)	(0.0040)	(0.0039)
Observations	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351
R-squared	0.04	0.04	0.04	0.04	0.04	0.04	0.04	0.04	0.04	0.04
Mean DV	38.4	38.4	38.4	38.4	38.4	38.4	38.4	38.4	38.4	38.4
Panel B. Whites										
Exposure	0.0008	-0.0046**	0.0011	0.0001	0.0018	-0.0027	-0.0009	-0.0020	0.0030	-0.0025
	(0.0022)	(0.0023)	(0.0021)	(0.0021)	(0.0020)	(0.0024)	(0.0022)	(0.0022)	(0.0021)	(0.0022)
Observations	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172
R-squared	0.15	0.15	0.15	0.15	0.15	0.15	0.15	0.15	0.15	0.15
Mean DV	39.2	39.2	39.2	39.2	39.2	39.2	39.2	39.2	39.2	39.2

Notes. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age, birth order, and child sex. Standard errors, two-way clustered by maternal birth state and maternal birth year, are in parentheses. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year by birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state (measured over the years preceding the 1963 introduction of the measles vaccine) interacted with maternal birth-year fixed effects to control for exposure to measles vaccination. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977. This table reports a placebo specification in which the state-specific legalization years are drawn at random from a normal distribution with the same mean and standard deviation as the actual policy distribution. Different appendix tables in this group report independent draws.
Second generation: Children whose mothers, born between 1955 and 1977, were exposed to the legalization of the contraceptive pill for minors.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table E-5 - Placebo Tests Using Fake Policy Sets Drawn from a Normal Distribution with the Same Mean and Standard Deviation as the Actual Policy Distribution

	Outcome: Preterm Birth, Iteration:									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A. Blacks										
Exposure	0.0005	-0.0008*	0.0001	-0.0003	-0.0005	0.0000	0.0004	0.0004	0.0002	0.0001
	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Observations	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351
R-squared	0.04	0.04	0.04	0.04	0.04	0.04	0.04	0.04	0.04	0.04
Mean DV	0.18	0.18	0.18	0.18	0.18	0.18	0.18	0.18	0.18	0.18
Panel B. Whites										
Exposure	-0.0002	-0.0000	-0.0001	0	0.0001	-0.0001	0.0002	-0.0002	0.0002	-0.0002
	(0.0002)	(0.0002)	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Observations	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172
R-squared	0.04	0.04	0.04	0.04	0.04	0.04	0.04	0.04	0.04	0.04
Mean DV	0.10	0.10	0.10	0.10	0.10	0.10	0.10	0.10	0.10	0.10

Notes. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age, birth order, and child sex. Standard errors, two-way clustered by maternal birth state and maternal birth year, are in parentheses. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year by birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state (measured over the years preceding the 1963 introduction of the measles vaccine) interacted with maternal birth-year fixed effects to control for exposure to measles vaccination. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977. This table reports a placebo specification in which the state-specific legalization years are drawn at random from a normal distribution with the same mean and standard deviation as the actual policy distribution. Different appendix tables in this group report independent draws.
Second generation: Children whose mothers, born between 1955 and 1977, were exposed to the legalization of the contraceptive pill for minors.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table E-6 - Placebo Tests Using Fake Policy Sets Drawn from a Normal Distribution with the Same Mean and Standard Deviation as the Actual Policy Distribution

	Outcome: Fetal Growth, Iteration:									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A. Blacks										
Exposure	0.01	0.02	0.01	-0.01	0.0015	0.02	0.02	0.01	-0.0042	-0.01
	(0.02)	(0.02)	(0.01)	(0.02)	(0.0145)	(0.02)	(0.02)	(0.01)	(0.0160)	(0.02)
Observations	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351
R-squared	0.10	0.10	0.10	0.10	0.10	0.10	0.10	0.10	0.10	0.10
Mean DV	80.5	80.5	80.5	80.5	80.5	80.5	80.5	80.5	80.5	80.5
Panel B. Whites										
Exposure	-0.01	0.0024	-0.0024	-0.0032	0.0091	-0.0034	0.0035	-0.01	0.0080	0.0040
	(0.0085)	(0.0083)	(0.0093)	(0.0079)	(0.0084)	(0.0081)	(0.0083)	(0.0086)	(0.0088)	(0.0086)
Observations	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172
R-squared	0.24	0.24	0.24	0.24	0.24	0.24	0.24	0.24	0.24	0.24
Mean DV	85.8	85.8	85.8	85.8	85.8	85.8	85.8	85.8	85.8	85.8

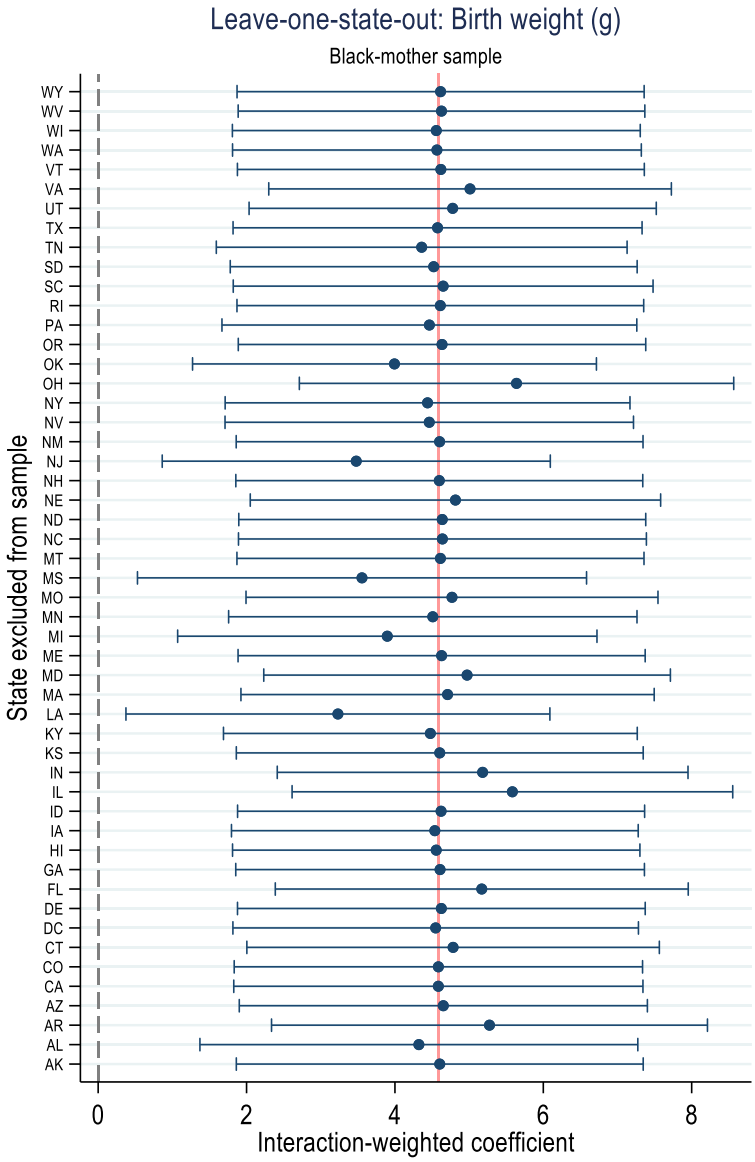
Notes. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age, birth order, and child sex. Standard errors, two-way clustered by maternal birth state and maternal birth year, are in parentheses. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year by birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state (measured over the years preceding the 1963 introduction of the measles vaccine) interacted with maternal birth-year fixed effects to control for exposure to measles vaccination. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977. This table reports a placebo specification in which the state-specific legalization years are drawn at random from a normal distribution with the same mean and standard deviation as the actual policy distribution. Different appendix tables in this group report independent draws.
Second generation: Children whose mothers, born between 1955 and 1977, were exposed to the legalization of the contraceptive pill for minors.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table E-7 - Placebo Tests Using Fake Policy Sets Drawn from a Normal Distribution with the Same Mean and Standard Deviation as the Actual Policy Distribution

