School Starting Age, Fertility, and Family Formation: Evidence from the School Entry Cutoff using Exact Date of Birth

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Abstract

This article analyzes the effect of school starting age on fertility and family formation by utilizing Norway's age-based school entry policy. Using individual-level register data and a regression discontinuity design, we find that being born after the age cutoff for school start results in an increased age at first birth of 2.9 months for women and 4.0 months for men, while completed cohort fertility was unchanged. Similarly, being born after the cutoff increased the age at first marriage by 4.7 months for women and 2.4 months for men, with no effect on the overall probability of having a partner. Results show that age at completed education and earnings development are important mechanisms in this fertility postponement. Additionally, we analyze detailed age- and parity-specific effects, providing important insights into how age at school start affects the timing of fertility, but not overall fertility.

1 Introduction

Over the last decades, there has been substantial shift in the age at first birth in Norway and other European countries, leading to concerns that individuals start childbearing too late to reach their intended family size. Understanding the causal drivers of this postponement, and its link with completed family size, is less than straightforward. There is a strong link between time in educational enrollment and timing of first birth (Lappegård & Rønsen, 2005), and studies using quasi-experimental variation in timing of educational enrollment suggest a causal relationship (Ní Bhrolcháin & Beaujouan, 2012; Skirbekk et al., 2004). Fertility in Nordic countries has until recent decades been lower among women with higher education than for women with low education (Jalovaara et al., 2019). At the same time, completed family size seems to be unaffected by timing of education (Ní Bhrolcháin & Beaujouan, 2012), likely because women "catch-up" after long educational enrollment by progressing to motherhood quicker than those with lower educational attainment (Lappegård & Rønsen, 2005; Ní Bhrolcháin & Beaujouan, 2012).

Entry into formal education is often determined by one's date of birth, with individuals born before the set cutoff date entering school in one year and individuals born after entering school a year later. This results in a discrepancy in the age at which children enter formal schooling of nearly one year, despite being born merely weeks or days apart.Studies exploiting random variation in timing of education from age-based school entry rules have provided valuable insights in the dynamics of fertility postponement and recuperation. Later school entry and higher graduation age has been linked to later births and partnership formation, while overall completed fertility and the probability of having a child was unaffected, both in Sweden (Skirbekk et al., 2004) and Finland (Fredriksson et al., 2022). On the contrary, in the United States, McCrary and Royer (2011) find no effect of females being born just before and after the school entry date on the likelihood to become a mother, nor an impact on the age at childbearing. Previous research has furthermore utilized this cutoff date to estimate causal effects of school starting age (SSA) on a variety of educational and health outcomes such as mental health, school performance, educational attainment, and income (Balestra et al., 2020; Matsubayashi & Ueda, 2015; Solli, 2017).

At the same time, there are several knowledge gaps. Most importantly, men's fertility patterns and their drivers are often distinct from those of women (Jalovaara et al., 2019; Kravdal & Rindfuss, 2008), and the impact of educational timing on male fertility has not been explored. Previous studies suggest rigidity in demographic transitions among females, with age at school entry acting as an important factor affecting the timing, but not the quantum, of fertility and family formation. As previous studies have focused on the effect of school entry and graduation age on women's fertility, little is known on whether this is also true for men's fertility. Research suggests that income is more important for male than female fertility outcomes (Bratsberg et al., 2021), which is one channel through which school starting age may affect the timing and number of children. Furthermore, the knowledge of drivers of postponement other than educational enrollment, such as earnings development, remains incomplete and indicative (Hart, 2015). Finally, we lack evidence of how the process of postponement and recuperation generate the combination of delayed fertility and unchanged family size. A precise understanding of this process requires detailed data on both parity and age-specific fertility rates.

In this paper, we analyze full population data for the Norwegian 1960-1973 birth cohorts in a regression discontinuity design (RDD) to examine causal effects of later school entry on fertility and family formation. Using an RDD with exact date of birth allows us to handle confounding better than most of the current literature which is based on comparing December-borns to January-borns, with the idea that birth month is as-good-as random. However, Dahlberg and Andersson (2018) show that there is considerable variation in parental characteristics by the child's birth month. Some of these characteristics may affect their offspring's fertility patterns, above and beyond the effect of SSA. Measurable parental characteristics can be controlled out, but their presence also suggests that unmeasured confounding is likely, and the latter cannot be handled in a control variable design. To address this, we estimate RDD models using exact date of birth, which compares those born just before the school entry cutoff to those born just after. We also perform multiple tests on the robustness of our models, and the validity of the assumption of no self-selection around the cutoff.

Beyond methodological advances, our study expands the literature in three main ways. First, we assess how SSA impacts men's family formation and risk of remaining childless, which to the best of our knowledge has not yet been studied. While effects on this outcome has previously been assessed for women, we argue that effects may be qualitatively different and potentially more detrimental for men. In particular, previous research has found that young SSA, particularly for boys, is associated with worse school performance and educational attainment, mental illness, and higher rates of ADHD and referral to special education services (Balestra et al., 2020; Black et al., 2011; Fredriksson & Öckert, 2014; Matsubayashi & Ueda, 2015; Solli, 2017). In turn, mental health and school performance are linked to lower educational attainment which is again linked to a higher probability of remaining childless among men, but not among women (Kravdal & Rindfuss, 2008).

Second, we explore the drivers of postponement, and the process of recuperation that ensures no effects on completed cohort fertility in more detail than has been previously done. Based on the literature, the main expectation is that fertility postponement is driven by later graduation from higher education (Lappegård & Rønsen, 2005) and thus a delayed income growth (Hart, 2015) among those who graduate at a higher chronological age. We explore this empirically by estimating both timing and overall effects on education and age-specific effects for income. As for recuperation of fertility, the general finding in the literature is that SSA affects fertility timing, but not completed fertility (Fredriksson et al., 2022; Skirbekk et al., 2004). Mechanically, this is only possible if the individuals that start school at a higher age recuperate fertility (have higher period birth rates than their earlier-starting peers) at some later time points. In this paper, we use detailed parity-specific and age-specific estimations of fertility outcomes to describe the dynamics of postponement and recuperation. Such descriptions are of general interest, as they are an important component of the education-fertility dynamic, with education linking stronger to fertility timing than to completed fertility. They also cast some light on the scope for late fertility, of interest in a period where the age at first birth is rising sharply.

Finally, we explore in more detail than is previously done whether postponement of fertility due to higher graduation age has negative consequences for children. Previous research has indicated that children of older mothers perform better in school, and that this is largely due to these children being born in later cohorts (Barclay & Myrskylä, 2016). Additionally, Fredriksson et al. (2022) show that increased SSA among females resulted in a decrease in gestational age and birth weight for their children, presumably due to their increased average age at first birth. The researchers did not find evidence that this initial birth weight and gestational age disadvantage translated to negative longer-term outcomes such as educational attainment and criminal activity (Fredriksson et al., 2022). However, despite the lack of evidence on long-term educational attainment, it is not known whether there may be differences in the earlier school performance among children of mothers or fathers born after the school entry cutoff. This may be an important factor, as previous studies have found poorer school performance among premature and low birth weight children (Kirkegaard et al., 2006; Saigal et al., 2003).

2 Institutional Context

2.1 The Educational System and School Enrollment in Norway

Prior to a reform introduced in 1997, compulsory school in Norway consisted of 9 years, beginning with enrollment in August of the calendar year children turn seven and ending with graduation in the calendar year students turn 16. Rather than setting a minimum school leaving age as in the United States, compulsory schooling was stipulated to 9 years in length. Students in compulsory school are exposed to the same curriculum with no tracking or grade-based student placement for the entire duration, and grade retention is rarely used. The relatively strict enforcement of school enrollment age and the lack of grade retention suggests that individuals within a given school cohort enroll in and graduate from compulsory school at the same time. As the school enrollment cutoff birth-date is set to January 1st, children born in December begin school on average a year earlier than children born just days or weeks later, in January. Consequently, while born just weeks or days apart, these children are often enrolled in different school cohorts, with the December-born children being the youngest in their class, and those born in January the oldest in their cohort.

Parents may apply to the municipality if they wish to alter their child's enrollment by one year either earlier or later than their stipulated enrollment date. Following this application, the municipality carries out an expert assessment to either grant or reject the request. Overall non-compliance rates are relatively low, with on average 5% children in the late 1960s postponing school start and less than 1% in the 1990's (Solli, 2017). Though noncompliance rates are low during this time, receiving a delayed or early school start is highly linked to the month of birth. Children, especially boys, who are born in December are most likely to receive a delayed school start while children, especially girls, born in January are more likely begin school early (Solli, 2017).

2.2 Education, Fertility, and Institutional Support

In Norway as in most Western countries, the Total Fertility Rate (TFR) declined sharply in the 1970s, from 2.5 children per woman in 1970 to 1.72 in 1980. At the same time, female labor force participation and educational attainment increased, and institutional support to dual-earner families were vastly expanded. A recuperation to fertility near replacement level in the late 1980s happened in conjunction with, and was probably partly due to, a major expansion in institutional support to families with children (Bergsvik et al., 2021; Rindfuss et al., 2010). Meanwhile, age at first birth has continued to increase: In 1975, mean age at first birth was 23 years for women and 26 for men. 40 years later, it had increased to 28 years for women and 31 for men. Between 2009 and 2022, the total fertility rate among women dropped from 1.98 children to a record low of 1.41 children (Statistics Norway, 2023).

The fertility decline throughout Europe, and in recent years throughout the Nordic countries, has led to great interest among both academics and politicians in understanding what mechanisms drive lower fertility rates, and to what degree. A large body of literature has described the role education plays on the fertility patterns of women in particular. Educational expansion has consistently been pointed to as a driver of fertility postponement, partly because fertility rates are very low for those enrolled in higher education (Lappegård & Rønsen, 2005). Among Norwegian men and women born 1940-1964, Kravdal and Rindfuss (2008) found that the negative impact of education on fertility for women weakened markedly over the cohorts. At the same time, substantial differences in the median age of first birth by educational attainment remained (Lappegård, 2000). For men, an increased likelihood for remaining childless emerged among the lower educated (Jalovaara et al., 2019; Kravdal & Rindfuss, 2008).

3 Conceptual Perspectives and Literature Review

The age at which an individual begins school may impact their future family formation and fertility through distinct mechanisms with potentially opposing effects. On the one hand, starting school later could give a domino effect where the transition to adulthood, including parenthood, consistently happens later. On the other hand, children who are older at enrollment have an advantage both academically and in the peer group, that can have positive effects that spill over into family formation in adulthood. Below, we elaborate on each of these perspectives, and how they may have different implications for men and women.

3.1 The Cohort Postponement Model

Children who enroll in school at a relatively older age will on average complete their education later as well. This age at completed schooling also impacts various mediating factors such as the age when beginning employment and the age at marriage, which in turn leads to delays in the age at first birth (Chang et al., 2021; Skirbekk et al., 2004). Chang et al. (2021) found that a one year delay in SSA led to a delay in first and second births among employed women by three and four months, respectively. The researchers identified the age at labor market entry as a mediating channel for this delay in childbearing (Chang et al., 2021).

