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School Entry Policies, Maternal Education, and Pregnancy Outcomes: New Causal Evidence*

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Abstract

Despite growing research interest in school entry policies, their effects on newborn well-being remain understudied. This study provides new causal evidence using restricted birth data from three states, which are based on the latest 2003 revision of birth certificate instruments. Our normalized-and-pooled regression discontinuity analysis demonstrates young mothers born just after the school entry date cutoff have considerably lower educational attainment than those born just before. These women also tend to experience poorer birth outcomes, including a modest decrease in birth weight and a large increase in the risks of multiple adverse birth outcomes. Maternal health behaviors, insurance coverage, and paternal age are key channels for these intergenerational health effects. Overall, in states where educational systems fall behind the national average in terms of advancing academic progress and retaining students, we find additional education can yield significant health benefits for young mothers and their newborns.

Keywords School entry policies, Educational attainment, Infant health, Maternal health behaviors, Regression discontinuity design

JEL Classification I10, I21, J13

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1 Introduction

The last two decades saw a great deal of research and policy discussion on school entry age laws. The state legislation typically specifies a date on or before which children must turn 5 years old in order to begin kindergarten in public schools each fall. Otherwise similar children born just after that date are not eligible to enter kindergarten until the following school year. On one hand, the policy has the potential to reduce the years of schooling for those born right after their state's eligibility date. Entering school almost a year older than those born just before the cutoff date, the older entrants have fewer years of compulsory education when reaching the school dropout age and are at risk of legally leaving school for a longer duration of time (Cook and Kang, 2016; Arnold and Depew, 2018).[1] On the other hand, being born just after eligibility cutoff dates means that children start school at an older age (higher absolute maturity) and may be more ready for instruction. However, they are also older relative to their classmates (higher relative maturity), and as a result, could be negatively affected by younger, more disruptive peers.

Prior studies using data from the United States (US) have documented individuals with birthdays after eligibility cutoffs tend to have lower educational attainment than those with birthdays immediately prior (Cascio and Lewis, 2006; Dobkin and Ferreira, 2010; Tan, 2017). Nevertheless, older entrants are inclined to have better academic performance, although it is debated whether the age effect on test scores dissipates as children advance through school (Bedard and Dhuey, 2006; Dhuey, Figlio, Karbownik, and Roth, 2019).[2] Late entry also reduces risky health behaviors, rates of juvenile delinquency and crime, and the likelihood of being misdiagnosed as having learning disabilities (Cook and Kang, 2016; Depew and Eren, 2016; Elder, 2010; Evans, Morrill, and Parente,

[1] Consider an example in which the school entry date is August 30th, the school year begins in early September, and the school leaving age is 17. Students born right after the cutoff date will turn 6 just after they begin kindergarten and become eligible to leave school upon turning 17, near the beginning of their 12th school year (11th grade). Those born just before the school start date will have just turned 5 when beginning kindergarten and won't turn 17 until just before their 13th school year (12th grade) begins. Even if they don't leave school immediately upon turning 17, older starters will have an extra year of exposure to the option of dropping out than younger starters, when we consider their timeline toward high school graduation. This prolonged dropout exposure may also subject older starters to increased pressure to prioritize work, family responsibilities, or other non-academic pursuits, potentially undermining their longer-term educational trajectories. Furthermore, other factors such as biological age and minimum working age could influence the gap in educational attainment between older and younger starters, in a way similar to school leaving age (Dobkin and Ferreira, 2010; McCrary and Royer, 2011).

[2] This overall age effect is a composite of three impacts which are difficult to separately estimate: that of age at entry, age at test, and age relative to the peer group (Black, Devereux, and Salvanes, 2011; Cascio and Schanzenbach, 2016). The first two effects pertain to absolute maturity and the third concerns relative maturity.

2010; Johansen, 2021). Nevertheless, to these older starters, there is evidence of negative spillovers from younger peers regarding classroom performance and substance misuse (Elder and Lubotsky, 2009; Johansen, 2021). A growing literature looks at the longer term effects of school starting age. The findings are mixed on labor market outcomes such as earnings and employment (Arnold and Depew, 2018; Bedard and Dhuey, 2012; Black et al., 2011; Dobkin and Ferreira, 2010; Fredriksson and Ockert, 2014). Prior work also shows a positive health effect of higher school starting age for adult males (Arnold and Depew, 2018; Black et al., 2011). Moreover, raising the state minimum school starting age lowers crime activity later in life (McAdams, 2016).

Less attention has been given to the intergenerational effects of school entry policies. There is limited evidence on fertility. Several studies using European data report late school entry reduces teenage pregnancy and increases maternal age at first birth, but doesn't affect completed fertility (Black et al., 2011; Beck, Hart, and Flato, 2024; Borra, Gonzalez, and Patino, 2024). In contrast, US-based research shows no impact on teenage pregnancy or age at first birth (McCrary and Royer, 2011; Tan, 2017). Because many European countries have compulsory schooling laws tied to educational attainment and not age, older female entrants in these countries spend more time in school than those in the US, which implies a stronger incapacitation effect on teen pregnancy. Even less is known about newborn well-being. Two Europe-based studies which analyze women of childbearing age find being born after cutoff dates has a modest and negative effect on infant health in Finland and Spain, primarily due to delayed motherhood (Borra et al., 2024; Fredriksson, Huttunen, and Ockert, 2022).[3] To the best of our knowledge, McCrary and Royer (2011) is the only relevant causal analysis in the US context. Using restricted birth data from California and Texas, they find that being born after statewide school start dates lowers educational attainment for first-time young mothers but, somewhat surprisingly, has small overall effects on health of their offspring.

The small literature regarding newborn health is noteworthy, given that health at birth has profound and lasting impacts on health, human capital, labor market performance, and well-being over one's lifecycle (Almond, Currie, and Duque, 2018; Bharadwaj, Lundborg, and Rooth, 2018;

[3] There is no evidence of an impact on mother's educational attainment. But unlike the US, both countries require that individuals achieve a certain level of education, regardless of the age when they start schooling.

Helgertz and Nilsson, 2019). Of course, a notable barrier for new research on this issue is accessing US data with mother's date of birth. Furthermore, we do anticipate school entry policies to make a difference in birth outcomes through schooling and age-for-grade effects. For expectant mothers, education enhances their permanent income, enabling them to afford quality health insurance and receive appropriate health services especially prenatal care (Currie and Moretti, 2003; Heckman, Humphries, and Veramendi, 2018). Second, a woman's education is causally connected to the quality of her mate (Anderberg and Zhu, 2014). Third, education improves women's mental and physical health, facilitates the acquisition of health knowledge, and promotes healthy pregnancy behaviors (Cutler and Lleras-Muney, 2010; Eide and Showalter, 2011; Brunello, Fabbri, and Fort, 2013). Moreover, absolute and relative maturity manipulated by school entry policies also impacts a mother's choice of health inputs, through channels such as cognitive abilities, partner selection, and substance use (Dhuey et al., 2019; Johansen, 2021; Borra et al., 2024).

In this study, we provide new causal evidence on the effects of school entry policies on pregnancy outcomes. Among the three study states, Nevada and Tennessee require individuals to be five years old before or on September 30th to begin kindergarten for the mother cohorts we examine, while New Mexico sets the cutoff at August 31st (Evans et al., 2010; Bedard and Dhuey, 2012).[4] The corresponding birth data we access includes mother's exact date of birth, which permits implementation of a regression discontinuity (RD) design.

We follow McCrary and Royer (2011) to focus on young women for whom the education discontinuities induced by school entry policies are most relevant. After validating our RD design, we estimate the impact on education at motherhood using the pooled and state-level samples.[5]

[4] We observe fairly high compliance with the entry laws across the three states, although parents may delay their child's enrollment in public kindergarten (redshirting), petition for early enrollment for children born after school start dates, or choose private kindergarten to circumvent public school cutoff dates (Taveras, 2025). Using census data, we find about 73 to 78 percent of 5-year-old females in these states who were age eligible for kindergarten attended public kindergarten between 1980 and 2000. In addition, as long as these noncompliance behaviors influence later education attainment and infant health solely through child's actual start age and introduce no extra direct effects of birthdate relative to the eligibility cutoff on the outcomes, they won't bias our estimated intention-to-treat effect of school entry policies.

[5] As mentioned above, school entry policies affect educational attainment of young women whose academic progression decisions are age dependent. The education discontinuities also pertain to young women born near the cutoff date whose schooling is interrupted by pregnancy. In addition, the mother cohorts in Nevada (Tennessee) for our analysis were mostly exposed to a school leaving age of 18(17), whereas those in New Mexico were consistently subject to a leaving age of 18. The results change little when we restricting the sample to women who were uniformly exposed to a given leaving age.

Next, five birth outcome measures are examined to give a comprehensive picture of the influence of school entry policies on newborn health. We then conduct multiple inference adjustments and complement the continuity-based approach with local randomization estimation, which does not require approximation of smooth regression functions. Furthermore, we explore the mechanisms through which the entry policies affect education and health, carry out a number of robustness checks, and perform additional analyses such as estimation of the heterogeneous effects and power calculations. The rich evidence provided in this study will offer valuable insights for policymakers, educators, and parents who seek a deeper understanding of the infant health consequences of school entry laws.

2 Data

We use restricted birth data with mother's date of birth from Nevada, New Mexico, and Tennessee, provided by each state's Department of Health. For Nevada and New Mexico, we have data on the universe of all within-state births occurring in 2010-2016 and 2008-2015, respectively. For Tennessee, due to the high cost of accessing data, we obtained access to the subset of statewide birth records for 2004-2015 that meet our sample selection criteria.[6] Through the sampling periods above, all three states used the latest 2003 revision of the Standard Certificate of Live Birth, which improves data quality and includes additional information about mothers and newborns than the old 1989 version.

We examine three sets of dependent variables. First, we check whether there is an effect of mother's birthdate relative to the school start cutoff on education, as measured in her baby's birth record. The 2003 revised birth certificate instruments include eight categories for the education level of the parents.[7] We use these categories to construct three indicators on mother's educational attainment: less than high school education, high school completion or less, some college experience (without a degree) or less. The first indicator allows us to assess whether school entry policies affect completing high school education or equivalent for young mothers. The second one focuses on the

[6]These records cover first-time Tennessee mothers who are no more than 24 years old and born two months before and after the state's school entry cutoff date. More information about the final sample selection will be given below.
[7]The 1989 version records years of schooling completed.

margin of college enrollment. The third one pertains to the impact on earning an associate degree or higher.

Second, we analyze five primary birth outcomes: birth weight, low birth weight (LBW, birth weight $<$ 2,500 grams), preterm birth (or prematurity, gestational age $<$ 37 weeks), 5-minute Apgar score $<$ 7 (or low Apgar score), and cesarean birth (or caesarean section). Birth weight is the primary measure of a baby's health in most economic research on infant well-being. LBW and preterm birth are two key indicators of poor health at birth. The 5-minute Apgar score evaluates a baby's five cardinal signs at 5 minutes after birth, with each sign being scored as 0, 1, or 2. A 5-minute Apgar score of 7 or above is considered normal, while a score below 7 indicates that the baby needs additional medical care. A cesarean birth is generally more costly than a vaginal birth, especially when there are complications during pregnancy for mothers or fetuses. Some cesarean sections can be prevented by enhancing maternal health status and behaviors.

Third, we look at a number of variables that give information about potential mechanisms. The first group measures maternal health and health behaviors. Three of these variables concern the preconception period: pre-pregnancy obesity (body mass index or BMI $\geq$ 30), overweight (BMI $\geq$ 25), and smoking. The other measures we consider include early prenatal care (care initiation in the first trimester), number of prenatal care visits, prenatal smoking (smoking in any trimester), and inadequate gestational weight gain. Both the timing and frequency of prenatal care visits are important for infant health production (Yan, 2020). The second group pertains to the income channel, which comprises three measures on mother's insurance for delivery payment and receipt of benefits from the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC). The third group includes indicators of father's education and age, which provides insights into how school entry policies influence mate selection and quality.

We begin our sample selection by, first, focusing on first-time mothers who have no history of pregnancy termination and who deliver singletons. Without this restriction, the effect of birth outcomes of the first pregnancy on future fertility and health of subsequent births would impact our estimates. Second, as we proxy the state where mothers begin their education by state of birth, our sample consists exclusively of mothers who were born in and currently reside in the same state where they give birth. Moreover, our primary sample is limited to mothers who are no older than 23 years, as the literature shows education discontinuities induced by the school entry policies are most relevant to youths whose academic progression decisions are age dependent (Dobkin and Ferreira, 2010; McCrary and Royer, 2011; Tan, 2017).[8] Finally, for the baseline estimation, we restrict the sample to mothers born 40 days around the first day after school start dates (more discussion below).

With the resulting pooled and state-level samples of first-time native young mothers, we report the means of the key variables in Table 1. The majority of the young women and their mates have completed secondary education, while a moderate percentage have college experience. There is a relatively high incidence of LBW and prematurity in all the states. Differences in maternal smoking rates are evident when we compare Tennessee to the other two states. Moreover, more than 70% of the mothers in the pooled sample are Medicaid or WIC beneficiaries.[9] In terms of race and ethnicity, Native American mothers who identify as American Indian or Alaska Native and are not Hispanic comprise 18% of the New Mexico sample. About 35% and 62% of the sampled mothers are Hispanic in Nevada and New Mexico, respectively. Non-Hispanic black mothers constitute the largest minority group in Tennessee. Finally, we note that mothers in Tennessee make up approximately 77% of the pooled sample. Appendix Table A1 further provides the means for the samples with a longer 60-day window on either side. The pattern is very similar.

[Insert Table 1 Here]

3 Method

We begin by considering the following model to compare mothers born near school start dates:

$$Y_i = \alpha_0 + \alpha_1 T_i + \alpha_2 B_i + \alpha_3 T_i B_i + \varepsilon_i \tag{1}$$

where Y_i is a measure of education at motherhood or a birth outcome for mother i. We normalize mother's day of birth using the eligibility cutoff, which gives the running variable B_i. It equals

[8] We see this pattern that the negative effect on education around the cutoff is stronger for young mothers than for old mothers, using the Nevada and New Mexico datasets which allow us to include older mothers (results not shown).

[9] About 3% of the mothers in the Tennessee sample have missing information on Medicaid and private insurance. But the corresponding fractions are much higher in Nevada (9%) and New Mexico (24%).

0 for mothers born on the first day after the cutoff date, -1 for mothers born on the cutoff date itself, 1 for mothers born on the second day after the cutoff, and so on. $T_i = \mathbf{1}(B_i \geq 0)$ indicates being born after the state's entry cutoff date for the mother. The parameter of interest is α_1 which captures the discrete change in an outcome variable due to the school entry policies.[10]

Eq.1 is estimated by local linear regression, which solves the following optimization:

$$min \sum_{i=1}^{N} (Y_i - \alpha_0 - \alpha_1 T_i - \alpha_2 B_i - \alpha_3 T_i B_i)^2 K_h(B_i) \tag{2}$$

where $K_h(B_i)$ is a kernel function that assigns non-negative weights to mothers born within a h-day window around $B_i = 0$. Our main estimates use the triangular kernel function, but there are empirical exercises with other kernel functions as well. Moreover, while the baseline analysis employs a 40-day bandwidth (h=40 and B_i ranges from -40 to 40), we also examine how sensitive the results are to a range of other bandwidths. The data we have access to for Nevada and New Mexico can accommodate a bandwidth of up to 182 days. Accordingly, for those two states, we also perform additional analyses with Mean Square Error (MSE) optimal bandwidths.[11] In addition, we pool observations of the three states in the main analysis to improve estimation precision. The parameter α_1 then gives a normalized-and-pooled RD effect, which is equal to a weighted average of the state-specific RD effects (Cattaneo, Idrobo, and Titiunik, 2024). The results on the state-specific RD effects will be presented, too.

