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Causal Impact of School Starting Age on the Tempo of Childbirths: Evidence from Working Mothers and School Entry Cutoff Using Exact Date of Birth

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Abstract

Many studies show that females' age at first childbirth affects important outcomes of these females and their offspring such as health- and socioeconomic-related variables. This paper analyzes whether there is a causal relationship between working mothers' school entry age and the timing at which they give birth by exploiting Korea's elementary school entry cutoff regulation. Using administrative employment insurance data that record the fertility history of female working mothers together with regression discontinuity design, we find that a year's delay in age at school starting increases age at first and second childbirth by approximately 3 and 4 months, respectively. We also find that one of the mechanisms that affects the relationship between these two variables is age at first employment. The estimated effects of SSA are likely to be salient in a country where educational sequence that a student experience is rigid.

Keywords School starting age · Fertility tempo · Age at first employment · Regression discontinuity design

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1 Introduction

It is widely accepted both among academic scholars and practitioners that delay in childbearing leads to many significant demographic consequences. One such ramification is that it weakens the reliability of indicators of fertility trends such as total fertility rate (TFR). If later childbearing is pervasive, changes in TFR at a certain period may not reflect the change in fertility level. Rather, it may merely reflect the change in fertility tempo (Ní Bhrolcháin, 2011; Shoen, 2004). Another issue associated with later childbirths is that it affects multifarious outcomes of children and mothers. Many studies have found that later childbirth affects children's health (e.g., Barclay & Myrskylä, 2016; Borra et al., 2016; Brion et al., 2008; Durkin et al., 2008; Jacobsson et al., 2004; Johnson et al., 2009; Myrskylä & Fenelon, 2012; Schulkind & Shapiro, 2014; Sutcliffe et al., 2012) and cognitive (e.g., Addo et al., 2016; Duncan et al., 2018; Fishman, 2018; Geronimus et al., 1994) outcomes. Many research findings have also shown that later childbirths influence mothers' health- (e.g., Jacobsson et al., 2004; Sauer, 2015), educational- (e.g., Addo et al., 2016), and labor-related (e.g., Bratti & Cavalli, 2014; Leung et al., 2016) outcomes.

Furthermore, delay in childbirths has implications for public finance-related issues. For example, changes in fertility tempo make fertility trends difficult to interpret (Neels et al., 2017) and accordingly may reduce the effectiveness of public policies that are linked to fertility trends. Murphy et al. (2006) point out that children born to old mothers are less likely to have surviving parents that are in good health when they become adults, which may lead to an increase in the financial burden of their households. In addition, some studies show that late childbirth is associated with higher medical costs (e.g., Triunfo et al., 2019) and consequently affects governments' healthcare costs. Research also shows that delay in childbirths affects the quantum of fertility (e.g., Schmidt et al., 2012; te Velde et al., 2012), which in turn affects the dependency ratio, so there are consequences in terms of financing social security systems.

As can be expected from many of the studies mentioned above, a large body of existing literature has engaged in identifying the factors that affect the timing of childbirths. For example, Adserá (2004) and Aassve et al. (2006) examined the effect of employment-related factors on fertility timing. Rindfuss et al. (2007) studied the impact of childcare availability on age at first childbirth. Furthermore, many studies have analyzed the relationship between educational participation and later childbearing (e.g., Amin & Behrman, 2014; Neels et al., 2017; Tropf & Mandemakers, 2017).

The purpose of this study is to analyze the causal impact of school starting age (SSA) on the timing of childbirths, a topic that, to the best of our knowledge, has not been studied to date. Isolating the causal impact of SSA on fertility tempo is important because SSA is a policy variable in many countries (Leuven & Oosterbeek, 2007) and may have ramifications for demographic-related outcomes. For instance, many studies document that delaying SSA is beneficial for children's educational achievement (e.g., Bedard & Dhuey, 2006; McEwan & Shaprio, 2008, Dhuey et al., 2019). Because of such benefits, many countries have implemented legislation aimed at promoting educational outcomes by delaying SSA (Deming & Dynarski, 2008). Such delay may then have spillover effects on fertility-related outcomes because fertility-related events are, in general, affected by school graduation age and age at first employment, both of which are affected by SSA.

Delaying SSA may delay school graduation age and age at first employment and consequently increase age at first childbirth. Contrarily, its effect on fertility timing may be trivial if such effects dissipate with age. If the former scenario is pervasive, policymakers, particularly in low-fertility countries with high incidence of late childbearing, should consider both the benefits and costs of changing SSA. From a policy perspective, therefore, it is necessary to determine whether there is a causal relationship between SSA and fertility timing.

The SSA effect with respect to fertility tempo is likely to be salient, especially in the South Korean context because children follow a well-sequenced educational trajectory during their school period with few interruptions due to issues such as repeating grades or dropouts. Elementary (6 years) and middle (3 years) school were compulsory for our analysis cohorts, and according to Statistics Korea, repeating and dropout rates are less than 1% during these two educational levels. Consequently, almost all the high school entering students are of the same school starting cohort. Moreover, the dropout rate in high school is also very low in Korea. The average high school dropout rate is about 1% for our analysis cohorts. We argue, therefore, that such non-interrupted sequence that South Korean children follow during their school period is likely to induce the fertility tempo effect of SSA.

Note, however, that isolating the causal impact of SSA on fertility timing is empirically challenging because SSA is an endogenous variable influenced by socioeconomic variables such as income and parental education. In this study, we exploit Korea's elementary school entry cutoff policy to estimate the causal impact of SSA on the timing of childbirths. In Korea, children born in February enter compulsory elementary school a year before those who are born in March of the same year. As a consequence of this policy, children who are born, say, on February 28 of a given year enter school a year earlier than children born, say, on March 1 of that same year.

The former children are likely to be similar to the latter both observably and unobservably because the exact date of birth is exogenous. To analyze an impact of a treatment, many studies use a child's quarter of birth as an instrument for a treatment arguing that children born in different quarters are similar systematically in terms of their observable and unobservable characteristics (e.g., Angrist & Krueger, 1991). If the quarter of birth instrument is exogenous as to unobservables and highly correlated with a treatment of interest, then one can causally isolate an effect of a treatment. Note, however, that some studies find that quarter of birth is endogenous because parents can time the childbirth (Buckles & Hungerman, 2013). In this study, we use date of birth as an instrument for SSA. While conflicting evidence exists as to whether date of birth is exogenous (Dickert-Conlin & Elder, 2010; Huang et al., 2020), we argue that exact date of birth is less likely to be endogenous, especially in our context because our analysis cohorts were born in the period 1979–1992, when the share of childbirths by cesarean section in Korea was very low, e.g., the share in 1985 was 6% according to government statistics provided by the National Health Insurance System in Korea.[1]

By exploiting the school entry cutoff policy mentioned above, we use a regression discontinuity design (RDD) to establish the causal relationship between SSA and fertility tempo. Because children born around the March 1 threshold are observably and unobservably similar in many aspects that affect the timing of when they give birth, with the only difference between those born just before and after the threshold being SSA, any observed difference in the timing of childbirths is likely to be driven by this difference in SSA. Our regression discontinuity analyses show that a year's delay in SSA leads to a postponement of the timing of first and second childbirths by approximately three and 4 months, respectively. Theoretically, the estimated impact of SSA on the timing of childbirths may have been driven by many channels such as school graduation age. Because of the data availability, we were able to identify one such mechanism, i.e., labor market entry age. Our results show that females who entered school a year earlier started their first job about 2 to 3 months sooner than those of the same biological age who entered school a year later.