Using Swedish administrative data, Skirbekk et al. (2004) find that women who were born in December prior to the school entry cutoff had their first child on average 4.9 months earlier than women born after the cutoff. This difference was similar when examining second births, with females born in December on average four months younger than females born in January of the following year. Furthermore, the age at first marriage was also younger for those born in December compared to those born the next month in January (Skirbekk et al., 2004). Similarly, Fredriksson et al. (2022) find using Finnish data that women who entered school one year later are on average 0.4 to 0.5 years older at first birth and 0.6 years older at first cohabitation than women born just before the cutoff.

The current literature suggests that for women, the postponement of first births does not have a lasting impact on family size. Given that men have a substantially longer reproductive window than women, effects on total number of children as a mechanical effect of postponement would seem surprising. However, it suggests that for both men and women, a higher SSA will be linked to a later transition to parenthood. Furthermore, it points to completed education and earnings growth as mediators of this postponement.

3.2 Educational Experience and Social Rank in Class

Another mechanism in which SSA can impact fertility is through educational experience and social rank in class. Relatively younger students have been shown to have lower levels of socio-emotional adjustment (Ensar & Keskin, 2014) and lower self-esteem levels (Thompson et al., 2004) than relatively older students. Older students are also more likely to score higher on cognitive and non-cognitive skills (Crawford et al., 2014) and often have better school performance compared to relatively young students (Aliprantis, 2014; Dicks & Lancee, 2018; Solli, 2017). When examining gender differences, relatively young boys perform significantly worse in school than relatively young girls (Diris, 2017; Hemelt & Rosen, 2016). This may be related to school readiness, as previous research has shown that the female education advantage throughout schooling may even be prevalent before school entry (Brandlistuen et al., 2021). As children's school readiness and capabilities at school entry have been shown to predict future academic performance (Duncan et al., 2007), this may signal a "double disadvantage" for relatively young boys entering school.

Children who start school at a younger age also experience a higher likelihood of experiencing mental health disorders (Black et al., 2011; Kuntsi et al., 2022; Matsubayashi & Ueda, 2015). This may have an impact on later family formation as internalizing disorders have been shown to be negatively associated with childlessness among men (Evensen & Lyngstad, 2020). Previous research found a significant increase in suicide rates between ages 15 and 23 for those born before the school entry cutoff compared to those born just after (Matsubayashi & Ueda, 2015). The researchers also found that relatively young individuals were less likely to attend upper-secondary or tertiary education, and were more likely to work lower wage jobs. Consequently, being the relatively youngest in a school cohort may translate into an academic disadvantage that may in turn translate to a socioeconomic disadvantage later in life (Matsubayashi & Ueda, 2015).

The possible impact of SSA on educational experience, mental health, and ultimately educational attainment and socioeconomic status may have important consequences for fertility and partnership formation. Men with low education levels exhibit the highest levels of childlessness (Jalovaara et al., 2019). Thus, any negative impact on educational attainment may translate into higher childlessness for men. For women, there is even some evidence that being among the oldest in the peer group comes with a *disadvantage*: McCrary and Royer (2011) find that mothers born just after the cutoff date have younger and less educated partners than those born just before (McCrary & Royer, 2011). This finding fits into a line of research that indicates a preference for hypogamy among men.

3.3 Expectations

As a result of the conceptual perspectives and prior empirical findings, we hypothesize that being born in December will lower the average age at childbearing among women with no impact on completed fertility. Previous research has shown that women's fertility is sensitive to major live events, such as completed education and initiation of labor force participation, but that this postponement effect is concentrated on the *timing* of births rather than the overall number of children (Fredriksson et al., 2022). Given the comparatively lower opportunity cost of fertility for men, we may expect men's fertility timing to be less sensitive to educational timing. However, if young relative age is associated with poorer academic achievement and educational experience, this may translate to an increase in childlessness and a decrease in completed cohort fertility among men.

Additionally, we examine whether the effect of SSA varies based on parental education level. Parents of more advantaged socioeconomic status may have additional resources to counteract the potential negative effects of young school start on school performance and educational attainment, and we may therefore expect the effect to differ based on parental education level (Bernardi & Grätz, 2015; Dobkin & Ferreira, 2010). Those with lower parental education level may have larger effects of SSA on educational attainment and earnings, which may translate into negative effects on childlessness and probability of finding a partner.

4 Data, Measures, and Methods

4.1 Data Sources and Outcome Variables

Data used in this analysis comes from the Norwegian Population Register, the National Education Database, and the Tax Registry. Information from these registers includes individual-level data on exact date of birth, parental and background characteristics, educational attainment, pensionable income, and information on births and union formation. We were additionally able to link individuals to their children and observe child educational outcomes such as lower-secondary school GPA and national test scores. Our analytical sample included all individuals born between 1960 and 1973 to two-Norwegian born parents and who were alive and living in Norway between the ages of 15 and 45. A small portion of observations were missing information on compulsory school and were therefore excluded ($\approx 0.5\%$), leaving the final sample at 767,528 individuals. Main analyses are presented by sex and further subgroup analyses were completed by parental education level.

We measure age at first birth and completed cohort fertility, along with age at first marriage and probability of having a spouse. To examine the age- and parity-specific effects, we utilize the age-specific fertility rates and indicators for whether an individual has had a first, second, or third and higher birth in 5-year age groups between ages 15 and 44. Additionally, we examine partnership characteristics of age and educational attainment at first birth, as well as total years of schooling and age at completed education. Earnings were measured as total pensionable income reported to the Tax Registry. This includes salary, unemployment benefits, sick leave benefits, parental leave benefits, and pensions. For regression analyses, earnings was measured yearly between ages 15 and 45, scaled by the National Insurance Scheme's basic amount per that year, and then transformed to log scale. [1]

4.2 Descriptive Statistics

Table 1 shows descriptive characteristics for the analytical sample of our main analysis. Fewer females than males were childless at age 45 (11% vs. 18%) and they had on average slightly more children (2.08 vs. 1.89). Females were on average 25.95 years old at first marriage and 26.18 years old at first birth. Males were on average 28.89 years old at first marriage and 29.17 years old at first birth, and a lower proportion of males ever had a spouse compared to females (90% vs. 94%). Educational characteristics between males and females were similar with 72% of females and 73% of males completing upper secondary school. Overall, individuals earned on average 572,524 NOK in 2016-equivalents at 45. Males earned on average 672,321 NOK in 2016-equivalents at 45, while females earned an average of 468,323 NOK 2016-equivalents. Those with high parental educational attainment were older at first birth and marriage, and had more years of schooling and higher income at 45 than those who had lower parental educational attainment.

[1] This basic amount is used to calculate pension payments, and is based on expected wage growth and adjusted each year.

Table 1: Descriptive Statistics

Parental characteristics	All	Females	Males	High parental education	Low parental education
	mean (sd)	mean (sd)	mean (sd)	mean (sd)	mean (sd)
Mother's age at birth	26.99	26.99	26.99	27.51	26.74
	(5.95)	(5.95)	(5.94)	(5.29)	(6.21)
missing (n)	178	81	97	26	152
Father's age at birth	30.35	30.35	30.36	30.36	30.35
	(6.96)	(6.97)	(6.96)	(6.22)	(7.29)
missing (n)	5,808	2,820	2,988	353	5,455
Birth order	2.15	2.15	2.15	1.95	2.25
	(1.25)	(1.25)	(1.25)	(1.05)	(1.32)
Mother's education	0.55	0.55	0.55	0.81	0.43
missing (n)	4,329	2,071	2,258	885	3,444
Father's education	0.62	0.61	0.62	0.96	0.45
missing (n)	15,378	7,576	7,802	1,464	13,914
Fertility and family formation					
Number of children	1.98	2.08	1.89	1.96	1.99
	(1.19)	(1.12)	(1.24)	(1.15)	(1.20)
Childless	0.15	0.11	0.18	0.15	0.15
Age at first birth	27.62	26.18	29.14	28.94	27.01
	(5.61)	(5.19)	(5.63)	(5.47)	(5.58)
Have a spouse	0.92	0.94	0.90	0.92	0.92
Age at first marriage	27.41	25.95	28.89	28.13	27.08
	(6.09)	(5.64)	(6.17)	(5.94)	(6.13)
Education and income					
Complete upper-secondary	0.72	0.72	0.73	0.85	0.66
Years of schooling	13.59	13.78	13.42	14.72	13.07
	(2.57)	(2.63)	(2.50)	(2.64)	(2.36)
missing (n)	3,449	1,286	2,163	599	2,850
Earnings at 45 (NOK)	57.25	46.83	67.23	65.25	53.50
	(38.46)	(24.84)	(45.84)	(49.33)	(31.41)
missing (n)	55,778	29,232	26,546	16,749	39,029
N	767,528	377,423	390,105	244,206	523,322

Note. Parental education is measured as attending upper-secondary schooling. Age at first birth and marriage are taken from a sub-sample with at least one birth or marriage, respectively. Earnings are presented in 2016-equivalent Norwegian kroner (10,000 NOK). Parental education subgroups were created according to whether at least one parent completed upper secondary education (high) or if neither parent completed upper secondary education (low).

4.3 Empirical Strategy

Despite low non-compliance rates, using actual SSA to directly estimate effects on family formation may be biased due to the selection by parental characteristics into non-compliance. For example, if more educated parents are more likely to successfully delay their child's school enrollment than less educated parents, results using actual SSA may be biased. We therefore follow the approach of Chang et al. (2021), Fredriksson et al. (2022), and McCrary and

Royer (2011) by using exact date of birth as an instrument for SSA. We begin by testing the first-stage relationship between exact date of birth and (estimated) SSA, following the approach by Dee and Sievertsen (2018). This first stage estimate confirms the relation between date of birth and school enrollment timing, before we turn to the reduced form estimates as the main specification. As actual SSA can suffer from the issues of selection outlined above, the reduced form specification circumvents these endogeneity problems by estimating the impact of birth date on the outcomes of interest. Finally, we examine the validity of the RDD design and perform multiple robustness checks on the main results.

Our empirical strategy is to use a regression discontinuity design to identify effects of being born right after vs. right before January 1st. It rests on the assumption that any potential confounder in the relationship between birth timing and the outcome is continuous across the discontinuity cut-off. For instance, fewer pregnancies may be planned as you approach the cut-off from either side, given a preference for avoiding December-born children. However, at the exact discontinuity point, we may assume that individuals giving birth have equal planning characteristics regardless of whether they gave birth in December or January. The continuous confounders are handled through linear control variables for the distance to the cut-off in days, whereas the effect of being born before or after the cut-off is captured with a dummy variable, identifying the size of the "jump" between the two lines. The regression equation becomes:

$$y_{it} = \alpha + \beta_1 NewYear_t + \beta_2 DoB_t + \beta_3 NewYear_t \times DoB_t + \beta_Y YoB_t + \beta_X X_i + \epsilon_{it} \quad (1)$$

Where y_{it} is the outcome of interest for individual i born at time t, β_1 is the reduced form coefficient of interest, $NewYear_t$ is a dummy variable equal to 1 if born after new year, DoB_t is a linear term reflecting the date of birth, YoB_t is the year of birth running from July to June, X_i are a set of control variables, and ϵ_{it} is the residual. Estimates are presented using bandwidths selected to optimize the mean-squared error (MSE) (Imbens & Kalyanaraman, 2012). Calonico et al. (2014) and Cattaneo et al. (2019) show that bandwidths used by typical bandwidth selectors may be too large to guarantee the validity of the underlying distributional approximations and that this may lead to biased estimates. The researchers present a solution by adjusting the standard errors for the estimated bias term (Calonico et al., 2014; Cattaneo et al., 2019). Therefore, we provide conventional MSE optimal point estimators and robust bias-corrected standard errors when using MSE optimal bandwidths. Estimates using 60 day bandwidths are presented in Appendix C, Tables C5 and C7. Estimates were calculated using the rdrobust package for R (Calonico et al., 2022).