With respect to birth outcomes, our focus on the reduced form specification of Eq.1 departs from McCrary and Royer (2011) which uses T_i to instrument for mother's education. The instrumental variable strategy assumes changes in pregnancy outcomes of the affected mothers are entirely attributable to the pure schooling effect of the entry policies. While education is likely the main driver for the impacts on birth outcomes, both absolute and relative maturity, as mentioned above, may also influence infant health. The reduced form RD model we opt for imposes no restriction

[10] T_i can be viewed as the treatment assignment. Depew and Eren (2016) use T_i as an instrumental variable for late school entry to estimate the effect of treatment received (or the local average treatment effect). In our context, lacking data on actual entrance age prevents us from estimating this effect for young mothers. We estimate the intention-to-treat effect as represented by α_1, which is most policy relevant. Moreover, employing T_i as an instrumental variable for mother's education instead raises concerns, as discussed below.

[11] As mentioned earlier, the Tennessee data we have spans only a 60-day window on either side of the cutoff date.

on how the entry policies work. In addition, the model will be useful when we investigate mother's health and health behaviors, access to health care and nutrition, and father's characteristics.

It is straightforward to employ other model specifications. Consider a generalized model of the form:

$$Y_i = \alpha_0 + \alpha_1 T_i + \alpha_2 F(B_i) + \alpha_3 T_i F(B_i) + \alpha_4 X_i + \upsilon_i \tag{3}$$

where $F(B_i)$ is a polynomial function of B_i and X_i is a vector of predetermined covariates. In some exercises, we additionally control for background characteristics X_i, such as indicators of mother's cohort and race/ethnicity. Similar to previous studies (Black et al., 2011; McCrary and Royer, 2011), each cohort of mothers is defined symmetrically about $B_i = 0$.[12] Of course, we test smoothness of these covariates before adding them to the model. Some other robustness checks incorporate a quadratic form for $F(B_i)$ in the specification, with or without additional controls. Specifications with higher-order polynomials are less preferred, as these models tend to yield less reliable results near boundary points (Cattaneo, Idrobo, and Titiunik, 2019).

We note the continuity-based approach above is based on approximation and extrapolation of smooth regression functions. The smoothing bias introduced by extrapolation will be negligible only with large sample size. For this study, it may be subject to debate whether there are sufficient mass points for the discrete running variable B_i about the cutoff. However, we also consider the local randomization approach which does not require approximation of unknown functions. With this framework, our RD design is regarded as a randomized experiment in a small window of the cutoff $\mathcal{W}$, in which the potential outcomes are additionally assumed to be unaffected by the running variable (Cattaneo et al., 2024). We implement a window selector which assesses covariate balance in a sequence of nested windows.[13] The following simple model is then estimated within $\mathcal{W}$:

$$Y_i = \beta_0 + \beta_1 T_i + \nu_i \tag{4}$$

[12] For instance, all the mothers born within a window of 182 days around the date when $B_i = 0$ in year 1991 belong to the 1991 cohort.

[13] Here, we search for the largest window around the eligibility cutoff where women assigned to treatment and control are comparable in terms of predetermined characteristics. Covariate balance is then consistent with randomization of treatment assignment within the chosen $\mathcal{W}$, which allows us to estimate the effect by comparing the average observed outcomes between the treatment and control groups. In contrast, for our continuity-based RD analysis, testing covariate balance provides insights into whether smoothness of regression functions holds and whether observed discontinuities in the outcome variables could be driven by factors other than school entry policies.

where $T_i = \mathbf{1}(B_i \geq 0)$ as in Eq.1. We will compare the results on β_1 with those on α_1 from Eq.1. Finally, for the outcome measures of maternal education and infant health which this study focuses on, we will calculate Romano-Wolf (RW) step-down p-values to adjust for multiple inference (Romano and Wolf, 2016). The RW correction not only controls family-wise error rate (FWER) but also offers more power than other corrections, because it takes into account dependence among unadjusted p-values by boostrap resampling (Romano and Wolf, 2005).[14]

4 Results

4.1 Validity of the RD design

Before we carry out the formal RD analysis, it is important to examine the distribution of birthdays of the first-time young mothers. Appendix Figure A1 plots density of their birthdays around the state-specific school entry cutoff or pooled cutoff. We don't observe any non-random sorting of birthdays near the cutoff within a 60-day window on either side. The pattern is unchanged for Nevada and New Mexico when the plots extend to a longer window of ± 90 days.[15] Additional local polynomial density tests also confirm no manipulation of the timing of these young mothers' own births (suppressed for brevity).[16]

We also conduct balance tests on predetermined characteristics of the young mothers. As most of the characteristics observed in the birth records could be considered as a response to the treatment assignment regarding school entry, a limited set of variables are available for this exercise. We consider indicators of mother's race/ethnicity and cohort. To conserve space, we present only the pooled discontinuity estimates with 90% confidence intervals in Appendix Figure A2. We see little evidence of discontinuity in the background characteristics, using triangle kernel weights and a 40-day bandwidth. The only exception is that the estimates for 1996 and 1997 cohorts are marginally

[14] Suppose we have a family of L null hypotheses, of which M are true ($M \leq L$). FWER is the probability that at least one of the M true null hypotheses in the family is rejected.

[15] For ease of illustration, we center each histogram on the cutoff date, which is represented as zero on the horizontal axis. The corresponding distribution variable is the difference in days between mother's birthday and the cutoff date. This variable "days from cutoff " is slight modification of B_i from Eq.1, since it equals $B_i + 1$.

[16] Dickert-Conlin and Elder (2010) also suggest US parents do not appear to systematically time childbirth, analyzing birth records from all the states. Furthermore, birth timing manipulation can lead to covariance imbalance, which is however ruled out by the smoothness test below.

significant at the 10% level. Moreover, none of the estimates are significant when we alternatively apply uniform kernel weights and a bandwidth of 50 days.

Finally, our decision to limit the sample to young mothers could shut down interesting channels, especially the timing of fertility. However, we find that school entry policies do not appear to affect age at motherhood. Appendix Table A2 shows the results of examining the age of first-time mothers in Nevada and New Mexico, when expanding the sample to include mothers of all ages.[17] The estimates are small, insignificant, and largely unaffected by the chosen kernel function or bandwidth.

4.2 Main results

4.2.1 Maternal education

Figure 1 provides a graphical display of the relationship between education at motherhood and normalized birthday, using the pooled sample described in Table 1. To reduce noise in the data, unconditional means of the education variables in 4-day bins are calculated and presented.[18] The fitting lines come from local linear regression interpolation of Eq.1. A 40-day bandwidth and the unbinned raw data are used for the regression. At the eligibility cutoff, we see a pronounced increase in the fraction of young mothers who do not complete high school and those who have high school or less education. There is also visual evidence of a discontinuity in the rate of having some college or less. Recall, there are competing mechanisms that could affect years of schooling at the threshold. Individuals born just after the cutoff experience prolonged dropout exposure, which allows them legally leave school before completing high school or, more broadly, influences their overall educational trajectories. In addition, for these older starters, the net age-for-grade impact is likely to be positive on grade progression.[19] Our results suggest that for the young mothers, the ability to leave school with fewer completed years dominates the age-for-grade effect on educational attainment.

[17]Tennessee is excluded from this analysis, since our data for this state only includes information on mothers up to age 24.

[18]Appendix Figure A3 displays daily averages of educational attainment, rather than 4-day bins.

[19]In terms of academic performance, the literature shows the advantage of absolute maturity for the older starters more than offsets the downside of having relatively younger peers (Elder and Lubotsky, 2009; Cascio and Schanzenbach, 2016).

[Insert Figure 1 Here]

Column (1) in panel A of Table 2 presents the corresponding discontinuity point estimates, which are also our baseline results. We find that being born after the cutoff elevates the probability of an unfinished high school education by 6.4 percentage points, or about 21% relative to the sample mean. The likelihood of having a high school diploma or less is estimated to increase by 3.4 percentage points (about 5%), and the probability of having at most some college experience rises by 1.3 percentage points (1.4%). All three estimates are statistically significant at the 1 percent level. In columns (2) and (3), we consider alternative bandwidths of 30 and 50 days. The results are similar to our baseline findings.

[Insert Table 2 Here]

The other panels of Table 2 report the education estimates by state. Focusing on column (1), we see a notable rise in high school non-completion at each state's school entry cutoff. The estimated effects are also remarkably consistent across states, in terms of magnitude (20-21%). For the other two education measures, the discontinuity estimates are small and insignificant for Nevada and New Mexico. In contrast, young mothers born right after the cutoff date in Tennessee experience a statistically significant increase of 4.2 percentage points of having no more than a high school diploma or equivalent. Similarly, the likelihood of having at most some college education increases by 1.5 percentage points, and this estimate is also statistically significant at the 1% level. The results are again insensitive to bandwidth choice (columns 2 and 3). Moreover, in theory, a state with more data observations around the cutoff is assigned a greater weight in the estimation of the normalized-and-pooled effects in panel A. That is why the Tennessee estimates have a larger influence on the pooled results. Finally, Figures A5, A7, and A9 provide a graphical treatment of the state-specific estimates from column (1) of Table 2.

McCrary and Royer (2011) find being born after the state's school start date increases the probability of not finishing high school by 4 to 6 percentage points, for first-time young mothers in California and Texas. Our baseline pooled and state-level estimates in Table 2 are above the higher end of their range. Moreover, for having a high school diploma or less, their estimate of about 2 percentage points in both California and Texas is less than our Tennessee and pooled estimates.[20] State-level differences in educational systems, socioeconomic conditions, and demographics can contribute to the discrepancy. For instance, our three states generally face considerable challenges with school funding, early intervention programs, and access to support services, which likely hinders student academic achievement and retention. As a result, women born after the cutoff date in these states may be more sensitive to the possibility of leaving school than their counterparts in California and Texas, leading to larger education discontinuities. Our findings are also comparable with Cook and Kang (2016) and Tan (2017), which document discontinuities on the order of 3 to 6 percentage points in attending the 12th grade.

4.2.2 Birth outcomes

Figure 2 demonstrates the profile of the newborn health outcomes by normalized birthday with 4-day bins, as with Figure 1 for education.[21] For nearly every outcome, there is obvious visual evidence of a discontinuity at the cutoff. Although the size of these discrete changes varies, they overall suggest that young women born just after the eligibility cutoff tend to have poorer birth outcomes. The corresponding estimates are presented in column (1) of Table 3 (Panel A). We see a statistically significant reduction of 28.3 grams (g) on birth weight and a 1-percentage-point increase in LBW (11%). There is also a precisely estimated rise in the risk of preterm birth (1.3 percentage point, or 14%) and cesarean birth (1.3 percentage points, or 5%). The increase in the likelihood of having a low-Apgar-score baby is small and statistically insignificant. The other results in column (1) come from estimation by state and the corresponding graphs are shown in Figures A6, A8, and A10. The pattern in Nevada is broadly consistent with the baseline results from the pooled sample; but with the limited sample size, the estimates are all imprecise. In New Mexico, we see some suggestive evidence of worse birth outcomes, except that the discontinuity is very small for birth weight and the estimate for low Apgar score is of the opposite of the expected sign. Moreover, none of the point estimates are statistically significant. Turning to Tennessee, there is evidence of a precisely estimated adverse effect on birth weight (-33.5 g), preterm birth

[20] The pattern remains unaltered when we account for the baseline difference in young mother's educational attainment between their study states and ours.

[21] Appendix Figure A4 displays daily averages of the birth outcomes.

(1.2 percentage points), and low Apgar score (1 percentage point). With a different bandwidth for the pooled and state-level analysis, the results in the other columns are generally similar to those in column (1).

[Insert Figure 2 Here]

[Insert Table 3 Here]

Our infant health estimates are larger than McCrary and Royer (2011)'s. They find very small effects of school entry laws on LBW and prematurity. One possible explanation for our larger intergenerational estimates is the more sizable change in maternal education reported above. Moreover, in the states McCrary and Royer (2011) focus on, California and Texas, there is also suggestive evidence that being young for grade considerably hinders skill acquisition for individuals born just before the cutoff date (Dobkin and Ferreira, 2010). To the extent that this negative age-for-grade effect endures throughout adolescence and into adulthood, it may offset the pure schooling impact on newborn health, resulting in a relatively small overall effect of school entry policies. In our three study states, the net effect of absolute and relative maturity may not counteract the schooling impact as strongly. In addition, the 95% confidence intervals of our pooled LBW and prematurity estimates do not rule out a small effect. It is worth noting the magnitude of our estimated effects on birth outcomes is generally comparable to those in Currie and Moretti (2003) which uses college openings to instrument for education and in Noghanibehambari, Salari, and Tavassoli (2022) which exploits variation in maternal education due to changes in minimum dropout age policies.

4.2.3 Multiple inference adjustments and local randomization estimation

Panel A of Table 4 reproduces the benchmark pooled estimates on education at motherhood and birth outcomes, and also reports the unadjusted p-values. The Romano-Wolf correction is then implemented with 1,000 bootstrap replications. According to the RW p-values in panel A, all the original precise estimates retain significance. Panel B shows the results from local randomization estimation. As the window selector recommends a 10-day window for this analysis, the sample size is much smaller. In this sample, women whose normalized birthdays are among the five integers between -5 and -1 are placed in the control group, while those whose normalized birthdays correspond to one of the five integers between 0 and 4 are in the treatment group.[22] Moreover, heteroskedasticity-robust standard errors are preferable to clustered standard errors, as the sampled mothers are randomly drawn from a large population within the 10-day window and random assignment takes place at the individual level (Abadie, Athey, Imbens, and Wooldridge, 2023).[23] The local randomization results are line with those in panel A, although we generally observe larger unadjusted p-values (except for LBW and prematurity). The estimates for some college or less and cesarean delivery are now imprecise. The RW correction results in even larger p-values. Still, we see significant impacts on high school non-completion, high school graduation or less, and prematurity.

[Insert Table 4 Here]

4.3 Potential mechanisms

In Table 5, we investigate the potential mechanisms by which school entry policies affect infant health, using the pooled sample in Table 1. Looking at the estimates based on a 40-day bandwidth in column (1), we see young mothers born after the cutoff are significantly more likely to be obese before pregnancy and to have inadequate gestational weight gain.[24] There is also clear evidence of a decline in the probability of early prenatal care and the number of prenatal care visits. While these estimates on prenatal care are precise, they are modest in magnitude. We see no difference in pre-pregnancy smoking at the school start cutoff. The estimate for prenatal smoking has the expected sign, but it is imprecise. Turning to the results on access to health care and nutrition, we find being born after the cutoff increases the likelihood of Medicaid coverage but lowers the chance of having private insurance. The discontinuity is small for receipt of WIC benefits. When examining father's characteristics, we find the estimated effects are small and statistically insignificant for paternal education. However, there is a statistically significant decrease in father's age. Moreover,

[22] We use a subset of the available indicators on mother's race/ethnicity and cohort for window search, and then apply a different subset for balance check which validates the chosen window. The results are available upon request.

[23] Intuitively, the sampled women are randomly assigned one of the ten values of the running variable, and accordingly, they are placed into the control or treatment group.

[24] For simplicity, we code a mother as having inadequate weight gain if she gains less than 8.5 kilograms during pregnancy. The cutoff of 8.5 kilograms roughly corresponds to the 15th percentile on the weight gain distribution of the first-time young mothers we focus on. However, the results are similar when we use alternative cutoffs for low weight gain, apply other kernel functions for weights, or define inadequate weight gain conditioning on pre-pregnancy BMI (Yan, 2015).

employing alternative bandwidths does not appreciably change the results, as shown in columns (2) and (3).