Many studies argue that fertility-related events, particularly during adulthood, are spaced in a rigid manner. Note, however, that many of the findings in these studies are correlational rather than causal, so our results help establish the internal validity of this argument because the effect estimates are identified off from the quasi-random variation. Knowing the time-rigidity of fertility events is beneficial from a policy perspective because if the events are spaced in a rigid manner, then policymakers may intervene earlier in people's life course to influence fertility-related outcomes. Note, furthermore, that although public policies such as SSA seem less likely to be related to fertility-related outcomes at first glance given the long time lag between the two factors, our study suggests that such policies can have significant spillover effects on the fertility choices made by families. As emphasized by Lopoo and Raissian (2012), considering the potential impact of many policies on fertility-related outcomes deserves more attention in the policy arena, particularly in developed countries that are creating strategies to promote fertility to support an aging population.

2 Theoretical Background and Literature Review

Theoretically, SSA may affect fertility tempo through several channels. One such channel is educational attainment. A large volume of literature shows that children who enter school relatively later tend to earn more years of education (e.g., Angrist & Krueger, 1992; Black et al., 2011; Dobkin & Ferreira, 2010). Women are less likely to have a child during their educational period due to the incapacitation or incarceration effect, so more education leads to delay in the timing of

[1] We tested whether the C-section rate affects effect estimates. Specifically, we derived effect estimates when the C-section rate was very low using the 1979–82 cohorts in which the rate is much lower than 1985. The results were qualitatively similar.

childbirths. Furthermore, women with more education tend to have fewer children (Kravdal & Rindfuss, 2008; Neels et al., 2017), channeling the tempo and quantum effect of SSA. Educational attainment also generates substitution and income effects on fertility-related events (Becker, 1960; Black et al., 2013), and depending on the magnitude of these two effects, it may either increase or decrease total fertility.

Another potential mechanism that mediates the relationship between SSA and the timing of fertility outcomes is through its effect on the timing of school graduation. Children who start school later are more likely to end formal education later during their lifetime (Bloom & Trussell, 1984). As the simple life-cycle model of human capital investment, fertility, and labor supply shows (Becker, 1981; Cigno, 1991), this has significant consequences on fertility-related events because age at school completion affects various mediating outcomes such as age at first employment and marriage, many of which affect the timing and spacing of fertility (Blossfeld & Huinink, 1991; Brien & Lillard, 1994; Gutiérrez-Domènech, 2008; Marini & Hodsdon, 1981; Raymo, 2003; Skirbekk, 2005).

The cohort postponement model can also be used to explain the relationship between SSA and fertility timing (Goldstein & Cassidy, 2014). A significant number of academic debates exist as to whether the cohort- or period-based postponement model explain more of the changes in fertility-related events (e.g., Bongaarts & Feeney, 1998; Kim & Schoen, 2000; Ní Bhrolcháin, 1992). If the cohort-based postponement model is more pervasive, it is likely that children who start school later than those of a similar biological age would initiate childbearing activities relatively later, on average, because their behavior is affected more by their social age than their biological age.

As can be expected from the discussion above, SSA is, in theory, related to fertility behavior. Note, however, that few studies have investigated the causal relationship between the two variables empirically. Most of the existing research has studied the relationship between years of education and fertility outcomes (e.g., Cygan-Rehm & Maeder, 2013; Martin, 2000; Ní Bhrolcháin & Beaujouan, 2012; Osili & Long, 2008; Rindfuss et al., 1996; Silles, 2011), while relatively little research analyzes the effect of the timing of education such as SSA. The exceptions are studies conducted by Skirbekk et al. (2004) and Neels et al. (2017), which analyzed the impact on fertility timing of the age at leaving education.

Our study contributes to previous studies in several key aspects. First, our study is, to our knowledge, the first analysis to isolate the causal impact of SSA on the timing of fertility using plausibly exogenous variation in SSA. Second, contrary to previous studies that use month of birth or educational reform as an exogenous variation, our research design exploits the exact date of birth. Hence, we argue that the internal validity of our estimated results is high (McCrary & Royer, 2011). Third, our data and analysis sample allow us to illuminate one potential mechanism that mediates SSA and fertility timing. Specifically, we analyze whether SSA first influences age at first employment, and whether it subsequently affects age at first and second childbirth. Such mechanism analysis will help examine whether many of the events during adulthood are spaced in a rigid manner.

3 Institutional Background

In Korea, the school year starts at the beginning of March, and the birth cohort for each school year is strictly determined by children's exact date of birth. According to Chapter 2 (Compulsory Education) of the Enforcement Decree of the Elementary and Secondary Education Act, children whose age reach elementary school age on March 1 are admitted to school. In Korea, children who become age seven after February of year t enter elementary school. That is, children who are born in January or February of year t enter elementary school in year $t + 6$ and those who are born in March of year t enter the school in year $t + 7$. Consequently, children who are born, say, between February 20 and 29 on a given year are admitted to school a year earlier than those who are born, say, between March 1 and 10 of the same year.[2] Article 15 of the Decree was amended in 2008 such that there is no difference in SSA between February- and March-born children. Note, however, that the birth cohorts examined in this study are unaffected by this amendment because the latest birth cohort employed for our analysis entered school before 2000.[3]

The rate of non-compliance with the Decree is extremely low in Korea, particularly for the analysis cohort in this study. According to the Statistical Yearbook of Education, the share of entering students whose age is outside the school entry age range is less than 0.008 for those born between 1979 and 1986 and approximately 0.012 for those born between 1987 and 1990. Thus, mothers who were born, say, on February 28 of year t must have entered school one year earlier than those who were born, say, on March 1 of year t. Given that their biological age is the same, and observable and unobservable baseline characteristics are likely to be homogenous between the two groups, we argue that any observed differences in fertility-related outcomes between the two groups are driven causally by the difference in SSA.

The public education system in Korea is threefold: 6 years of elementary school, 3 years of middle school, and 3 years of high school. Elementary and middle school are compulsory, and repeating and dropout rates is extremely low in high school (less than 2%). Also, high school graduation rate is over 95% (during 1998–2002), according to Statistics Korea, and such shares are stable across academic years. Thus, almost all students within the same cohort graduate high school at the same

[2] Article 15 (Making List of Schoolchildren) of Chapter 2 of the Decree stipulates the following: "The head of an Eup/Myeon/Dong (i.e., administrative district) shall investigate children living in his jurisdiction as of November 1 who reach elementary school age (excluding those who attend school at the age of five pursuant to Article 13 (2) of the Act) on March 1 of the following year and make a list of schoolchildren by November 30 of the year concerned.".