	Outcome: Birth Weight Rank, Iteration:									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A. Blacks										
Exposure	0.0071**	-0.0011	-0.0039	-0.0045	0.0004	0.0018	-0.0021	-0.0016	-0.0042	0.0015
	(0.0028)	(0.0027)	(0.0030)	(0.0030)	(0.0031)	(0.0029)	(0.0029)	(0.0030)	(0.0029)	(0.0026)
Observations	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351	1,519,351
R-squared	0.16	0.16	0.16	0.16	0.16	0.16	0.16	0.16	0.16	0.16
Mean DV	4.7	4.7	4.7	4.7	4.7	4.7	4.7	4.7	4.7	4.7
Panel B. Whites										
Exposure	-0.0020	0.0008	0.0043**	-0.0011	-0.0001	0.0004	0.0004	-0.0017	-0.0030*	-0.0000
	(0.0019)	(0.0017)	(0.0018)	(0.0018)	(0.0018)	(0.0018)	(0.0018)	(0.0016)	(0.0017)	(0.0018)
Observations	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172	5,206,172
R-squared	0.29	0.29	0.29	0.29	0.29	0.29	0.29	0.29	0.29	0.29
Mean DV	5.7	5.7	5.7	5.7	5.7	5.7	5.7	5.7	5.7	5.7

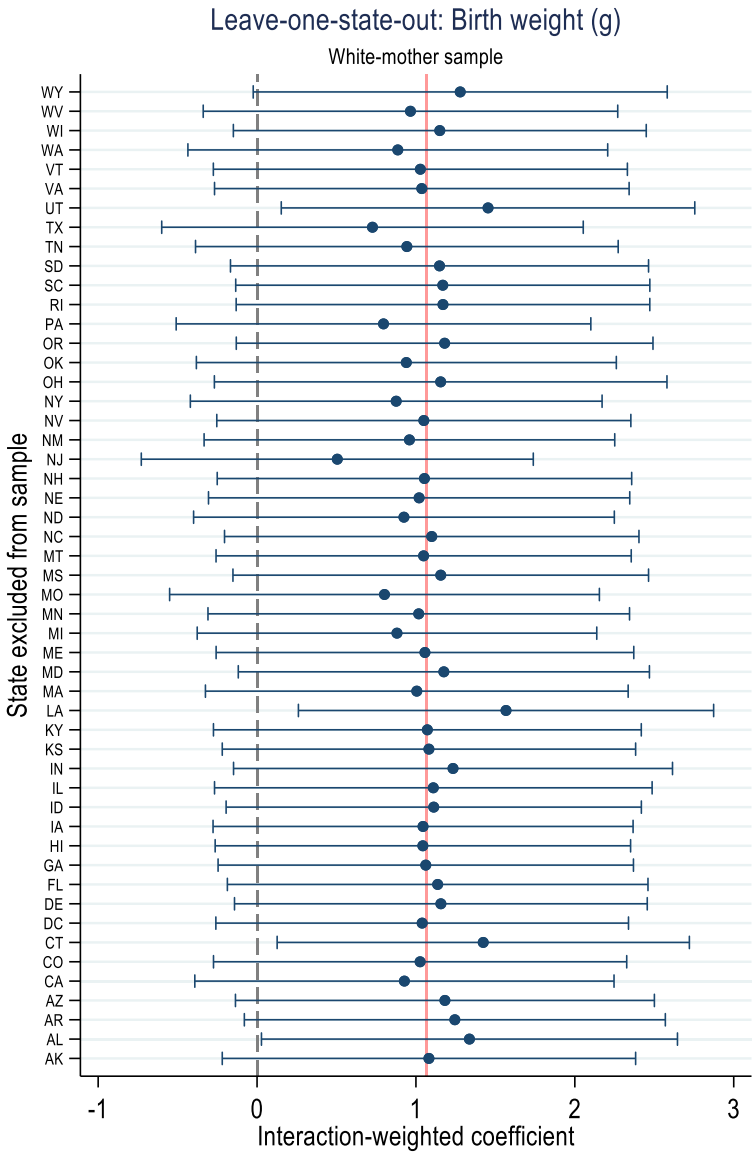
Notes. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age, birth order, and child sex. Standard errors, two-way clustered by maternal birth state and maternal birth year, are in parentheses. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year by birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state (measured over the years preceding the 1963 introduction of the measles vaccine) interacted with maternal birth-year fixed effects to control for exposure to measles vaccination. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977. This table reports a placebo specification in which the state-specific legalization years are drawn at random from a normal distribution with the same mean and standard deviation as the actual policy distribution. Different appendix tables in this group report independent draws.
Second generation: Children whose mothers, born between 1955 and 1977, were exposed to the legalization of the contraceptive pill for minors.
*** p<0.01, ** p<0.05, * p<0.1