[Insert Table 5 Here]

Our results on pre-pregnancy obesity, weight gain, prenatal care, and insurance coverage align with the income and health channels which are tied to mother's education. For instance, for young mothers born just after the cutoff who tend to have less education and income, reduced access to private insurance helps explain why they experience poorer birth outcomes than those born right before, since private insurance typically provides a broader network of providers and more comprehensive healthcare services than Medicaid. Of course, Medicaid serves as an important safety net for low-income mothers who might otherwise be left uninsured. In our context, increased Medicaid coverage may mitigate the differences in education and income and the associated infant health effects for mothers born near the cutoff. For smoking, the age-for-grade effect may weaken the impact of education. Given that many partnerships are formed at the grade level, the discontinuity for father's age appears to be driven by relative maturity: women born just after the cutoff interact with younger peers or potential partners while those born just before have older peers.

We next summarize the results for these mechanism variables by state (not shown to conserve space). In Nevada, being born after the entry date leads to a higher rate of obesity and overweight prior to pregnancy and low weight gain, and a lower chance of having early prenatal care or private insurance. The estimated impacts for New Mexico are generally imprecise. The only exception is a large and statistically significant rise in pre-pregnancy obesity. For Tennessee, a pattern similar to Nevada emerges for prenatal care initiation, inadequate weight gain, and private insurance coverage. Moreover, young mothers from Tennessee born after the cutoff date are more likely to use WIC and have partners who are less educated and younger. For these women, the discontinuous decline in paternal education is consistent with both the manipulation of school entry policies on their peer groups and the tendency for positive assortative mating based on education.

4.4 Robustness of the main results

Figure 3 provides further evidence on the sensitivity of the results to differing bandwidth choices, when we pool observations across the three states. Since the bandwidth available to us in the Tennessee dataset is limited, we focus on a range of 15-55 days in the figure. It is worth mentioning that when the selection procedure by Calonico, Cattaneo, and Titiunik (2014) and specifications with lower-order polynomials are employed, recent research has also reported relatively small optimal bandwidths (Cook and Kang, 2016; Depew and Eren, 2016; Johansen, 2021; Borra et al., 2024). In addition, for very small bandwidths below 20 days, the confidence intervals in the figure may be less reliable due to the limited number of clusters. We see the point estimates are generally stable for the education outcomes, especially starting at a bandwidth of roughly 25 days. Likewise, the estimated discontinuities are robust to varying bandwidths for birth weight, LBW, and having a low-Apgar-score baby. The estimates for prematurity initially decline before stabilizing as the bandwidth increases, while the results for cesarean birth do appear to be somewhat sensitive to bandwidth choice, with the estimated impact steadily shrinking as the window expands.[25]

[Insert Figure 3 Here]

Table 6 displays the results of a variety of additional robustness checks. Again, we focus on estimation of the pooled RD effect with a 40-day bandwidth. Columns (1) and (2) use an alternative kernel function (Uniform or Epanechnikov). The pattern is broadly consistent with the baseline findings in panel A (column 1) of Tables 2 and 3, with some minor differences. For example, the estimates for LBW and cesarean birth become less precise. But the estimated discontinuities for having a low-Apgar-score baby turn significant. As shown in column (3), employing a quadratic polynomial specification does not materially alter the results; however, we now observe stronger

[25] Moreover, suppose the underlying optimal bandwidth for an outcome lies between 15 and 55 days, for instance, 38 days. Then, the resulting discontinuity estimate will numerically equal the point estimate based on h=38 in the figure. However, when estimates are based on MSE-optimal bandwidths, robust bias-corrected confidence intervals are necessary for valid inference (Cattaneo et al., 2019). Another bandwidth t is needed for the correction. Direct estimation of t is infeasible for the pooling analysis, as it requires data spanning the entire domain of the running variable ($\pm$182 days) for each state. But using the full $\pm$182 days, we find h/t is typically around 0.7 when analyzing the Nevada and New Mexico data with optimal bandwidth selection. Assuming this ratio holds for the pooling case, we compute the implied t for a given h. Finally, applying these bandwidths of t and h to re-scale and recenter the corresponding original confidence intervals, we find nearly all of the precise estimates in the figure retain significance (results available upon request).

effects on birth weight, prematurity, and cesarean birth. The benchmark results are also robust to inclusion of predetermined covariates, when we sequentially add the mother cohort fixed effects (FE) and race/ethnicity FE in columns (4) and (5).[26] Column (6) shows adding these controls to the model with a quadratic polynomial produces estimates analogous to those in column (3) without the controls. Moreover, the results again conform to the baseline when we try controlling for maternal age FE (not shown).

[Insert Table 6 Here]

The last two columns of Table 6 consider alternative age restrictions for the sample (no more than 22 or 24 years old for the sampled mothers). The findings again conform to the baseline, except that the estimated effects for birth weight are smaller and less precise. We also repeat the nine excises above with the state-level samples and find the results are similar to the counterparts in Tables 2 and 3 (not shown). Moreover, Appendix Tables A3 and A4 provide evidence for Nevada and New Mexico when we implement the bandwidth selection procedure by Calonico et al. (2014). For simplicity, we present the bias-corrected point estimates and standard errors, by which robust bias-corrected confidence intervals can be easily constructed (Cattaneo et al., 2019).[27] Under the baseline specification with triangle kernel weights, the results for the two states in column (1) of Tables A3 and A4 are generally in line with their counterparts (based on a 40-day bandwidth) in Tables 2 and 3.[28] The other columns yield analogous results, when we employ an alternative kernel function or include controls.

4.5 Additional analysis

To explore the heterogeneous effects by mother's race/ethnicity, we begin by splitting the pooled sample in Table 1 into two subsamples (non-Hispanic White mothers and non-White mothers) and

[26] The set of mother race/ethnicity FE for columns (4) and (5) consists of dummy variables which correspond to non-Hispanic Black, Hispanic, and Native American mothers, respectively. The results change little when we use an alternative set of three dummy variables, such as White, non-Hispanic Black, and Hispanic.

[27] The significance level for each estimate in the tables is based on these robust bias-corrected confidence intervals. Furthermore, as with the original RD estimator, the bias-corrected estimator is also asymptotically consistent.

[28] Similar to the recent studies mentioned above, the optimal bandwidths are also relatively small for the two states. This holds especially true for the birth outcomes: all the corresponding optimal bandwidths are less than 55 days in column (1) of Tables A3 and A4.

estimate the baseline model for both subsamples. For ease of illustration, the results are graphed in Figure 4. Being born after the cutoff significantly increases high school non-completion for both subgroups, almost to the same extent. For the other two education outcomes, the estimated impacts are greater for White mothers. We also observe a pattern of poorer birth outcomes, although the estimated impacts are generally less precise than the pooled estimates above. Moreover, the estimated discontinuities are typically of similar magnitude for the two subgroups, except for preterm birth. Next, we separately examine non-Hispanic Black, Hispanic, and Native American mothers, who collectively represent over 95% of all the non-White mothers. Appendix Figure A11 again shows educational attainment is negatively affected, especially for Black and Hispanic mothers. In addition, we see suggestive evidence that mothers from the three minority groups who were born after the cutoff tend to have worse birth outcomes.[29]

[Insert Figure 4 Here]

As with recent longer term studies (Borra et al., 2024; Fredriksson et al., 2022), auxiliary analysis also looks at mothers of all ages in Nevada and New Mexico (not shown). This exercise produces weaker impacts on education at motherhood. In addition, the estimates on newborns are insignificant and indicate no deterioration in health. The pattern is not surprising: as mentioned earlier, the education discontinuities are smaller for older mothers, who are now included in the samples for the auxiliary analysis. The older first-time mothers may have been less likely to have made their school exit decision according to the legal leaving age, choosing to stop schooling based on completed education instead. Finally, Appendix Table A5 calculates statistical power for a set of hypothesized effects against the null hypothesis of zero impact, using the pooled sample in Table 1. Effect sizes under the alternative hypotheses typically come from multiplying the dependent variable means by a fraction.[30] We focus on the baseline specification used for column (1) of Tables

[29] The estimated effects for Native American mothers above are mainly driven by those in New Mexico, due to the small number of Native Americans in the other two states. Likewise, the results for Black mothers are primarily driven by those in Tennessee. Moreover, the contribution of Hispanic mothers in Tennessee to the overall estimates of this subgroup is small.

[30] For almost all the dependent variables, these fractions are 35% (a very large effect), 25%, 15%, 10%, and 5% (a small effect). The only exception is birth weight, as even a reduction of 5% of the mean or about 160 g represents an enormous effect, which is rarely documented (Fredriksson et al., 2022; Gage, Fang, O'Neill, and DiRienzo, 2013; Noghanibehambari et al., 2022). Therefore, we begin the power analysis with a more plausible yet still sizable impact of -80 g, followed by smaller effects down to -10 g.

2 and 3 and consider power of two-sided tests with 10% size. For the educational outcomes, we have sufficient power to detect even small effects, except high school non-completion.[31] Moreover, provided the true effect is not small, the sample allows us to correctly rule out zero effect for birth weight, prematurity, and cesarean birth with an about 80 percent or higher chance. For LBW and low Apgar score, we have limited statistical power to detect modest effects.

5 Conclusion

This study contributes to the limited body of research examining the impact of school entry policies on pregnancy outcomes, leveraging birth data from three states based on the latest 2003 revision of the US birth certificate. Our normalized-and-pooled regression discontinuity analysis shows young mothers born just after the school entry date have considerably lower educational attainment than those born just before. These women also tend to have poorer birth outcomes, including a modest reduction in birth weight and a large increase in LBW, prematurity, and cesarean birth. When we adjust for multiple testing and implement local randomization estimation, the pattern is consistent with that from the baseline analysis. The education discontinuities on high school non-completion are evident across the three states. Moreover, we observe precisely estimated adverse impacts on the other indicators: earning a high school diploma or less, having at most some college experience. With respect to newborn health, the most compelling findings are seen in Tennessee, while there is suggestive evidence of higher risks for several adverse infant health outcomes in Nevada and New Mexico.

We find maternal pre-pregnancy obesity, weight gain, use of prenatal care, insurance coverage, and paternal age are key mechanisms through which the school entry policies influence infant health. Of course, while mother's education is likely the primary driver of the discontinuities in many of these inputs, absolute and relative maturity may also be a contributing factor, especially regarding partner quality. The baseline results are also robust to varying bandwidths, different choices of kernel function or polynomial order, additional controls, and alternative sample restrictions. For future research, scholars may provide additional evidence using a national sample with mother's

[31] Also recall the baseline estimated effect for some college or less education is 0.013, which is very small relative to the sample mean. Using 0.013 as the hypothesized effect, we find the corresponding power is 0.93.

birthday. Another fruitful avenue is to investigate the infant health consequences of late school entry in low-income countries. In addition, it will be interesting to compare mental health, alcohol consumption, and earnings of expectant mothers born near the entry date, when data become available. Overall, our results suggest academic redshirting for female children may lead to long term costs of having poorer birth outcomes. In states where educational systems fall behind the national average in terms of advancing academic progress and retaining students, additional education can yield significant health benefits for young mothers and their newborns.

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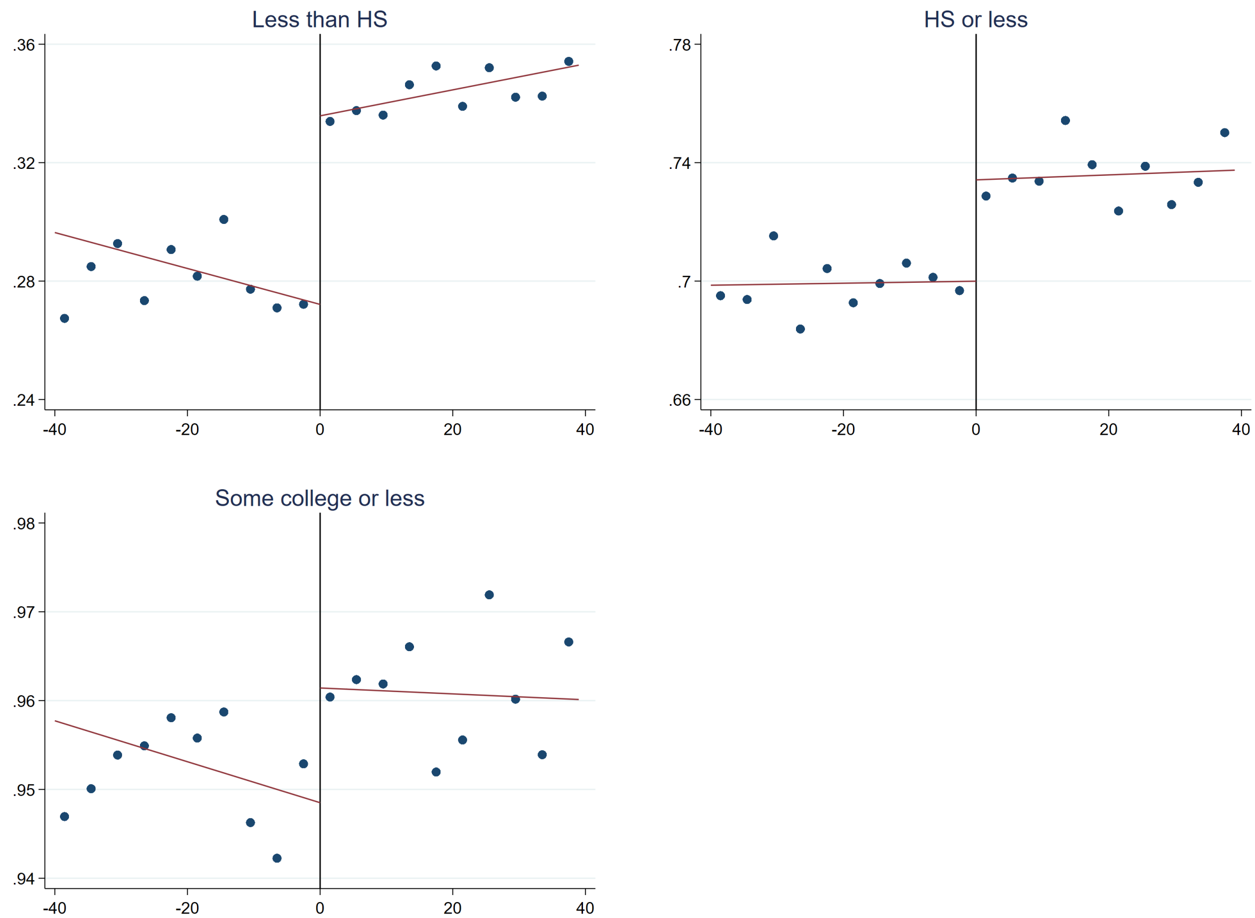
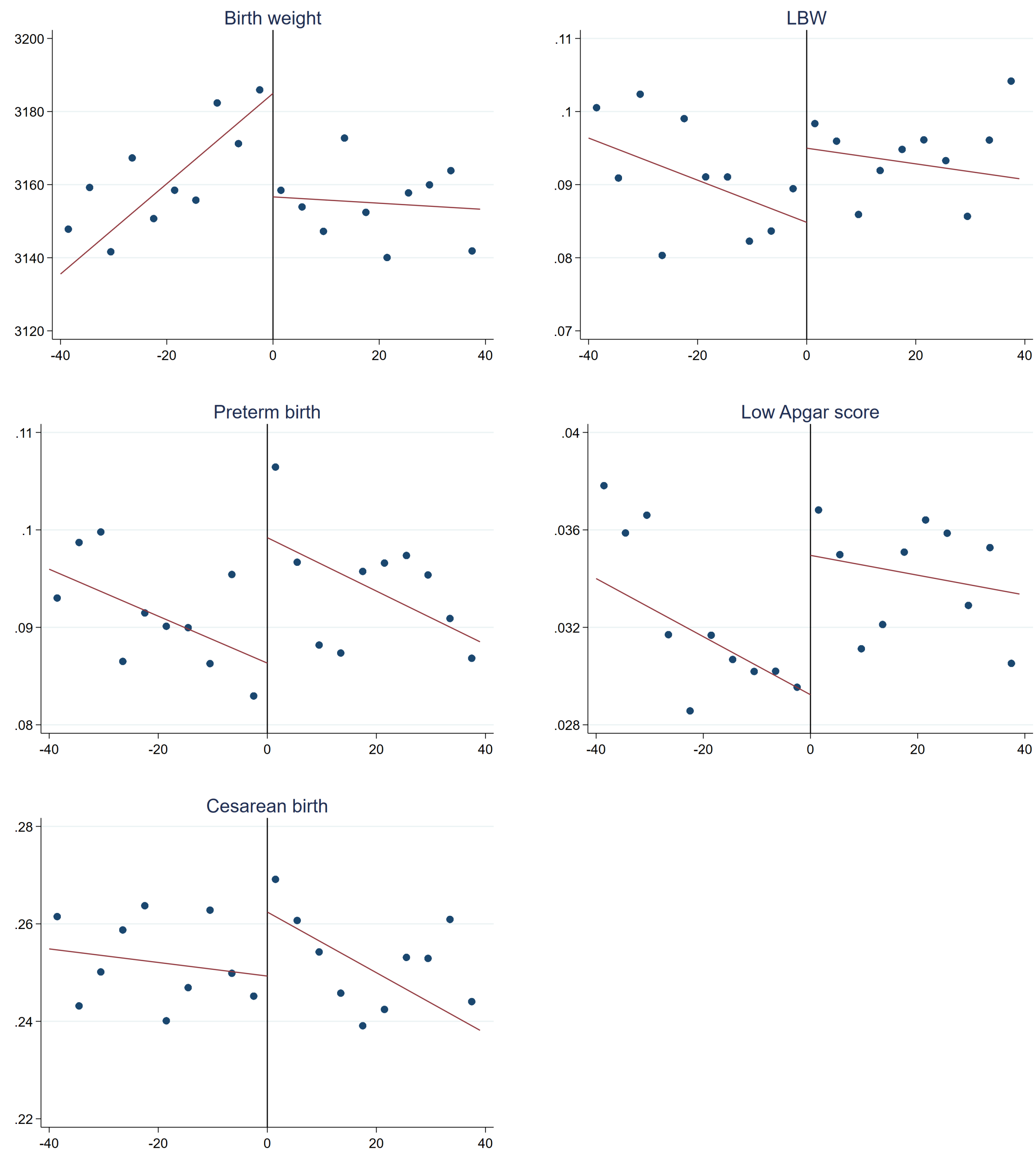
Figure 1: Education at motherhood by normalized birthday



