



School starting age, maternal age at birth, and child outcomes[☆]

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ABSTRACT

This paper analyses the effects of maternal age at birth on children's short and long-term outcomes using Finnish register data. We exploit a school starting age rule for identification. Mothers who are born after the school entry cut-off give birth at higher age, but total fertility and earnings are unaffected. Being born after the cut-off reduces gestation and, hence, child birth weight. The effects on birth weight and gestation are rather small, however, suggesting that the long-run impacts may be limited. Accordingly, we find no impacts on longer-term child outcomes, such as educational attainment and adolescent crime rates. Thus, using this source of variation, we find no favorable average effects of maternal age at birth on child outcomes.

1. Introduction

There is a large literature on the association between maternal age at birth and child outcomes. Much of this literature has focused on teen motherhood. But socioeconomic outcomes also tend to be worse for younger mothers in general. For example, our data (from Finland) show that the probability of having a secondary degree is 16 percentage points lower for children born to 20-year-old mothers compared with children born to 30-year-old mothers; moreover, the probability of committing crime in late adolescence is 7 percentage points higher for children born to 20-year-olds compared with children born to 30-year-olds.¹

However, there is reason to believe that a substantial portion of the strong relationship between child outcomes and maternal age at birth can be attributed to selection; young mothers simply have different observed and unobserved characteristics compared with older mothers. To control for selection, a recent paper by [Aizer et al. \(2020\)](#) compares the outcomes of children born to mothers who were sisters. The authors show that this approach reduces the strength of the association between teen motherhood and child future outcomes. For instance, the probability of finishing high-school, which is 16 percentage points lower for children of teen mothers in the cross section, is reduced to a deficit of 6 percentage points when the comparison is between the children of mothers who were sisters. Overall, [Aizer et al. \(2020\)](#) conclude that the effect of being born to a teen mother is negative.² Being born to a younger mother may thus have negative ramifications for children later on in life, which provides an argument for policies aimed at delaying childbearing.

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¹ These outcomes pertain to children of mothers who were born between 1945-1960. Children have secondary degrees if they graduated from a general or vocational upper-secondary school. Children are coded as having committed a crime if they were convicted in district courts at least once between the ages 18-20. [Section 2](#) describes the data in greater detail.

² [Aizer et al. \(2020\)](#) also show that child outcomes improve with mothers' age until the mothers are in their mid 20s.

Our paper revisits the question of the causal impact of maternal age at birth on children's outcomes in the short and long run. We do this using variation induced by school starting age legislation – an approach to identification which is new to this literature. Women who were born after the school age entry cut-off (January 1st) postpone fertility significantly. For example, the probability of giving birth by age 30 is reduced by 3 percentage point for those born after the cut-off. Indeed, being born after the school entry cut-off affects fertility through ages 20 to 35 but leaves total fertility unaffected.

A challenge associated with our identification approach is that the school starting age may have a direct effect on child outcomes. For example, the literature has shown that children who start school at older ages tend to have higher educational attainment (e.g., [Black et al., 2011](#), and [Fredriksson and Öckert, 2014](#)). The increase in parental schooling can directly affect child outcomes by, e.g., increasing the amount of resources available and by increasing the ability to process information regarding the effects of parental behavior ([Aizer and Stroud, 2010](#)). A direct effect of school starting age on child outcomes would violate the exclusion restriction required for instrumental variables.

In the Finnish context, the direct effects of the school starting age appear to be limited: Educational attainment is only weakly affected; moreover, the school entry cut-off does not affect earnings (prior to giving birth) and the characteristics of the father. Nevertheless, we present reduced form effects of being born after the school entry cut-off, since one may be concerned with the validity of the exclusion restriction. With that said, we mainly interpret the results in terms of the impact of maternal age at birth.

Through what mechanisms can maternal age at birth affect the child? We think it is useful to distinguish two types of channels. One channel may be referred to as the direct (or biological) effect of age at birth. This channel would typically suggest that it is better to be born to younger mothers, since medical risk factors increase with mother's age (e.g., [Sutcliffe et al., 2012](#)). Another channel may be referred to as the indirect (or resource) effect of age at birth. This channel would suggest that it is better to be born to older mothers, since they are wealthier, have more life experience, and live in more stable family relationships.

We think that our estimates mainly capture the direct effect of age at birth. The reason for this is that we do not see an impact of the projected school starting age on maternal and paternal earnings prior to giving birth. While mothers born after the school entry cut-off give birth at a higher age, their family income is thus unaffected.

A prerequisite for our analysis is the availability Finnish register data that allow us to track women over their life-cycle and their children into early adulthood. The register data contain information on traditional birth outcomes, such as birth weight and gestation, but also on maternal behaviors, such as smoking. The administrative data also includes later child outcomes such as having a secondary degree, and whether the child has been convicted of any crime. The analysis of how crime is related to maternal age at birth is also new to this literature.

We find that the school entry rule affects the age at family formation. Thus, the age at cohabitation and the age of giving birth both increase as a result of being born after the school entry cut-off. In particular, mothers born just after the school-entry cut-off have their first births when they are around half a year older than those born just before this cut-off. Completed fertility, however, is unaffected, and there is thus no effect on selection into motherhood.

Birth weight and gestation fall as a result of the mother being born after the school-entry cut-off. The birth weight effect is rather small, however: interpreted as the effect of being born to an older mother, the estimates suggest that an increase in the maternal age by 1 year lowers birth weight by 1.4 percent relative to the mean. Also, being born after the school-entry cut-off only has a minor effect on the likelihood of extreme birthweight outcomes. And it does not affect the probability of smoking during pregnancy.

The negative effect on birthweight may translate to worse longer run outcomes; see [Black et al. \(2007\)](#) for evidence. However, given that the birth weight effects are small, it would be surprising to see major effects on the educational attainment of the child. In principle, there could also be a counteracting resource effect of maternal age at birth, but such resource effects are likely small in our context. Consistent with this reasoning we find no impact of the school-entry cut-off on children's long-run outcomes, implying, for example, that educational attainment and teenage crime rates are unrelated to maternal age.

Our paper adds to the large and rather descriptive literature that has examined the association between maternal age and child outcomes. We do this by briefly considering some of the other approaches that have been used in the literature hitherto. In particular, we consider a mother fixed effects approach, comparing siblings born to the same mother at different maternal ages;³ and a maternal sister fixed effects approach, comparing cousins, i.e., children born to mothers who are sisters.⁴ These two approaches allow better control for unobserved factors of mothers giving birth at different ages than conventional ordinary least squares estimates. Nevertheless, one may still be concerned that there is selection into different maternal ages. Indeed, the two fixed effects approaches yield the impression that postponing child birth may benefit children in the long-run. Consistent with such upward bias, and in contrast to our IV-estimates, we also show that the fixed effects approaches yield a positive association between maternal age at birth and spousal earnings.

Although not the main focus of the paper, we also make two contributions to the literature on school entry age policies. First, we provide new estimates on the effect of school entry age on fertility. Previous studies have focused on fertility during school enrolment ([McCrary and Royer, 2011](#), [Black et al., 2011](#), [Johansen, 2021](#)). By contrast, we analyze the effect of the school entry rule on fertility patterns throughout a woman's fertile ages and thus observe the effects on completed fertility. We show that the effects on fertility extend well beyond the school years⁵. Second, we provide new estimates on the relationship between the school entry

³ This approach has been used by several papers in the literature, e.g., [Myrskylä and Felon \(2012\)](#), [Myrskylä et al. \(2013\)](#), [Duncan et al. \(2018\)](#), and [Favara and Perez \(2019\)](#).

⁴ This is the approach used by, e.g., [Geronimus et al. \(1994\)](#), [Rosenzweig and Wolpin \(1995\)](#), [Lopez Turley \(2003\)](#), and [Aizer et al. \(2020\)](#).

age and offspring outcomes.⁶ We find that the school entry age has no intergenerational impact on the children of mothers affected by the entry-age policies.

The remainder of the paper is organized as follows. Section 2 describes the data and provides a descriptive analysis of the association between maternal age at birth and child outcomes. In Section 3, we first present the empirical set-up, and then move on to present our results on the relationship between school entry rules, maternal age at birth, and child outcomes. Section 4 concludes.

2. Data and descriptive analysis

2.1. Data

We use administrative data from Finland, containing information on entire cohorts of women and their children. In the main analysis we focus on cohorts of women born between 1950-59 and 1971-78. The younger birth cohorts (those born 1971-78) are used to shed light on the relationship between maternal age and child outcomes at birth. The reason for focusing on these cohorts is that the Medical Birth Registry is available during 1987-2018. Given this restriction, the earliest cohort we can do the analysis for consists of women born in 1971 (who were 16 in 1987). We also want to cover the upper-range of the fertile age range; this puts an upper limit on those born in 1978 who are 40 years-of-age in 2018. The older cohorts (those born 1950-59) are used to shed light on the longer run effects of maternal age at birth on education and crime for their children.