Appendix F



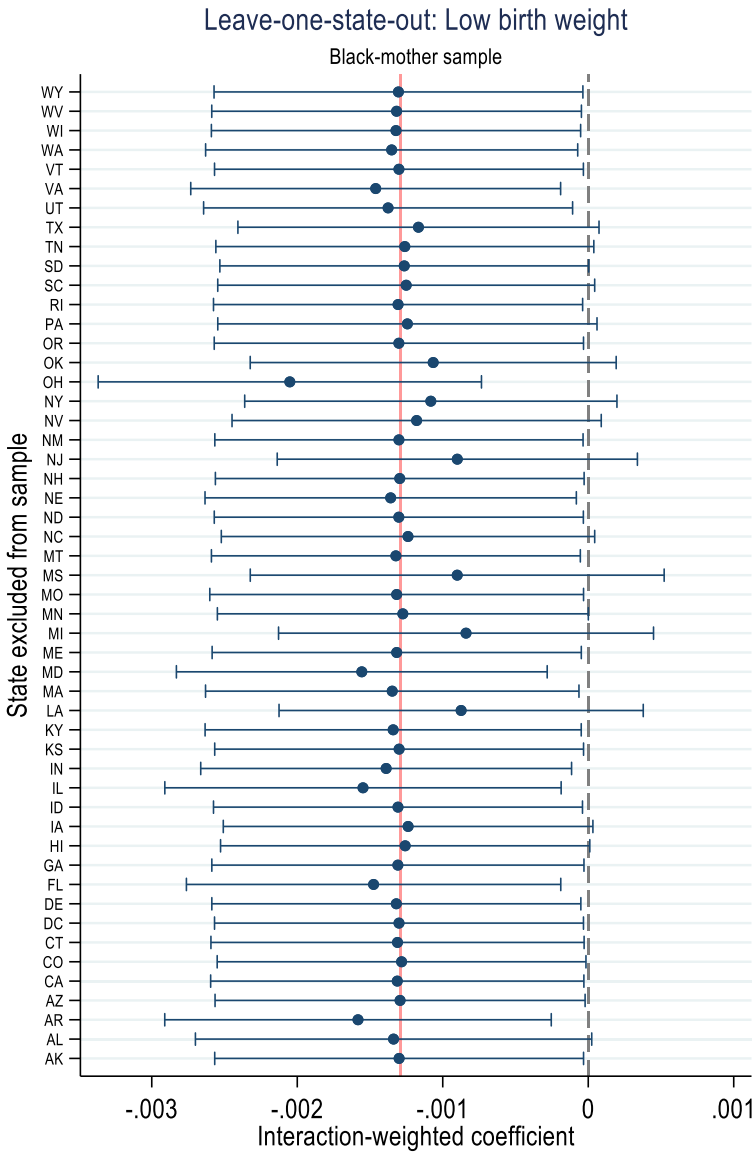
Appendix Figure F-1 - Leave-One-State-Out Robustness: Birth Weight among Children of Black Mothers

Notes. This figure reports leave-one-state-out estimates for the effect of maternal cohort exposure to minors' contraceptive pill access on second-generation birth weight among children of Black mothers. Each row excludes one maternal birth state from the estimation sample. Point estimates and 95 percent confidence intervals are shown. The vertical red line indicates the full-sample interaction-weighted estimate, while the gray dashed line marks zero. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age group, birth order, and child sex. Confidence intervals are constructed using standard errors two-way clustered by maternal birth state and maternal birth year. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year-by-birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state, measured over 1952–1963, interacted with maternal birth-year fixed effects. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977.



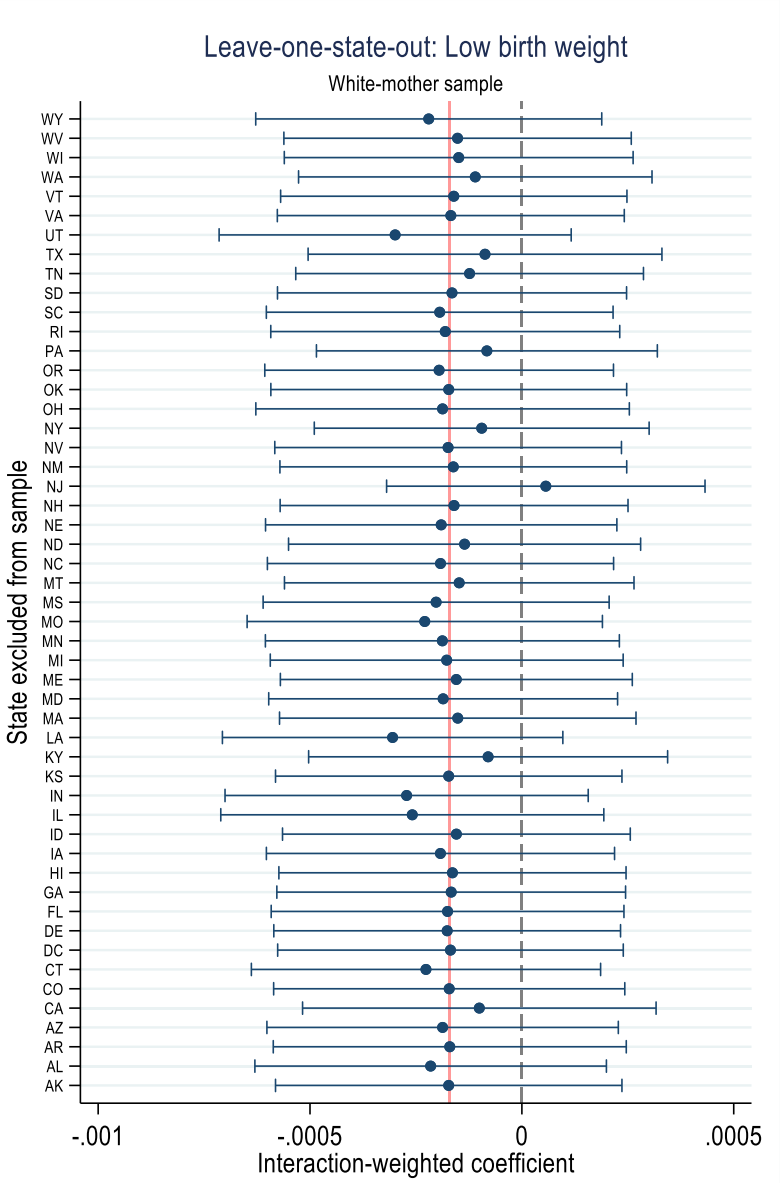
Appendix Figure F-2 - Leave-One-State-Out Robustness: Birth Weight among Children of White Mothers

Notes. This figure reports leave-one-state-out estimates for the effect of maternal cohort exposure to minors' contraceptive pill access on second-generation birth weight among children of White mothers. Each row excludes one maternal birth state from the estimation sample. Point estimates and 95 percent confidence intervals are shown. The vertical red line indicates the full-sample interaction-weighted estimate, while the gray dashed line marks zero. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age group, birth order, and child sex. Confidence intervals are constructed using standard errors two-way clustered by maternal birth state and maternal birth year. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year-by-birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state, measured over 1952–1963, interacted with maternal birth-year fixed effects. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977.