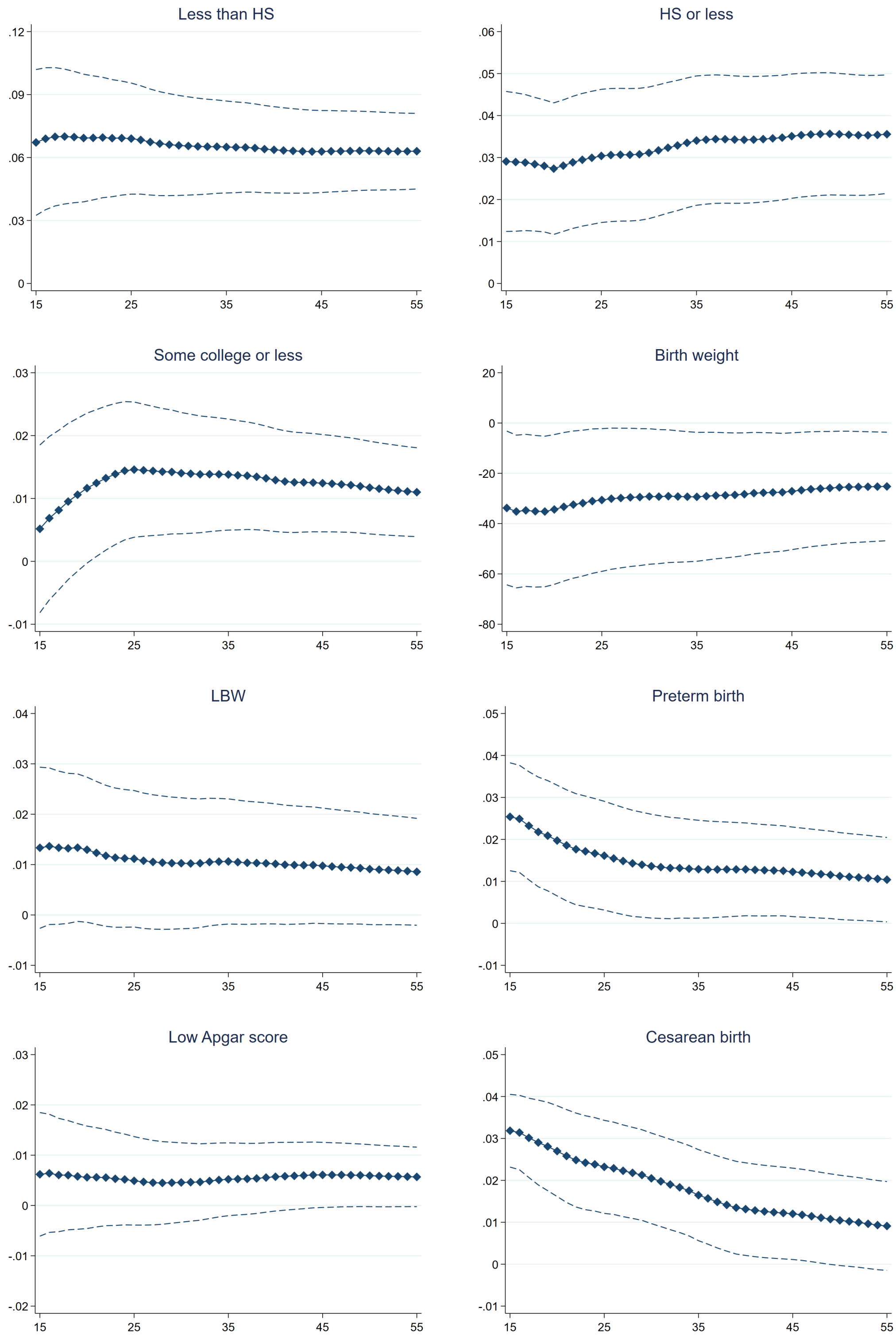
Note. The horizontal axis indicates the normalized mother's day of birth in Eq.1. The dots are averages of mother's educational attainment over 4-day bins. The fitting lines are from interpolation of local linear regressions which use triangle kernel weights and a 40-day bandwidth. The pooled sample used for the regressions is described in Table 1.

Figure 2: Birth outcomes by normalized birthday



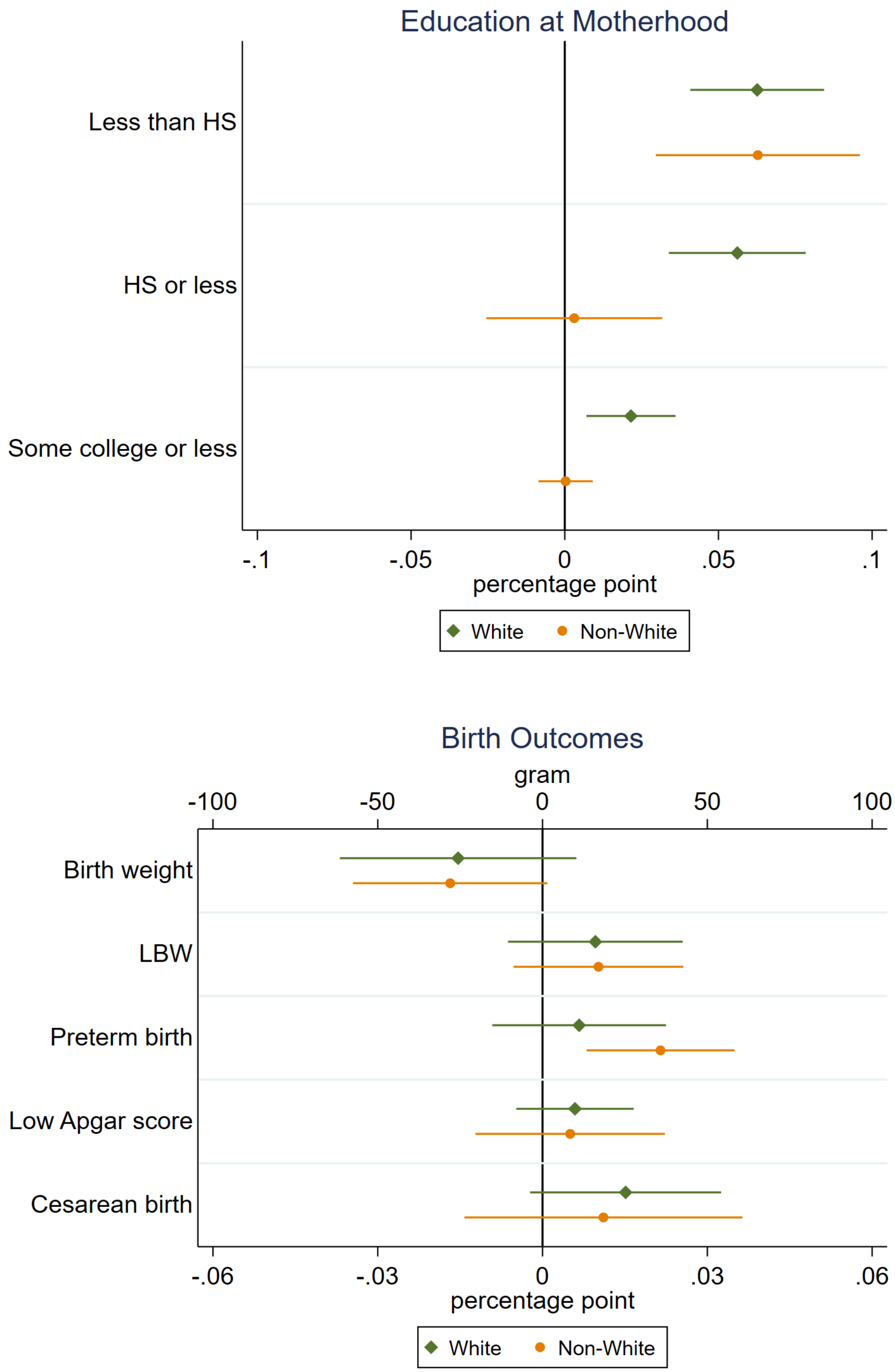
Note. The horizontal axis indicates the normalized mother's day of birth in Eq.1. The dots are averages of the birth outcomes over 4-day bins. The fitting lines are from interpolation of local linear regressions which use triangle kernel weights and a 40-day bandwidth. The pooled sample used for the regressions is described in Table 1.

Figure 3: Robustness to alternative bandwidths



Note. The horizontal axis indicates bandwidth. The solid line denotes the estimates for α_1 from Eq.1, for the first-time young mothers of the three states. The model is estimated with triangle kernel weights and differing bandwidths. The dash lines represent the 95% confidence intervals. Standard errors are clustered by normalized mother's birthday.

Figure 4: Effects of being born after the eligibility cutoff: White and non-White mothers



Note. The figure plots the discontinuity estimates with 95% confidence intervals, when we split the pooled sample in Table 1 into two subsamples (non-Hispanic White and non-White mothers). We estimate Eq.1 for either group with a 40-day bandwidth and triangle kernel weights. Standard errors clustered by normalized mother's birthday are used to construct the confidence intervals.

Table 1: Descriptive statistics

	Pooled	Nevada	New Mexico	Tennessee
Mother's education				
Mother less than HS	0.311	0.350	0.346	0.301
Mother HS or less	0.717	0.762	0.702	0.715
Mother some college or less	0.956	0.963	0.952	0.957
Birth outcomes				
Birth weight	3158.197	3189.235	3144.165	3158.129
Low birth weight (LBW)	0.093	0.082	0.081	0.096
Preterm birth	0.093	0.075	0.069	0.100
Low Apgar score	0.033	0.022	0.055	0.030
Cesarean birth	0.252	0.236	0.162	0.272
Mother's health and health behaviors				
Pre-pregnancy obesity	0.188	0.183	0.160	0.193
Pre-pregnancy overweight	0.405	0.386	0.403	0.407
Pre-pregnancy smoking	0.236	0.085	0.108	0.274
Early prenatal care	0.657	0.589	0.628	0.669
Number of prenatal visits	11.217	10.766	10.366	11.432
Prenatal smoking	0.178	0.072	0.062	0.211
Inadequate weight gain	0.151	0.138	0.153	0.152
Mother's access to health care and nutrition				
Mother on Medicaid	0.753	0.567	0.800	0.762
Mother on private insurance	0.213	0.266	0.112	0.224
Mother on WIC	0.750	0.573	0.747	0.767
Mother's other demographic characteristics				
Mother Non-Hispanic Black	0.250	0.149	0.012	0.308
Mother Hispanic	0.127	0.348	0.616	0.009
Mother Native American	0.030	0.016	0.180	0.001
Mother's age	19.562	19.625	19.494	19.569
Father's characteristics				
Father less than HS	0.238	0.272	0.296	0.224
Father HS or less	0.739	0.759	0.692	0.746
Father some college or less	0.942	0.941	0.938	0.943
Father's age	22.716	22.649	22.448	22.779
Max N	35684	2558	5524	27602

Note. Each state-level sample consists of first-time mothers who are no more than 23 years old and were born 40 days around the first day after the state's school entry date. The pooled sample comes from combining the three state-level samples.