5 Results

5.1 First-Stage Estimates

As our data does not contain information on the age at which individuals actually enter school, we create an estimated school starting age (ESSA) variable using information on the graduation date from compulsory schooling and the individual's exact date of birth. Figure 1 shows the discontinuity plot for the first stage relation between the exact date of birth, re-centered around the January 1st cutoff, and the ESSA for those born ±60 days from the cutoff. As expected, the ESSA shows a clear discontinuity at the cutoff date. Additionally, as the date of birth approaches the cutoff, a slight increase in ESSA is shown, likely indicating the higher probability for children born right before the cutoff of receiving a delayed school start during the time our cohorts entered schooling. The first stage estimates show that those born after the cutoff date were on average 6.1 months older at school entry compared to those born before.[2]

[2] Information on the construction of the ESSA variable and the first stage estimates are provided in Appendix A.

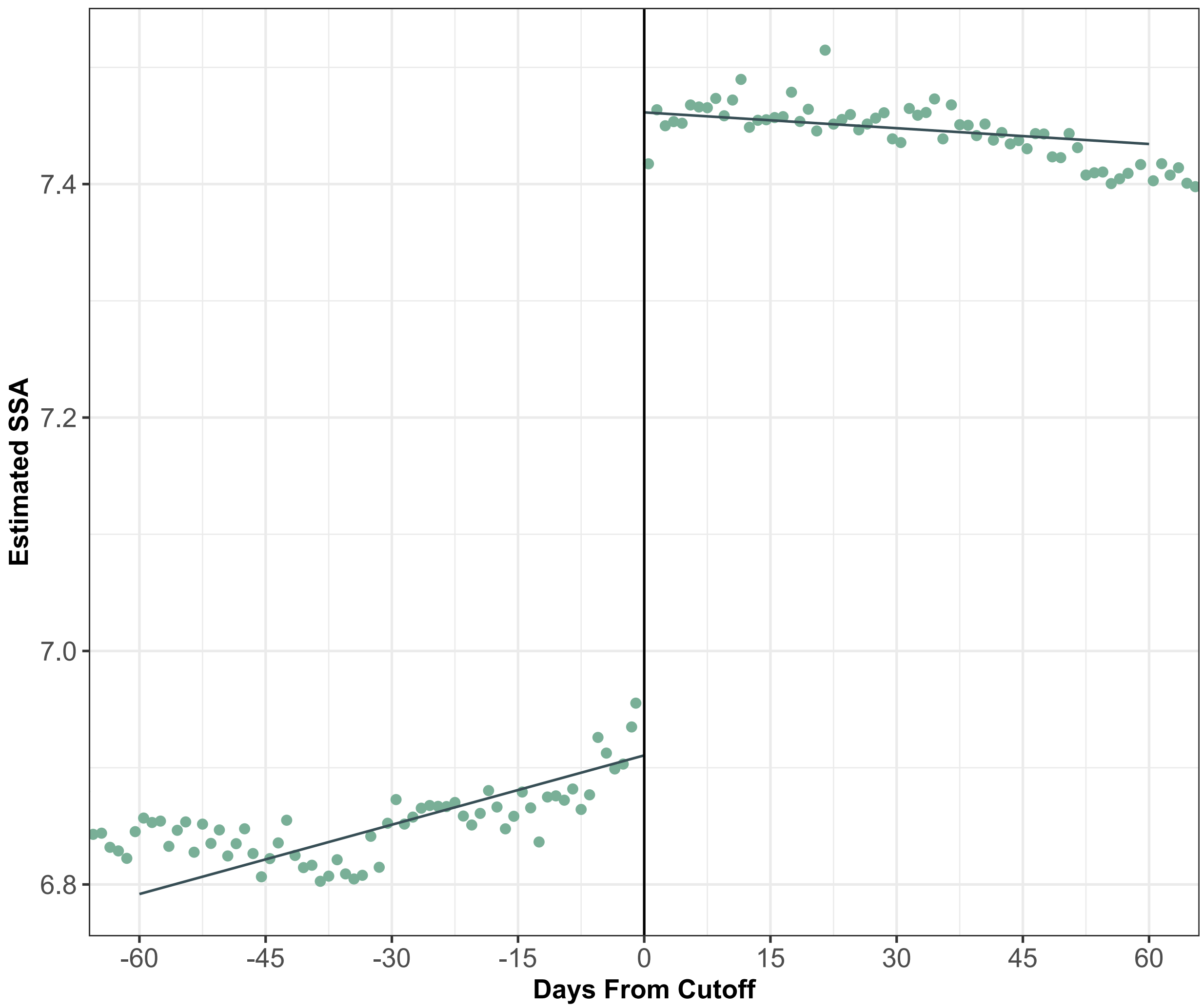

Figure 1: Discontinuity plot for first-stage estimates. *Note.* Dots represent sample averages by date of birth. Solid lines represent local linear regression estimated separately on each side of the cutoff with triangular kernel weighing and adjusted for year of birth dummies (year redefined as running from July until the following June).

5.2 The Effect of School Starting Age on Fertility and Family Formation

Table 2 presents the effects of date of birth on fertility and family formation from our main specification. Being born after the cutoff resulted in an average increase in the age at first birth of 2.9 months for females and 4.0 months for males. When scaled by the first stage estimates[3], a one year delay in education is associated with an increased age at first birth of 5.4 months for females and 8.3 months for males. The average age across all births was also increased for both males and females born after the cutoff, while the total number of children and probability to be childless by 45 was not significantly different. Similarly, females born after December 31st were on average 4.7 months older at their first marriage and males were on average 2.4 months older, while the probability to ever have a spouse by age 45 was not significantly different for either sex. When looking at the timing of first births and first marriages we may introduce bias into the estimates if there is selection into parenthood or partnership. Though as we do not find an effect on total fertility or probability to have a partner, we find no evidence of selection into parenthood/partnership. When examining partner characteristics, the age of partners of neither females nor males born after the cutoff were statistically significantly different compared to the partners of those born before the cutoff.

[3] See Appendix A for first stage estimates.

Table 2: Estimates for the effect of being born after December 31st on fertility

Females

Variable	(1) Age at 1st birth	(2) Age at birth	(3) Total fertility	(4) Childless
Born after cutoff	0.244*	0.227*	-0.008	-0.001
	(0.103)	(0.092)	(0.015)	(0.004)
h	36	35	74	74
N	63,429	61,728	145,955	145,955

Variable	(5) Age at 1st marriage	(6) Have a spouse	(7) Partner's age	(8) Partner's education
Born after cutoff	0.389***	-0.001	0.031	-0.033
	(0.096)	(0.004)	(0.116)	(0.054)
h	50	56	38	32
N	92,752	110,581	65,950	50,713

Males

Variable	(9) Age at 1st birth	(10) Age at birth	(11) Total fertility	(12) Childless
Born after cutoff	0.331**	0.302**	0.012	-0.005
	(0.101)	(0.089)	(0.017)	(0.006)
h	52	55	73	61
N	86,614	91,587	148,174	123,378

Variable	(13) Age at 1st marriage	(14) Have a spouse	(15) Partner's age	(16) Partner's education
Born after cutoff	0.201†	-0.001	0.107	0.065
	(0.109)	(0.004)	(0.101)	(0.042)
h	49	74	42	61
N	89,445	150,238	69,957	92,490

Note. Numbers in parentheses are standard errors. Effects estimated with local linear regression (p=1) fit separately on both sides of the cutoff and triangular kernel weighing. h and p denote bandwidth used and degree of polynomial, respectively. Bandwidths used are mean-squared error optimal with robust bias-corrected standard errors (in parentheses). Adjusted for year of birth dummies (year redefined as running from July until the following June).
Estimates of age at first birth, average age at birth, and partner characteristics are taken from a sub-sample restricted to those with at least one birth. Estimates of age at first marriage are taken from a sub-sample restricted to those with at least one marriage. Estimates for partner's age and education (measured in years of schooling) are measured at first birth.
$\dagger p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$

Figure 2 presents the age-specific fertility rates in 5-year age groups for males and females from ages 15 to 44. For those born after the cutoff, there are fewer births in the earlier ages compared to those born before the cutoff, until age 30. After 30, the relatively older groups appear to recuperate with more births than the relatively younger group in the later reproductive ages. This initial postponement and later recuperation explains why those who were born after the cutoff and thus began school at an older age, were older at the time of first birth while overall fertility levels were not affected by the age at school start.