[3] Article 15 of the Decree was amended as the following: "The head of an Eup/Myeon/Dong shall investigate children living in his/her jurisdiction as of October 1 every year who reach 6 years of age from January 1 to December 31 of the year (excluding those who are going to school by entering an elementary school in the next year of the year to which the date on which they reach 5 years of age belongs pursuant to the forepart of Article 13 (2) of the Act) and make a list of schoolchildren by October 31 of the year. In such cases, children excluded from a list of schoolchildren because they have re-quested postponement of entering a school in the year to which the date on which they reach 6 years of age belongs pursuant to paragraph (3) shall be included therein.".

Table 1 Demographic indicators of South Korea by cohort

Cohort	% married during the age of 25–29	% married during the age of 30–34	% of women with no childbirth experience	Average total number of childbirths
1979	0.391	0.217	0.278	1.296
1980	0.391	0.238	0.311	1.209
1981	0.390	0.230	0.357	1.095
1982	0.385	0.209	0.411	0.953
1983	0.385	0.170	0.480	0.812
1984	0.388	0.113	0.571	0.648
1985	0.382	0.038	0.647	0.513
1986	0.330	0.000	0.726	0.384
1987	0.244	0.000	0.798	0.282
1988	0.150	0.000	0.861	0.192
1989	0.082	0.000	0.895	0.145
1990	0.021	0.000	0.922	0.104
1991	0.000	0.000	0.945	0.075
1992	0.000	0.000	0.966	0.041

The estimates are computed from the 2015 census data (2% random sample)

age (i.e., age of 19), and it is less likely that repeating and dropout rates are significantly different between those who are born in February and March.

The TFR in Korea was 4.53 in 1970, and it hit the replacement level fertility in 1983. Since then, the TFR declined most of the time, and as of 2019, the TFR in Korea is 0.918, the lowest fertility rate in the world. Table 1 presents information, by cohort, on the percentage of females who are married during the age of 25 to 29 and 30 to 34, share of women with no childbirth experience, and average total number of childbirths. We estimated these statistics using the 2015 census data (2% random sample), the latest census data we can obtain at this point. As can be seen from Table 1, the share of age at first marriage is similar for older cohorts (1979–1984). The share of women with no childbirth experience is 27.8% for the earliest cohort (1979), and the share is gradually increasing for later cohorts. The average total number of childbirths is 1.29 for the earliest cohort, and again the rate of decline in the average is quite stable for later cohorts.

4 Empirical Strategy and Data

4.1 Empirical Strategy

We exploit Korea's school entry policy based on exact date of birth to isolate the causal impact of SSA on our outcomes of interest. The policy allows us to use RDD because our treatment variable, SSA, is a discontinuous function of date of birth, the assignment variable. Specifically, treatment variable D_{it} is a dummy variable indicating whether a mother entered school a year later and is defined as follows:

$$D_{it} = \mathbb{1}\{X_{it} \geq 0\}$$

,where X_{it} is the distance function that denotes the distance (in days) from the cutoff point (i.e., March 1) with subscripts i and t indicating mothers and birth year. For example, the value of X_{it} for mothers born on February 20 is − 9 and consequently, these mothers can be considered to be in the control group. The value of X_{it} is non-negative for those born on March 1 or after in a given year, and these mothers constitute the treatment group. The distance variable is generated within the birth year.

The estimation of the treatment effect in the context of RDD amounts to estimating two regressions each using data points left and right of the cutoff point (c). The literature on RDD proposes estimating the regression using local polynomial regression (Cattaneo et al., 2017; Imbens & Lemieux, 2008), so this requires choices on kernel functions, degree of polynomials (p), and bandwidth (h). For kernel functions, which determine the weight of each observation, the literature suggests using uniform or triangular kernel functions because of their simplicity and desirable statistical properties; thus, we follow this practice. Regarding the polynomial specification, which determines the functional form, many studies suggest using low-order polynomials. For example, Gelman and Imbens (2019) suggest estimating local linear or quadratic regression because higher-order polynomials lead to noisy estimates and poor coverage of confidence intervals. In this study, we follow their suggestions and present discontinuity estimates based on local linear and quadratic regressions.[4] For the bandwidth choice, which determines the analysis sample, we provide effect estimates based on various bandwidth choices to avoid specification searching and for the sake of transparency of the estimated results, rather than resorting to a single bandwidth choice, the practice often recommended by many studies (e.g., Lee & Lemieux, 2010).

All in all, in the case of local linear regression using the uniform kernel function, we estimate the following regression model using the observations within $c - h < X_{it} < c + h$:

$$Y_{it} = \alpha + \tau D_{it} + \beta\left(X_{it} - c\right) + \gamma\left(X_{it} - c\right)D_{it} + \gamma_t + \varepsilon_{it}$$

,where Y_{it}, D_{it}, $X_{it} - c$, and $\left(X_{it} - c\right)D_{it}$ denote outcome, treatment, assignment variables, and interaction term between treatment and assignment variables, respectively. We also include γ_t, birth year fixed effects, so that the comparison is conducted within the same birth year. ε_{it} is an error term. The coefficient of interest—the discontinuity estimate at the cutoff point—is $\hat{\tau}$, the effect of SSA on Y_{it}. The local quadratic specification is the same with two additional variables, $\left(X_{it} - c\right)^2$ and $\left(X_{it} - c\right)^2 D_{it}$, added in the model above.

[4] Effect estimates based on other kernel functions (e.g., Epanechnikov) and higher-order polynomials are qualitatively similar and available upon requests.

4.2 Data

This study uses restricted administrative employment insurance data administered by the Korea Employment Information Service. These data contain information on female workers who used maternity leave between 2002 and 2016. The data are advantageous in analyzing the effect of SSA on fertility-related outcomes for several reasons. First, the data record the exact date of birth of mothers, which is unavailable in many of the survey data. We can use this variable as an assignment variable in an RDD and compare the two groups with almost identical biological age. Second, the data include information on the exact birthdate of these female' children for which we can measure the fertility tempo in exact days. Third, the data contain information on date of first employment, though there are some missing values. We can therefore analyze one possible mechanism (i.e., age at first employment), and whether the birth events are sequenced in a rigid manner. Fourth, the measurement error issue is minimal because the data are administratively managed.

Note, however, that the data do have some limitations. First, we cannot keep track of each birth cohort until the end of childbearing age (e.g., 50). Thus, the data allow for analyzing the effect of SSA only on the tempo, but not the quantum, of fertility. Second, while the treatment and outcome variables are recorded in detail, the data contain little information on baseline characteristics, which is often the case in administrative data. While we argue that baseline covariates such as gender are likely to be balanced between the two groups given the exogenous nature of the exact date of birth, we complement our analysis using falsification tests to validate our results. The third limitation is related to truncation issues (Wooldridge, 2012). By design, the administrative data contain information on only working females who used maternity leave, so we do not observe non-working females and working females without childbirth experience (i.e., data are truncated). While the existence of this kind of truncation does not pose a threat to internal validity of our research, it must be noted that the analysis results cannot be generalized to the non-working females and working females without childbirth experience.