Table 2: Effects of being born after the eligibility cutoff: Mother's education

	(1) RD band=40	(2) RD band=30	(3) RD band=50
Panel A: Pooled			
Less than HS	0.064***	0.066***	0.063***
	(0.010)	(0.012)	(0.010)
HS or less	0.034***	0.031***	0.036***
	(0.008)	(0.008)	(0.007)
Some college or less	0.013***	0.014***	0.012***
	(0.004)	(0.005)	(0.004)
Max N	35533	26647	44215
Panel B: Nevada			
Less than HS	0.073*	0.087**	0.073**
	(0.039)	(0.043)	(0.036)
HS or less	-0.012	-0.028	-0.004
	(0.033)	(0.037)	(0.031)
Some college or less	-0.004	-0.006	-0.003
	(0.021)	(0.026)	(0.018)
Max N	2530	1906	3169
Panel C: New Mexico			
Less than HS	0.068***	0.060**	0.069***
	(0.026)	(0.029)	(0.024)
HS or less	0.017	0.010	0.026
	(0.021)	(0.021)	(0.020)
Some college or less	0.011	0.012	0.010
	(0.009)	(0.011)	(0.008)
Max N	5454	4126	6764
Panel D: Tennessee			
Less than HS	0.062***	0.066***	0.061***
	(0.012)	(0.013)	(0.010)
HS or less	0.042***	0.040***	0.041***
	(0.008)	(0.009)	(0.008)
Some college or less	0.015***	0.016**	0.013***
	(0.005)	(0.006)	(0.005)
Max N	27549	20615	34282

Note. Each entry reports a regression discontinuity estimate of α_1 from Eq.1, for the first-time young mothers of all three states or in each state . The model is estimated with triangle kernel weights and a bandwidth of 30, 40, or 50 days. Standard errors clustered by normalized mother's birthday are in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Effects of being born after the eligibility cutoff: Birth outcomes

	(1) RD band=40	(2) RD band=30	(3) RD band=50
Panel A: Pooled			
Birth weight	-28.309**	-29.211**	-25.591**
	(12.446)	(13.744)	(11.399)
LBW	0.010*	0.010	0.009
	(0.006)	(0.007)	(0.006)
Preterm birth	0.013**	0.014**	0.011**
	(0.006)	(0.006)	(0.005)
Low Apgar score	0.006	0.005	0.006*
	(0.003)	(0.004)	(0.003)
Cesarean birth	0.013**	0.020***	0.010*
	(0.006)	(0.006)	(0.005)
Max N	35684	26771	44410
Panel B: Nevada			
Birth weight	-28.729	-25.160	-30.188
	(41.750)	(46.120)	(37.810)
LBW	0.003	-0.003	0.004
	(0.022)	(0.024)	(0.020)
Preterm birth	0.009	0.009	0.012
	(0.020)	(0.022)	(0.018)
Low Apgar score	0.003	0.006	0.0002
	(0.012)	(0.013)	(0.012)
Cesarean birth	0.024	0.013	0.026
	(0.040)	(0.048)	(0.034)
Max N	2558	1928	3206
Panel C: New Mexico			
Birth weight	0.086	-9.243	0.261
	(28.321)	(31.755)	(26.028)
LBW	0.004	0.011	0.002
	(0.016)	(0.018)	(0.015)
Preterm birth	0.018	0.024	0.008
	(0.016)	(0.019)	(0.015)
Low Apgar score	-0.014	-0.019	-0.011
	(0.014)	(0.016)	(0.013)
Cesarean birth	0.018	0.019	0.018
	(0.011)	(0.012)	(0.012)
Max N	5524	4182	6855
Panel D: Tennessee			
Birth weight	-33.468**	-33.050**	-29.854**
	(13.677)	(15.167)	(12.486)
LBW	0.012	0.011	0.011
	(0.008)	(0.009)	(0.007)
Preterm birth	0.012*	0.011	0.012*
	(0.007)	(0.008)	(0.007)
Low Apgar score	0.010**	0.009	0.010**
	(0.005)	(0.006)	(0.004)
Cesarean birth	0.011	0.021***	0.008
	(0.008)	(0.008)	(0.007)
Max N	27602	20661	34349

Note. Each entry reports a regression discontinuity estimate of α_1 from Eq.1, for the first-time young mothers of all three states or in each state. The model is estimated with triangle kernel weights and a bandwidth of 30, 40, or 50 days. Standard errors clustered by normalized mother's birthday are in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Mother's education and birth outcomes: Multiple inference adjustments and local randomization analysis

	(1) Less than HS	(2) HS or less	(3) Some college or less	(4) Birth weight	(5) LBW	(6) Preterm birth	(7) Low Apgar score	(8) Cesarean birth
Panel A: Continuity-based RD with RW correction								
Born afer cutoff	0.064^{***}	0.034^{***}	0.013^{***}	-28.309^{**}	0.010^{*}	0.013^{**}	0.006	0.013^{**}
	(0.010)	(0.008)	(0.004)	(12.446)	(0.006)	(0.006)	(0.003)	(0.006)
Unadjusted p-value	<0.0001	<0.0001	0.0020	0.0229	0.0956	0.0225	0.1000	0.0198
RW p-value	0.0010	0.0010	0.0060	0.0400	0.0959	0.0400	0.0959	0.0400
N	35533	35533	35533	35684	35684	35487	35420	35684
Panel B: Local randomization with RW correction								
Born afer cutoff	0.070^{***}	0.036^{***}	0.010	-39.536^{**}	0.016^{*}	0.024^{***}	0.007	0.022
	(0.014)	(0.014)	(0.006)	(17.687)	(0.009)	(0.009)	(0.006)	(0.013)
Unadjusted p-value	<0.0001	0.0087	0.1107	0.0254	0.0721	0.0071	0.2054	0.1016
RW p-value	0.0010	0.0410	0.2767	0.1109	0.2617	0.0350	0.2767	0.2767
N	4401	4401	4401	4415	4415	4384	4368	4415

Note. In panel A, each continuity-based RD estimate comes from estimating Eq.1, when we use the pooled sample described in Table 1. The model is estimated with triangle kernel weights and a bandwidth of 40 days. Standard errors clustered by normalized mother's birthday are in parentheses. In panel B, each local randomization estimate comes from estimating Eq.4. The sample used is a subset of the one for panel A, which includes the mothers whose B_i range from -5 to 4 (in a 10-day window). Heteroskedasticity-robust standard errors are in parentheses. In both panels, ***, **, and * denote statistical significance at 1%, 5%, and 10%, based on the unadjusted p-values. The RW p-values come from implementating the Romano-Wolf multiple hypothesis correction with 1,000 bootstrap replications.

Table 5: Potential mechanisms

	(1) RD band=40	(2) RD band=30	(3) RD band=50
Panel A: Mother's health and health behaviors			
Pre-pregnancy obesity	0.014*	0.015*	0.014**
	(0.008)	(0.009)	(0.007)
Pre-pregnancy overweight	0.018	0.023	0.014
	(0.013)	(0.014)	(0.011)
Pre-pregnancy smoking	-0.002	0.001	-0.004
	(0.010)	(0.011)	(0.009)
Early prenatal care	-0.027***	-0.032***	-0.021**
	(0.009)	(0.011)	(0.008)
Number of prenatal visits	-0.199**	-0.189*	-0.193**
	(0.099)	(0.113)	(0.090)
Prenatal smoking	0.006	0.012	0.002
	(0.009)	(0.010)	(0.008)
Inadequate weight gain	0.016**	0.012	0.018***
	(0.007)	(0.008)	(0.007)
Panel B: Mother's access to health care and nutrition			
Mother on Medicaid	0.018*	0.021*	0.015
	(0.011)	(0.012)	(0.010)
Mother on private insurance	-0.019*	-0.025**	-0.016*
	(0.010)	(0.011)	(0.009)
Mother on WIC	0.015	0.019*	0.013
	(0.011)	(0.011)	(0.010)
Panel C: Father's characteristics			
Father less than HS	0.010	0.015	0.007
	(0.010)	(0.012)	(0.009)
Father HS or less	0.009	0.007	0.010
	(0.011)	(0.013)	(0.010)
Father some college or less	-0.002	-0.005	-0.0003
	(0.005)	(0.006)	(0.004)
Father's age	-0.257***	-0.268***	-0.228***
	(0.086)	(0.092)	(0.084)

Note. Each entry reports a regression discontinuity estimate of α_1 from Eq.1, for the first-time young mothers for the first-time young mothers of all three states. The model is estimated with triangle kernel weights and a bandwidth of 30, 40, or 50 days. Standard errors clustered by normalized mother's birthday are in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

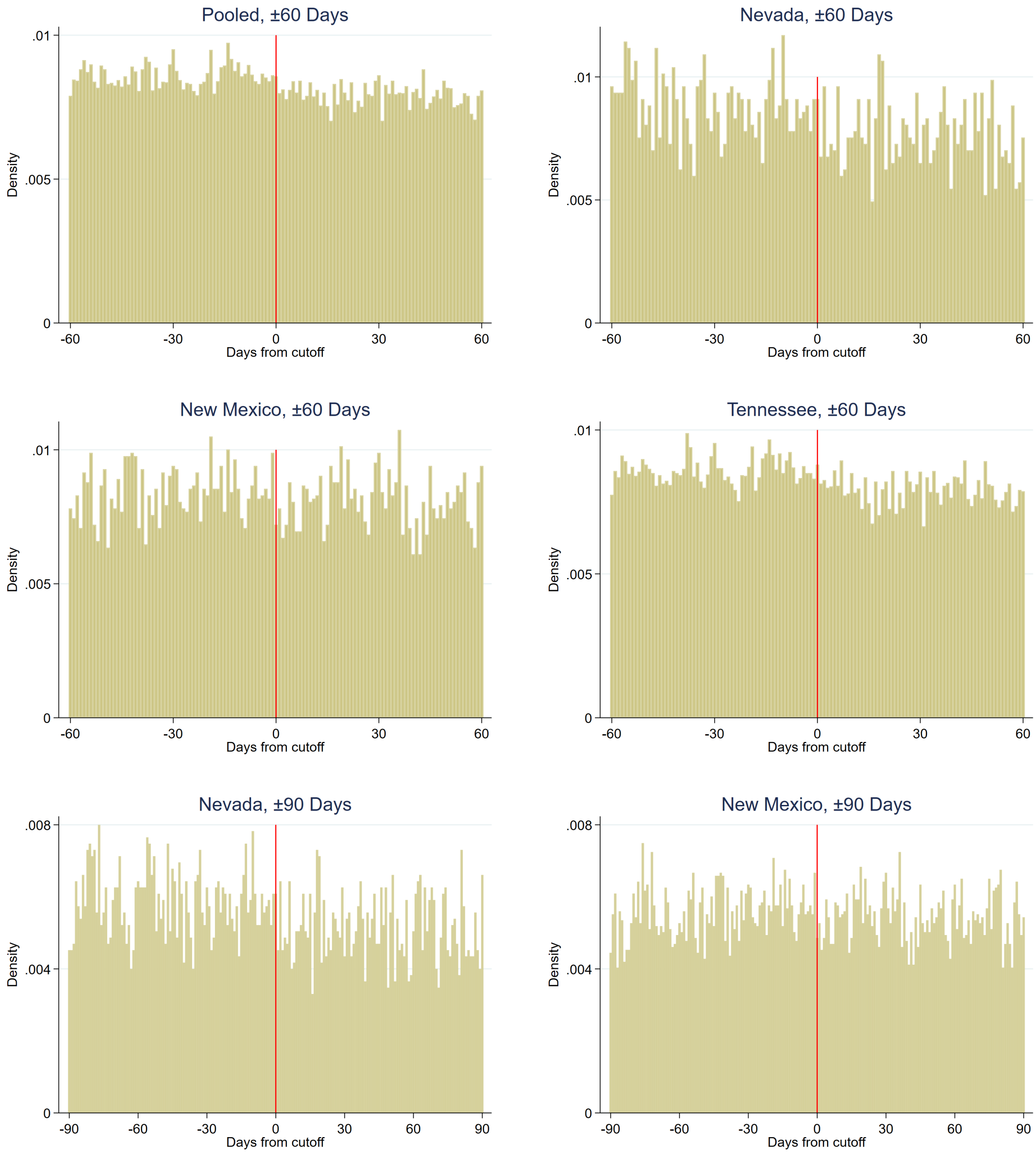
Table 6: Robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Less than HS	0.060***	0.063***	0.070***	0.064***	0.063***	0.066***	0.069***	0.062***
	(0.010)	(0.010)	(0.016)	(0.010)	(0.009)	(0.014)	(0.011)	(0.009)
HS or less	0.035***	0.035***	0.029***	0.035***	0.034***	0.026***	0.036***	0.039***
	(0.008)	(0.008)	(0.009)	(0.008)	(0.008)	(0.009)	(0.007)	(0.008)
Some college or less	0.010**	0.013***	0.014**	0.013***	0.013***	0.013**	0.008**	0.015***
	(0.004)	(0.004)	(0.006)	(0.004)	(0.004)	(0.006)	(0.003)	(0.006)
Birth weight	-22.484*	-27.751**	-32.707**	-28.192**	-25.920**	-29.617*	-22.124*	-17.450
	(11.494)	(12.334)	(15.722)	(12.303)	(12.507)	(16.312)	(12.911)	(11.566)
LBW	0.008	0.010	0.012	0.010*	0.010	0.011	0.010	0.007
	(0.006)	(0.006)	(0.008)	(0.006)	(0.006)	(0.008)	(0.007)	(0.006)
Preterm birth	0.011**	0.012**	0.017**	0.013**	0.013**	0.017**	0.012*	0.009*
	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)	(0.007)	(0.006)	(0.005)
Low Apgar score	0.007**	0.006*	0.004	0.006	0.005	0.004	0.008*	0.005
	(0.003)	(0.003)	(0.005)	(0.004)	(0.004)	(0.005)	(0.004)	(0.003)
Cesarean birth	0.009	0.010*	0.029***	0.013**	0.014**	0.030***	0.014*	0.011*
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)	(0.005)
Kernel function	Uni	Epa	Tri	Tri	Tri	Tri	Tri	Tri
Polynomial order	1	1	2	1	1	2	1	1
Mother cohort FE	N	N	N	Y	Y	Y	N	N
Mother race/ethnicity FE	N	N	N	N	Y	Y	N	N
Mother age restriction	≤ 23	≤ 23	≤ 23	≤ 23	≤ 23	≤ 23	≤ 22	≤ 24

Note. Each entry reports a regression discontinuity estimate of α_1 from Eq.3, when we use the pooled sample described in Table 1. All the models are estimated with a bandwidth of 40 days. When indicated, models use different kernel functions, a first or second order polynomial, additional controls, or alternative sample restrictions on mother's age. Standard errors clustered by normalized mother's birthday are in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