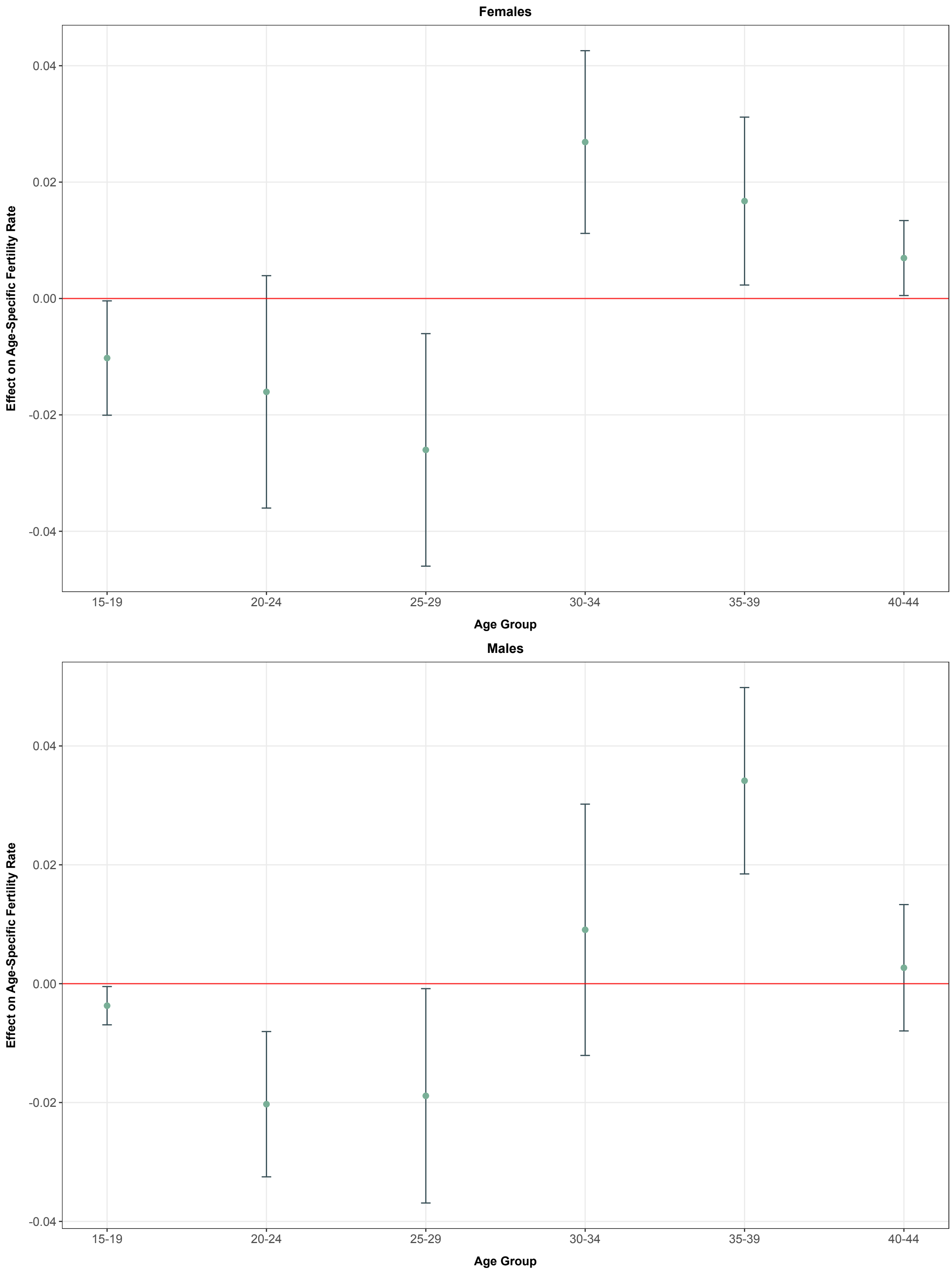

Figure 2: Effect of being born after December 31st on the age-specific fertility rate for males and females. *Note.* Estimates from local linear regression estimated separately on either side of the cutoff with MSE optimal bandwidth and triangular kernel weighing. Estimates are adjusted for year of birth dummies (year redefined as running from July until the following June). Dots represent regression coefficients and lines indicate 95% confidence intervals.

5.2.1 Parity-Specific Effects

Figure 3 shows the effect of being born after the cutoff on the parity-specific birth probabilities by 5-year age groups from ages 15 to 44 (see Appendix B for estimates and optimal bandwidths). This graph shows that the postponement of births in the younger ages by those born after the cutoff is consistent across the first, second, and third or higher parities. Females who are relatively older are less likely than those who start school at a younger age to have a first birth or to transition to a second or third birth until age 30. In the older ages, however, they are more likely to have a first, second, or third birth compared to the relatively younger females, recuperating from their earlier postponement. Interestingly, we also see a similar pattern among males, where during the younger ages, those born after the cutoff experience a lower probability to have a first, second, or third birth, with a recuperation at the later ages. For the third and higher parity, the trend appears to be slightly shifted to even older ages, where the recuperation appears to begin in the 35 to 39 age group, and in the 40 to 44 age group men born after the cutoff are still significantly more likely to have a birth compared to those born before, likely due to the longer reproductive window for men.

5.2.2 Heterogeneous Effects

Previous research has mainly focused on the effects of school entry policies on fertility, with little known of the potential effect heterogeneity by socioeconomic status. Parents from more advantaged socioeconomic positions may have more resources to counteract the potential negative effects of young SSA on their child's future educational attainment and earnings (Bernardi & Grätz, 2015; Dobkin & Ferreira, 2010). As these are potentially key mechanisms in the impact of school timing on fertility, we may find differing effects of SSA on these mediating factors, and potentially on completed cohort fertility or fertility postponement, between individuals with higher and lower parental education. Thus, we analyzed the effect of SSA separately by parental education level, i.e. through using information on three generations in the data. Subgroups were created according to whether at least one parent completed upper secondary education or if neither parent completed upper secondary education. Table 3 shows the estimates by parental education subgroups. Among individuals with better educated parents, the first stage relation between date of birth and ESSA showed that those born after the cutoff were on average 5.7 months older at school start than those born before. This was stronger for individuals with less educated parents, with those born after December 31st on average 6.4 months older at school start than those born before. Among those with higher parental education, those born after the cutoff were on average 5.5 months older at first birth compared to those born before. Among those with lower parental education, those born after were on average 3.4 months older at the time of first birth compared to those born before the cutoff. Those born after the cutoff were on average 5.3 months older at first partnership formation compared to those born before, among those with at least one parent with completed upper-secondary school. This effect was smaller among those with lower educated parents, with those born after the cutoff on average 3.1 months older at the time of first partnership formation compared to those born before. There was no statistically significant effect found for completed cohort fertility among either group, nor for the probability of having a partner. Among those with higher parental education, those born after December 31st had an increase in the probability to complete upper secondary education by 1.6 percentage points and on average 1.6 months more schooling by the age of 45 compared to those born before. Interestingly, no statistically significant effect on probability to complete upper secondary education was found among those with lower parental education, and no effect on earnings at age 45 was found for either group.

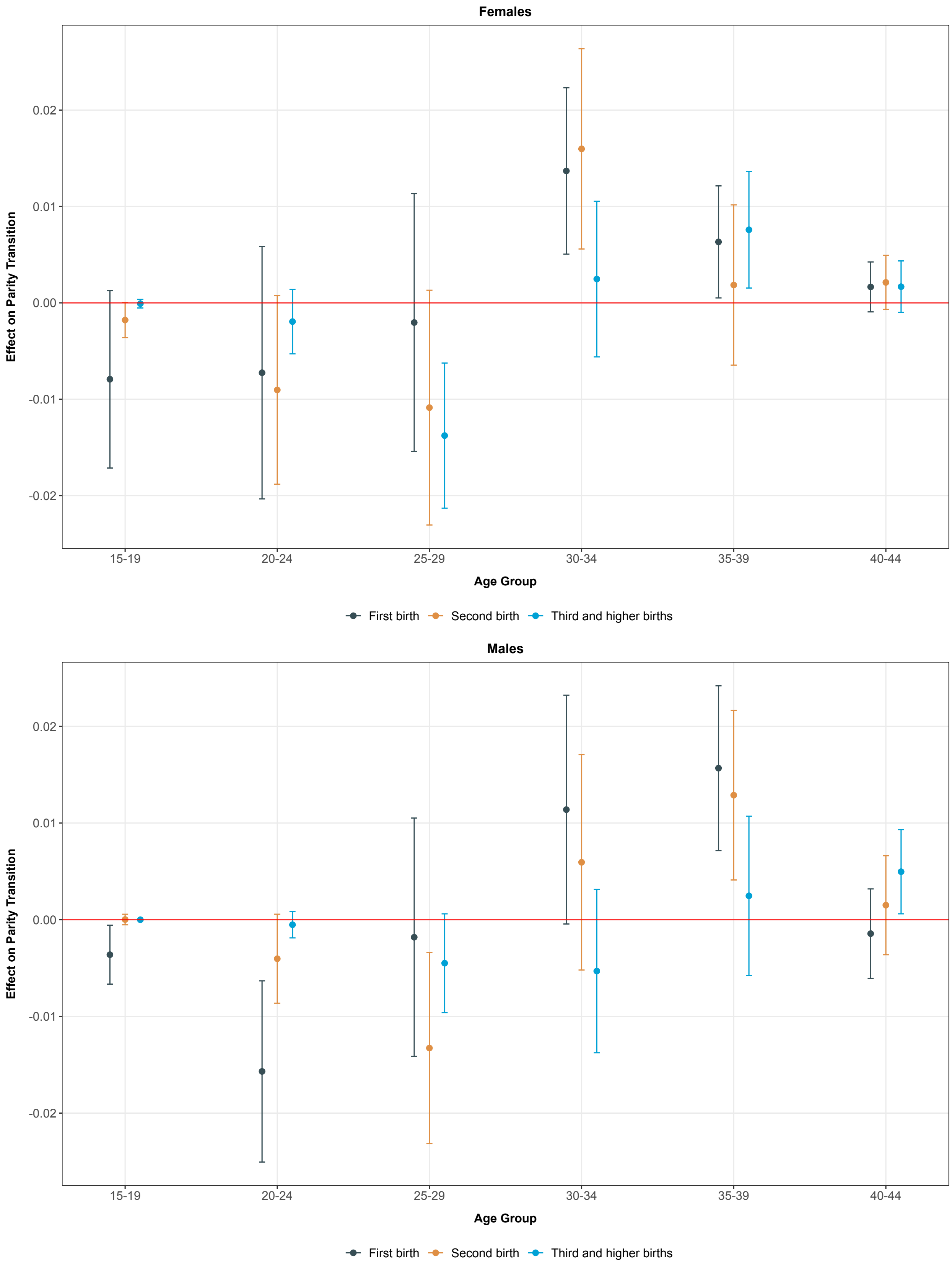

Figure 3: Effect of being born after December 31st on the parity transitions by 5-year age group for males and females. *Note.* Estimates from local linear regression estimated separately on either side of the cutoff with MSE optimal bandwidth and triangular kernel weighing. Estimates are adjusted for year of birth dummies (year redefined as running from July until the following June). Dots represent regression coefficients and lines indicate 95% confidence intervals.

Table 3: Estimates for the effect of being born after December 31st by parental education level

At least one parent with completed upper-secondary education

Variable	(1) First stage: ESSA	(2) Age at 1st birth	(3) Total fertility	(4) Age at 1st marriage
Born after cutoff	0.471***	0.459***	-0.009	0.438**
	(0.013)	(0.129)	(0.020)	(0.138)
h	25	44	68	44
N	31,744	39,785	47,412	50,954
	(5) Have a partner	(6) Log earnings at 45	(7) Upper-secondary	(8) Years of school
Born after cutoff	-0.002	-0.011	0.016*	0.132*
	(0.005)	(0.015)	(0.008)	(0.059)
h	66	51	36	39
N	83,360	60,223	48,546	49,064

Neither parent with completed upper-secondary education

Variable	(9) First stage: ESSA	(10) Age at 1st birth	(11) Total fertility	(12) Age at 1st marriage
Born after cutoff	0.530***	0.287**	0.004	0.260**
	(0.011)	(0.086)	(0.015)	(0.079)
h	22	46	67	65
N	61,235	108,057	182,651	162,369
	(13) Have a partner	(14) Log earnings at 45	(15) Upper-secondary	(16) Years of school
Born after cutoff	-0.001	-0.008	0.005	0.063†
	(0.003)	(0.010)	(0.007)	(0.033)
h	70	46	46	50
N	191,090	117,052	126,412	136,575

Note. Numbers in parentheses are standard errors. Effects estimated with local linear regression (p=1) fit separately on both sides of the cutoff and triangular kernel weighing. h and p denote bandwidth used and degree of polynomial, respectively. Mean-squared error (MSE) optimal bandwidth used. Adjusted for year of birth dummies (year redefined as running from July until the following June).
Estimates of age at first birth and age at first marriage is taken from a sub-sample restricted to those with at least one birth or marriage. Estimates for education are measured as the probability to complete upper-secondary education and years of schooling by age 45. Earnings were scaled by the National Insurance Scheme's basic amount per that year, then transformed to log scale.
†$p<0.1$; $^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$

5.3 Drivers and Consequences of Delayed Childbearing

5.3.1 Drivers of Postponement: Education and Income

Previous research has shown that age at first employment acts as a mechanism for the effect of SSA on the timing of childbearing (Chang et al., 2021). Females' childbearing has shown to be particularly impacted by education, and the age at school start subsequently impacts the age at finished compulsory education, which has also been shown to impact the timing of childbearing among females (James & Vujić, 2019; Skirbekk et al., 2004). Similarly, earnings growth is known to be an important determinant of fertility timing (Hart, 2015). To examine these mechanisms in our sample , Figure 4 presents the discontinuity plot for age at completed education (plot a) and total years of schooling by age 45 (plot b) along with the effect of being born after the cutoff on yearly earnings for females (plot c) and males (plot d).[4] Individuals born after the school cutoff are on average older when they finished their education compared to those born before the cutoff. However, this timing effect does not appear to translate to differences in the total years of schooling. RDD estimates for total years of schooling and educational attainment both at 45 and by first birth are provided in Appendix C. When looking at earnings across the reproductive lifespan, individuals born after the cutoff on average earned less than those born before the cutoff until around age 30. From the early 30's until age 45 this difference disappeared, and there was no statistically significant difference in (log) pensionable income between those born before and after the cutoff date. Plots C and D in Figure 4 show that a higher graduation age is linked to a significant delay in earnings development, potentially explaining some of the postponement of childbearing. In short, our findings support the expectations from the Cohort Postponement Model, where delayed childbearing is linked to delayed educational completion and later wage growth.