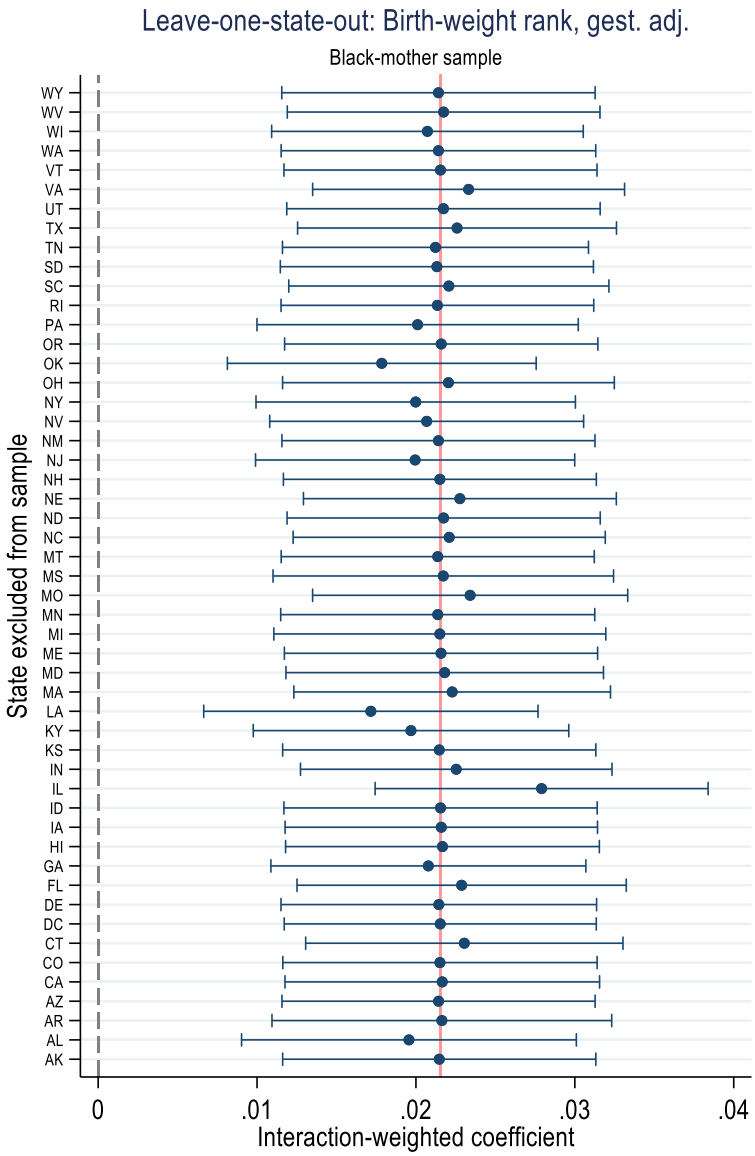
Appendix Figure F-3 - Leave-One-State-Out Robustness: Low Birth Weight among Children of Black Mothers

Notes. This figure reports leave-one-state-out estimates for the effect of maternal cohort exposure to minors' contraceptive pill access on second-generation low birth weight among children of Black mothers. Each row excludes one maternal birth state from the estimation sample. Point estimates and 95 percent confidence intervals are shown. The vertical red line indicates the full-sample interaction-weighted estimate, while the gray dashed line marks zero. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age group, birth order, and child sex. Confidence intervals are constructed using standard errors two-way clustered by maternal birth state and maternal birth year. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year-by-birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state, measured over 1952–1963, interacted with maternal birth-year fixed effects. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977.



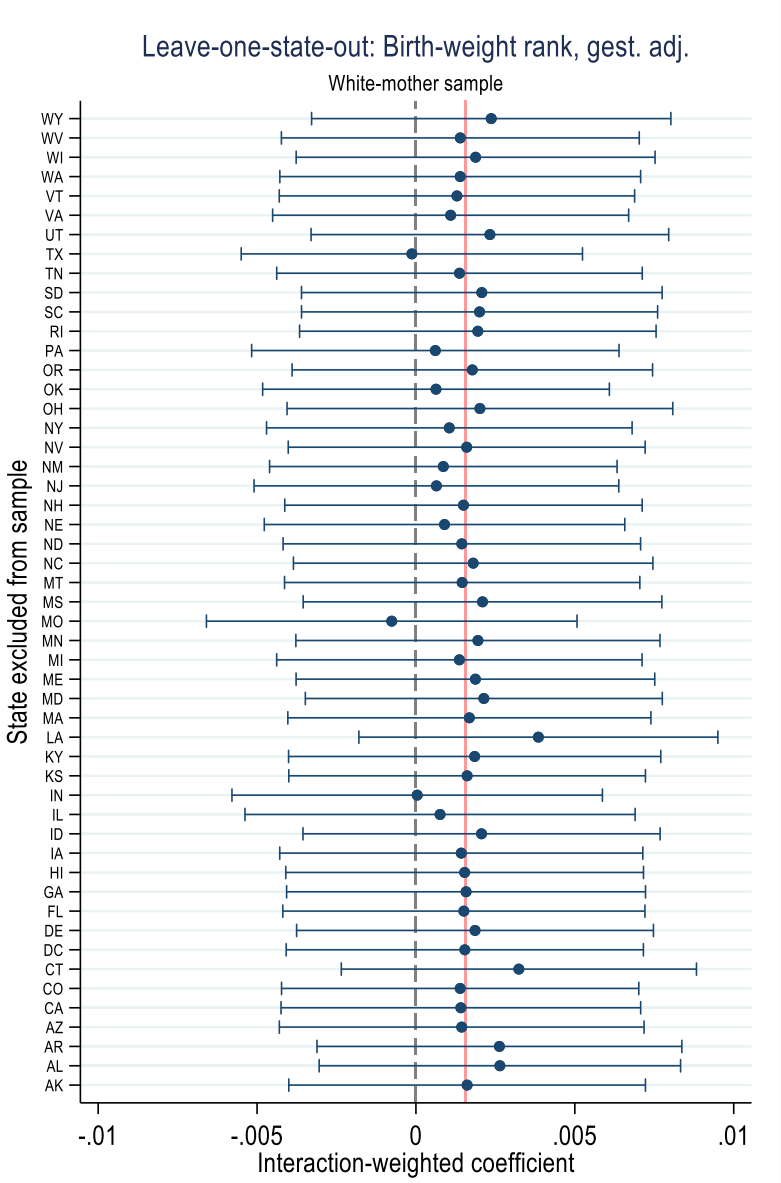
Appendix Figure F-4 - Leave-One-State-Out Robustness: Low Birth Weight among Children of White Mothers

Notes. This figure reports leave-one-state-out estimates for the effect of maternal cohort exposure to minors' contraceptive pill access on second-generation low birth weight among children of White mothers. Each row excludes one maternal birth state from the estimation sample. Point estimates and 95 percent confidence intervals are shown. The vertical red line indicates the full-sample interaction-weighted estimate, while the gray dashed line marks zero. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age group, birth order, and child sex. Confidence intervals are constructed using standard errors two-way clustered by maternal birth state and maternal birth year. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year-by-birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state, measured over 1952–1963, interacted with maternal birth-year fixed effects. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977.