The Medical Birth Registry has information on children's and mother's outcomes at birth or during pregnancy. The main outcomes used are birth weight, gestation, and information on whether the birth was induced, or performed as a c-section. To measure whether the weight of the child corresponds to gestation weeks, we also calculate the so-called BW-ratio by dividing observed birth weight by median birth weight for gestational age (see, e.g., Voskamp et al. 2014). Moreover, the registry has information on pre-natal outcomes, and we create an indicator for whether the mother smoked during the pregnancy.

Information on demographics, cohabitation, education, and labor market outcomes (for both parents and their children) are obtained from the Finnish Longitudinal Employer–Employee Data (FLEED), available for 1988-2016, or from the so-called FOLK registry for 1988-2019. The educational module of the FOLK registry has information on attained degrees, and when they were obtained. Years of schooling is constructed using information on the length of education for each degree. We also use an indicator for a secondary degree, which gets value one if the individual has obtained an upper-secondary (high school or vocational) degree. For the children, we construct an inactivity-indicator that gets value one if individual is not employed nor enrolled in school. For the parents, we measure real annual earnings (in 2013 Euros) before birth.

The crime information comes from sentence records that cover all sentences in Finnish district courts during 1987-2015. We create an indicator that gets value one if the individual has been convicted in a district court at least once between ages 18-20.

Table 1 shows descriptive statistics for mothers and their children in our main analysis data. Mothers were almost 28 years old when they gave birth to their first child; see column (1). Their children weighed on average 3.5 kg., 10 percent of the babies were delivered by C-section, and 14 percent of mothers smoked during pregnancy; see column (2). The final column shows longer-run child outcomes (available for children of mothers born 1950-59). It shows that 82 percent of the children had attained a secondary degree, that 18 percent of the children were “inactive” (i.e., not in employment or education) at age 21, and that 7 percent of children committed a crime when they were aged 18-20.

2.2. The association between child outcomes and maternal age at birth

A large (and mainly descriptive) literature has documented that children born to young mothers tend to have worse outcomes from birth to adulthood. A concern with this literature is that the results reflect selection: different type of mothers give birth to children at different ages.

We begin by replicating these descriptive results with our data (from Finland). We use three different, and commonly used, approaches: Ordinary Least Squares (OLS), Maternal sister (i.e., cousin) fixed effects (FE), and Mother (i.e., sibling) fixed effects. The sample is restricted to mothers that have at least 2 children, who were born between 1960-1980 (short-run outcomes) or between 1945-1960 (long-run outcomes).⁷ To describe the relationship between the outcomes/characteristics of individual, i , who is connected to a mother, j , we use variants of the following regression:

$$y_{ij} = \sum_k \beta_k MAB_i^k + \sum_l \gamma_l BO_i^l + \sum_m \alpha_m YOB_j^m + \varepsilon_{ij}, \quad (1)$$

where MAB denotes maternal age at birth indicators (2-year intervals), BO birth order indicators, and YOB maternal year of birth indicators. Notice that mother fixed effects hold constant maternal year of birth. For comparability, we therefore control for YOB when using OLS and maternal sister FE.⁸

⁵ Skirbekk et al. (2004) investigate the relationship between a woman's month of birth, timing of marriages, timing of births and completed fertility using Swedish data. They show that women born in the early part of the year had higher age at motherhood, but there was no difference in completed fertility.

⁶ McCrary and Royer (2011) use school entry age policies to identify the effect of education on infant health. Their reduced form estimates suggest that being born after the school entry cut-off has no impact on infant outcomes.

⁷ Identification using Mother FE requires that the mother has at least two children. Since we do not want differences in sample composition to contaminate the comparison across different estimation approaches, we stick to this sample restriction throughout the analysis in this sub-section.

Table 1
Descriptive statistics.

| | Mother characteristics | Birth outcomes | Long-run child outcomes |
|---------------------------------------|------------------------|------------------|-------------------------|
| Mothers born | 1971-78 (1) | 1971-78 (2) | 1950-59 (3) |
| Mother's age at 1 st birth | 27.95 (5.27) | | |
| Mother's age at birth | | 29.84 (5.31) | 27.59 (5.24) |
| Years of schooling | 14.34 (2.78) | | |
| Earnings € (before 1st birth) | 20976.19 (16873.97) | | |
| # Children | 2.34 (1.27) | | |
| Have a spouse | 0.98 | | |
| Spousal years of schooling | 13.44 (2.90) | | |
| Spousal earnings € (before 1st birth) | 27947.75 (23852.60) | | |
| Maternal mother secondary ed. | 0.64 | | |
| Maternal father secondary ed. | 0.61 | | |
| Birthweight (grams) | | 3510.29 (571.87) | |
| Gestation (days) | | 277.82 (13.27) | |
| Induction | | 0.17 | |
| C-section | | 0.10 | |
| Mother smokes | | 0.14 | |
| Child secondary ed. | | | 0.82 |
| Child inactive | | | 0.18 |
| Any crime age 18-20 | | | 0.07 |
| #observations | 187,464 | 439,582 | 616,044 |

Notes: The table reports means and standard deviations (within parentheses). Column (1) is representative of mothers born 1971-78. Column (2) is representative of all children to mothers born 1971-78. Column (3) is representative of all children to mothers born 1950-59. Inactivity is defined as not being employed, nor being enrolled in education at age 21 (or the first year a child can be found in the FLEED data). Earnings are measured in real 2013 Euros.

Figure 1 shows how a number of child outcomes relate to maternal age at birth. The upper left panel pertains to birth weight. The black circles (reporting OLS-estimates) show that higher maternal ages tend to be associated with better child outcomes than teen motherhood. Moreover, the descriptive relationship between maternal age and birth outcomes follows an inverse u-shaped pattern as documented in earlier studies.⁹ Children born to very young or very old mothers tend to have the lowest birth weight. When we include cousin or sibling fixed effects in the regression, the relationship becomes linearly decreasing, indicating that the lower birth weight for children of young mother is largely driven by selection. The mother FE estimates suggest that an increase in maternal age at birth by a year is associated with a reduction in birth weight of 12 grams.

The upper right panel illustrates how gestation relates to maternal age at birth. The pattern of these estimates is broadly similar to the pattern for birth weight. The OLS estimates show an inverse u-shape, while the other two approaches suggest that gestation falls with mother's age at birth – at least beyond age 20.

The bottom panel of Figure 1 shows the relationship between maternal age at birth and two outcomes for children observed in the run – the probability of having at least an upper-secondary degree, and the propensity to commit any crime. The OLS relationships suggest strong improvements in later outcomes for children of older mothers. A child born to a 30-year-old is 5 percentage points less likely to have been sentenced for a crime than a child born to a 22-year-old, for example. Controlling for maternal sister or mother fixed effects, weakens the correlations. Nevertheless, even with mother fixed effects, the estimates suggest that children born to older mothers have higher educational attainment and are less likely to have committed a criminal offence. It is noteworthy that, when crime is the outcome, the mother fixed effects estimates are significantly different from the maternal sister fixed effects estimates, suggesting that the latter suffers from more selection on maternal age.

Figure 2 examines whether selection on maternal age is a potential concern. Concretely, we investigate whether education and earnings of the spouse are correlated with mother's age at birth. Both OLS and maternal sister FE estimates suggest that older mothers are systematically different than younger mothers, since they have partnered up with more educated spouses (see left panel). Since there is basically no variation in paternal education within a mother, the maternal fixed effects deal with such selection.

The concern for mother fixed effects estimates is selection on time-varying unobserved characteristics. Thus, if a positive innovation in family earnings cause mothers to postpone child birth, mother FE estimates of maternal age at birth are likely positively biased. The right-hand panel reports results that use father's earnings one year before childbirth as the outcome. The OLS estimates (black circles) indicate that spouse's earnings are positively correlated with age-at-motherhood. When maternal sister and mother fixed effects are included, this correlation remains. That earnings grow with maternal age at birth can reflect selection – higher earning couples (both in terms of earnings levels and earnings growth) are more likely to have children at older ages – as well as a causal (resource) effect

⁸ Notice that (child year of birth) = (maternal age of birth) + (maternal year of birth). This implies that maternal age at birth also captures any child year of birth effects holding constant maternal year of birth. For child outcomes, it does not really matter whether we control for child year of birth or maternal year of birth. Moreover, note that the results are robust to controlling for completed family size, which is also held constant using mother FE.

⁹ See, e.g., [Barbuscia et al. \(2020\)](#), [Hvide et al. \(2021\)](#), and [Wang et al. \(2020\)](#).