5.3.2 Consequences of Postponement: GPA in the Next Generation

We examined the GPA and national test scores of children born to individuals born around the cutoff date to assess whether the differences in average age at first birth and first marriage translate to educational (dis)advantages for their children. For these analyses, we examined the educational outcomes for children whose parent was born after the cutoff compared to children whose parent was born before the cutoff date. Table 4 presents the results of these analyses. There were no significant differences in the GPA or national test scores of children with a parent born after December 31st compared to those with a parent born before the cutoff threshold. Thus, we did not find any evidence that the average increase in age at first birth among individuals born after the cutoff translated to differences in the school performance of their children. As such, and in line with previous studies, we cannot detect that postponement at the margin we study comes with any disadvantage to children.

[4] Age at completed education is measured as the age at which an individual attained their highest degree.

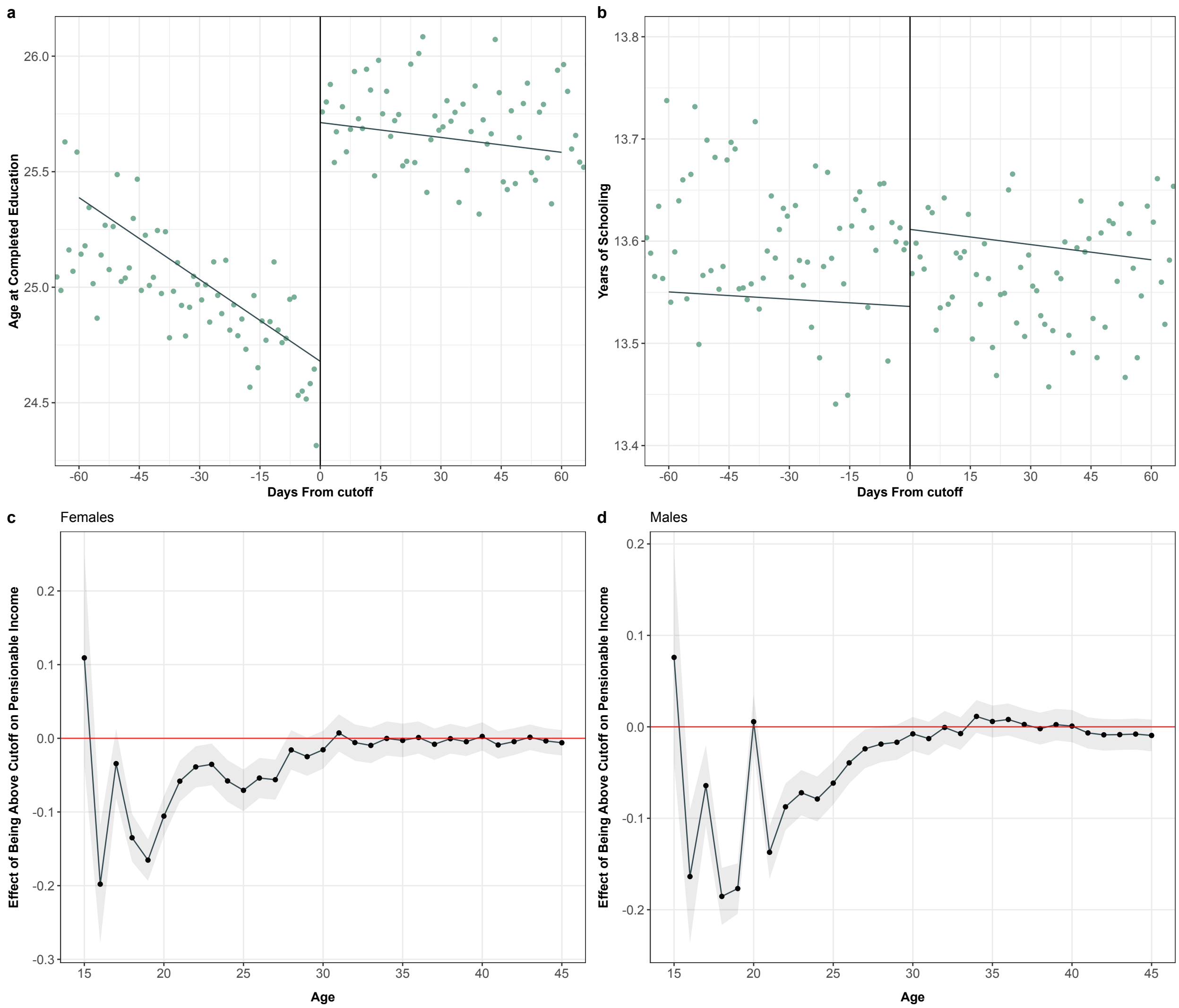

Figure 4: Effect of being born after December 31st on pensionable income and education. *Note.* Plot a represents the discontinuity in age at completed education, measured as age in which individuals obtained their highest degree. Plot b represents the discontinuity in total years of schooling by age 45. Dots represent sample averages by date of birth. Solid lines represent local linear regression estimated separately on each side of the cutoff. Plots c and d represent the yearly effects of being born after the cutoff on income for females and males with estimates from local linear regression estimated separately on either side of the cutoff with a bandwidth of ±60 days. Shaded region indicates 95% confidence intervals. Earnings were scaled by the National Insurance Scheme's basic amount per that year, and then transformed to log scale. All plots use triangular kernel weighing and are adjusted for year of birth dummies (year redefined as running from July until the following June).

Table 4: Estimates for the effect of being born after December 31st on children's education

Females

Variable	(1) GPA rank	(2) GPA (z-score)	(3) English 5th Exam (z-score)	(4) English 8th Exam (z-score)	(5) Reading 5th Exam (z-score)
Born after cutoff	0.004	0.013	-0.018	-0.003	-0.018
	(0.005)	(0.017)	(0.035)	(0.028)	(0.034)
h	56	57	47	42	44
N	80,551	81,986	20,098	29,467	20,558

Variable	(6) Reading 8th Exam (z-score)	(7) Reading 9th Exam (z-score)	(8) Math 5th Exam (z-score)	(9) Math 8th Exam (z-score)	(10) Math 9th Exam (z-score)
Born after cutoff	-0.023	-0.009	0.000	-0.020	-0.017
	(0.030)	(0.028)	(0.032)	(0.029)	(0.033)
h	36	53	49	40	43
N	25,154	27,820	23,181	28,161	22,575

Males

Variable	(5) GPA rank	(6) GPA (z-score)	(7) English 5th Exam (z-score)	(8) English 8th Exam (z-score)	(9) Reading 5th Exam (z-score)
Born after cutoff	0.002	0.005	0.004	-0.019	0.008
	(0.005)	(0.017)	(0.024)	(0.022)	(0.027)
h	71	69	70	65	52
N	97,160	94,306	41,602	55,628	33,879

Variable	(6) Reading 8th Exam (z-score)	(7) Reading 9th Exam (z-score)	(8) Math 5th Exam (z-score)	(9) Math 8th Exam (z-score)	(10) Math 9th Exam (z-score)
Born after cutoff	0.025	-0.005	0.027	0.007	0.002
	(0.024)	(0.027)	(0.029)	(0.027)	(0.026)
h	48	49	44	41	55
N	41,264	33,265	28,918	35,421	37,395

Note. Effects estimated with local linear regression (p=1) fit separately on both sides of the cutoff and triangular kernel weighing. h and p denote bandwidth used and degree of polynomial, respectively. Mean-squared error optimal bandwidth with bias-corrected standard errors. Adjusted for year of birth dummies (year redefined as running from July until the following June). GPA rank was calculated by cohort and then normalized to a 0-1 scale.

5.4 RDD Validity and Robustness Checks

To help determine whether the assumptions of the regression discontinuity design are met, we perform a number of robustness checks. These include assessing the possibility of selection bias in birth dates around the cutoff, sensitivity to the choice of bandwidth, and using a placebo cutoff date. If our results remain robust to these additional tests, this increases our confidence in the internal validity of the regression discontinuity design.

One concern is the potential for parents to strategically plan the timing of birth to avoid their children being born in December and therefore being the youngest in their class. This would mean that December born children are systematically different from those born in January, most likely in parental and socioeconomic characteristics. For example, parents with higher education may be more likely to be knowledgeable of the relative age effect and then strategically plan to avoid December births. Previous research has shown that date of birth may not be truly randomly distributed across the cutoff, and that particularly advantaged women are more likely to push births across the school entry cutoff (Huang et al., 2020). This planning of births could happen through two main channels. First, through strategic conception to avoid a due date in December, or second, through postponement of induced births or cesarean sections in late December. In our application, these two types of strategic timing will only bias estimates if there is a discontinuity around the cutoff of January 1st. General differences in parental characteristics by season of birth will be netted out by our running variable.

Parents may strategically plan the timing of birth to avoid their children being born before the January 1st cutoff date through another channel than strategic conception. Parents with due dates near the end of December may be able to delay planned cesarean sections or induced births until after the cutoff date. In terms of cesarean sections, Norway has historically quite low rates, increasing from around 2% in 1967 to 16% in 2016 (Norwegian Institute of Public Health, 2017). As a result, we do not believe there to be a substantial proportion of births in our cohorts of interest born via cesarean section around the cutoff, and even less so the proportion of cesarean sections that possibly were delayed until after the January 1st.

If parents with certain characteristics are more likely to avoid a December birth, either through strategic conception or shifting of births across the cutoff, this may result in selection bias as children born in December will be systematically different than children born in January. To assess for this potential selection bias, we plot sample averages for each day of birth in December and January for pre-determined characteristics. These characteristics would not be influenced by SSA, and therefore would provide information on whether parents with certain characteristics are able to time either conception or delivery to land just after the cutoff of January 1st. In Appendix D, Figure D1 shows the discontinuity plots for the individuals in our cohorts of interest born ±60 days around the cutoff. Here we see that children born before the cutoff have on average a slightly younger mother and father than those born after, though we cannot reject statistically that this is continuous across the cutoff. Additionally, we do not see any clear discontinuity in parental education levels across the cutoff. Table D8 in Appendix D provides the regression estimates for these parental characteristics. We do not find any statistically significant difference between those born before and after the cutoff in parental age and education, supporting the assumption that individuals around the cutoff do not differ on pre-determined characteristics.