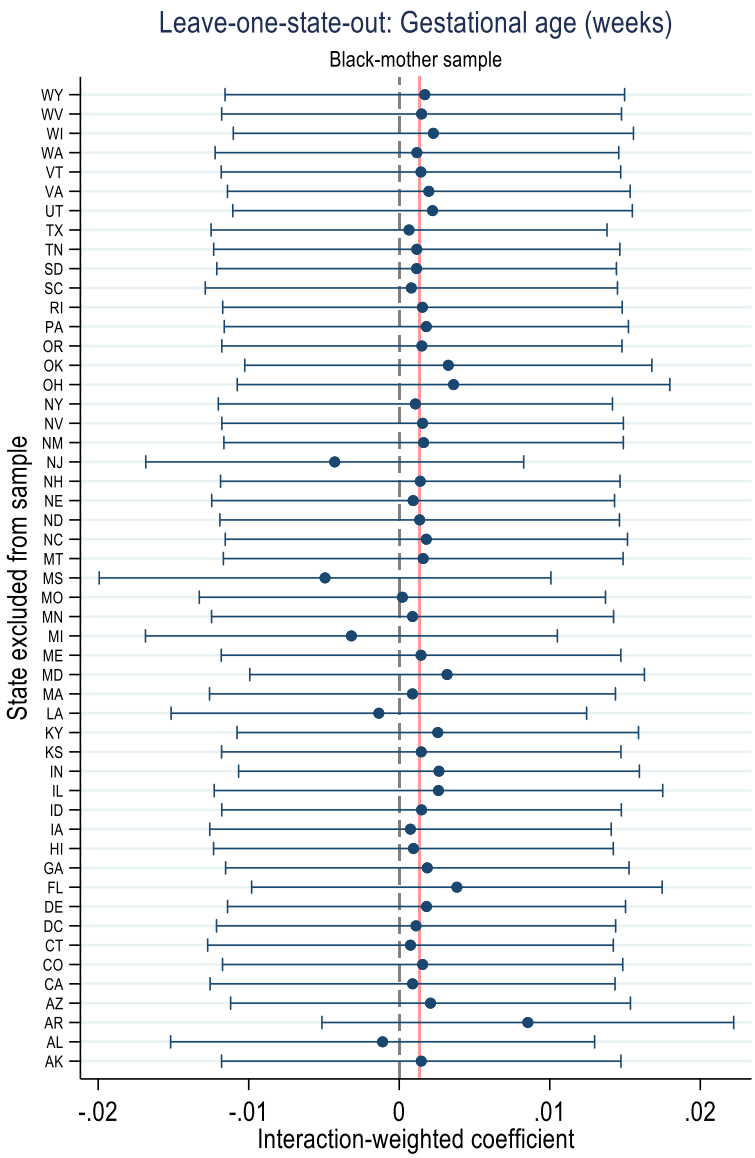
Appendix Figure F-5 - Leave-One-State-Out Robustness: Birth Weight Rank among Children of Black Mothers

Notes. This figure reports leave-one-state-out estimates for the effect of maternal cohort exposure to minors' contraceptive pill access on second-generation birth weight rank among children of Black mothers. Each row excludes one maternal birth state from the estimation sample. Point estimates and 95 percent confidence intervals are shown. The vertical red line indicates the full-sample interaction-weighted estimate, while the gray dashed line marks zero. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age group, birth order, and child sex. Confidence intervals are constructed using standard errors two-way clustered by maternal birth state and maternal birth year. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year-by-birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state, measured over 1952–1963, interacted with maternal birth-year fixed effects. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977.



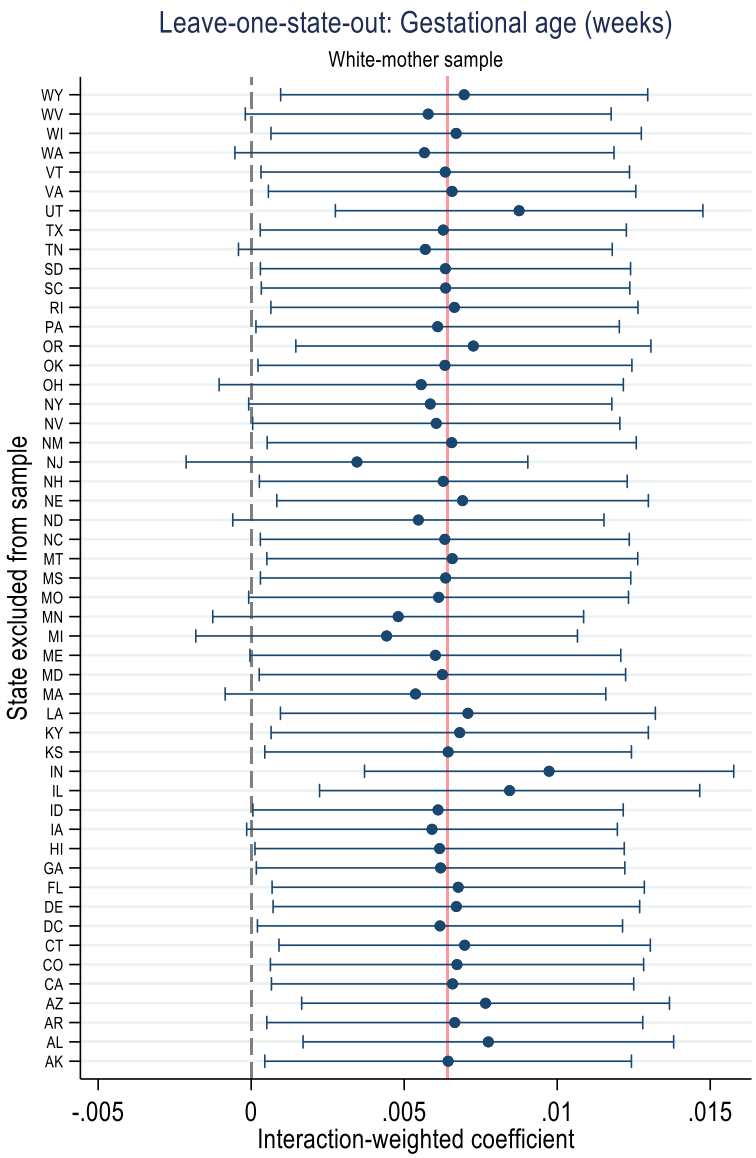
Appendix Figure F-6 - Leave-One-State-Out Robustness: Birth Weight Rank among Children of White Mothers

Notes. This figure reports leave-one-state-out estimates for the effect of maternal cohort exposure to minors' contraceptive pill access on second-generation birth weight rank among children of White mothers. Each row excludes one maternal birth state from the estimation sample. Point estimates and 95 percent confidence intervals are shown. The vertical red line indicates the full-sample interaction-weighted estimate, while the gray dashed line marks zero. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age group, birth order, and child sex. Confidence intervals are constructed using standard errors two-way clustered by maternal birth state and maternal birth year. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year-by-birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state, measured over 1952–1963, interacted with maternal birth-year fixed effects. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977.