The last limitation is related to censoring issues. In Table 2, we present information on timeline and cohort setup of the research setting. As can be seen from Table 2, we observe our analysis cohorts during the periods of 2002 to 2016. For older cohorts (i.e., 1979–1982), we observe them from the age of 20. Because some females who did not go to college may have entered the labor market at the age of 19, we do not observe reliably the age at first employment for the earlier cohorts. Hence, the analysis of age at first employment using the 1979 to 1982 cohorts may suffer from left-censoring issues. To address this issue, we estimate the effect of SSA on age at first employment using the younger cohorts (i.e., 1983–1992). Because we observe these cohorts from the age of 19, we believe that focusing on these cohorts is less likely to suffer from the left-censoring issue. On the other hand, there is a right-censoring issue when analyzing the effect of SSA on age at "second" childbirth, because we do not observe the timing of second childbirth for females who have not yet experienced "second" childbirth during our analysis period. Note that the effect estimate derived from an RD design with local linear specification using uniform kernel function is equivalent to the estimate obtained from the conventional

Table 2 Timeline and cohort setup

Cohort year	Periods of observations	Range of age of observations	% analysis sample covered by the administrative data
1979	2002–2016	23–37	0.174
1980	2002–2016	22–36	0.174
1981	2002–2016	21–35	0.175
1982	2002–2016	20–34	0.171
1983	2002–2016	19–33	0.158
1984	2002–2016	18–32	0.138
1985	2002–2016	17–31	0.116
1986	2002–2016	16–30	0.087
1987	2002–2016	15–29	0.063
1988	2002–2016	14–28	0.042
1989	2002–2016	13–27	0.025
1990	2002–2016	12–26	0.014
1991	2002–2016	11–25	0.007
1992	2002–2016	10–24	0.004

The percentage of birth cohorts covered by the administrative data is estimated by dividing the total number of females in the administrative data by the total number of female births by cohort. The data on the total number of female births by cohort are obtained from Korean Statistical Information Service (KOSIS)

regression model with the analysis sample restricted to a chosen bandwidth (Lee & Lemieux, 2010). Hence, to account for the right-censoring issue in the context of an RDD, we conduct the duration analysis based on censored regression model illustrated in Wooldridge (2012).

The maternity leave data we obtained from the Service were retrieved from the data repository as of 2016, and birth cohorts included in the data are those born between 1979 and 1992. Thus, we can keep track of the earliest cohort from 23 (as of 2002) to 37 (as of 2016) and the latest cohort from 10 (as of 2002) to 24 (as of 2016). One issue associated with using the earlier cohorts (e.g., 1979 to 1982) is that we do not observe their maternity leave records before 2002. Note, however, that this issue is less likely to be a problem in our setting. In 2001, for example, the age range of these four birth cohorts was from 19 to 22. According to the vital statistics, the share of childbirths by females aged between 19 and 22 is only 4%. The very small share of childbirths during the early 20 s is driven by the fact that the crude marriage rate for females aged between 19 and 22 is only 9%, while the share of births outside marriage is less than 1%. In sum, we argue that using the 1979 to 1982 birth cohorts in our analysis sample does not create bias in our estimator.[5]

[5] The effect estimate on the timing of first childbirth based on cohorts without the 1979 to 1982 birth cohorts is very similar with the full analysis sample (available upon request). Note, however, that if we drop these cohorts, it would be very difficult to analyze the effect on the timing of second childbirth because our data time frame does not allow for keeping track of second childbirth for later birth cohorts.

Table 3 Descriptive statistics

Variable	Mean	Std. Dev.	Min	Max	No. of obs.
Panel A. Control group (born in January and February)					
Age at first childbirth	29.76	2.89	17.39	38.63	117,320
Age at second childbirth	31.57	2.65	20.71	38.39	33,203
Age at first employment	22.83	2.33	16.67	33.52	24,939
Total ordinary monthly wage ($)	1,599.55	775.82	6.00	25,354.55	115,301
Panel B. Treated group (born in March or later)					
Age at first childbirth	29.76	2.82	13.73	38.27	384,782
Age at second childbirth	31.54	2.58	19.12	38.18	104,389
Age at first employment	22.90	2.20	16.07	33.23	90,630
Total ordinary monthly wage ($)	1,597.01	763.08	0.00	46,371.21	378,060

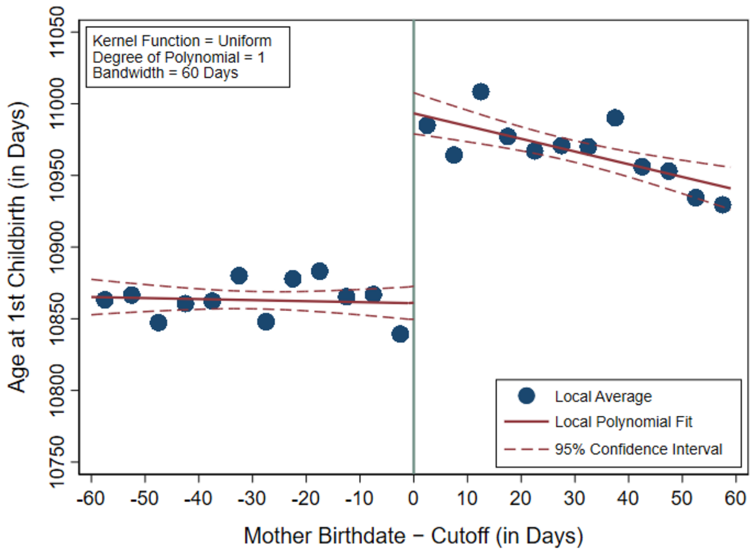
"Std. dev." and "No. of obs." denote standard deviations and the number of observations, respectively

Table 3 presents descriptive statistics of some key variables separately by the treated status. Females born in March or later constitute the treatment group, while those born in January and February are considered the control group. The statistics show that the differences in the average age at first and second childbirth between the treated and control groups are almost 0. The simple comparison of the two groups indicates that the timing of first and second childbirths is similar between the two groups. Note, however, that we cannot argue that SSA does not affect the timing of childbirths based on simple comparison because the two groups may not be similar in many dimensions that affect fertility-related behavior. The differences in the average age at first employment and total ordinary monthly wage between the two groups are also similar.