Since parents may theoretically be capable of manipulating the birth date even after conception, we repeat the analyses by removing the observations that are most likely to be susceptible to this manipulation. Previous research has shown that when manipulation of births across the school entry cutoff occurred, this was mainly among births ±7 days around the cutoff (Huang et al., 2020). Our results are robust to removing observations that are ±7 days from the cutoff. It seems unlikely that the parents' preference for mode of delivery or assistance can move delivery date with more than 14 days. Our main results remain robust to removing the observations closest to the cutoff, providing support for the assumption that individuals have not self-selected to either side of the cutoff through delayed inductions or cesarean-sections.

Results may also be sensitive to the selection of the bandwidth used, and although utilizing the MSE-optimal bandwidths is suggested to avoid bias associated with bandwidth selection, plotting the results across a variety of bandwidths is important to discover possible sensitivity to bandwidth selection. In Figure D2, the main results for both males and females with varying bandwidths are plotted. We see that the results remain robust to the bandwidth selection. Finally, one of the main assumptions of the regression discontinuity approach is that individuals on either side of the cutoff would have similar outcomes if not for the treatment. While this cannot be directly tested, we can instead examine whether observations differ in the outcomes around an artificial, or placebo, cutoff point (Cattaneo et al., 2019). The intuition behind this approach is that if the discontinuity induced at the cutoff is indeed a result of the change in school starting age we would not expect to see a discontinuity in the outcomes at a placebo cutoff date. For this check, we estimated the outcomes for individuals born on either side of a placebo cutoff point set at May 1st. We do not find any statistically significant discontinuity in the outcomes of individuals around this artificial threshold. Results from robustness checks are presented in Appendix D, Table D9 and Figure D2.

6 Discussion and Conclusion

In this study we utilized Norway's school entry policy with exact dates of birth to examine the effects of school entry age on fertility and family formation among both men and women and expanded the literature with three main contributions. First, we examined how age at school enrollment affects the fertility and family formation of men, which to the best of our knowledge has not previously been done. We find that increased SSA led to an increase in age at first birth and age at first marriage for not just women, but also and to a similar degree among men. Despite impacts on the timing of men's fertility and family formation, overall fertility, childlessness, and probability to find a partner was not affected, in contrast to our expectations. The postponement effect of education for women's childbearing patterns has been found in many previous studies, however, interestingly we find that men's fertility timing is also sensitive to changes in age at school start. Our findings support the cohort postponement model, which states that one's age at school start will impact later fertility and family formation through delays in mediating factors and the importance of one's social age.

Our second main contribution is that the extraordinarily rich register data allow us to explore the mechanisms linking school starting age to delayed parenthood, according to the cohort postponement model. We found that those born after the school entry cutoff are on average older when finishing their education, while total years of schooling is unaffected. Both men and women born after the school entry cutoff experience a significant delay in earnings development, lasting until around age 30. They are also older when they form their first partnership. All these factors are known to be important drivers of postponed parenthood (Hart, 2015; Lappegård & Rønsen, 2005), but is rarely possible to address how this whole machinery works in conjunction in a plausibly causal design.

We do not, however, find support for the educational experience and social rank in class theory for either men or women. Despite effects on the timing of family formation and childbearing, we find little evidence for overall impacts on completed cohort fertility, probability of having a spouse, childlessness, long-term income or educational attainment. As previous research has found effects of SSA on school performance (Mavilidi et al., 2022; Pehkonen et al., 2015), mental health (Black et al., 2011; Matsubayashi & Ueda, 2015), and mixed results on educational attainment (Arnold & Depew, 2018; Bedard & Dhuey, 2006; Fredriksson & Öckert, 2014), educational experience could be a potential mechanism for the effect of SSA on childlessness and cohort fertility. However, for the cohorts in our analysis, it appears that despite the potential negative impacts of SSA on educational experience, educational attainment remains unaffected overall. As educational attainment and earnings are strong determinants of fertility and family formation among men, this is a likely reason why we do not find impacts on completed fertility.

Third, we described in detail the age- and parity-specific effects of school entry timing for both men and women. Our results show that SSA has an impact on not just the timing of the first birth, but across second and third or higher births, where those who enter school at an older age experience a postponement of births in the younger reproductive ages and a recuperation in the later ages in both men and women. To the best of our knowledge, this is the first study to examine the age- and parity-specific effects of school start in such detail. This provides important insights into how age at school start affects the *timing* of fertility, and the recuperation processes that reconcile postponed fertility with unchanged total number of children.

We also find evidence suggesting differences in the strength of the postponement effect induced by SSA between those from different parental education levels. We find that among those with parents of higher education, the postponement effect on first birth is on average 5.5 months compared to 3.4 months among those with lower parental educational attainment. This suggests that the postponement effect may be partially mediated through educational enrollment beyond compulsory school.

We also investigated whether postponement of fertility due to higher SSA had negative consequences for children's school performance. A large literature has examined maternal age at birth and how this may impact their children's health and educational outcomes (Barclay & Myrskylä, 2016; Pinheiro et al., 2019). Increased maternal age could be seen as a risk factor for health consequences at birth, suggesting a negative effect of being born to older mothers. Alternatively, older parents may have more resources, which would suggest a positive effect of higher maternal or paternal age at birth. Previous research using the school entry cutoff to isolate effects of increased maternal age at birth on child outcomes has found small negative effects on birth weight and gestational age, but with no impact on educational attainment or crime rates (Fredriksson et al., 2022). We find no evidence of negative effects of increased age at birth induced by school entry age on school performance measured in national test scores and GPA. Our results are in line with previous research, while providing new estimates on the potential impact of increased parental age at birth on children's outcomes.

Overall, the postponement effect we find is relatively large, at 4.0 months for men and 2.9 months for women. To put this in context, the average change in paternal age at first birth during the period 2010-2020 was 1.53 months, while for women the average change in maternal age at first birth during the same period was 1.96 months. The size of the maternal estimates are within the range found in previous studies (Chang et al., 2021; Fredriksson et al., 2022; Skirbekk et al., 2004). However, the results from this study should be interpreted within the context of its limitations. The effects represent the local average treatment effect (LATE) and therefore we cannot be certain that they would be as large or the same for individuals born farther from the cutoff. Similarly, the results may have limited generalizability beyond the Norwegian context. The effects of SSA likely vary based on the enforcement of the school entry cutoff, thus the effects may be different in education systems that are less rigid in the age at school entry or education sequencing. However, we note that effects for women are very similar in South Korea (Chang et al., 2021), which in some aspects can be considered a contrasting case.

Our study provides causal evidence for the impact of increased SSA on family formation and fertility in Norway. This is to the best of our knowledge, the first study to extend the effects of SSA to include estimates on men's fertility and family formation. It is increasingly important to consider how men's fertility is affected by education as the proportion of men with low educational attainment experiencing childlessness has been steadily rising over recent decades (Jalovaara et al., 2019). By including men's fertility, we can gain novel insights into the determinants of fertility dynamics and their gendered patterns. In future research on SSA it will be valuable to include men's fertility outcomes with more recent cohorts, and to examine whether it impacts the timing of men's fertility and family formation in contexts beyond Norway.

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Appendices

A Estimated School Starting Age

To create the estimated school starting age variable, we gathered the individual's year of graduation from compulsory school. First, we set the month of graduation at August, to account for the fact that while graduation typically occurs in June, enrollment occurs in August. Next, we subtracted the individual's exact date of birth from this graduation date, which gives the age at which the individual completed compulsory school. Finally, we subtracted 9 years from this age, which is the length of compulsory school for the cohorts in this analysis. Since grade retention is rarely practiced in Norway and students cannot fail compulsory school, virtually all individuals are exposed to 9 years of compulsory education from the time they enter schooling. Therefore, we believe this to be a reliable approximation of the age the individual enrolled in school. The estimates for the first stage relation between date of birth and estimated school starting age are presented in table A1. Those born after the cutoff were on average 6.1 months older when starting school compared to those born before.

Table A1: First-stage estimates

	Overall		**Females**	**Males**
Born after cutoff	0.551^{***} (0.005)	0.508^{***} (0.009)	0.545^{***} (0.012)	0.481^{***} (0.011)
h	60	20	24	23
N	234,726	81,463	47,915	47,276

Note. Effects estimated with local linear regression (p=1) fit separately on both sides of the threshold and triangular kernel weighing. h and p denote bandwidth (measured in days) used and degree of polynomial, respectively. Estimates are adjusted for year of birth dummies (redefined as running from July to June). Mean-squared error (MSE) optimal bandwidth and robust bias-corrected standard errors (in parentheses) are used. Estimate with 60 day bandwidth is also included.
*** indicates statistical significance at the 0.1% level, ** indicates statistical significance at the 1% level, * indicates statistical significance at the 5% level, and †indicates statistical significance at the 10% level.

B Age-specific fertility rates and parity-specific trends

Table B2 provides the regression discontinuity estimates on age-specific fertility rates for males and females by 5-year age groups. Tables B3 and B4 provide the estimates for the effect of being born after the cutoff on the probability to have a first, second, and third or higher birth by 5-year age groups for females and males.

Table B2: Estimates for age-specific fertility rates for males and females

Females

	(1)	(2)	(3)	(4)	(5)	(6)
Age-group	**15-19**	**20-24**	**25-29**	**30-34**	**35-39**	**40-44**
Born after	-0.0010	-0.0016	-0.0260	0.0269	0.0167	0.0070
cutoff	[-0.0020, -0.0004]	[-0.0360, 0.0039]	[-0.0460, -0.0060]	[0.0112, 0.0426]	[0.0023, 0.0312]	[0.0005, 0.0134]
h	41	41	51	74	48	43
Adj. R^2	0.00284	0.00399	0.00130	0.00163	0.00167	0.00015
N	80,910	80,910	100,716	143,880	92,808	84,821

Males

	(7)	(8)	(9)	(10)	(11)	(12)
Age-group	**15-19**	**20-24**	**25-29**	**30-34**	**35-39**	**40-44**
Born after	-0.0037	-0.0203	-0.0189	0.0091	0.0341	0.0027
cutoff	[-0.0069, -0.0005]	[-0.0325, -0.0081]	[-0.0369, -0.0008]	[-0.0121, 0.0302]	[0.0185, 0.0498]	[-0.0080, 0.0133]
h	66	63	57	44	54	52
Adj. R^2	0.00013	0.00269	0.00547	0.00022	0.00174	0.00028
N	131,510	127,435	116,294	87,723	110,220	106,108

Note. Numbers in brackets are 95% confidence intervals. Effects estimated with local linear regression (p=1) fit separately on both sides of the cutoff and triangular kernel weighing. h and p denote bandwidth used and degree of polynomial, respectively. Bandwidths used are mean-squared error optimal. Adjusted for year of birth dummies (year redefined as running from July until the following June).