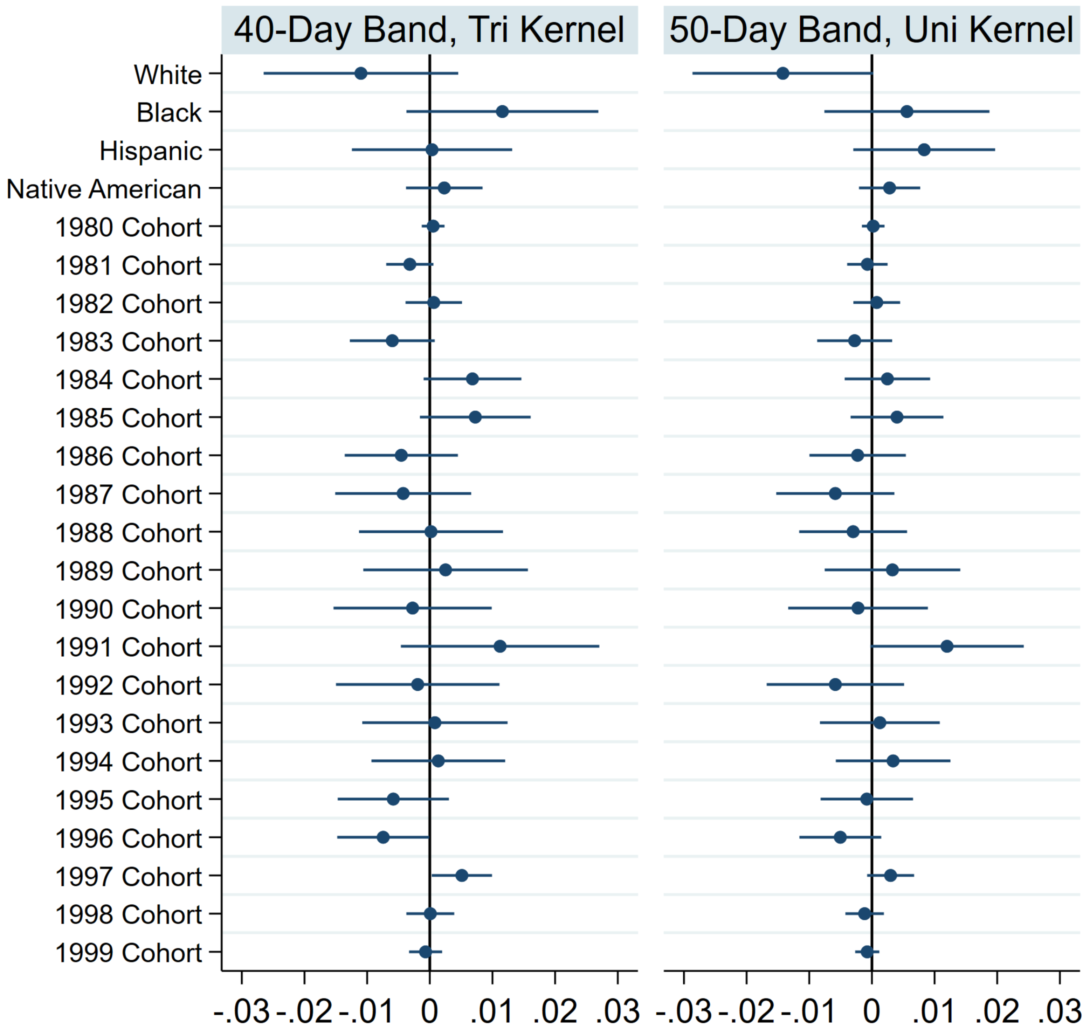
Appendix

Figure A1: Distribution of mother's birthday



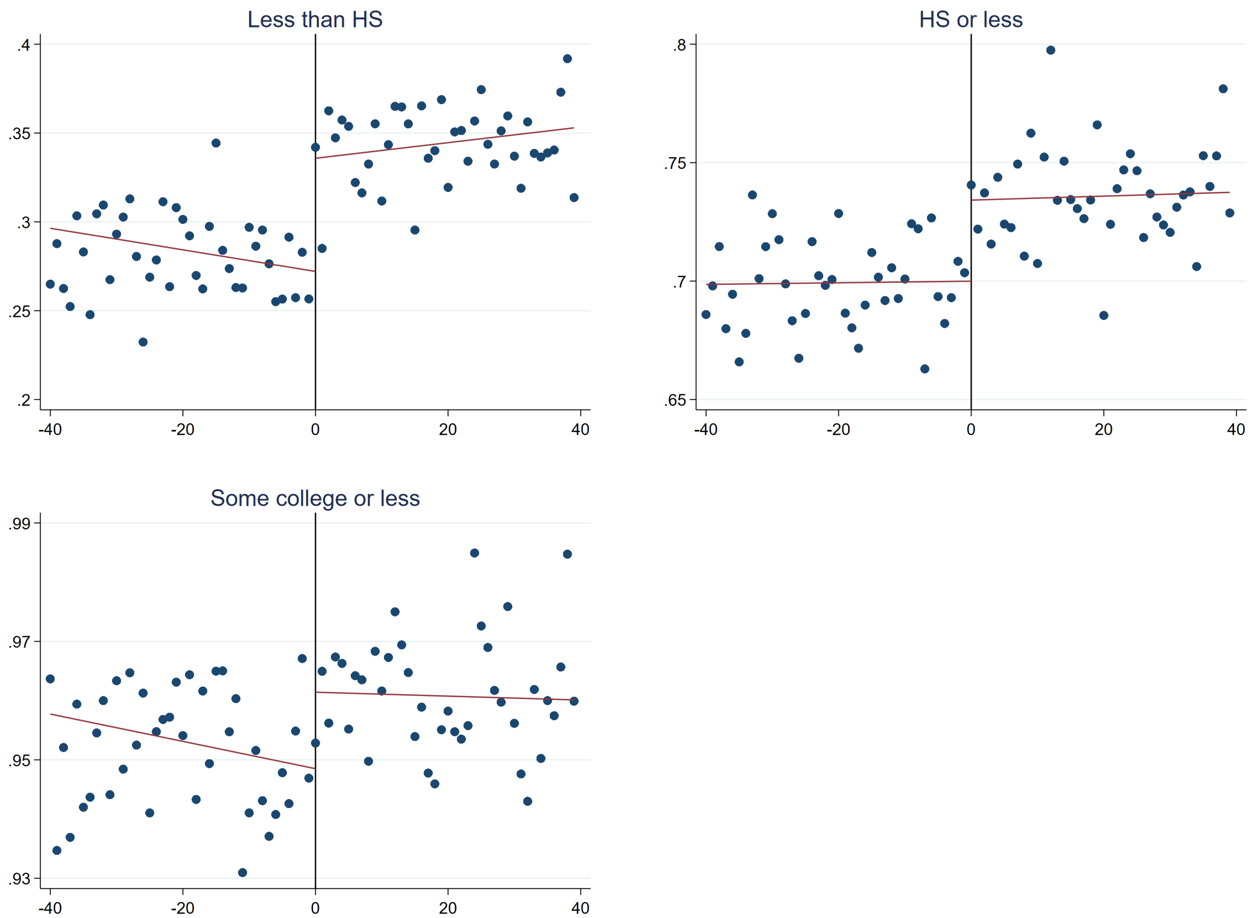
Note. The figure plots density of birthdays of the first-time young mothers around the state-specific school entry cutoff or pooled cutoff.

Figure A2: Smoothness of maternal predetermined characteristics



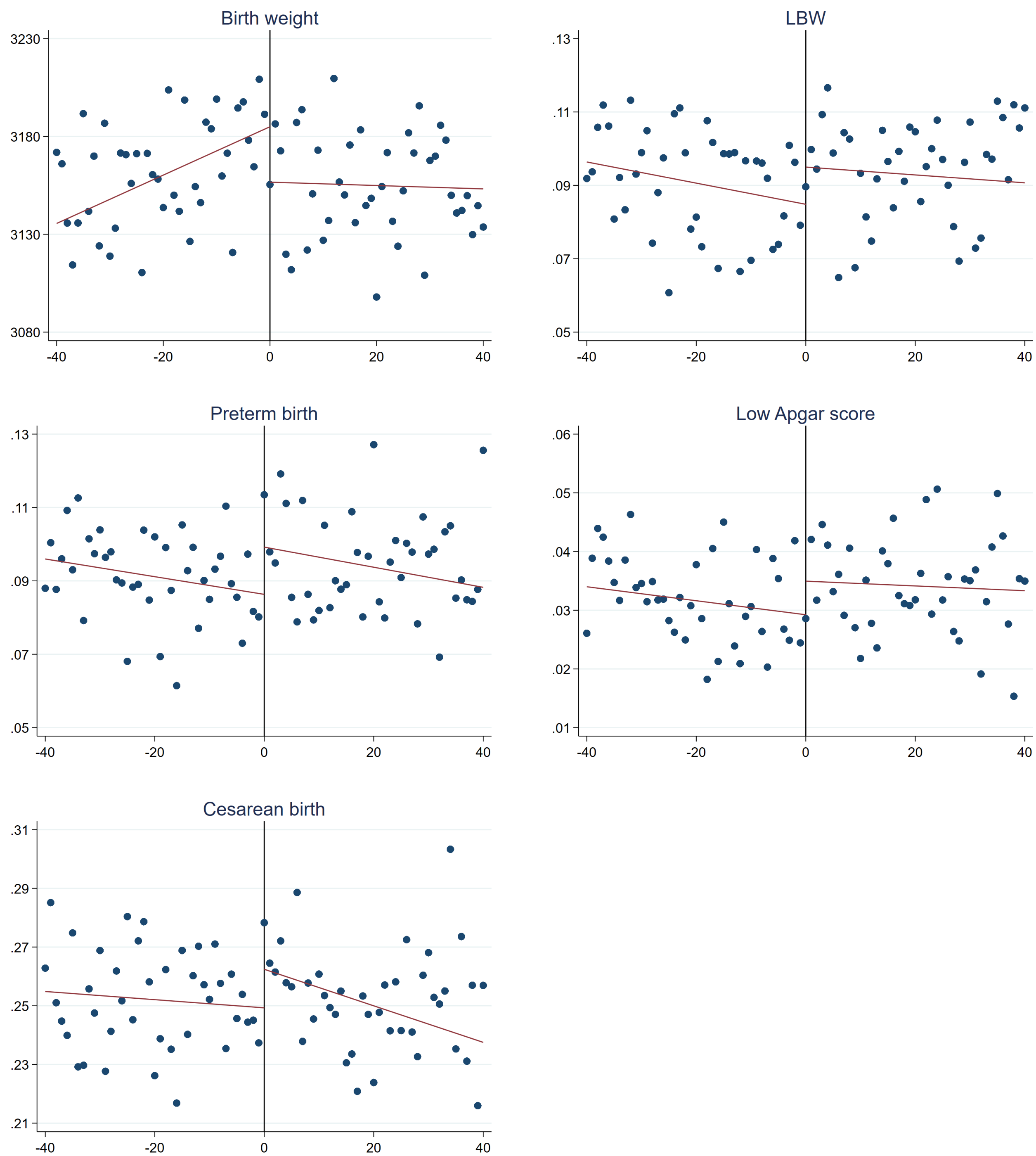
Note. The figure plots the discontinuity estimates with 90% confidence intervals for the first-time young mothers of all the three states. The subfigures use different kernel functions and bandwidths to estimate Eq.1. The corresponding standard errors used for the confidence intervals are clustered by normalized mother's birthday.

Figure A3: Education at motherhood by normalized birthday: Daily averages



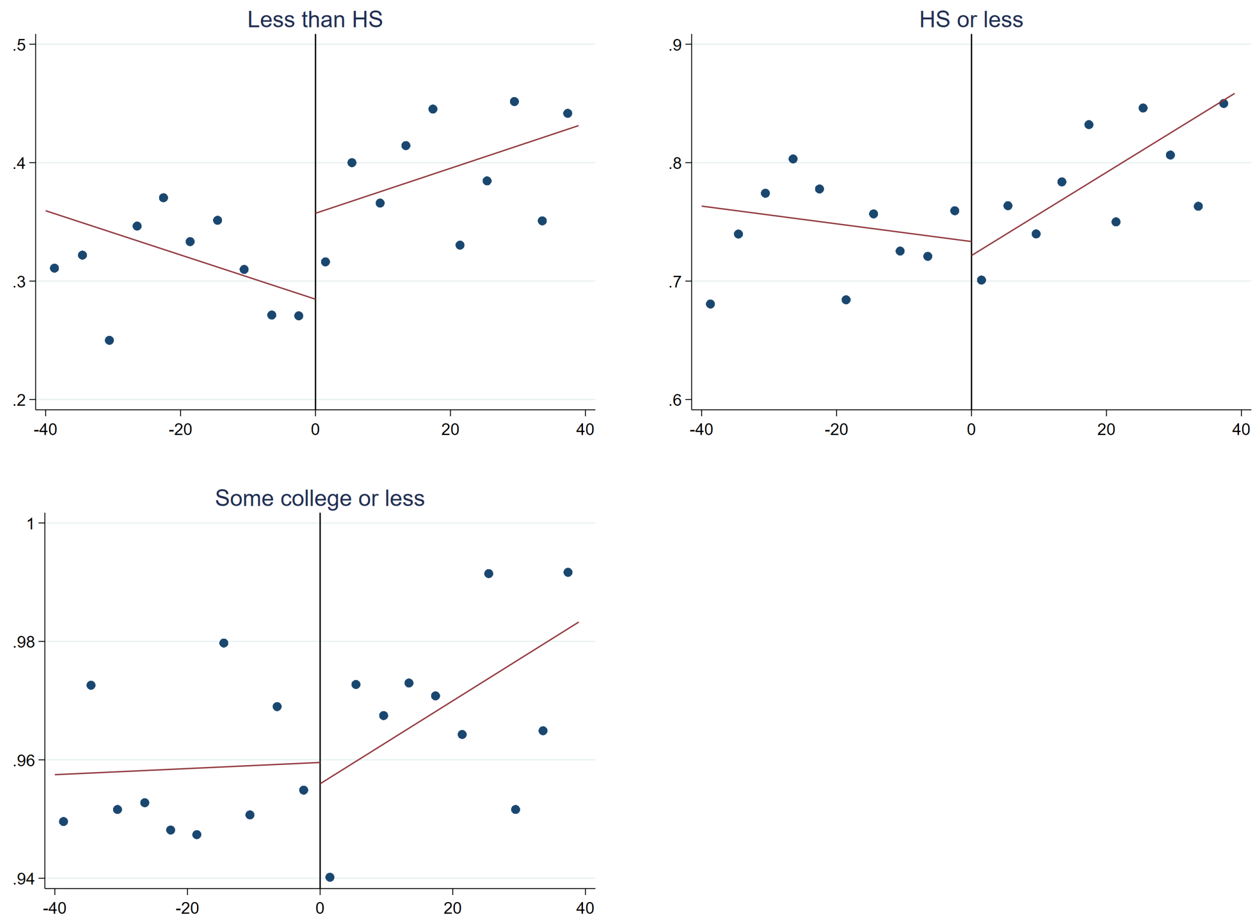
Note. The horizontal axis indicates the normalized mother's day of birth in Eq.1. The dots are daily averages of mother's educational attainment. The fitting lines are from interpolation of local linear regressions which use triangle kernel weights and a 40-day bandwidth. The pooled sample used for the regressions is described in Table 1.

Figure A4: Birth outcomes by normalized birthday: Daily averages



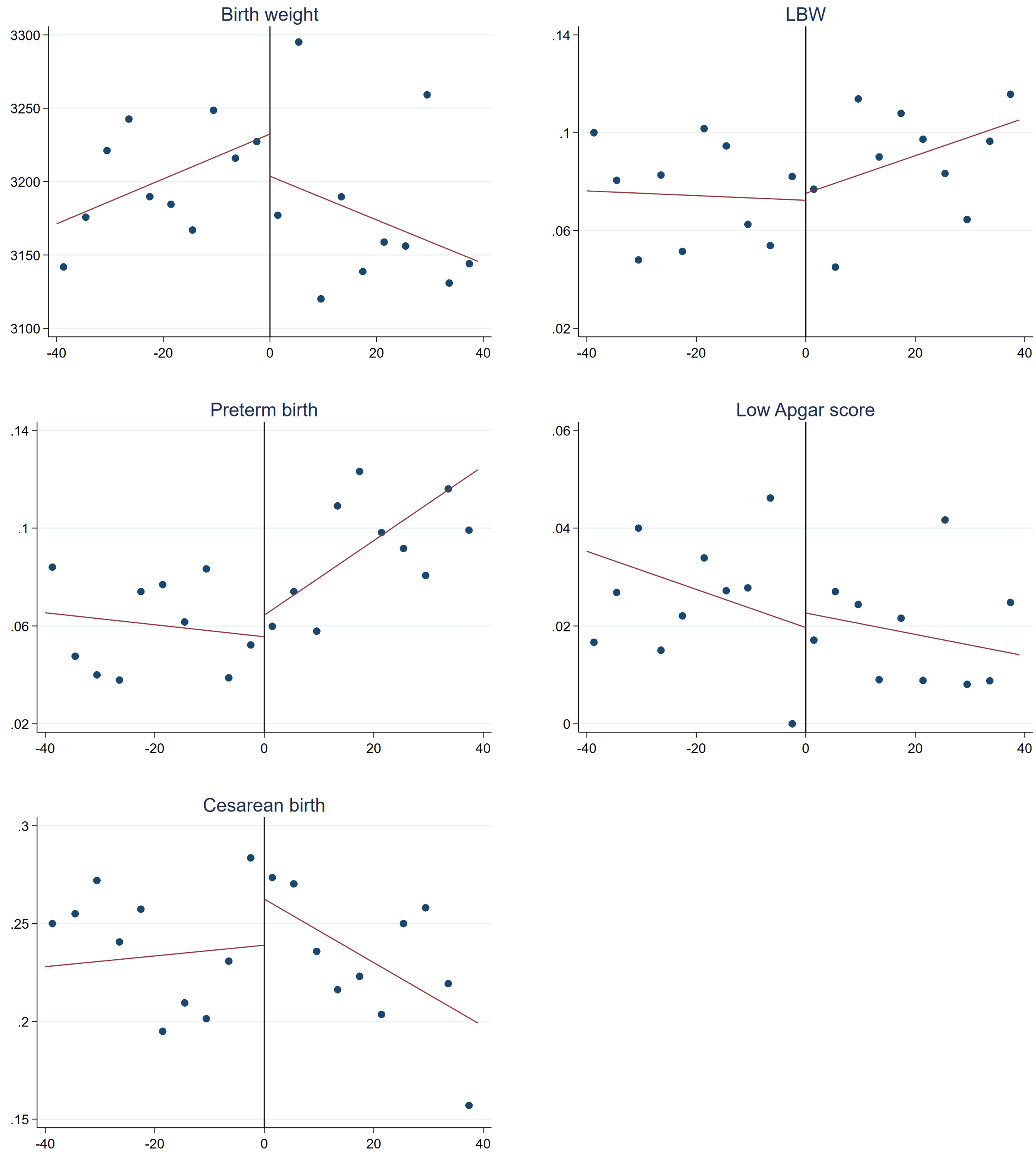
Note. The horizontal axis indicates the normalized mother's day of birth in Eq.1. The dots are daily averages of the birth outcomes. The fitting lines are from interpolation of local linear regressions which use triangle kernel weights and a 40-day bandwidth. The pooled sample used for the regressions is described in Table 1.