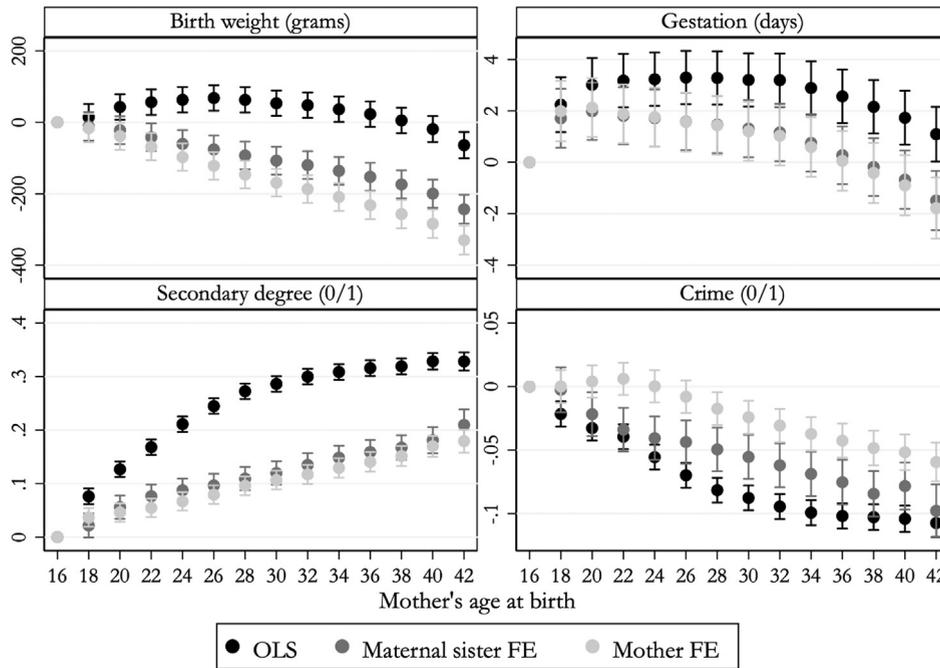


Figure 1. Child outcomes and mother’s age at birth *Notes:* The figures plot the coefficients on mother’s birth age dummies (2-year intervals) using three different specifications: OLS, Maternal sister FE regressions and Mother FE regressions, along with 90 percent confidence intervals. “Maternal sister FE” refers to a specification where we have indicator for mother’s sister FE (i.e., child’s cousin FE). OLS and Maternal sister FE regressions also control for birth year dummies for the mother, and child birth order dummies. Mother FE regressions include child birth order dummies. Birth weight and gestation regressions include children whose mothers were born between 1960-1980 and had at least two children. Regressions with secondary degree and any crime as outcomes are for children whose mothers were born between 1945-1960 and had at least two children.

of age – older mothers tend to partner with older spouses and earnings grow with age.¹⁰ Thus, even when comparing children born to same mother, the one that was born later, had access to greater resources.

That spousal characteristics are systematically different for older and younger mothers pose a challenge for the fixed effects approaches. That spousal education grows with age is a clear sign of selection potentially conflating the maternal sister fixed effects estimates. That spousal earnings are higher for older mothers may also reflect selection. If so, maternal fixed effects estimates are likely biased upwards.¹¹ In the sequel, we show that the education and (pre-birth) earnings of the mothers as well as their spouses are unrelated to the school starting age rule for the cohorts born in the 1970s. Our identification strategy, described in the next section, may thus allow for a cleaner interpretation of the impact of maternal age at birth (subject, of course, to the exclusion restriction).

3. The school entry cut-off, maternal age at birth, and child outcomes

3.1. Specification

We are mainly interested in how the maternal age at birth (*MAB*) affects child outcomes (*y*):

$$y_{ic} = \alpha_c + \beta MAB_{ic} + f(DoB_{ic}) + \epsilon_{ic} \tag{2}$$

where *DoB* denotes day of birth (of the mother), α_c is a cohort (*c*) fixed effect, and y_{ic} is the outcome for a child born to a mother from cohort *c*. A cohort is defined as running from July to June.

Since maternal age at birth is endogenous, we use variation coming from the school starting age legislation. With *DoB* normalized to zero at the school entry cut-off (1st of January), the instrument is defined as $Z_{ic} = 1\{DoB_{ic} \geq 0\}$.

The reduced form corresponding to equation (1) then is:

$$y_{ic} = \theta_c + \pi Z_{ic} + g(DoB_{ic}) + \epsilon_{ic}, \tag{3}$$

where $g(DoB_{ic})$ is a control function in the assignment variable (day of birth).

¹⁰ Note also that spousal earnings grow with maternal age at birth, holding constant the age of the spouse.

¹¹ Mother FE estimates may also be upward biased if complications surrounding the birth and development of the older sibling cause the couple to postpone having a second child.

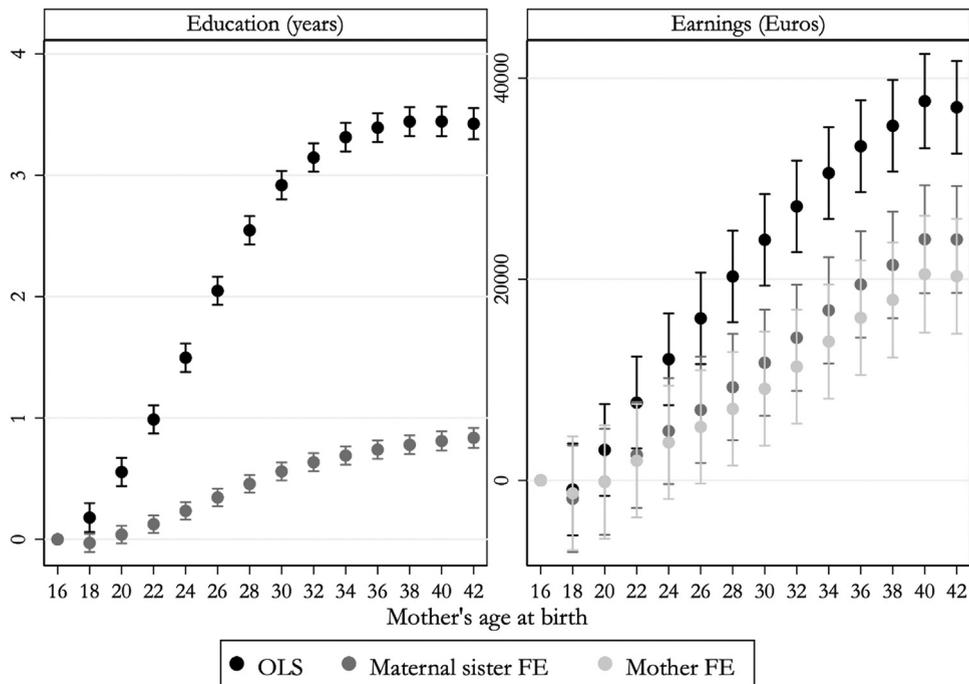


Figure 2. Spousal characteristics and mother's age at birth *Notes:* The figures plot the coefficients on mother's birth age dummies (2-year intervals) in three different regressions: OLS, Maternal sister FE regressions and Mother FE regressions, along with 90 percent confidence intervals. "Maternal sister FE" refers to specification where we have indicator for mother's sister FE (i.e., child's cousin FEs). OLS and Maternal sister FE regressions also control for birth year dummies for the mother, and child birth order dummies. Mother FE regressions include child birth order dummies. Spousal earnings are measured in real 2013 Euros. Regressions include children whose mothers were born between 1960-1980 and had at least two children.

Since the school entry cut-off may affect other maternal outcomes (e.g., educational attainment and earnings) which may have a direct effect on child outcomes, we mainly present reduced form results. Below we show that education, earnings, and the characteristics of the partner, are generally unaffected by the cut-off. We therefore think maternal age at birth is the main mechanism, even though we cannot strictly rule out other mechanisms. We mostly estimate [equation \(3\)](#) using a 2nd order parametric control function in day-of-birth on a window that roughly corresponds to the optimal bandwidth (in the [Calonico et al. 2014](#) sense, given the 2nd order control function). We have also estimated all outcome equations via local linear regression with bandwidths chosen so as to minimize mean-squared error; these results are reported in the Appendix, and the results do not change appreciably.

We first examine a wide range of outcomes for potential mothers, that is, their years of schooling, earnings, and the probability to give birth at a given age using the specification in (2). We then examine the outcomes of children born to these mothers, both at birth and in the longer run. Note that compliance with the school starting age legislation is very high in Finland. Only 5.9 percent of students leave compulsory school outside their normal age range.¹²

3.2. Validity of the research design

A fundamental assumption in the regression discontinuity design is that individuals cannot exactly manipulate the assignment variable, which implies that they are as good as randomly assigned relative to the school-entry cut-off. If so, there should be no shifts in the distribution of birth dates around this threshold;¹³ analogously, pre-determined covariates should be balanced at the cut-off.

The assignment variable in the RD-design is the day of birth of mothers. [Figure 3](#) shows the distribution of birth dates among mothers born 1971-78. There is a dearth of births around Christmas which has to do with C-sections not being planned then.¹⁴ Other than that, the number of births evolve smoothly through the cut-off. Consequently, the [McCrary \(2008\)](#) test does not reject smoothness of the distribution of mother's birth dates around the threshold.¹⁵

¹² This number comes from the cohorts of mothers born 1975-78. Unfortunately, we do not observe the school starting age in our data. But since grade retention is rarely practiced in Finnish schools (in 2016, 0.3 % of compulsory school students repeat a grade) we infer that 95 percent is a ballpark estimate of the compliance with the school starting age legislation.