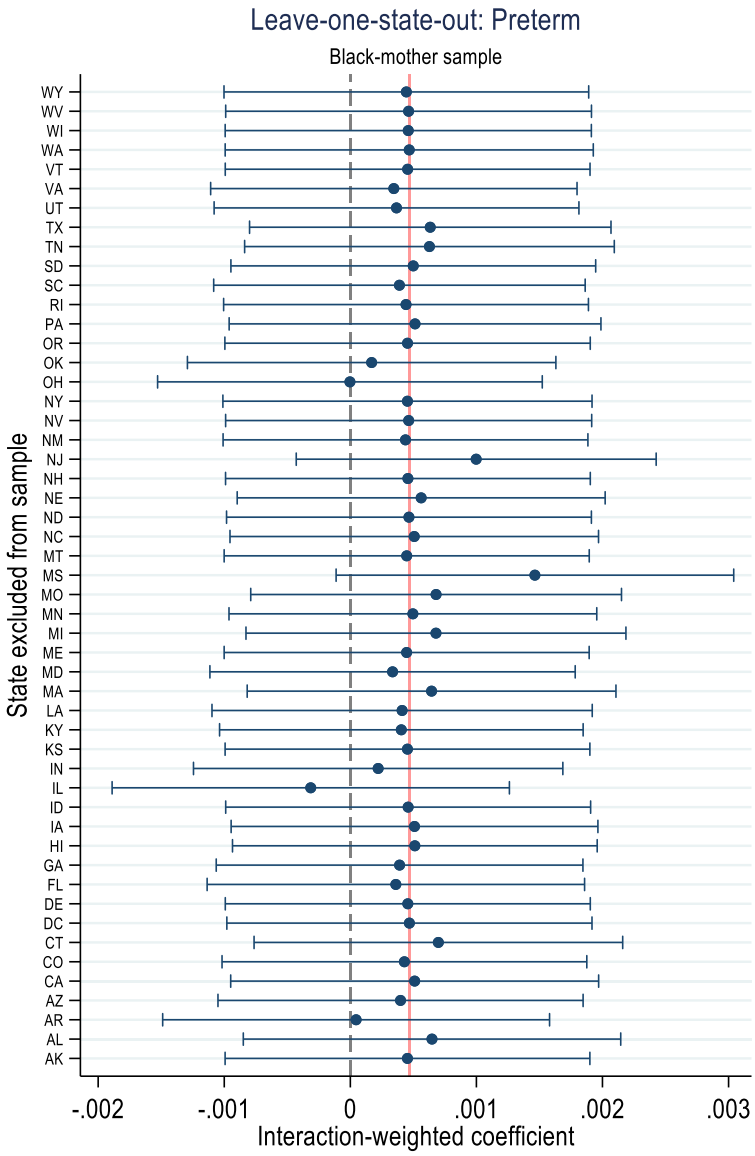
Appendix Figure F-7 - Leave-One-State-Out Robustness: Gestational Age among Children of Black Mothers

Notes. This figure reports leave-one-state-out estimates for the effect of maternal cohort exposure to minors' contraceptive pill access on second-generation gestational age among children of Black mothers. Each row excludes one maternal birth state from the estimation sample. Point estimates and 95 percent confidence intervals are shown. The vertical red line indicates the full-sample interaction-weighted estimate, while the gray dashed line marks zero. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age group, birth order, and child sex. Confidence intervals are constructed using standard errors two-way clustered by maternal birth state and maternal birth year. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year-by-birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state, measured over 1952–1963, interacted with maternal birth-year fixed effects. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977.



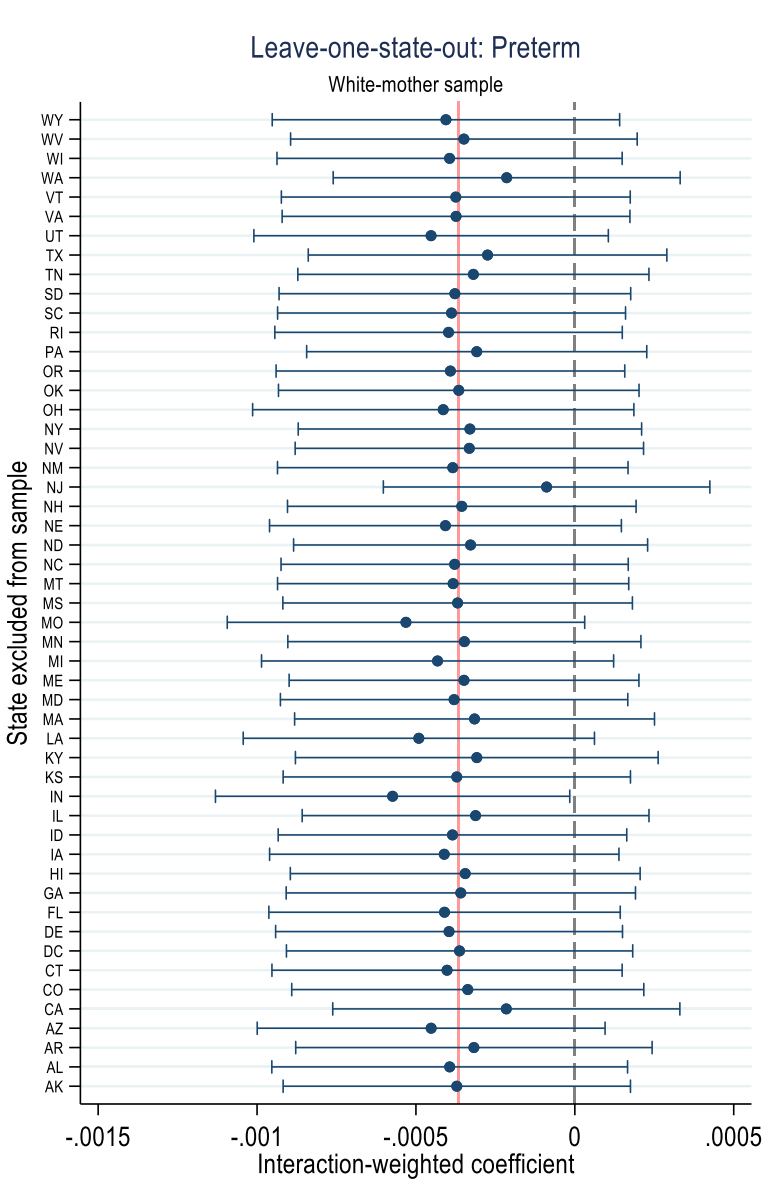
Appendix Figure F-8 - Leave-One-State-Out Robustness: Gestational Age among Children of White Mothers

Notes. This figure reports leave-one-state-out estimates for the effect of maternal cohort exposure to minors' contraceptive pill access on second-generation gestational age among children of White mothers. Each row excludes one maternal birth state from the estimation sample. Point estimates and 95 percent confidence intervals are shown. The vertical red line indicates the full-sample interaction-weighted estimate, while the gray dashed line marks zero. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age group, birth order, and child sex. Confidence intervals are constructed using standard errors two-way clustered by maternal birth state and maternal birth year. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year-by-birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state, measured over 1952–1963, interacted with maternal birth-year fixed effects. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977.