Table B3: Estimates for parity transitions by age group for females

Females

	First birth					
	(1)	(2)	(3)	(4)	(5)	(6)
Age-group	**15-19**	**20-24**	**25-29**	**30-34**	**35-39**	**40-44**
Born after	-0.0079	-0.0072	-0.0020	0.0137	0.0063	0.0017
cutoff	[-0.0171, 0.0013]	[-0.0203,0.0058]	[-0.0154, 0.0113]	[0.0051, 0.0223]	[0.0005, 0.0121]	[-0.0009, 0.0042]
h	39	46	44	60	52	48
Adj. R^2	0.00264	0.00232	0.00018	0.00382	0.00148	0.00019
N	74,956	90,854	86,804	117,523	100,716	92,808

	Second birth					
	(7)	(8)	(9)	(10)	(11)	(12)
Age-group	**15-19**	**20-24**	**25-29**	**30-34**	**35-39**	**40-44**
Born after	-0.00178	-0.0090	-0.0109	0.0160	0.0019	0.0021
cutoff	[-0.0036, 0.0000]	[-0.0188, 0.0008]	[-0.0230, 0.0013]	[0.0056, 0.0264]	[-0.0065, 0.0102]	[-0.0007, 0.0049]
h	72	46	53	62	42	68
Adj. R^2	0.00061	0.00284	0.00118	0.00203	0.00181	0.00017
N	141,847	88,829	102,771	121,606	82,888	133,701

	Third and higher births					
	(13)	(14)	(15)	(16)	(17)	(18)
Age-group	**15-19**	**20-24**	**25-29**	**30-34**	**35-39**	**40-44**
Born after	-0.0001	-0.0019	-0.0138	0.0023	0.0076	0.0017
cutoff	[-0.0005, 0.0004]	[-0.0053, 0.0014]	[-0.0213, -0.0062]	[-0.0056, 0.0105]	[0.0015, 0.0136]	[-0.0010, 0.0044]
h	54	56	57	70	70	53
Adj. R^2	0.00002	0.00046	0.00154	0.00059	0.00026	0.00008
N	106,721	108,634	110,581	137,762	137,762	102,771

Note. Numbers in brackets are 95% confidence intervals. Effects estimated with local linear regression (p=1) fit separately on both sides of the cutoff and triangular kernel weighing. h and p denote bandwidth used and degree of polynomial, respectively. Bandwidths used are mean-squared error optimal. Adjusted for year of birth dummies (year redefined as running from July until the following June).

Table B4: Estimates for parity transitions by age group for males

Males

	First birth					
	(1)	(2)	(3)	(4)	(5)	(6)
Age-group	**15-19**	**20-24**	**25-29**	**30-34**	**35-39**	**40-44**
Born after	-0.0036	-0.0157	-0.0018	0.0114	0.0157	-0.0014
cutoff	[-0.0067, -0.0006]	[-0.0251, -0.0063]	[-0.0141, 0.0105]	[-0.0004, 0.0232]	[0.0072, 0.0242]	[-0.0061, 0.0032]
h	69	63	51	43	40	47
Adj. R^2	0.00013	0.00278	0.00161	0.00239	0.00233	0.00040
N	137,778	124,435	101,932	87,723	81,509	93,771

	Second birth					
	(7)	(8)	(9)	(10)	(11)	(12)
Age-group	**15-19**	**20-24**	**25-29**	**30-34**	**35-39**	**40-44**
Born after	0.0000	-0.0040	-0.0133	0.0059	0.0129	0.0015
cutoff	[-0.0005, 0.0006]	[-0.0086, 0.0006]	[-0.0232, -0.0034]	[-0.0052, 0.0171]	[0.0041, 0.0217]	[-0.0036, 0.0066]
h	50	71	60	55	55	62
Adj. R^2	0.00003	0.0008	0.00419	0.00023	0.00195	0.00054
N	101,932	144,004	121,266	112,206	112,206	123,378

	Third and higher births					
	(13)	(14)	(15)	(16)	(17)	(18)
Age-group	**15-19**	**20-24**	**25-29**	**30-34**	**35-39**	**40-44**
Born after	NA	-0.0005	-0.0045	-0.0053	0.0025	0.0050
cutoff	[NA, NA]	[-0.0019, 0.0009]	[-0.0096, 0.0006]	[-0.0138, 0.0031]	[-0.0058, 0.0107]	[0.0006, 0.0093]
h	54	67	58	50	48	71
Adj. R^2	NA	-0.00000	0.00144	0.00211	0.00032	0.00015
N	110,220	135,617	116,294	101,932	95,787	144,004

Note. Numbers in brackets are 95% confidence intervals. Effects estimated with local linear regression (p=1) fit separately on both sides of the cutoff and triangular kernel weighing. h and p denote bandwidth used and degree of polynomial, respectively. Bandwidths used are mean-squared error optimal. Adjusted for year of birth dummies (year redefined as running from July until the following June).

C Additional Estimates

As results are presented using mean-squared error (MSE) optimal bandwidths, below we present the main results using ±60 day bandwidths. Table C5 presents the results on family formation and fertility using 60 day bandwidths. The results are in line with estimates using MSE-optimal bandwidths. For females, being born after the cutoff results in an increased age at first birth of 4.1 months, and for males this is 4.0 months. Females born after the cutoff are also on average 4.7 months older at first marriage, and males are on average 2.4 months older. The probability to have a spouse and overall fertility are not affected.

Table C6 provides estimates for the effect of school starting age on male's and female's educational attainment and income using MSE optimal bandwidths. For females, individuals born after the cutoff were on average 11.0 months older when leaving school and had on average 1.1 more months of education by age 45, but were not more likely to complete upper-secondary schooling by age 45 or have higher educational attainment in years of schooling by first birth compared to those born before the cutoff. For males, those born after the cutoff were on average 14.2 months older when leaving school, but were not statistically significantly different in terms of years of schooling either by age 45 or first birth, or in the probability to complete upper-secondary schooling by age 45 compared to those born before the cutoff. Estimates using 60 day bandwidth are presented in Table C7. The results are in line with the estimates using MSE optimal bandwidths.

Table C5: Estimates for the effect of being born after December 31st on fertility

Females

Variable	(1) Age at 1st birth	(2) Age at birth	(3) Total fertility	(4) Childless
Born after	0.346***	0.313***	-0.008	-0.001
cutoff	(0.070)	(0.062)	(0.014)	(0.004)
h	60	60	60	60
N	102,993	102,993	115,480	115,480

Variable	(5) Age at 1st marriage	(6) Have a spouse	(7) Partner's age	(8) Partner's education
Born after	0.394***	-0.001	0.119	0.040
cutoff	(0.074	(0.003)	(0.080)	(0.035)
h	60	60	60	60
N	108,419	115,480	101,567	92,471

Males

Variable	(9) Age at 1st birth	(10) Age at birth	(11) Total fertility	(12) Childless
Born after	0.329***	0.299***	0.009	-0.005
cutoff	(0.078)	(0.071)	(0.016)	(0.005)
h	60	60	60	60
N	97,358	97,358	119,246	119,246

Variable	(13) Age at 1st marriage	(14) Have a spouse	(15) Partner's age	(16) Partner's education
Born after	0.204*	-0.002	0.147*	0.064†
cutoff	(0.082)	(0.004)	(0.072)	(0.036)
h	60	60	60	60
N	106,697	119,246	97,303	89,391

Note. Numbers in parentheses are standard errors. Effects estimated with local linear regression (p=1) fit separately on both sides of the cutoff and triangular kernel weighing. h and p denote bandwidth used and degree of polynomial, respectively. Adjusted for year of birth dummies (year redefined as running from July until the following June).
Estimates of age at first birth, average age at birth, and partner characteristics are taken from a sub-sample restricted to those with at least one birth. Estimates of age at first marriage are taken from a sub-sample restricted to those with at least one marriage. Estimates for partner's age and education (measured in years of schooling) are measured at first birth.
†$p<0.1$; $^{*}p<0.05$; $^{***}p<0.001$

Table C6: Estimates for the effect of being born after December 31st on education: MSE bw

Females

Variable	(1) Upper-secondary by age 45	(2) YOS by age 45	(3) Upper-secondary by first birth	(4) YOS by first birth	(5) Age at Completed Education
Born after cutoff	0.009	0.093*	-0.007	0.011	0.916***
	(0.006)	(0.042)	(0.010)	(0.047)	(0.169)
h	70	54	31	40	47
N	137,762	106,335	54,864	70,084	92,808

Males

Variable	(6) Upper-secondary by age 45	(7) YOS by age 45	(8) Upper-secondary by first birth	(9) YOS by first birth	(10) Age at Completed Education
Born after cutoff	0.010	0.056	0.008	0.045	1.182***
	(0.008)	(0.043)	(0.009)	(0.046)	(0.127)
h	43	46	45	46	47
N	87,723	93,224	74,962	76,181	95,787

Note. Effects estimated with local linear regression (p=1) fit separately on both sides of the cutoff and triangular kernel weighing. h and p denote bandwidth used and degree of polynomial, respectively. Bandwidths used are mean-squared error optimal with robust bias-corrected standard errors (in parentheses). Adjusted for year of birth dummies (year redefined as running from July until the following June). Estimates of years of schooling (YOS) and upper-secondary school completion by first birth are taken from a sub-sample restricted to those with at least one birth. Age at completed education is measured as the age at which an individual attained their highest degree.
$^{*}p < 0.05$; $^{***}p < 0.001$

Table C7: Estimates for the effect of being born after December 31st on education: 60 day bw

Females

Variable	(1) Upper-secondary by age 45	(2) YOS by age 45	(3) Upper-secondary by first birth	(4) YOS by first birth	(5) Age at Completed Education
Born after	0.006	0.097^{**}	0.005	0.050	0.963^{***}
cutoff	(0.006)	(0.033)	(0.007)	(0.034)	(0.185)
h	60	60	60	60	60
N	115,480	115,056	102,993	102,560	115,480

Males

Variable	(6) Upper-secondary by age 45	(7) YOS by age 45	(8) Upper-secondary by first birth	(9) YOS by first birth	(10) Age at Completed Education
Born after	0.009	$0.056^{\dagger}$	0.009	0.046	1.134^{***}
cutoff	(0.006)	(0.032)	(0.007)	(0.034)	(0.150)
h	60	60	60	60	60
N	119,246	118,544	97,358	96,847	119,246

Note. Effects estimated with local linear regression (p=1) fit separately on both sides of the cutoff and triangular kernel weighing. h and p denote bandwidth used and degree of polynomial, respectively. Adjusted for year of birth dummies (year redefined as running from July until the following June). Estimates of years of schooling (YOS) and upper-secondary school completion by first birth are taken from a sub-sample restricted to those with at least one birth. Age at completed education is measured as the age at which an individual attained their highest degree.
$\dagger p < 0.1$; $^{**}p < 0.01$; $^{***}p < 0.001$

D RD Validity and Robustness Checks

D.1 Pre-determined characteristics

To examine whether children born just before the cutoff are different in parental characteristics to those born just after the cutoff, we plot sample averages for each day of birth in December and January for pre-determined characteristics in Figure D1. Regression estimates for parental characteristics are presented in Table D8. We do not find any evidence that individuals born on either side of the cutoff date vary by parental pre-determined characteristics.