¹³ [Huang et al. \(2020\)](#) show that Chinese mothers systematically time births relative to the school entry cut-off.

¹⁴ [Jacobson et al. \(2020\)](#) document a similar pattern using Californian data.

¹⁵ The discontinuity estimate is -0.020 (standard error: 0.021). Accounting for the fact that the assignment variable is discrete, does not alter this conclusion. [Frandsen \(2017\)](#) shows that the McCrary test tends over-reject a true null hypothesis when the assignment variable is discrete. This over-rejection problem is likely small in the current context, as day-of-birth is a finely grained assignment variable.

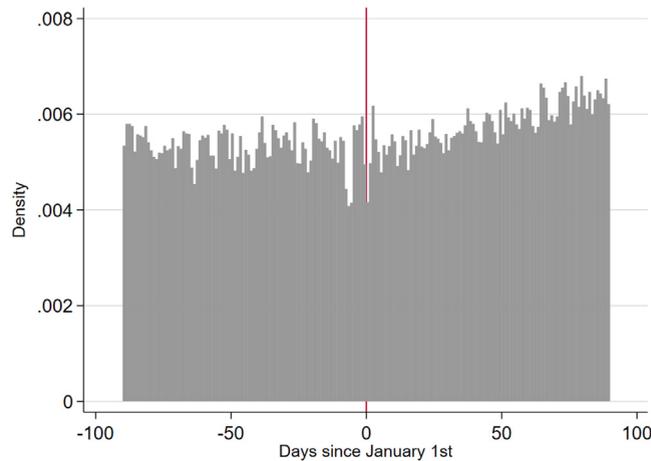


Figure 3. Distribution of birth dates *Notes:* The figure plots the distribution of all births to mothers born in 1971-1978 in Finland (90 days before/after the January 1st cutoff).

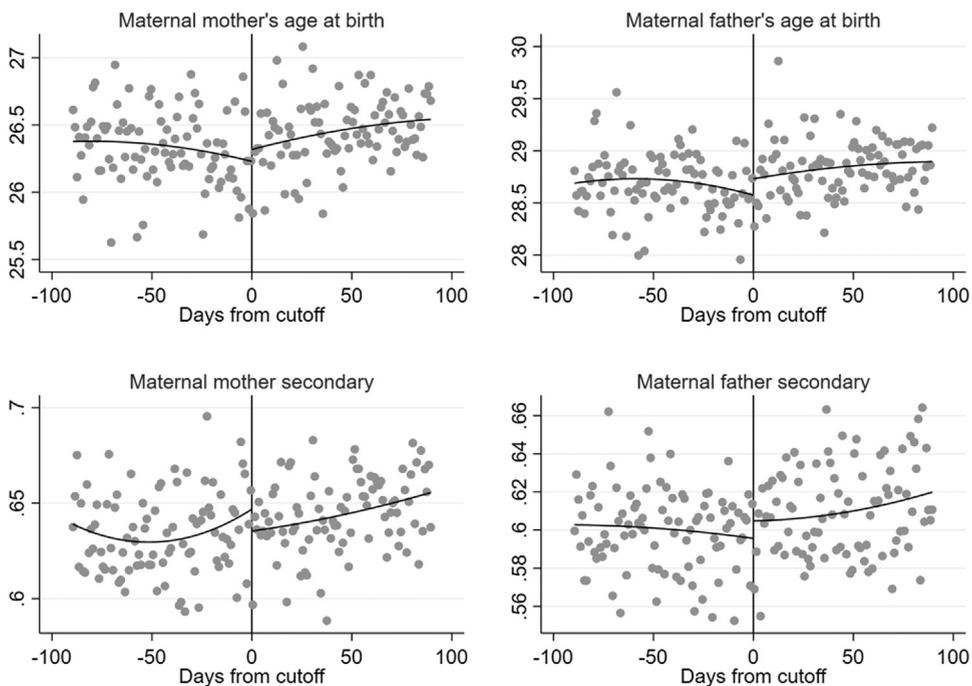


Figure 4. Mother’s birth dates and pre-determined characteristics *Notes:* The sample includes the *parents* of all mothers born 90 days before/after January 1st in 1971-1978 in Finland. Dots are sample averages by day of birth. The solid lines represent a second order polynomial regression.

Another way to check the validity of the research design is to investigate whether background characteristics change at the threshold. Figure 4 examines whether there is a discontinuity in the characteristics of the parents of the mothers around the cut-off. It shows that there are no differences in the age of birth of the parents of the mothers; the same conclusion applies to their education levels.¹⁶

Table 2 provides a slightly more detailed analysis of whether baseline covariates are balanced for the mothers born 1971-78.¹⁷ The first two columns examine whether the parents are present in the data; a non-present parent is basically equivalent to the parent not being alive. As the first two columns show, there are no jumps at the cut-off. The next two columns look at parental age at birth – age at birth is also balanced at the threshold. The final two columns examine whether parental education is balanced. Education

¹⁶ Analogous analyses for *all women* (rather than *all mothers*) born 1971-78 show that there are no discontinuities in the number of births and baseline characteristics around the school entry threshold.

¹⁷ Table A6 reports on balancing in the sample of mothers born in the 1950s.

Table 2
Balance of pre-determined characteristics at cut-off.

| | Parent observed in data | | Parental age at birth | | Parent has secondary ed. | |
|--------------------|-------------------------|------------------|-----------------------|------------------|--------------------------|------------------|
| | Mother | Father | Mother | Father | Mother | Father |
| Born after cut-off | 0.002 (0.002) | 0.005 (0.003) | 0.101 (0.105) | 0.168 (0.118) | -0.011 (0.010) | 0.010 (0.010) |
| Mean dep. var. | 0.987 | 0.970 | 26.335 | 28.692 | 0.634 | 0.600 |
| # observations | 89,876 | 89,876 | 88,756 | 87,248 | 87,890 | 84,291 |

Notes: The table relates the characteristics of the *parents* of the mothers to an indicator of the mother being born after the school entry cut-off. Sample includes the parents of mothers who were born in 1971-1978. Estimates were obtained using a second-order polynomial in the assignment variable (day of birth) and observations 90 days before and after cutoff date. Regressions include cohort fixed effects (July/June). Table A1 in appendix report the analogous table for the local linear specification.

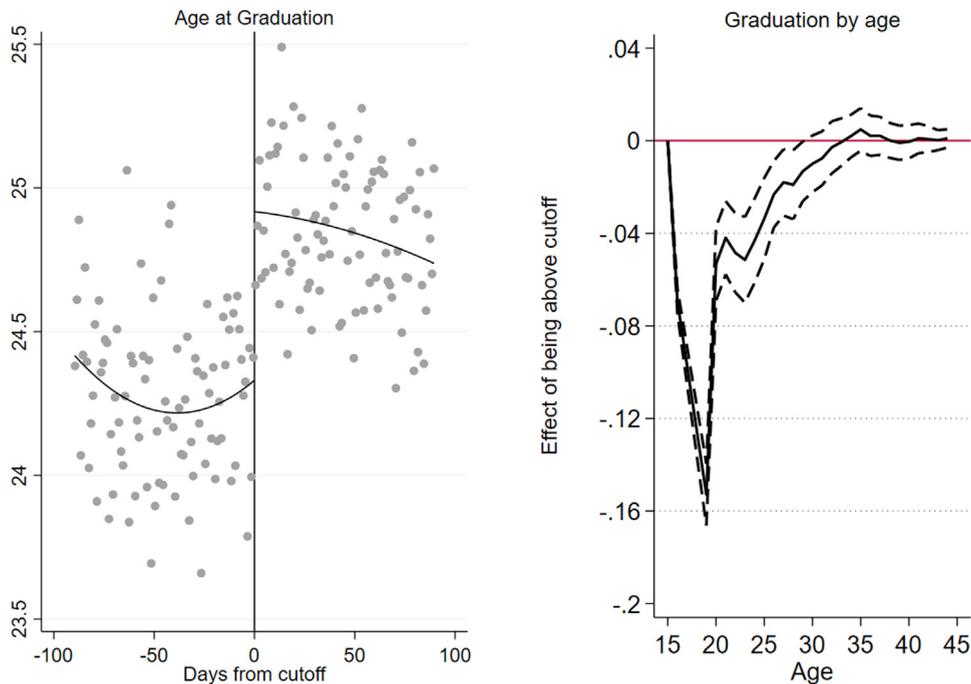


Figure 5. Age at graduation and school entry age cutoff Notes: The left-hand panel plots mean age at graduation by day of birth, with a regression line obtained from fitting a 2nd order polynomial separately on each side of the threshold. The RD-estimate of being born after the cut-off is 0.680 (standard error: 0.136). The right-hand panel plots the estimated effects of being born after cutoff on the probability to graduate from school by age that were obtained from separate polynomial regressions. The specification only includes births 90 days before and after the school starting age cut-off (January 1st).

for the maternal parents is the same on both sides of the school entry cutoff. Taken together, there is no evidence of any systematic differences for mothers around the threshold.