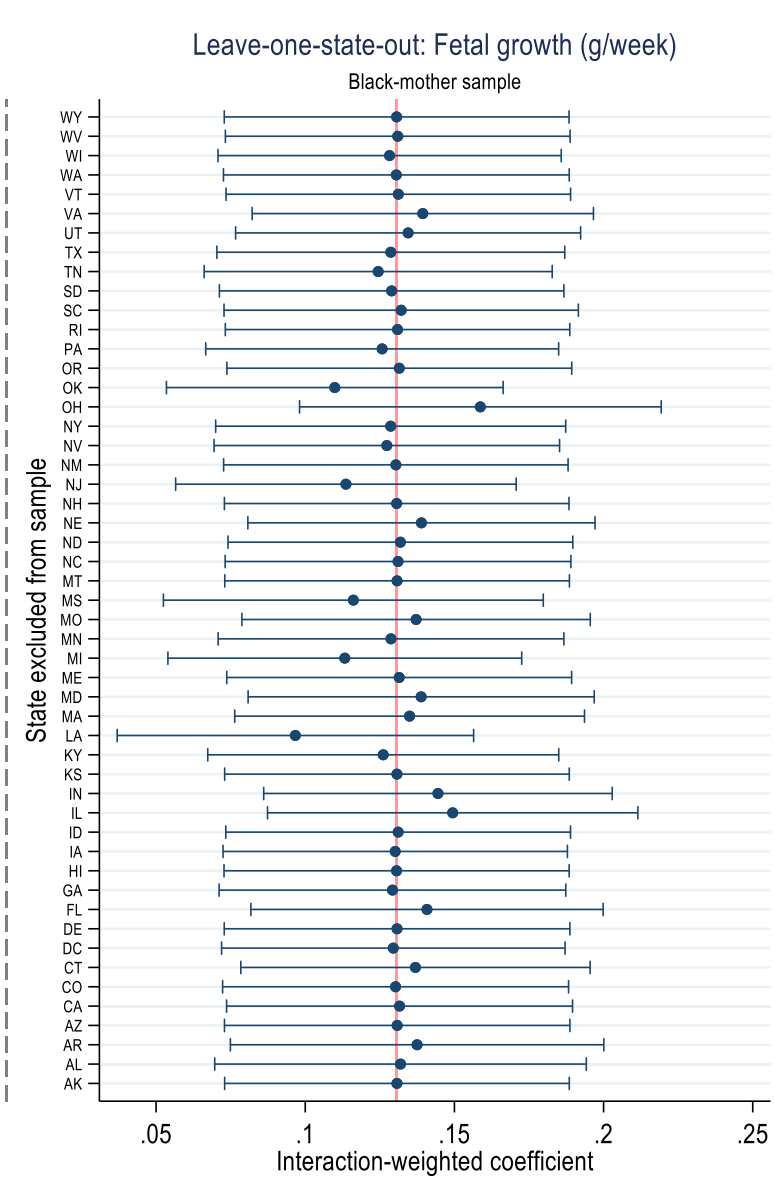
Appendix Figure F-9 - Leave-One-State-Out Robustness: Preterm Birth among Children of Black Mothers

Notes. This figure reports leave-one-state-out estimates for the effect of maternal cohort exposure to minors' contraceptive pill access on second-generation preterm birth among children of Black mothers. Each row excludes one maternal birth state from the estimation sample. Point estimates and 95 percent confidence intervals are shown. The vertical red line indicates the full-sample interaction-weighted estimate, while the gray dashed line marks zero. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age group, birth order, and child sex. Confidence intervals are constructed using standard errors two-way clustered by maternal birth state and maternal birth year. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year-by-birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state, measured over 1952–1963, interacted with maternal birth-year fixed effects. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977.



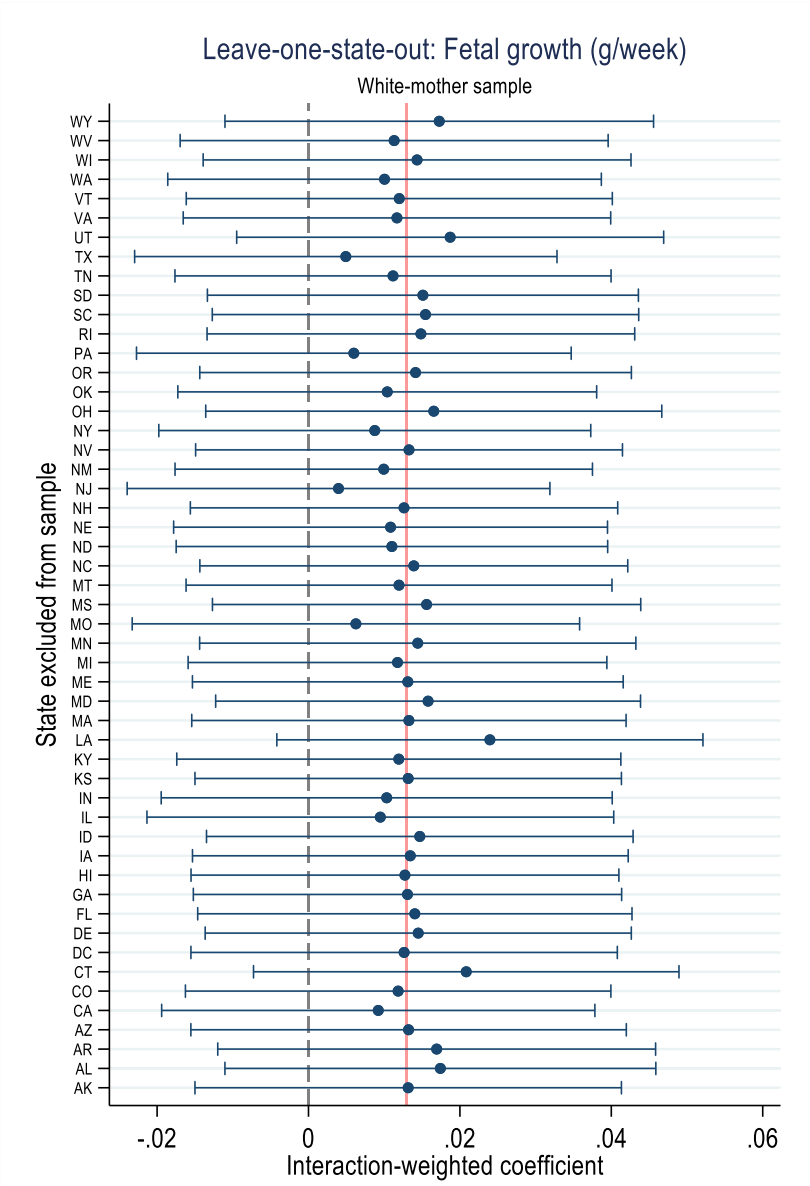
Appendix Figure F-10 - Leave-One-State-Out Robustness: Preterm Birth among Children of White Mothers

Notes. This figure reports leave-one-state-out estimates for the effect of maternal cohort exposure to minors' contraceptive pill access on second-generation preterm birth among children of White mothers. Each row excludes one maternal birth state from the estimation sample. Point estimates and 95 percent confidence intervals are shown. The vertical red line indicates the full-sample interaction-weighted estimate, while the gray dashed line marks zero. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age group, birth order, and child sex. Confidence intervals are constructed using standard errors two-way clustered by maternal birth state and maternal birth year. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year-by-birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state, measured over 1952–1963, interacted with maternal birth-year fixed effects. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977.