Figure A5: Education at motherhood by normalized birthday: Nevada



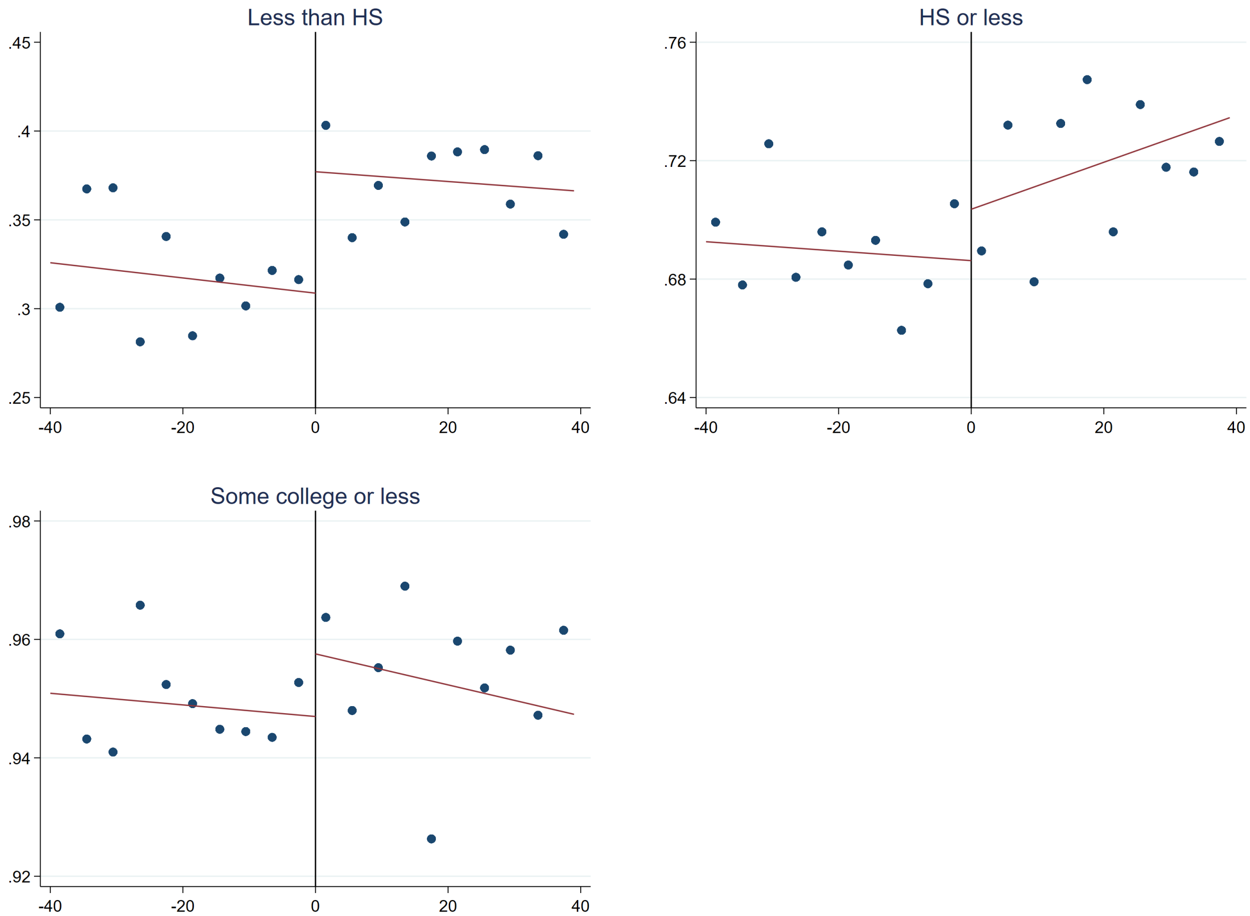
Note. The horizontal axis indicates the normalized mother's day of birth in Eq.1. The dots are averages of mother's educational attainment over 4-day bins. The fitting lines are from interpolation of local linear regressions which use triangle kernel weights and a 40-day bandwidth. The Nevada sample used for the regressions is described in Table 1.

Figure A6: Birth outcomes by normalized birthday: Nevada



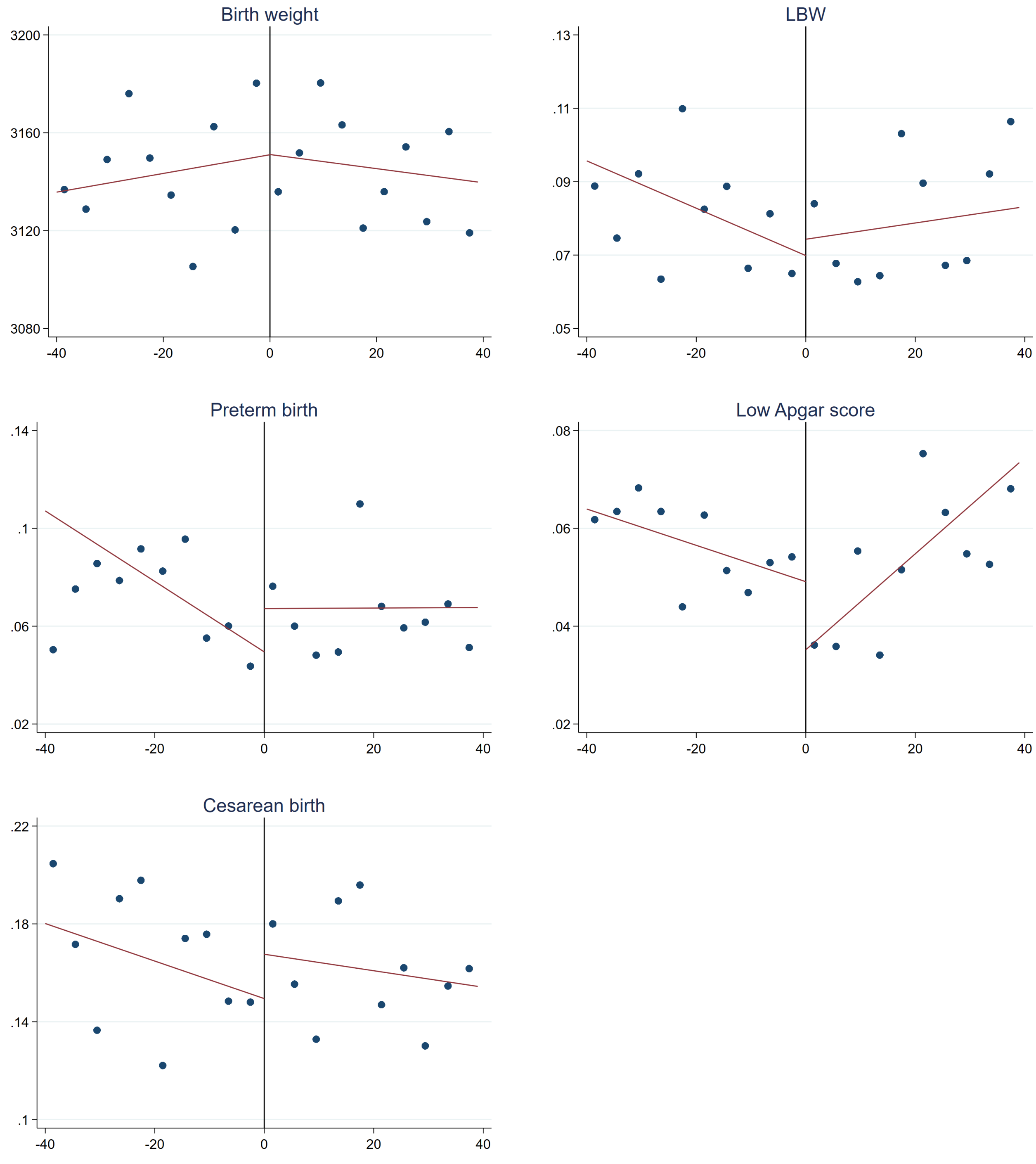
Note. The horizontal axis indicates the normalized mother's day of birth in Eq.1. The dots are averages of the birth outcomes over 4-day bins. The fitting lines are from interpolation of local linear regressions which use triangle kernel weights and a 40-day bandwidth. The Nevada sample used for the regressions is described in Table 1.

Figure A7: Education at motherhood by normalized birthday: New Mexico



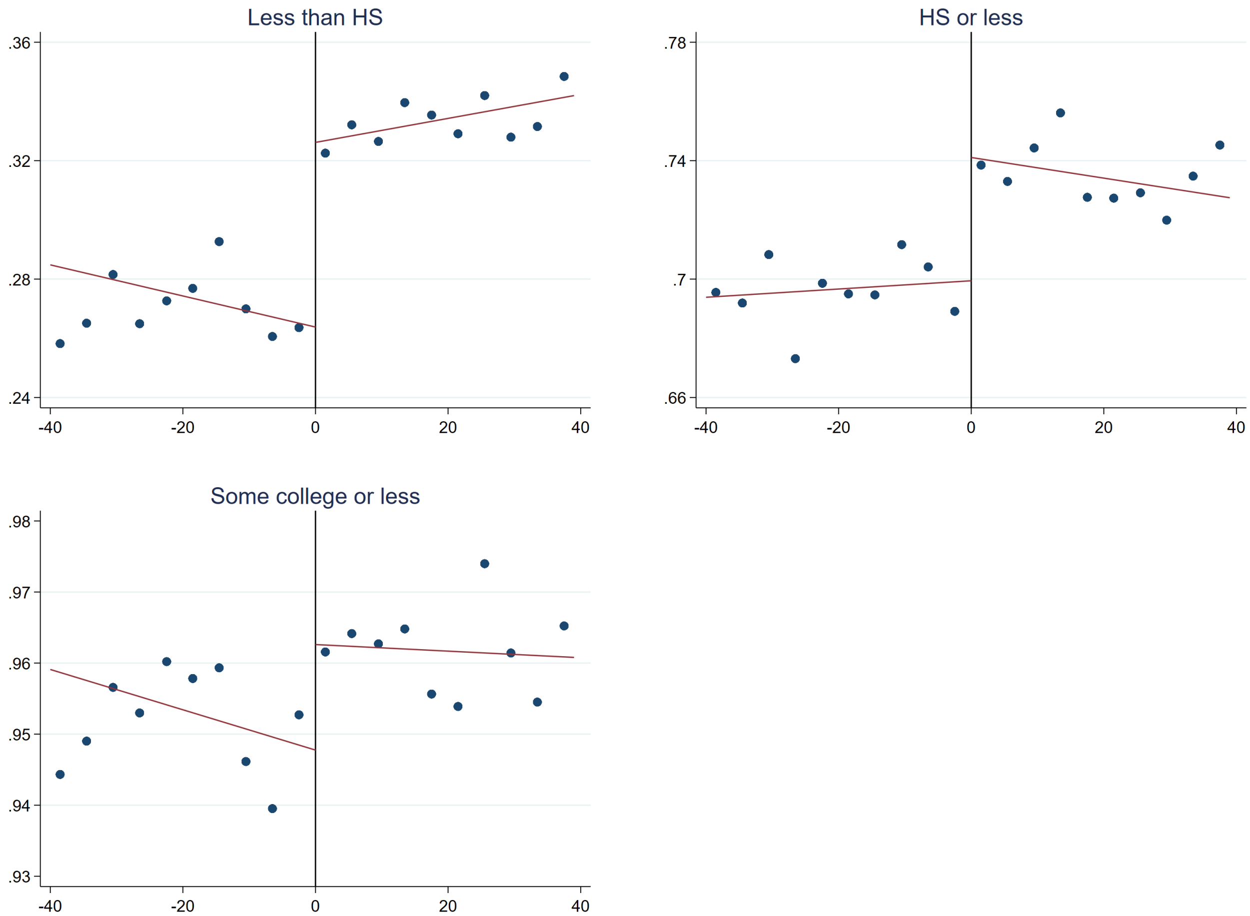
Note. The horizontal axis indicates the normalized mother's day of birth in Eq.1. The dots are averages of mother's educational attainment over 4-day bins. The fitting lines are from interpolation of local linear regressions which use triangle kernel weights and a 40-day bandwidth. The New Mexico sample used for the regressions is described in Table 1.

Figure A8: Birth outcomes by normalized birthday: New Mexico



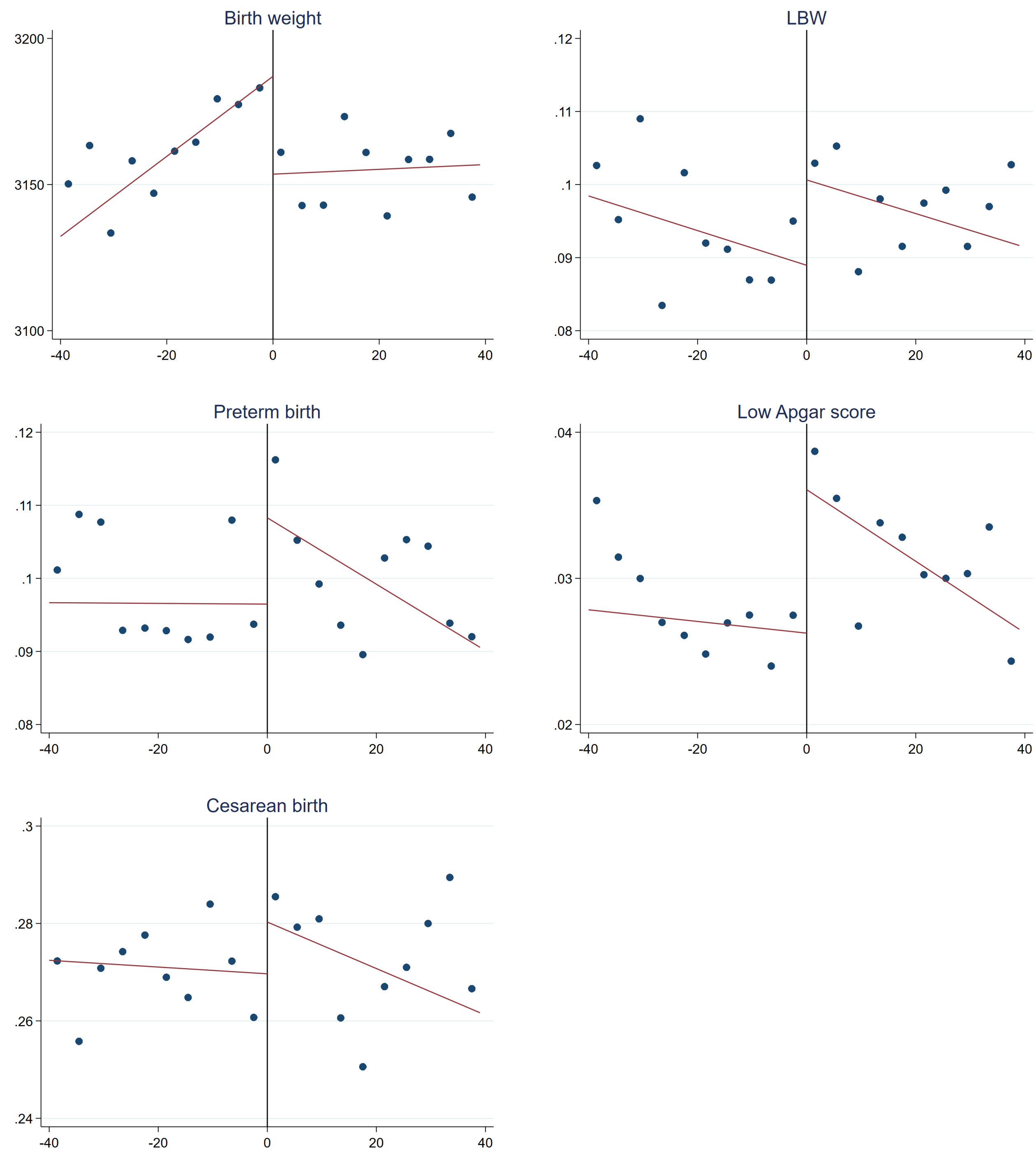
Note. The horizontal axis indicates the normalized mother's day of birth in Eq.1. The dots are averages of the birth outcomes over 4-day bins. The fitting lines are from interpolation of local linear regressions which use triangle kernel weights and a 40-day bandwidth. The New Mexico sample used for the regressions is described in Table 1.