3.3. Effects of being born after the school entry cut-off on outcomes for mothers

How does the school entry legislation affect maternal outcomes? We begin by illustrating the process through which the school entry cut-off affects maternal age at birth. We thus show how the cut-off affects age of school completion, age of cohabitation, and age of motherhood.

Figure 5 examines how being born after the school entry cut-off affects the age of school completion.¹⁸ The left-hand panel illustrates that those who are born after the cut-off – who start school when they are one year older – leave school when they are 0.68 years older; the average school-leaving age increases from around 24.3 to almost 25 years-of-age. The right-hand panel shows

¹⁸ The outcome is constructed using information on when the individuals obtained their highest post-compulsory educational degree. For those with no post-compulsory degrees, the time of graduation is the expected time of graduation from compulsory school (June in the year when turning 16).

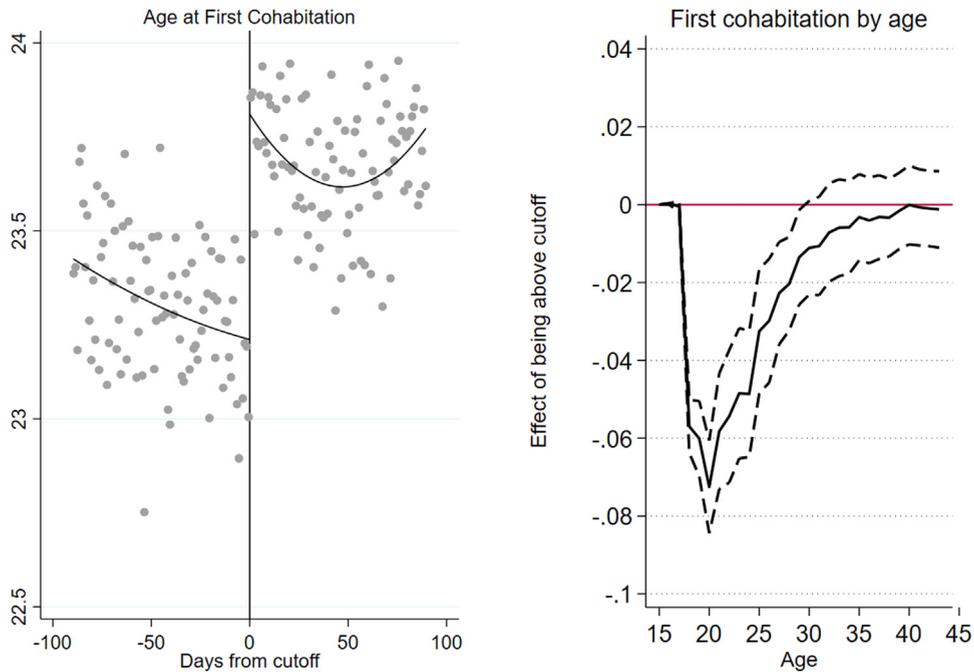


Figure 6. Age at first cohabitation and school entry age cutoff *Notes:* The left-hand panel plots mean age at cohabitation by day of birth, with a regression line obtained from fitting a 2nd order polynomial separately on each side of the threshold. The RD-estimate of being born after cut-off is 0.616 (standard error: 0.082). The right-hand panel plots the estimated effects of being born after cutoff on the probability of cohabitation by age that were obtained from separate polynomial regressions. The specification only includes births 90 days before and after the school starting age cut-off (January 1st).

that being born after the cut-off lowers the probability of leaving school at younger ages, in particular at age 16 (when compulsory school ends) and age 19 (when upper-secondary school ends). The differences in the graduation age persist into the 20s; by age 30 there are no differences left, however.

Since differences in the school entry age translate into differences in the school-leaving age, it is likely that the school entry age affects partnership and family formation. Figure 6 examines the impact on when the first cohabiting relationship is formed. Being born after the school entry cut-off thus only affects the timing of partnership formation, and individuals form their first cohabiting relationship at 23.8 years of age rather than at 23.2 years-of-age, as shown in the left-hand panel. The right-hand side shows how the probability of cohabiting by age is affected by the school-entry cut-off, while the right-hand side shows the mean impact on age at first cohabitation. The right-hand panel illustrates a stronger impact at younger ages, that the effect persists when the mothers are in their 20s, and that there is no differential selection into partnerships in the longer run.

Figure 7 turns to age at motherhood. The left-hand-side of Figure 7 examines how maternal age at birth is affected by the school starting age.¹⁹ It illustrates a clear jump in maternal age at birth at the school entry cut-off. Just to the right side of the cut-off, age at birth is 0.42 years higher than just to the left. The right-hand-side shows how the distribution of maternal ages at first birth changes at the school-entry cut-off. It illustrates that there is no effect for young females (below age 20), that being born after the school entry cut-off affects fertility through ages 20 to 35, but leaves total fertility unaffected (the effect dissipates by age 40). Since total fertility is unaffected by the school starting age, there is no effect on selection into motherhood.

Figures 6 and 7 provide clear evidence that being born after the school entry cut-off postpones family formation: age at cohabitation and age at birth increase. This raises the question of whether any effects on the child is due to age at birth or age at partnership formation. Strictly, we cannot tell these two potential impacts apart, although we find it most plausible that any effect on the child is driven by age at birth.²⁰

Table 3 examines how a set of maternal outcomes are related to the school entry cut-off. Columns (1) and (2) show that age at first birth and average age at birth increases by 0.46 years and 0.42 years, respectively.^{21,22} Table 3 also illustrates that there is no effect

¹⁹ Since total fertility is unaffected by mother’s age at school start we include all births at this stage.

²⁰ Conditional on age at birth, age at cohabitation could matter for child outcomes, if longer relationships prior to first birth (say) improve child outcomes.

²¹ The effect of the school entry cut-off on average age at birth is slightly lower than the effect on average age at first birth since there is a small reduction in birth-spacing for those born after the cut-off. In particular, the parametric estimates show that: the effect on average age at birth (0.42) = the effect on age at first birth (0.46) – the effect on birth spacing (0.04).

²² Age at first cohabitation and age at graduation (with highest degree) increase with 0.62 and 0.68 years, respectively.

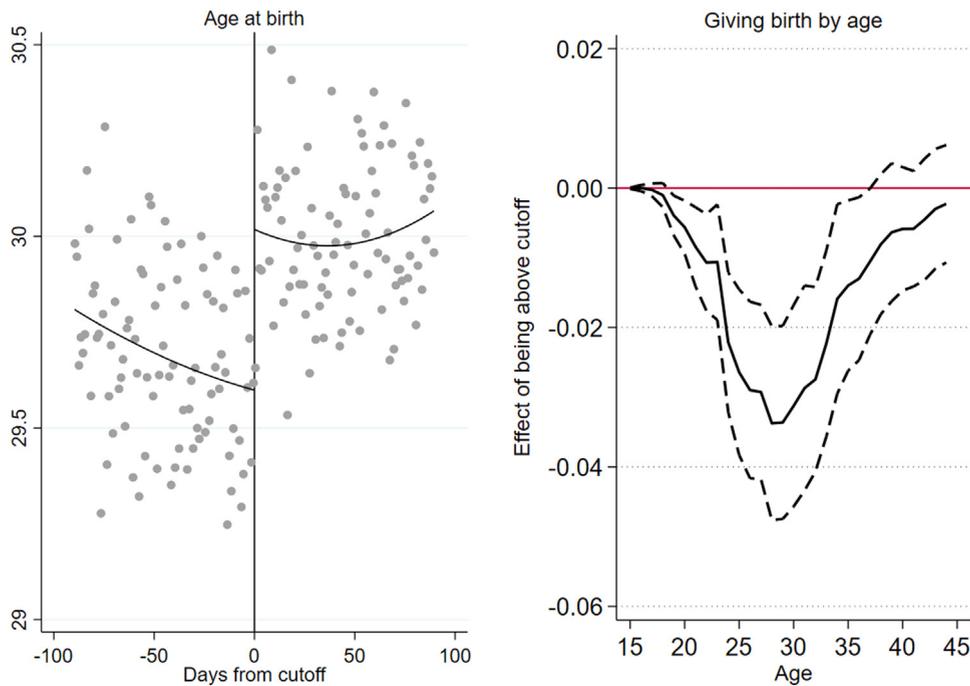


Figure 7. Fertility, age at birth, and school entry age cutoff *Notes:* The left-hand panel plots mean age at birth by day of birth, with a fit regression line obtained from fitting a 2nd order polynomial separately on data from each side of the threshold. The RD-estimate of being born after cut-off is 0.419 (standard error: 0.071). The right-hand panel plots the estimated effects of being born after cutoff on the probability to give birth by age that were obtained from separate polynomial regressions. The specification only includes births 90 days before and after the school starting age cut-off (January 1st). The corresponding figure using local linear regression is reported in appendix (Figure A1).