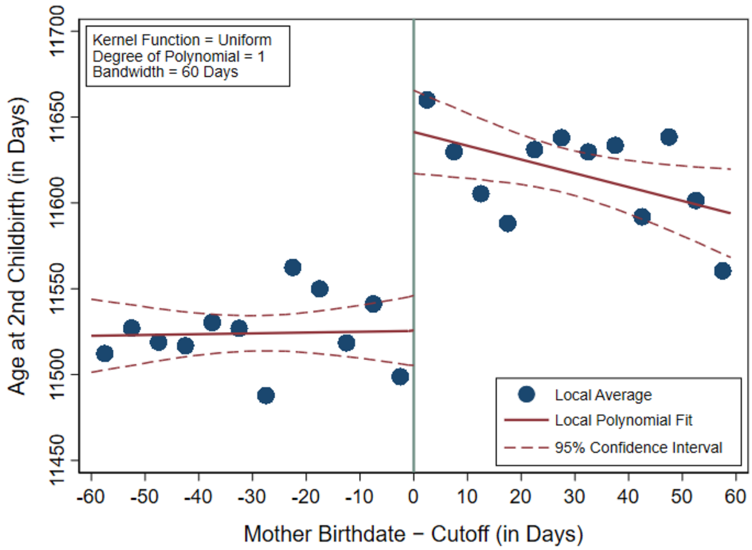
5 Results

5.1 Main Results

We first provide graphical evidence to analyze the effect of SSA on the fertility tempo, which is the norm in any studies that use RDD (Lee & Lemieux, 2010). Figure 1 shows the densities of the two outcomes: age of mothers at first and second childbirths measured in days. The horizontal axis denotes the difference in days between mothers' birthdate and the March 1 cutoff. In the figure, − 10 and 0, for example, indicate mothers born on February 19 and March 1, respectively. The vertical axis shows the distance between mothers' birthdate and their children's birthdate, so 10,900, for instance, indicates that mothers' age at first childbirth is approximately 29.86 (= 10,900/365). Each dot in the figure corresponds to the mean within 5-day binwidth. For example, the dot just left of the cutoff is the mean age at first



Panel A. Age at First Childbirth



Panel B. Age at Second Childbirth

Fig. 1 Densities of first and second childbirths at the true cutoff

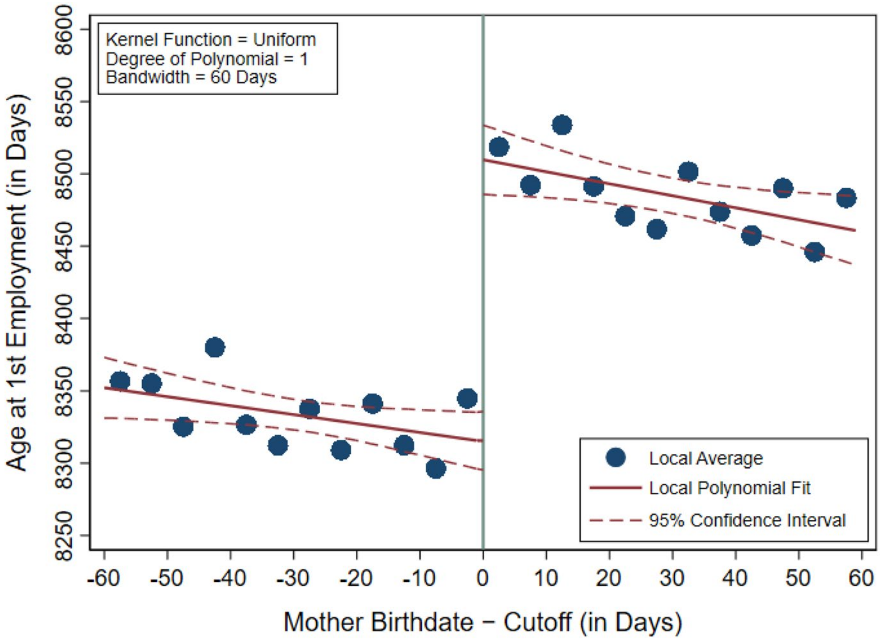


Fig. 2 Densities of age at first employment at the true cutoff

childbirth for mothers born between February 24 and 28.[6] The regression lines at the left and right of the cutoff are estimated under three specifications: uniform kernel functions, first-order polynomials, and bandwidth of 60 days. The dashed lines indicate 95% confidence interval.

Panel A in Fig. 1 shows the timing of first childbirth. The densities of this outcome variable are smooth across the assignment variable with a very sharp jump at the cutoff. The graph shows that mothers born in January and February (i.e., left of the cutoff) delivered their first child 10,875 days, on average, after their birth. Mothers born in March or April, on the other hand, delivered their first child 10975 days, on average, after their birth. The figure implies that those who started school a year earlier delivered their first child approximately 100 days sooner than those who entered school a year later. The regression discontinuity estimate is statistically significant at the 5 percent level because the two confidence intervals do not overlap at the cutoff. Panel B in Fig. 2 displays the timing of second childbirth. Compared with the first childbirth case, the densities of the outcome variable are slightly noisier. Yet, we again observe visually clear discontinuity at the cutoff point that is statistically significant. The magnitude of the discontinuity is approximately 130 days. Mothers born in February are approximately 11,520 days old (approx. 31.5 years

[6] Mothers who are born on February 29 are coded as being born on February 28 for the sake of simplicity.

Table 4 Regression discontinuity estimates

Outcome	Local linear fit (p = 1)			Local quadratic fit (p = 2)		
	h = 10	h = 20	h = 30	h = 10	h = 20	h = 30
Panel A. Uniform kernel function						
First childbirth	84.801***	79.901***	73.349***	70.512***	66.611***	83.919***
	(23.320)	(17.955)	(14.489)	(25.181)	(21.446)	(20.122)
	[37,944]	[70,648]	[100,571]	[37,944]	[70,648]	[100,571]
Second childbirth	130.075***	91.798***	55.378***	122.585***	131.679***	126.805***
	(31.284)	(23.880)	(23.842)	(32.911)	(31.651)	(27.617)
	[10,675]	[19,859]	[28,276]	[10,675]	[19,859]	[28,276]
First employment	45.523***	76.502***	87.330***	92.459***	47.439***	52.650***
	(18.800)	(12.566)	(12.720)	(14.148)	(19.906)	(15.589)
	[8,321]	[15,234]	[22,032]	[8,321]	[15,234]	[22,032]
Panel B. Triangular kernel function						
First childbirth	78.185***	74.154***	77.810***	50.596***	75.490***	73.480***
	(21.594)	(17.976)	(15.801)	(26.565)	(21.644)	(19.904)
	[34,716	[67,416	[98,077	[34,716	[67,416	[98,077
Second childbirth	129.974***	109.465***	86.648***	102.472***	134.359***	128.239***
	(27.307)	(24.006)	(21.918)	(29.401)	(29.854)	(27.899)
	[9,810]	[18,957]	[27,563]	[9,810]	[18,957]	[27,563]
First employment	65.810***	63.056***	72.098***	81.677***	55.110***	57.335***
	(14.903)	(12.783)	(11.138)	(13.321)	(18.298)	(15.905)
	[7,590]	[14,508]	[21,406	[7,590]	[14,508]	[21,406

Standard errors clustered at the distance level are in parentheses. The number of effective sample used for estimating the regression discontinuity is in brackets. p and h denote the degree of polynomial and bandwidth, respectively. All the outcome variables are defined as the age in days since the date of birth

***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels

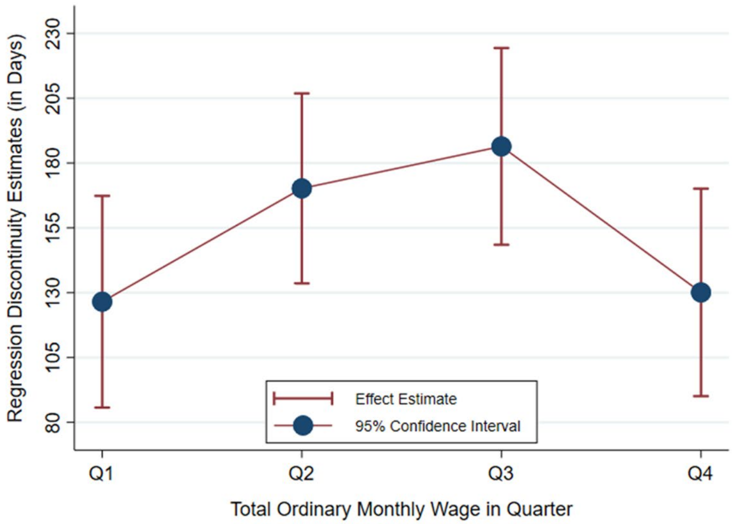
old), and those born in March are approximately 11,650 days old (approx. 31.9 years old).