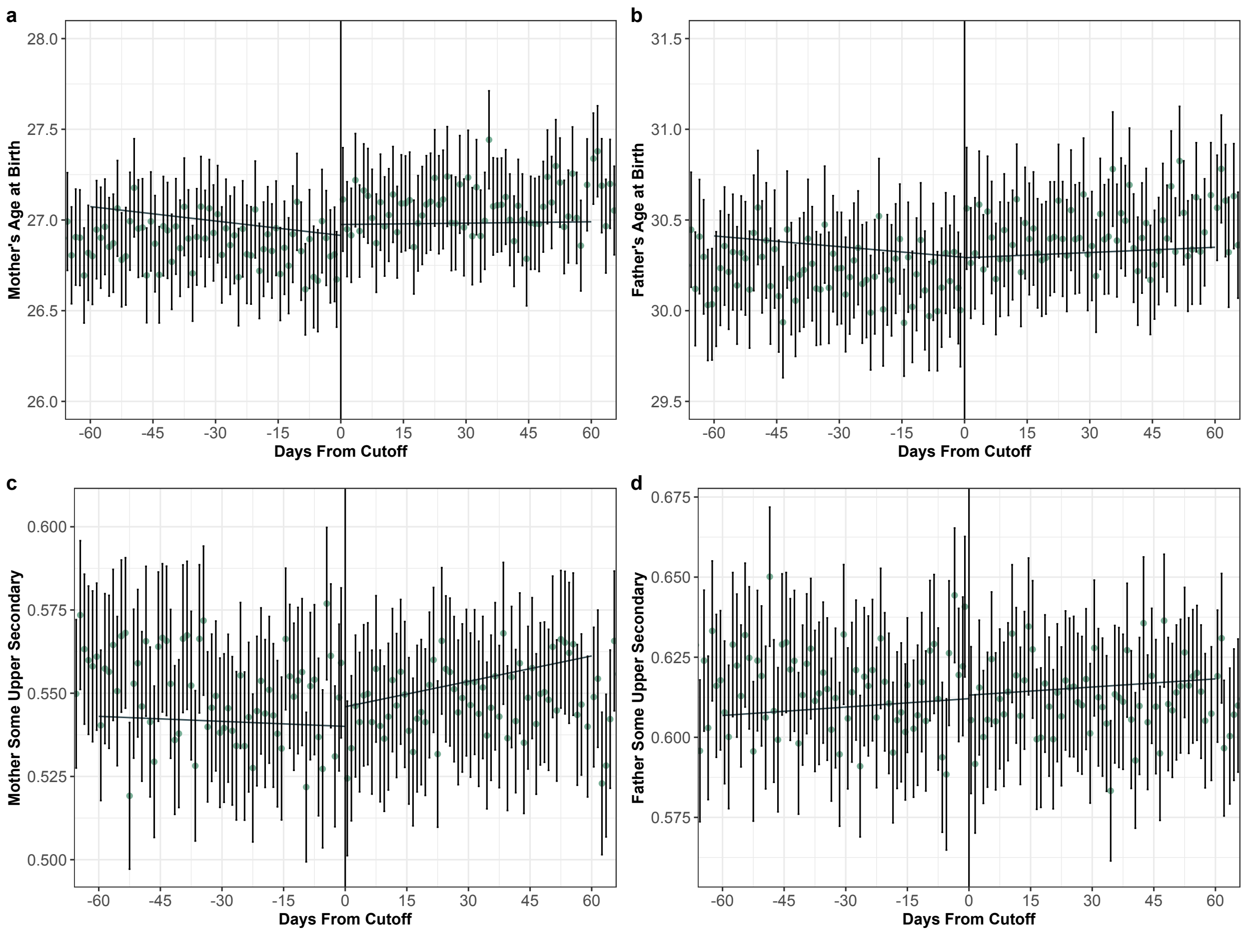

Figure D1: Individual's birth dates and pre-determined characteristics. *Note.* Dots represent sample averages with 95% confidence intervals by date of birth. Solid lines represent local linear regression estimated separately on each side of the cutoff with triangular kernel weighing and adjusted for year of birth dummies (year redefined as running from July until the following June).

Table D8: Robustness check with parental characteristics

Variable	(1) Mother's age at birth	(2)	(3) Father's age at birth	(4)
Born after cutoff	0.060	0.069	-0.002	0.001
	(0.054)	(0.071)	(0.063)	(0.080)
h	60	45	60	52
N	234,671	180,535	232,887	207,226
Variable	(5) Mother's education	(6)	(7) Father's education	(8)
Born after cutoff	0.006	0.008	0.001	-0.006
	(0.004)	(0.005)	(0.004)	(0.006)
h	60	68	60	37
N	233,411	269,950	229,889	145,309

Note. Effects estimated with local linear regression (p=1) fit separately on both sides of the cutoff and triangular kernel weighing. h and p denote bandwidth used and degree of polynomial, respectively. Adjusted for year of birth dummies (year redefined as running from July until the following June). Estimates of parental education are measured as probability to attend upper-secondary school.

D.2 Robustness Checks

Table D9 presents the results of the ”donut” RD and placebo cutoff robustness checks. For the ”donut” RD estimates, observations most likely to be strategically manipulated around the threshold are removed (±7 days from the cutoff). The results were robust to removing these observations, where females born after the cutoff were on average 5.6 months older at first birth, and males were on average 4.4 months older. In order to assess the RD assumption that individuals on either side of the threshold would on average exhibit similar outcomes if not for the treatment induced at the cutoff, the main outcomes were estimated using a placebo cutoff date of May 1st. We did not find any statistically significant differences in the age at first birth nor total fertility across the placebo cutoff date. Finally, to assess whether the results are sensitive to the choice of bandwidth, Figure D2 presents the estimates using various bandwidths from ±14 days to ±180 days around the threshold. The results were robust to variations in the choice of bandwidth.

Table D9: Robustness checks for main estimates

Females	*Donut-RD*		*Placebo Cutoff: May 1st*	
Variable	(1) Age at 1st birth	(2) Total fertility	(3) Age at 1st birth	(4) Total fertility
Born after	0.466***	-0.023	-0.014	0.009
cutoff	(0.083)	(0.017)	(0.117)	(0.029)
h	60	60	26	18
N	91,608	102,747	54,054	42,454

Males	*Donut-RD*		*Placebo Cutoff: May 1st*	
Variable	(5) Age at 1st birth	(6) Total fertility	(7) Age at 1st birth	(8) Total fertility
Born after	0.370***	0.008	-0.051	-0.015
cutoff	(0.093)	(0.019)	(0.145)	(0.029)
h	60	60	21	19
N	86,811	106,366	42,097	46,693

Note. Effects estimated with local linear regression (p=1) fit separately on both sides of the cutoff and triangular kernel weighing. Those born ±7 days from the cutoff are removed in donut estimates. h and p denote bandwidth used and degree of polynomial, respectively. MSE optimal bandwidth and robust bias-corrected errors used for placebo cutoff estimates. Adjusted for year of birth dummies (year redefined as running from July until the following June). Estimates of age at first birth are taken from a sub-sample restricted to those with at least one birth.
$^{***}p < 0.001$

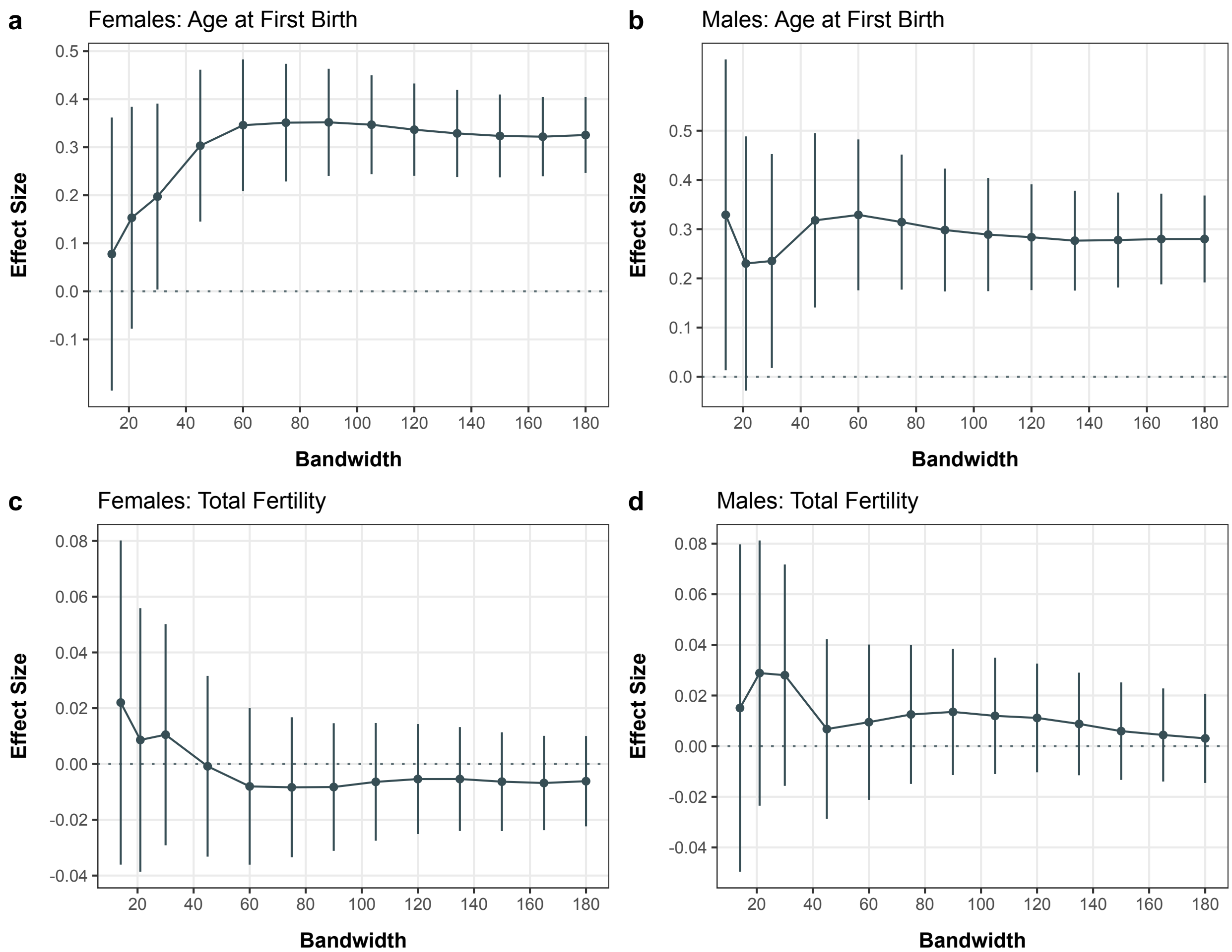

Figure D2: Bandwidth Variations. *Note.* Dots represent estimates from local linear regression fit separately on each side of the cutoff with triangular kernel weighing and adjusted for year of birth dummies (year redefined as running from July until the following June). Lines represent 95% confidence intervals. Bandwidth measured in days relative to January 1st cutoff.