Table 3
Effect of being born after school entry on outcomes for mothers.

| Variable | (1) Age at 1 st birth | (2) Age at birth (all births) | (3) Spacing | (4) # Children | (5) Years of schooling |
|--------------------|--|----------------------------------|----------------------|----------------------------------|--|
| Born after cut-off | 0.459*** (0.108) | 0.419*** (0.071) | -0.040 (0.054) | -0.002 (0.026) | 0.077 (0.057) |
| Mean | 27.778 | 29.691 | 3.236 | 1.793 | 14.207 |
| # observations | 89,876 | 210,712 | 71,034 | 89,876 | 89,876 |
| | (6) Earnings before 1 st birth | (7) Have spouse | (8) Age of spouse | (9) Spousal yrs. of schooling | (10) Spouse earnings before 1 st birth |
| Born after cut-off | -74.804 (347.429) | 0.000 (0.003) | -0.051 (0.128) | 0.058 (0.060) | 119.933 (499.518) |
| Mean | 20709.817 | 0.904 | 30.273 | 13.358 | 30337.304 |
| # observations | 89,095 | 89,876 | 87,319 | 87,319 | 85,720 |

Notes: Sample includes women who were born in 1971-1978. Estimates were obtained using a second order polynomial in the assignment variable (day of birth) and observations 90 days before and after cutoff date. Regressions include cohort fixed effects (July/June). Table A2 in appendix reports the analogous table for the local linear specification. Earnings are measured in real 2013 Euros.

on birth spacing or total fertility, as shown by columns (3) and (4). Moreover, there is no effect on years of schooling for the cohorts of mothers born 1971-78; this result contrasts with many other countries where educational attainment is typically positively affected by the school starting age (e.g. Black et al, 2011, and Fredriksson and Öckert, 2014). Similarly, the level of earnings is unrelated to the school-starting age; the estimated impact (-0.36% relative to the mean) and far from being statistically significant.

Columns (7)-(10) illustrate that spousal characteristics are unrelated to the school entry age. Again, the estimated impacts are small: the effect on spousal earnings corresponds to 0.43% relative to the mean, for example. This reinforces the conclusion that the school-entry age legislation only affects maternal age at birth in our study cohorts.

3.4. Child birth outcomes

We have documented above that the school entry rule affects age-at-motherhood. As discussed in the introduction, age-at-motherhood can affect child outcomes in various ways. On the one hand, birth complications increase with advanced maternal

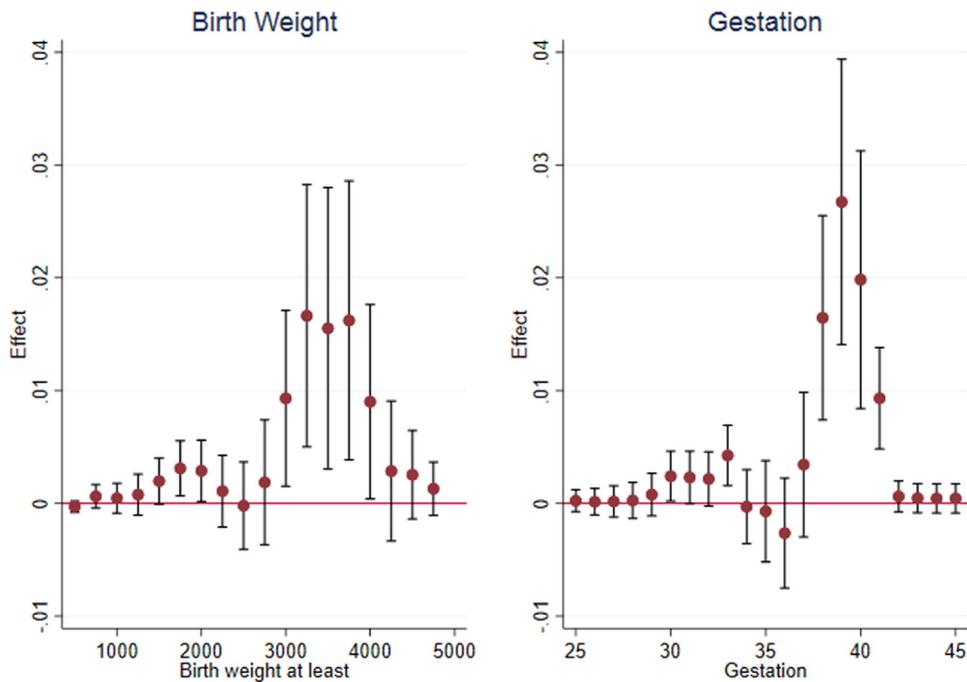


Figure 8. Effect of the school-entry rule on the probability to give birth by birth weight and gestation interval *Notes:* The figure plots the estimates from separate regressions of the probability to have birth weight of at least the amount on the x axis (left-hand-side figure) or gestation (weeks) of at least the amount on the x axis (right-hand-side figure) on an indicator for the mother being born after the school entry cut-off (January 1st). Sample includes all women born 90 days before or after school entry cutoff during 1971-78. The regression control for a second order polynomial in the assignment variable (day-of-birth) and cohort fixed effects (July/June). [Figure A2](#) in appendix reports the analogous figure for the local linear specification.

Table 4
Effect of mother being born after school entry cutoff on the child’s birth outcomes.

| | Mother’s age at birth | Birthweight (grams) | Gestation (days) | BW-ratio | Induction | C-section | Mother smokes |
|--------------------|-----------------------|-----------------------|----------------------|-------------------|-------------------|------------------|-------------------|
| Born after cut-off | 0.419*** (0.071) | -21.015*** (7.683) | -0.515*** (0.179) | -0.003 (0.002) | -0.004 (0.005) | 0.001 (0.004) | -0.004 (0.005) |
| Mean | 29.691 | 3512.219 | 277.886 | 0.998 | 0.169 | 0.100 | 0.143 |
| # observations | 210,712 | 210,473 | 210,019 | 209,966 | 204,443 | 210,712 | 210,712 |

Notes: Sample includes all children to women born in 1971-78. Estimates were obtained using a second-order polynomial in the assignment variable (day of birth) and observations 90 days before and after cutoff date. The regressions include cohort fixed effects (July/June). BW-ratio is defined as observed birth weight divided by the median birth weight for gestational age (see, e.g., [Voskamp et al. 2014](#)). [Table A3](#) in appendix reports the analogous table for the local linear specification.

age. On the other hand, older mothers have access to more resources, and can also have more patience and better parental practices. [Figure 8](#) shows how the distribution of birth weights and gestation, respectively, is affected by the school entry rule. Regarding birth-weight, there is a shift in the number of births from around 4000 grams to 3500 grams. Similarly, the school-entry cut-off reduces gestation, and the effects are concentrated around full-term (40 weeks).

[Table 4](#) goes on to show how a wider set of birth outcomes depends on whether the mother is born after the school entry cut-off.²³ As already noted, birthweight and gestation fall as a result of the mother being born after the school entry cut-off. The table also shows that the probability of the birth being induced or the result of a C-section is unaffected by the cut-off; there is, thus, nothing to suggest that gestation periods are cut shorter, using C-sections or inductions, for older mothers. Moreover, birth-weight by gestation is unaffected; see column headed “BW-ratio”, which suggests that the decrease in birthweight is driven by the reduction in gestation, and that fetal growth is unaffected by maternal age at birth. The final column of [Table 4](#) suggests that health behaviors, measured here using an indicator of whether the mother smokes during pregnancy, is unaffected by being born after the cut-off.

Our preferred interpretation of the results in [Table 4](#) is that an increase in the maternal age at birth causes a reduction in birth-weight. Women born earlier in the calendar year are 0.4 months older when giving birth and give birth to 21 grams lighter children.

²³ The results do not change if we focus on first-born children only.

Table 5
Effect of mother being born after school entry cutoff on child's long-term outcomes.

| | Mother's age at birth | Secondary degree | Inactivity | Any crime |
|--------------------|-----------------------|-------------------|-------------------|-------------------|
| Born after cut-off | 0.211*** (0.056) | -0.000 (0.004) | -0.007 (0.004) | -0.002 (0.003) |
| Mean dep. var. | 27.514 | 0.821 | 0.188 | 0.069 |
| # observations | 325,751 | 325,751 | 325,751 | 325,751 |

Notes: The sample includes women who were born in 1950-1959 and their children. Estimates were obtained using a second-order polynomial in the assignment variable (day of birth). All regressions include cohort fixed effects (July/June). Inactivity is defined as not being employed, nor being enrolled in education. Secondary degree and inactivity are measured when the child is 21 (or older if not observed at age 21). Crime is any crime committed between ages 18-20. [Table A4](#) in appendix reports the analogous table for the local linear specification.

Thus, a one-year increase in age causes a reduction in birthweight by 1.4 percent (-50 grams). The maternal fixed effects estimates (see [Figure 1](#)) suggest that the corresponding change would reduce birthweight by 0.3 percent (-12 grams), which would then suggest that the maternal fixed effects estimates are biased upwards.