While the graphical analysis presented in Fig. 1 suggests that a year delay in SSA delays fertility tempo, we examine the effect of SSA more formally using local polynomial regression estimators. The estimated effects are presented in Table 4. In the table, we provide effect estimates under different regression specifications for the sake of the transparency and reliability of our estimated results. The first three columns present estimates under local linear specification (p = 1), while the last three columns display estimates under local quadratic specification (p = 2). The estimates in the first three rows in Panel A are derived using the uniform kernel function, while those in Panel B are obtained using the triangular kernel function. For each specification, we present effect estimates under three bandwidth choices: 10, 20, and 30 days. For statistical inference, we estimated robust standard errors clustered at the values of assignment variable. As seen from the table, all the estimated effects are statistically significant. The effective sample size within the chosen bandwidth is displayed in brackets.

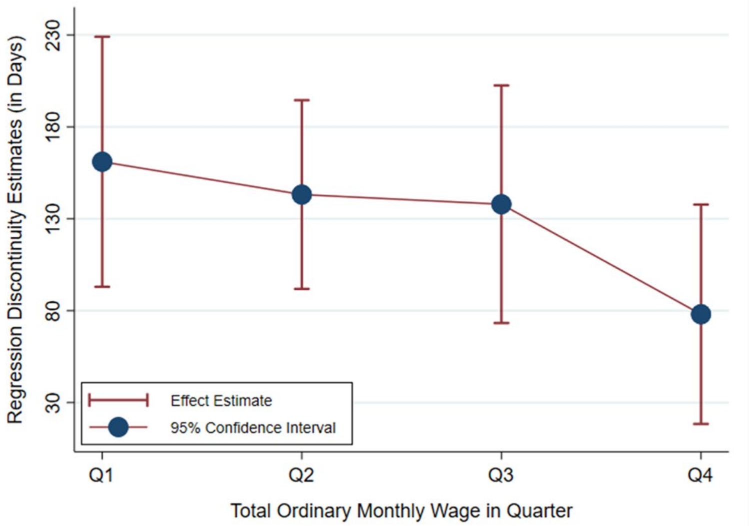
The estimated effect of SSA on the timing of first childbirth is approximately 80 days when mothers within 10 days of the cutoff point are compared with. The effect estimate is quite insensitive to the choice of polynomial and kernel function, though the effect estimate under the local quadratic fit with the triangular kernel function turned out to be significantly smaller. The magnitude of the estimated effects varies slightly depending on the bandwidth choice, but the difference is mostly trivial. The estimated results on the timing of first childbirth imply that mothers who entered school a year earlier delivered their first child approximately 2 to 3 months earlier than those with the same biological age who entered school a year later. Turning to the timing of second childbirth, we again see similar effects. Although effect estimates are somewhat sensitive to the choice of bandwidth, all the effect estimates are highly statistically significant, and the magnitude of all estimates suggests that delay in SSA leads to postponement of the timing of second childbirth. Females who entered school a year later experienced a second childbirth about three to 4 months later compared with those who entered a school a year earlier.

To shed some light on possible mechanisms that mediate SSA and the timing of childbirth, we analyzed whether SSA affects age at first employment. One of the advantages of the administrative data used in this analysis is that they contain information on the exact employment history of all the working mothers. Accordingly, we can analyze the timing of females' first labor market entry and whether this timing mediates the subsequent fertility-related events. Figure 2 shows the densities of age at first employment (in days) by the assignment variable. As seen from the figure, the discontinuity at cutoff is significantly salient. The results of statistical inference are presented in Table 4. Again, we provide discontinuity estimates under various specifications. Though estimated discontinuities vary by specifications, all the estimates imply that females who entered school a year later tended to start their first formal jobs approximately 2 to 3 months later. In Korea, the share of students who repeat certain grades during the formal education period is extremely low (less than 2% in high school). Furthermore, the college matriculation rate is very high for these birth cohorts (approximately 70%) and most female college matriculants graduate college in 4 years; 4-year graduation for students attending 4-year college is very high in Korea. Consequently, if a child enters school earlier, it is likely that the child will also enter the labor market earlier. All in all, these results suggest that age at first employment mediates the relationship between SSA and the timing of childbirths.

The next set of analyses we conducted is the estimation of heterogeneous impact of SSA by earnings. Theoretically, SSA induces income and substitution effects, and consequently, it is possible that the effect of SSA on the fertility tempo is heterogeneous by income. Analyzing the heterogeneous impact of SSA by wage level is also desirable from a policy perspective because such information would allow governments to develop fertility-related policies that are more effective. For example, suppose the postponement effect of SSA is more pervasive in low-income families. Then, governments may intervene more actively in such groups if it were to advance



Panel A. Effects on Age at First Childbirth



Panel B. Effects on Age at Second Childbirth

Fig. 3 Regression discontinuity estimates by wage level

the average timing of childbirth. Note, however, that because wage can be affected by SSA, the analysis by earnings is meant to be descriptive rather than causal.