Figure A9: Education at motherhood by normalized birthday: Tennessee



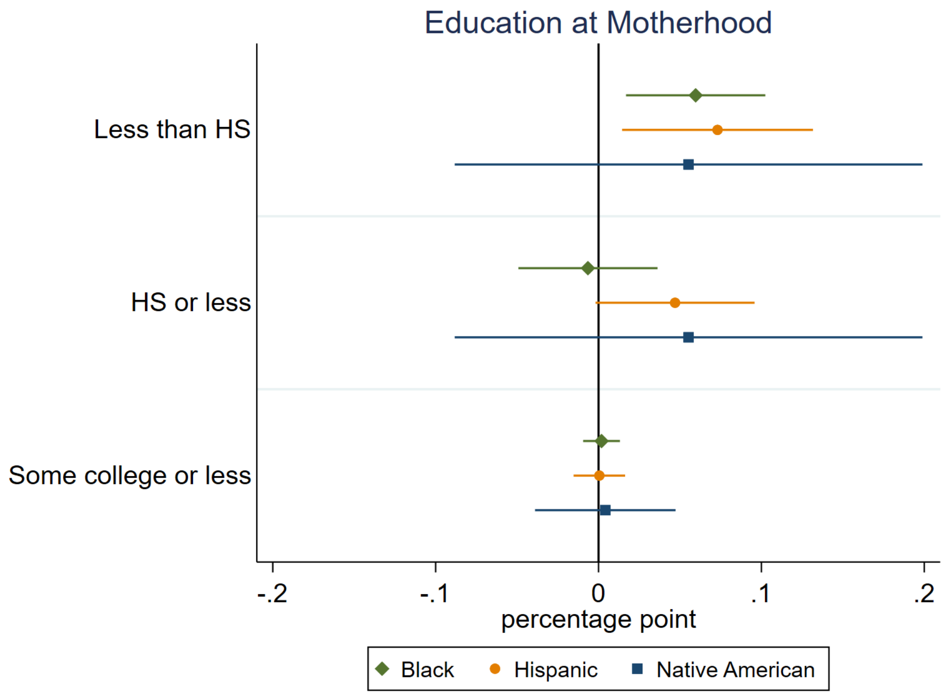
Note. The horizontal axis indicates the normalized mother's day of birth in Eq.1. The dots are averages of mother's educational attainment over 4-day bins. The fitting lines are from interpolation of local linear regressions which use triangle kernel weights and a 40-day bandwidth. The Tennessee sample used for the regressions is described in Table 1.

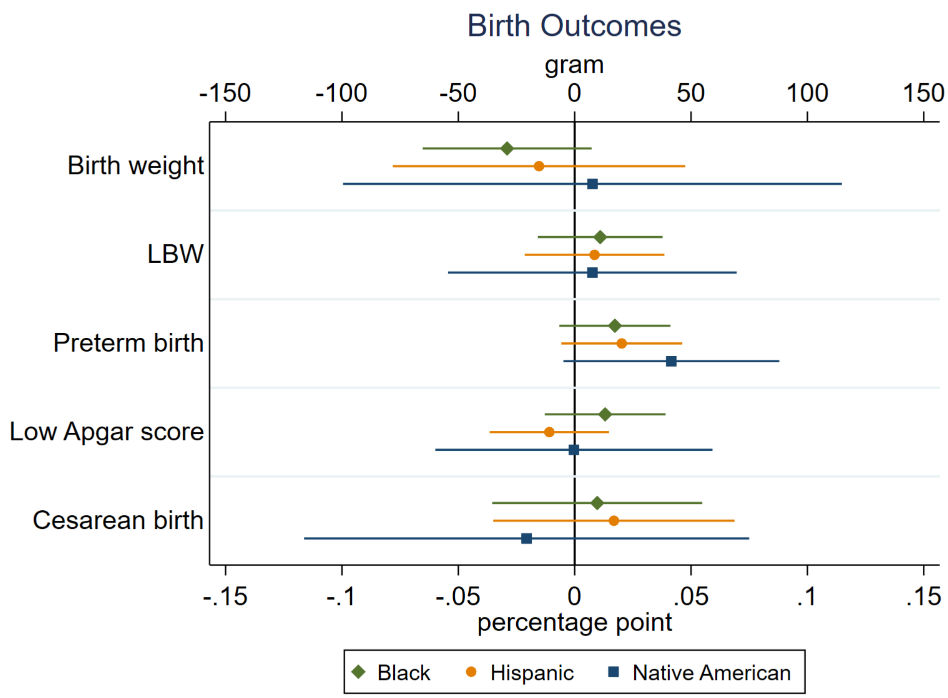
Figure A10: Birth outcomes by normalized birthday: Tennessee



Note. The horizontal axis indicates the normalized mother's day of birth in Eq.1. The dots are averages of the birth outcomes over 4-day bins. The fitting lines are from interpolation of local linear regressions which use triangle kernel weights and a 40-day bandwidth. The Tennessee sample used for the regressions is described in Table 1.

Figure A11: Effects of being born after the eligibility cutoff: Three subgroups of non-White mothers





Note. The figure plots the discontinuity estimates with 95% confidence intervals, when we consider three main subgroups of the non-White mothers examined in Figure 4. We estimate Eq.1 for each subgroup with a 40-day bandwidth and triangle kernel weights. Standard errors clustered by normalized mother's birthday are used to construct the confidence intervals.

Table A1: Descriptive statistics: Samples with 60-day bandwidth

	Pooled	Nevada	New Mexico	Tennessee
Mother's education				
Mother less than HS	0.309	0.341	0.344	0.300
Mother HS or less	0.715	0.762	0.695	0.715
Mother some college or less	0.956	0.962	0.950	0.957
Birth outcomes				
Birth weight	3159.505	3189.997	3149.241	3158.697
Low birth weight (LBW)	0.093	0.081	0.082	0.096
Preterm birth	0.094	0.074	0.070	0.101
Low Apgar score	0.033	0.019	0.052	0.030
Cesarean birth	0.252	0.238	0.162	0.271
Mother's health and health behaviors				
Pre-pregnancy obesity	0.188	0.176	0.161	0.194
Pre-pregnancy overweight	0.404	0.379	0.407	0.406
Pre-pregnancy smoking	0.235	0.087	0.109	0.274
Early prenatal care	0.657	0.591	0.626	0.668
Number of prenatal visits	11.213	10.728	10.361	11.431
Prenatal smoking	0.179	0.074	0.062	0.211
Inadequate weight gain	0.150	0.137	0.146	0.152
Mother's access to health care and nutrition				
Mother on Medicaid	0.752	0.560	0.795	0.762
Mother on private insurance	0.213	0.265	0.116	0.224
Mother on WIC	0.749	0.575	0.739	0.767
Mother's other demographic characteristics				
Mother Non-Hispanic Black	0.253	0.148	0.010	0.311
Mother Hispanic	0.127	0.345	0.623	0.008
Mother Native American	0.029	0.016	0.179	0.001
Mother's age	19.556	19.609	19.490	19.564
Father's characteristics				
Father less than HS	0.238	0.269	0.296	0.224
Father HS or less	0.738	0.756	0.693	0.745
Father some college or less	0.942	0.944	0.935	0.943
Father's age	22.727	22.577	22.446	22.801
Max N	53123	3846	8192	41085

Note. Each state-level sample consists of first-time mothers who are no more than 23 years old and were born 60 days around the first day after the state's school entry date. The pooled sample comes from combining the three state-level samples.

Table A2: Regression discontinuity estimates of age at motherhood

	(1)	(2)	(3)
Panel A: Nevada			
Bandwidth=40	-0.062	-0.011	-0.052
	(0.401)	(0.347)	(0.380)
MSE optimal bandwidth	-0.050	-0.008	-0.041
	(0.407)	(0.355)	(0.397)
Kernel function	Tri	Uni	Epa
Panel B: New Mexico			
Bandwidth=40	0.299	0.258	0.288
	(0.240)	(0.221)	(0.235)
MSE optimal bandwidth	0.341	0.309	0.302
	(0.242)	(0.263)	(0.247)
Kernel function	Tri	Uni	Epa

Note. Each entry reports a regression discontinuity estimate of α_1 from Eq.1, for the first-time mothers in New Mexico or Nevada. The model is estimated with weights from the indicated kernel function. The bandwidth is either 40 days or determined by minimizing the MSE of the RD estimator. The MSE optimal bandwidth takes on a value between 38 and 57. Standard errors clustered by normalized mother's birthday are in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: RD analysis with optimal bandwidth selection: Nevada

	(1)	(2)	(3)	(4)
Less than HS	0.072*	0.061	0.075**	0.071**
	(0.037)	(0.043)	(0.037)	(0.033)
Bandwidth	68	42	68	69
HS or less	-0.016	0.014	-0.0002	-0.021
	(0.034)	(0.033)	(0.032)	(0.034)
Bandwidth	51	59	62	47
Some college or less	-0.003	-0.001	-0.006	-0.002
	(0.017)	(0.017)	(0.019)	(0.017)
Bandwidth	68	53	53	70
Birth weight	-32.393	-17.153	-33.452	-31.477
	(43.847)	(38.633)	(43.903)	(42.748)
Bandwidth	52	54	51	57
LBW	0.000003	0.011	0.002	0.001
	(0.024)	(0.025)	(0.025)	(0.023)
Bandwidth	47	38	43	48
Preterm birth	0.001	0.002	-0.0002	0.003
	(0.022)	(0.023)	(0.022)	(0.023)
Bandwidth	39	30	37	39
Low Apgar score	0.004	-0.007	0.002	0.002
	(0.014)	(0.011)	(0.014)	(0.013)
Bandwidth	47	62	48	51
Cesarean birth	0.032	0.045	0.038	0.031
	(0.041)	(0.037)	(0.039)	(0.041)
Bandwidth	49	41	46	51
Kernel function	Tri	Uni	Epa	Tri
Mother cohort FE	N	N	N	Y
Mother race/ethnicity FE	N	N	N	Y

Note. Each entry reports a bias-corrected estimate of α_1 from Eq.1, for the first-time young mothers in Nevada. The baseline model in column (1) is estimated with triangle kernel weights. When indicated, models in the other columns use different kernel weights or additional controls. The bandwidth is determined by minimizing the MSE of the RD estimator. Bias-corrected standard errors clustered by normalized mother's birthday are in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: RD analysis with optimal bandwidth selection: New Mexico

	(1)	(2)	(3)	(4)
Less than HS	0.059**	0.052**	0.057**	0.060**
	(0.025)	(0.025)	(0.025)	(0.027)
Bandwidth	68	61	65	58
HS or less	0.025	0.029	0.026	0.019
	(0.022)	(0.024)	(0.023)	(0.023)
Bandwidth	60	49	61	62
Some college or less	0.011	0.004	0.011	0.007
	(0.010)	(0.010)	(0.010)	(0.010)
Bandwidth	53	49	51	53
Birth weight	-2.116	3.694	1.244	-3.899
	(30.747)	(33.643)	(31.113)	(31.246)
Bandwidth	50	43	48	49
LBW	0.007	0.012	0.005	0.009
	(0.018)	(0.019)	(0.018)	(0.018)
Bandwidth	46	32	42	45
Preterm birth	0.027	0.012	0.023	0.030
	(0.019)	(0.018)	(0.018)	(0.019)
Bandwidth	37	40	38	37
Low Apgar score	-0.013	-0.008	-0.012	-0.013
	(0.016)	(0.017)	(0.016)	(0.016)
Bandwidth	44	37	42	44
Cesarean birth	0.023*	0.021	0.022	0.025*
	(0.013)	(0.016)	(0.014)	(0.013)
Bandwidth	50	47	49	49
Kernel function	Tri	Uni	Epa	Tri
Mother cohort FE	N	N	N	Y
Mother race/ethnicity FE	N	N	N	Y

Note. Each entry reports a bias-corrected estimate of α_1 from Eq.1, for the first-time young mothers in New Mexico. The baseline model in column (1) is estimated with triangle kernel weights. When indicated, models in the other columns use different kernel weights or additional controls. The bandwidth is determined by minimizing the MSE of the RD estimator. Bias-corrected standard errors clustered by normalized mother's birthday are in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Power analysis

	(1)	(2)	(3)	(4)	(5)
Less than HS					
Power against alternative	1.000	1.000	0.998	0.907	0.437
Effect under alternative	0.109	0.078	0.047	0.031	0.016
HS or less					
Power against alternative	1.000	1.000	1.000	1.000	0.999
Effect under alternative	0.251	0.179	0.107	0.072	0.036
Some college or less					
Power against alternative	1.000	1.000	1.000	1.000	1.000
Effect under alternative	0.335	0.239	0.143	0.096	0.048
Birth weight					
Power against alternative	1.000	0.999	0.778	0.485	0.207
Effect under alternative	-80.000	-60.000	-30.000	-20.000	-10.000
LBW					
Power against alternative	1.000	0.985	0.740	0.453	0.197
Effect under alternative	0.032	0.023	0.014	0.009	0.005
Preterm birth					
Power against alternative	1.000	0.993	0.797	0.503	0.213
Effect under alternative	0.033	0.023	0.014	0.009	0.005
Low Apgar score					
Power against alternative	0.955	0.771	0.417	0.250	0.138
Effect under alternative	0.012	0.008	0.005	0.003	0.002
Cesarean birth					
Power against alternative	1.000	1.000	1.000	0.998	0.723
Effect under alternative	0.088	0.063	0.038	0.025	0.013

Note. The table presents statistical power of two-sided tests with 10% percent size, for a set of hypothesized effects against the null hypothesis of zero impact. The power analysis is based on the baseline specification with a 40-day bandwidth and triangle kernel weights. In addition, the pooled sample in Table 1 is used.