Could a reduction in birthweight by 1.4 percent matter for outcomes in the longer run? Exploiting within-twins variation, [Black et al. \(2007\)](#) show that birth weight is more or less linearly related to long-run outcomes such as earnings and education. If we take the estimates in [Black et al. \(2007\)](#) literally, a reduction in birthweight by 1.4 percent would: (i) reduce IQ by 0.4 percent of a standard deviation; and (ii) reduce high-school completion rates by 0.13 percentage points.²⁴ Thus, the reduction in birthweight induced by maternal age could matter in the longer-run, but it is unlikely that the long-run effects are large. In the next sub-section, we directly examine if the long-run outcomes for children are affected by maternal age at birth.

3.5. Child longer-run outcomes

Is there an effect of maternal age at birth on the longer run outcomes for children? To address this question, we now focus on mothers born earlier, i.e., during 1950-59. For children of these mothers we can obtain information on their outcomes at ages 18-20. For these cohorts of mothers, maternal age at birth increases by 0.21 years as a consequence of the mother being born after the school-entry cut-off (see column (1) of [Table 5](#)).²⁵

Remaining columns of [Table 5](#) shows that there are no long-run implications for children of having their mothers being born after the school entry cut-off. Given the moderate size of the effect of maternal age on birth weight, it would be surprising if we would have found large longer run effects.

In fact, one may argue that the estimates in [Table 5](#) give a too favorable impression of the impact of higher maternal ages. As discussed above, advanced maternal age may be associated with higher income around child birth, and the school starting age may also have an effect on mother's outcomes. [Table A5](#) in the Appendix reports the effects of being born after the cut-off date on mother's own outcomes.²⁶ For these cohorts of mothers, being born after the cut-off increases years of schooling by 0.129 years (see panel B). This estimate is statistically significant and substantially larger than the corresponding estimate for the cohorts born in the 1970s; the likely reason for the difference across the cohorts is that an educational reform postponed tracking for those born in the 1970s.²⁷ Thus, for these cohorts the school entry cut-off can affect child outcomes both through maternal age at birth and through maternal education. Since we expect mother's education to improve child outcomes, the results in [Table 5](#) reinforces the view that there are no favorable effects of maternal age at birth on child long-run outcomes.

4. Conclusions

How are child outcomes affected by being born by an older mother? To study this question, we exploit the school starting age rule in Finland and analyse its impact on fertility patterns and offspring outcomes.

We show that women who are born after the school-entry cut-off are older when they give birth. The effect of being born after the cut-off is to increase maternal age at birth by 0.4-0.5 years for the cohort of mothers born in the 1970s. Completed fertility,

²⁴ We use the estimates in Table III in [Black et al. \(2007\)](#). They report that the effect of an increase of birth weight by 1% on the stanine of IQ is 0.0062. Since the stanine has a standard deviation of 2, the effect of the increase we consider is $(0.0062/2) \times 1.4 = 0.004$ of the standard deviation of IQ.

²⁵ The reduction in the effect on maternal age has to do with educational attainment being lower in older cohorts. If females complete schooling before the onset of the fertile ages, we would not expect the school starting age to matter much.

²⁶ The table also contains the balance test results for the 1950-59 cohorts. Background characteristics of the mothers appear to be balanced. It also shows that there is no direct effect of being born after school cut-off on completed fertility.

²⁷ [Fredriksson and Öckert \(2014\)](#) show that the effect of the school entry age in Sweden fell from 0.21 years to 0.11 years as a result of a reform pushing ability tracking from age 11 (or 13) to age 16. Finland implemented a similar reform starting with the cohorts born 1961; see [Pekka et al. \(2009\)](#). The 1950-59 cohorts were thus affected by the more selective system while 1971-78 cohorts pursued their education in a system where tracking was implemented at age 16 rather than age 11.

however, is unaffected; there is thus no effect on selection into motherhood. Birth weight and gestation fall as a result of being born after the school-entry cut-off. The birth weight effect must be considered small, however: interpreted as the effect of being born to an older mother, the estimates suggests that an increase in the maternal age by 1 year lowers birth weight by 1.4 percent (-50 grams). Moreover, we detect only minor effects on the likelihood of extreme birthweight outcomes. We also show that there are no impacts on children's long-run outcomes. Educational attainment is unrelated to the school entry cut-off as are teenage crime rates.

Our paper has revisited the question of the causal impact of maternal age at birth on children's outcomes using an identification approach which we believe is new to this literature. We have also briefly considered some of the other approaches – e.g., comparing siblings born to the same mother – that have been used previously. In general, it seems that these fixed effects approaches yield a too favourable impression of the effect of being born to an older mother. For instance, the within siblings estimates suggest a positive association between the probability of having a secondary degree and maternal age. Our estimates, that are building specifically on shifts in maternal age due to school starting age cut-off, suggest no favourable impact on the child long-run outcomes on average. (Eqn. 3)

Author Statement

We declare that we have no relevant or material financial interests that relate to the research described in this paper. All authors have seen and approved the final version of the manuscript being submitted. Since the paper uses administrative data, without identifiable private information, IRB approval was not obtained for the project.

Appendix

Table A1, A2, A3, A4, A5, Figure A1, A2

Table A1

Balance of pre-determined characteristics at cut-off (local linear).

| | Parent observed in data | | Parental age at birth | | Parent has secondary ed. | |
|------------------------------|-------------------------|------------------|-----------------------|------------------|--------------------------|------------------|
| | Mother | Father | Mother | Father | Mother | Father |
| Panel a) | | | | | | |
| Born after cut-off | 0.001 (0.002) | 0.004 (0.003) | 0.092 (0.087) | 0.138 (0.093) | -0.006 (0.009) | 0.007 (0.008) |
| Mean dep. var. ^{a)} | 0.987 | 0.970 | 26.317 | 28.693 | 0.635 | 0.600 |
| # observations | 47,594 | 65,842 | 66,792 | 72,932 | 49,434 | 64,350 |
| Bandwidth left/right | 50/50 | 69/69 | 71/71 | 79/79 | 53/53 | 72/72 |

Notes: Sample includes mothers who were born in 1971-1978. Estimates were obtained using local regression with a 1st order polynomial in the assignment variable (day of birth). Bandwidths are determined by minimizing mean squared error following [Calonico et al. \(2014\)](#). Regressions include cohort fixed effects (July/June). ^{a)} Mean below cut-off within optimal bandwidth.

Table A2

Effect of being born after school entry on outcomes for mothers (local linear).

| Variable | (1) Age at 1 st birth | (2) Age at birth(all births) | (3) Spacing | (4) # Children | (5) Years of schooling |
|--------------------|--|---------------------------------|---|----------------------------------|--|
| Born after cut-off | 0.540*** (0.116) | 0.471*** (0.078) | -0.041 (0.042) | 0.000 (0.020) | 0.083 (0.058) |
| Mean ^{a)} | 27.716 | 29.634 | 3.257 | 1.786 | 14.217 |
| # observations | 39,338 | 87,462 | 57,084 | 74,290 | 44,804 |
| BW left/right | 41/41 | 39/39 | 76/76 | 78/78 | 47/47 |
| | (6) Earnings before 1 st birth | (7) Have spouse | (8) Age of spouse(1 st birth) | (9) Spousal yrs. of schooling | (10) Spouse earnings before 1 st birth |
| Born after cut-off | -58.763 (359.872) | -0.000 (0.002) | -0.024 (0.126) | 0.077 (0.063) | 408.725 (498.510) |
| Mean ^{a)} | 20,826.668 | 0.904 | 30.312 | 13.336 | 30,213.328 |
| # observations | 41,656 | 72,446 | 45,308 | 40,832 | 39,176 |
| BW left/right | 44/44 | 76/76 | 49/49 | 44/44 | 43/43 |

Notes: Sample includes mothers who were born in 1971-1978. Estimates were obtained using local regression with a 1st order polynomial in the assignment variable (day of birth). Bandwidths are determined by minimizing mean squared error following [Calonico et al. \(2014\)](#). Regressions include cohort fixed effects (July/June). ^{a)} Mean below cut-off within optimal bandwidth. Earnings are measured in real 2013 Euros.

Table A3

Effect of mother being born after school entry cutoff on the child's birth outcomes (local linear).

| | Mother's age at birth | Birthweight | Gestation (days) | BW-ratio | Induction | C-section | Mother smokes |
|------------------------------|-----------------------|----------------------|---------------------|-------------------|-------------------|------------------|-------------------|
| Born after cut-off | 0.471*** (0.078) | -14.828** (7.160) | -0.378** (0.154) | -0.002 (0.002) | -0.002 (0.004) | 0.004 (0.004) | -0.006 (0.005) |
| Mean dep. var. ^{a)} | 29.634 | 3513.856 | 277.920 | 0.998 | 0.169 | 0.101 | 0.145 |
| # observations | 87,462 | 123,380 | 144,524 | 113,496 | 167,178 | 94,260 | 92,218 |
| Bandwidth left/right | 39/39 | 55/55 | 65/65 | 51/51 | 77/77 | 42/42 | 41/41 |

Notes: Sample includes all children to women born in 1971-78. Estimates were obtained using local regression with a 1st order polynomial in the assignment variable (day of birth). Bandwidths are determined by minimizing mean squared error (following [Calonico et al. 2014](#)). Regressions include cohort fixed effects (July/June). BW-ratio is defined as observed birth weight divided by the median birth weight for gestational age (see, e.g., [Voskamp et al. 2014](#)). ^{a)} Mean below cut-off within optimal bandwidth.