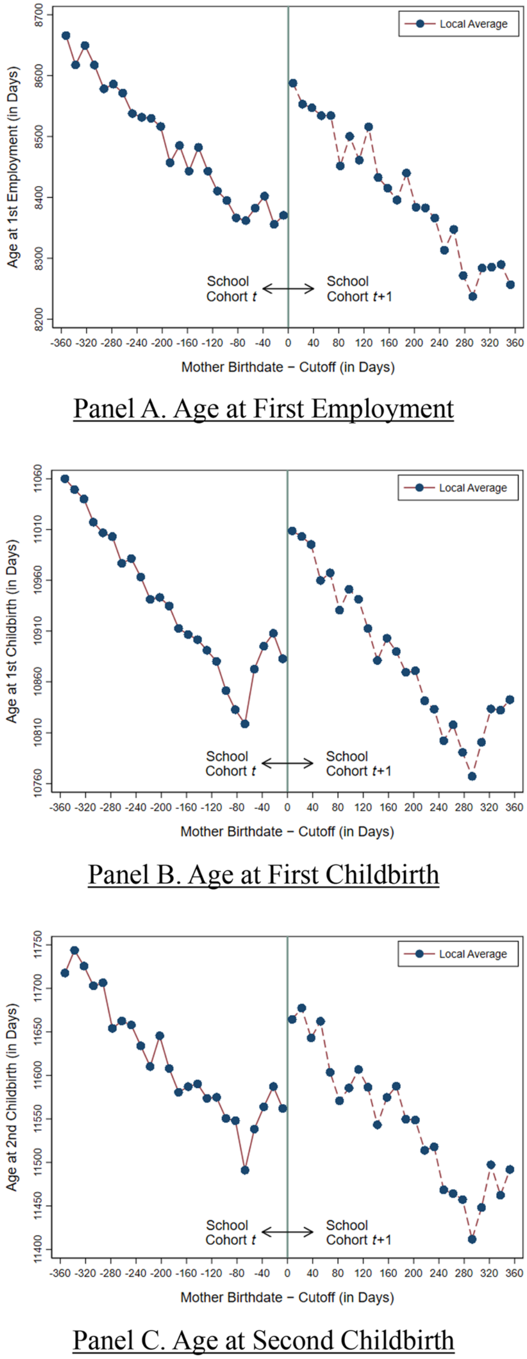
The data contain information on the total ordinary monthly wage of females, so we divided the analysis sample into four equal quarters based on wage level, and estimated the discontinuities in the timing of childbirths at the cutoff for each subsample. Panel A in Fig. 3 corresponds to the analysis results by the wage level for age at first childbirth. Each dot indicates the regression discontinuity estimate, and we juxtaposed the 95% confidence interval. As seen from Panel A, there is an inverse-U relationship between SSA and the timing of first childbirth. The heterogeneous impact by earnings for age at second childbirth is presented in Panel B. For this outcome, we see a downward sloping relationship between the two variables. In sum, the postponement effect of delay in SSA is more pronounced in low-income families, particularly for the timing of second childbirth. We argue that the postponement effect of SSA on the timing of first childbirth is less likely to be heterogeneous by income because the range of estimated discontinuities is only 50 days. This effect becomes more salient, however, when it comes to deciding upon the timing of second childbirth. The range of estimated discontinuities is 90 days. From a policy perspective, therefore, governments may develop fertility-related policies targeted at fertility tempo differently by income in order to promote the effectiveness of such policies.

The last set of analyses we conducted is to examine a more complete picture of the distribution of the three outcome variables using adjacent cohorts. In each of the panels in Fig. 4, we juxtaposed the densities of outcome variables (in 15-day bins) by the assignment variable. In the figure, the left of the cutoff point corresponds to school cohort t and the right of the cutoff displays school cohort $t + 1$. For instance, for the March 1 1979 cutoff, the left of the cutoff shows the densities of outcomes for females born between March 1978 and February 1979, while the right of the cutoff shows the densities for those born between March 1979 and February 1980. As can be seen from all the panels, the patterns of distribution of the densities are extremely similar between the two adjacent cohorts. Two points are noteworthy, in particular. First, the timing of each outcome is decreasing by the assignment variable, implying that the earlier the birthdate, the later the timing of each outcome. Second, the downward sloping pattern changes to upward sloping at around the −60 and 300 cutoff points. These cutoffs correspond to January 1, in which the New Year begins. These two results coincide with the findings of many studies in the demography literature that fertility-related events are affected by social age. Social age can be defined in many ways, and the panels in Fig. 4 suggest that fertility-related events are affected by school cohort as well as the year in which people are born.

5.2 Tests of Validity of the Results

The internal validity of our estimated results hinges critically on the identifying assumptions of RDD. That is, it must be the case that observable and unobservable baseline characteristics of those born around the March 1 threshold are continuously distributed at the cutoff point. Because our assignment variable is mothers' date of birth, the test of such assumptions requires variables that are determined prior to the realization of mothers' date of birth (e.g., parental income and education of mothers), which are unavailable in our administrative data. Another way to test such an assumption is to show that mothers have imprecise control over their date of birth, and such a test is often fulfilled by conducting the density test pioneered by McCrary

Fig. 4 Cohort comparison of outcome variables



(2008). Note, however, that conducting the density test to validate the identifying assumption in this study is inappropriate. The test uses the discontinuity in the densities of the assignment variable at the cutoff point as evidence of manipulation. As mentioned by McCrary (2008), however, the fact that we observe discontinuities in the densities may not always imply the prevalence of manipulation because many reasons other than manipulation may exist for observing such discontinuities. This is extremely salient in our context because the data we used are employment insurance data. People are automatically included in the insurance data as soon as they enter the labor market, and because those who enter school earlier are more likely to appear in the data (i.e., enter the labor market), the densities of assignment variable are more likely to be larger for the treatment group.

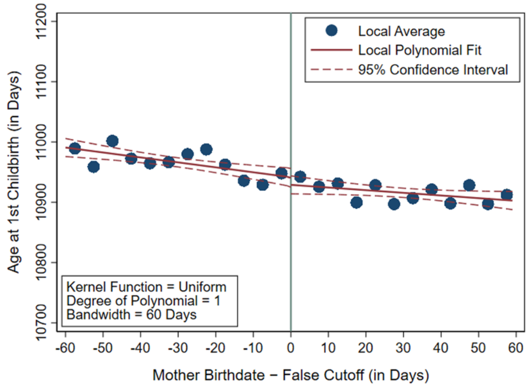
We use two pieces of evidence to argue in favor of the internal validity of our estimated results, rather than resorting to conventional statistical tests of identifying assumptions mentioned above. First, note that our assignment variable is mothers' exact date of birth, and our effect estimates are derived from analyzing the differences in fertility outcomes of those born, say, between February 19 to 28 and between March 1 and 10. The only convincing method that might have enabled the parents of our analysis mothers to manipulate the exact date of birth is through cesarean section. It is, however, difficult to believe that the parents of our analysis mothers would have engaged in childbirth through *C*-section simply to manipulate the timing of school entry. We present official statistics to back up this assertion. According to the Yearbook of Medical Insurance Statistics, the share of deliveries through *C*-section was only 3% in 1980, 6% in 1985, and 13% in 1990. Though the share is increasing over time, it is less likely that parents of our analysis mothers (born in 1979 to 1992) manipulated the exact date of birth through *C*-section in order to change the timing of school entry.

The other evidence we present is the results of falsification tests. The idea of our falsification tests is that if the discontinuities observed in fertility-related outcomes at the March 1 cutoff point are driven causally by the difference in SSA, we must not see any discernible discontinuities in fertility outcomes at other cutoffs such as May 1 because SSA does not change discontinuously at these cutoffs. The graphical results of the falsification tests based on the May 1st cutoff are presented in Fig. 5. The results of falsification tests based on other months are available in an online supplementary appendix. As can be seen from all the panels in the figure, we do not see any statistically significant discontinuities that are visually clear at the false cutoff for all the outcome variables. All the densities are distributed smoothly across the assignment variable with little volatility in the distribution. We also conducted statistical inference as in Table 4, and most of the estimated discontinuities are statistically and practically insignificant. All in all, we argue that the results of falsification tests ascertain the internal validity of our results.