Appendix Figure F-11 - Leave-One-State-Out Robustness: Fetal Growth among Children of Black Mothers

Notes. This figure reports leave-one-state-out estimates for the effect of maternal cohort exposure to minors' contraceptive pill access on second-generation fetal growth among children of Black mothers. Each row excludes one maternal birth state from the estimation sample. Point estimates and 95 percent confidence intervals are shown. The vertical red line indicates the full-sample interaction-weighted estimate, while the gray dashed line marks zero. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age group, birth order, and child sex. Confidence intervals are constructed using standard errors two-way clustered by maternal birth state and maternal birth year. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year-by-birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state, measured over 1952–1963, interacted with maternal birth-year fixed effects. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977.



Appendix Figure F-12 - Leave-One-State-Out Robustness: Fetal Growth among Children of White Mothers

Notes. This figure reports leave-one-state-out estimates for the effect of maternal cohort exposure to minors' contraceptive pill access on second-generation fetal growth among children of White mothers. Each row excludes one maternal birth state from the estimation sample. Point estimates and 95 percent confidence intervals are shown. The vertical red line indicates the full-sample interaction-weighted estimate, while the gray dashed line marks zero. The unit of observation is a collapsed cell defined by maternal birth state, maternal birth year, child birth state, child birth year, maternal race, maternal education, maternal age group, birth order, and child sex. Confidence intervals are constructed using standard errors two-way clustered by maternal birth state and maternal birth year. Regressions are weighted by the number of births in each cell. All regressions include maternal birth-state fixed effects, maternal birth-year-by-birth-region fixed effects, and sociodemographic controls for maternal race, maternal education, maternal age, birth order, and child sex. Regressions also include maternal birth-state-by-birth-year controls for per capita income, hospitals per capita, hospital beds per capita, and reported disease rates, together with the 12-year average pre-vaccine measles rate in the maternal birth state, measured over 1952–1963, interacted with maternal birth-year fixed effects. Estimates use the interaction-weighted difference-in-differences estimator of Sun and Abraham (2021). The sample covers births from 1970 to 2020 to maternal cohorts born between 1955 and 1977.

Appendix G

Appendix Table G-1 - Effects on Fertility of Young Women Ages 15-21

	Outcomes:		
	Log Birthrate, Total	Log Birthrate, Whites	Log Birthrate, Non-Whites
	(1)	(2)	(3)
$I(year > State\ Pill\ Year) \times$ Age 15-21	-0.05***	-0.05***	-0.04***
	(0.02)	(0.02)	(0.0099)
Observations	7,606	3,803	3,803
R-Squared	0.95	0.98	0.98
Mean DV	4.0	3.9	4.7
	Log Birthrate, First Births, Total	Log Birthrate, First Births, Whites	Log Birthrate, First Births, Non-Whites
	(4)	(5)	(6)
$I(year > State\ Pill\ Year) \times$ Age 15-21	-0.05***	-0.05***	-0.04***
	(0.01)	(0.02)	(0.01)
Observations	7,560	3,779	3,779
R-Squared	0.92	0.98	0.97
Mean DV	3.6	3.6	4.1
	Fertility Count, Total	Fertility Count, Whites	Fertility Count, Non-Whites
	(7)	(8)	(9)
$I(year > State\ Pill\ Year) \times$ Age 15-21	243.8	272.8	-54.5*
	(149.0)	(170.4)	(29.3)
Observations	7,606	3,803	3,803
R-Squared	0.89	0.98	0.98
Mean DV	4121.9	4562.4	1558.3
	Fertility Count, First Births, Total	Fertility Count, First Births, Whites	Fertility Count, First Births, Non-Whites
	(10)	(11)	(12)
$I(year > State\ Pill\ Year) \times$ Age 15-21	-71.4	-72.8	-69.2***
	(68.1)	(78.7)	(14.9)
Observations	7,560	3,779	3,779
R-Squared	0.89	0.98	0.98
Mean DV	2691.3	3006.5	895.1

Notes. The unit of observation is a state-by-year-by-age cell. Standard errors, two-way clustered by state and year, are in parentheses. Regressions are weighted by the female population in each state-year-age cell. All regressions include state-by-year fixed effects, age fixed effects, and an indicator for maternal race. Exposure is an indicator for being in the age range 15-21, the group directly affected by minors' pill access, in a state-year that had extended legal access to the contraceptive pill to minors. Women aged 23-35 serve as the comparison group. The sample covers state-years 1968-1985. Data are from NCHS birth data and the Surveillance, Epidemiology, and End Results Program (SEER, 2019).

*** p<0.01, ** p<0.05, * p<0.1

Appendix Table G-2 - Effects on Fertility of Young Women Ages 15-21 Using DiD Method of the Main Results

	Outcomes:		
	Log Birthrate, Total	Log Birthrate, Whites	Log Birthrate, Non-Whites
	(1)	(2)	(3)
$I(year > State\ Pill\ Year) \times$ Age 15-21	-0.03	-0.03*	-0.04
	(0.02)	(0.02)	(0.03)
Observations	8,308	4,154	4,154
R-Squared	0.93	0.97	0.92
Mean DV	4.0	3.9	4.7
	Log Birthrate, First Births, Total	Log Birthrate, First Births, Whites	Log Birthrate, First Births, Non-Whites
	(4)	(5)	(6)
$I(year > State\ Pill\ Year) \times$ Age 15-21	0.0022	-0.0023	0.02
	(0.0261)	(0.0244)	(0.05)
Observations	7,570	3,785	3,785
R-Squared	0.84	0.92	0.58
Mean DV	3.6	3.6	4.1
	Fertility Count, Total	Fertility Count, Whites	Fertility Count, Non-Whites
	(7)	(8)	(9)
$I(year > State\ Pill\ Year) \times$ Age 15-21	-22.4	-9.1	39.6
	(171.0)	(185.4)	(59.7)
Observations	8,308	4,154	4,154
R-Squared	0.73	0.80	0.85
Mean DV	4097.1	4537.1	1560.9
	Fertility Count, First Births, Total	Fertility Count, First Births, Whites	Fertility Count, First Births, Non-Whites
	(10)	(11)	(12)
$I(year > State\ Pill\ Year) \times$ Age 15-21	12.5	24.8	50.0
	(145.8)	(158.2)	(43.9)
Observations	7,570	3,785	3,785
R-Squared	0.76	0.83	0.82
Mean DV	2690.8	3005.9	894.9

Notes. The unit of observation is a state-by-year-by-age cell. Standard errors, two-way clustered by state and year, are in parentheses. Regressions are weighted by the female population in each state-year-age cell. All regressions include state-by-year fixed effects, age fixed effects, and an indicator for maternal race. Exposure is defined as in the main specification: an indicator equal to one for women in the age range 15-21 born in or after the year their state extended legal access to the contraceptive pill to minors, with women aged 23-35 as the comparison group. The sample covers state-years 1968-1985. Data are from NCHS birth data and the Surveillance, Epidemiology, and End Results Program (SEER, 2019).
*** p<0.01, ** p<0.05, * p<0.1