Table A4

Effect of mother being born after school entry cutoff on child's long-term outcomes (local linear).

| | Mother's age at birth | Secondary degree | Inactivity | Any crime |
|------------------------------|-----------------------|-------------------|-------------------|-------------------|
| Born after cut-off | 0.173*** (0.056) | -0.002 (0.004) | -0.002 (0.004) | -0.003 (0.002) |
| Mean dep. var. ^{a)} | 27.525 | 0.821 | 0.184 | 0.069 |
| # observations | 170,688 | 177,106 | 177,106 | 235,092 |
| Bandwidth left/right | 54/54 | 56/56 | 56/56 | 75/75 |

Notes: Sample includes all children to women born in 1950-1959. Estimates were obtained using local regression with a 1st order polynomial in the assignment variable (day of birth). Bandwidths are determined by minimizing mean squared error. All regressions include cohort fixed effects (July/June). Inactivity is defined as not being employed, nor being enrolled in education. Secondary degree and inactivity are measured at age 21 (or older if not observed at age 21). Crime is any crime the child committed between ages 18-20. ^{a)} Mean below cut-off within optimal bandwidth.

Table A5

Being born after school entry, balance of predetermined covariates, and outcomes for mothers, 1950-59 cohorts.

| Panel A) Local linear | Balance | | | Outcomes | |
|------------------------------|--------------------------|--------------------------|--------------------------------------|--------------------|---------------------|
| | Maternal mother observed | Maternal father observed | Maternal mother's years of schooling | Number of children | Years of schooling |
| Born after cut-off | 0.004 (0.003) | 0.006 (0.004) | 0.039 (0.030) | -0.022 (0.014) | 0.091*** (0.029) |
| Mean dep. var. ^{a)} | 0.902 | 0.844 | 9.839 | 2.671 | 12.648 |
| # observations | 127,392 | 139,116 | 75,486 | 167,392 | 145,472 |
| Bandwidth left/right | 40/40 | 44/44 | 39/39 | 53/53 | 46/46 |
| Panel B) Parametric | Balance | | | Outcomes | |
| | Maternal mother observed | Maternal father observed | Maternal mother's years of schooling | Number of children | Years of schooling |
| Born after cut-off | 0.001 (0.003) | 0.007* (0.004) | 0.033 (0.026) | -0.009 (0.015) | 0.129*** (0.029) |
| Mean dep. var. | 0.901 | 0.845 | 9.830 | 2.664 | 12.625 |
| # observations | 292,148 | 292,148 | 178,776 | 291,805 | 292,148 |

Notes: Sample includes women who were born in 1950-1959 and their parents. Estimates in Panel A) were obtained using local regression with a 1st order polynomial in the assignment variable (day of birth). Bandwidths are determined by minimizing mean squared error. Estimates in panel B) are parametric estimates using a second-order polynomial and observations 90 days before and after cutoff date. All regressions include cohort fixed effects (July/June). ^{a)} Mean below cut-off within optimal bandwidth.

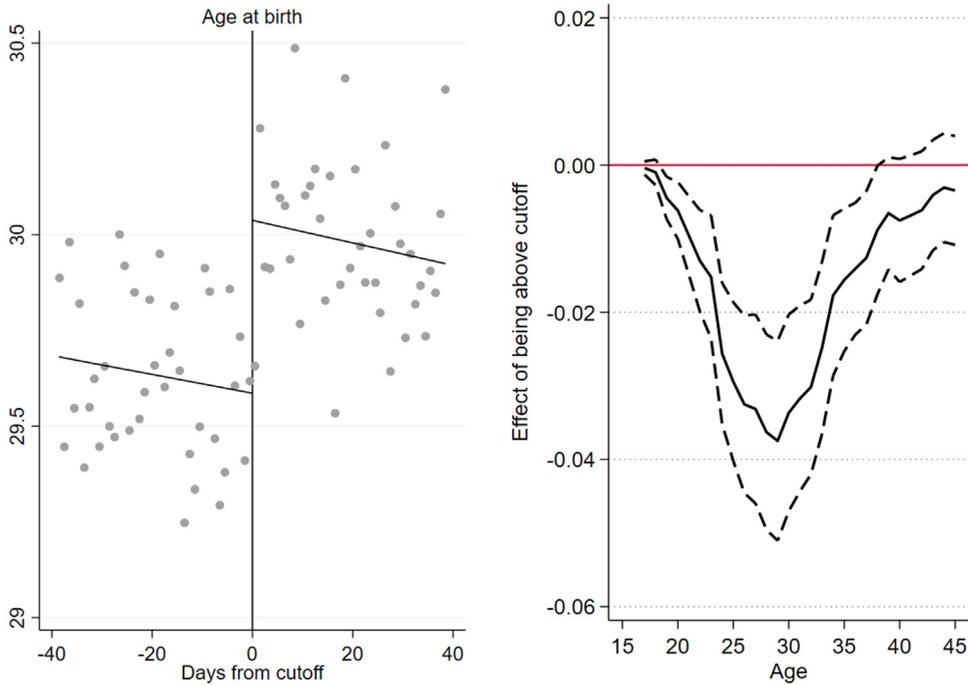


Figure A1. Fertility, age at birth, and school entry age cutoff (local linear) *Notes:* The left-hand panel plots mean age of giving birth by mother's day of birth, with a fit (solid line) obtained from fitting local linear regression within the optimal bandwidth around the January 1st cut-off. RD-estimate of being born after cut-off in the left-hand side figure is 0.471 (standard error: 0.078). The right-hand side figure plots the estimates from separate local linear regressions of the probability to give birth by age on an indicator for being born after the school entry cut-off (January 1st). The sample includes women born 1971-78 and the regressions control for cohort fixed effects (July/June).

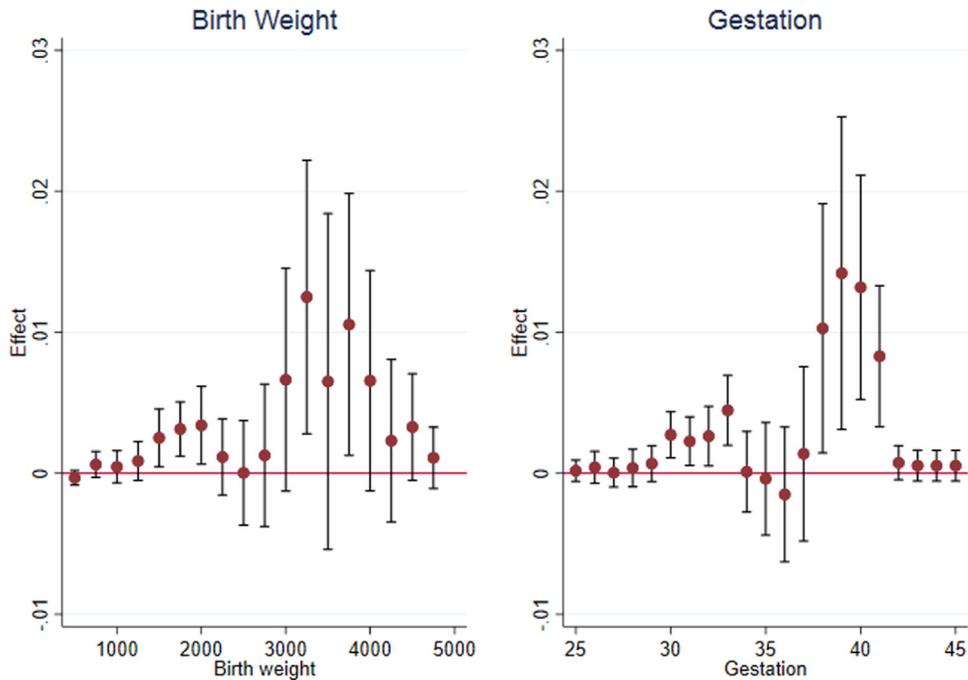


Figure A2. Effect of the school-entry rule on the probability to give birth by birth weight and gestation interval (local linear) *Notes:* The figure plots the estimates from separate local linear regressions of the probability to have birth weight of at least the amount on the x axis (left-hand-side figure) or gestation (weeks) of at least the amount on the x axis (right-hand-side figure) on an indicator for the mother being born after the school entry cut-off (January 1st). Sample includes women born 1971-78. The regressions also control cohort fixed effects (July/June).

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