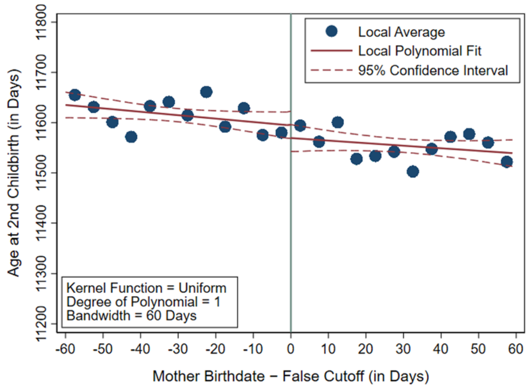
5.3 Robustness Check

As mentioned in the Data section, one of the limitation associated with analyzing the effect of SSA on age at second childbirth is the right-censoring issue.

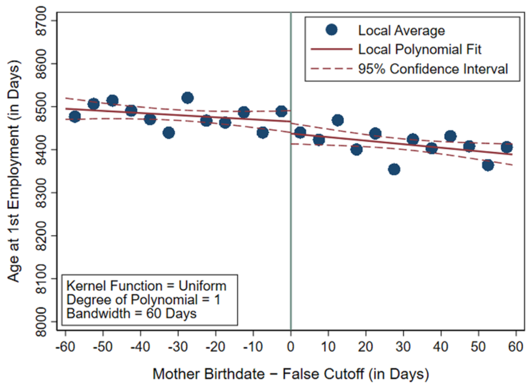
Fig. 5 Densities of outcome variables at the false cutoff



Panel A. First Childbirth (False Cutoff = May)



Panel B. Second Childbirth (False Cutoff = May)



Panel C. Employment (False Cutoff = May)

Because we do not observe the timing of second childbirth for females who have not yet experienced "second" childbirth during our analysis period, the bias that may arise from such right censoring must be accounted for in an estimator. To account for the right-censoring issue in the context of an RD design, we conducted the duration analysis based on censored regression model illustrated in Wooldridge (2012). To be more specific, to derive the regression discontinuity estimate that account for right censoring, we estimate censored regression model using the analysis sample restricted to a chosen bandwidth. The results obtained from the censored regression model are comparable to the RD estimates derived from local linear specification with the uniform kernel function.

Table 5 shows the results of the duration analysis for age at second childbirth. For the distribution assumption, we used lognormal specification. The effect estimates are quite insensitive to the choice of distribution assumptions. When the bandwidth choice is within 10 days, the estimated coefficient is 0.011 implying that females who entered school a year later have estimated durations that are about 1.1% longer than those who entered a school a year earlier. Because average age (in days) at second childbirth for control groups (i.e., females within − 10 days to − 1 day) is 12,388 days, the coefficient estimate implies that females in treated groups (i.e., females within 0–9 days) experienced a second childbirth about 137 days later. The associated effect estimates under the bandwidth choice of 20 and 30 days are 108 and 88 days, respectively. The estimated effects under the censored regression model are qualitatively similar with those obtained under the conventional regression discontinuity design setting in which the analysis is conducted without females who do not experience second childbirth experience.

6 Discussion and Conclusion

Identifying the long-term impact of children's age at school entry on various socioeconomic outcomes is one of the primary concerns for families, policymakers, and educators because of its possible consequences for the demand for publicly produced goods and intergenerational wealth transfers. In this study, we analyzed the long-term impact of mothers' SSA on the timing of their childbirths and age at first employment by exploiting Korea's school entry policy using exact date of birth. Our regression discontinuity analysis that compares mothers born in February and March shows that a year's delay in SSA leads to a postponement of age at first employment, first childbirth, and second childbirth.

Overall, our analysis confirms a causal link between SSA and the timing of employment and childbirths. While our results show that delay in SSA leads to a postponement of employment and fertility timing, we refrain from drawing strong conclusion about the existence of SSA effects on these outcomes in other contexts other than South Korea. As mentioned previously, the observed SSA effects in this study are likely to be driven by the fact that the educational system being rigid in South Korea (i.e., few dropouts and repeating rates). Hence, the SSA

Table 5 Analysis of the duration of age at second childbirth using censored regression model

Variable	Average age for control group (in days)	Coefficient	Effect estimates (in days)
Treatment indicator	12,388	Panel A: $-10 \leq h \leq 10$ 0.011*** (0.003)	137
Number of observations		[37,944]	
Treatment indicator	12,410	Panel B: $-20 \leq h \leq 20$ 0.009*** (0.003)	108
Number of observations		[70,648]	
Treatment indicator	12,408	Panel C: $-30 \leq h \leq 30$ 0.007** 0.003	88
Number of observations		[100,571]	

Standard errors clustered at the assignment variable level are in parentheses. The number of observations in brackets. The "treatment indicator" is equal to one if a female's distance value is equal to or greater than 0 and zero if the value is less than 0. The censored regression models are all estimated conditional on the distance variable (i.e., assignment variable), the interaction term between the treatment indicator and distance variable, and birth cohort fixed effects.

*** and ** indicate statistical significance at the 1% and 5% levels

effects may not be pervasive if there is no rigidity in educational sequence that children experience during their school period.

We also do not engage in drawing normative conclusions that governments should decrease SSA simply to advance the timing of fertility-related events because there are many benefits associated with delaying SSA. Many studies find that delaying SSA is associated with various social benefits such as improved human capital and health, increased wealth, and reduced crimes (e.g., Arnold & Depew, 2018; Bedard & Dhuey, 2012; Dee & Sievertsen, 2018; McAdams, 2016; McEwan & Shapiro, 2008). Rather than lowering SSA, therefore, we instead argue that government policies should be directed more toward eliminating barriers that prevent children from finishing a formal education on time. As our results show, fertility-related events, particularly during adulthood, are spaced in a rigid manner. If policymakers can incentivize people to graduate earlier and invest in eliminating labor market entry barriers such that people can start their careers earlier, then people are more likely to engage in fertility-related events earlier in life.

Our study is not without limitations. As mentioned previously, this study does not shed light on the effect of SSA on the quantum of fertility due to data limitations. Identifying the effect of SSA on total fertility is clearly policy relevant, and we hope to extend our analysis in the future as data become available. The external validity of our results is also worth mentioning. Because our analysis sample is the female working population with childbirth experience, it must be emphasized that our results may not be generalized to the overall female population. It is likely that working females with childbirth experience differ in many aspects that affect fertility outcomes from those who are not working and who are working but do not have childbirth experience, such as career motivation. Hence, the effect of SSA on fertility tempo may differ for the non-working females and working females without childbirth experience. Finally, care is required when drawing conclusions about the effect of SSA on fertility tempo in other countries. Korea is unique in many aspects in terms of gender roles, the degree of population homogeneity, and education systems. Korea is also the lowest fertility country in the world. We therefore suggest some caution in generalizing our results to other countries that are significantly different from Korea.

Supplementary Information The online version contains supplementary material available at https://doi.org/10.1007/s10680-021-09597-x.

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Data availability We have received the administrative data from the Ministry of Labor in Korea conditional on not disclosing the data. We will assist researchers who are interested in obtaining data from the Institute.

Code availability We will provide STATA codes for those who are interested in receiving the do file used for creating figures and tables.

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