



Causal Impact of Having a College Degree on Women's Fertility: Evidence From Regression Kink Designs

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Abstract

An important factor speculated to affect fertility level is education. Theoretical predictions regarding whether education increases or decreases fertility are ambiguous. This study analyzes the causal impact of higher education on fertility using census data administered by Statistics Korea. To account for the endogeneity of education, this study exploits the Korean higher education reform initiated in 1993 that boosted women's likelihood of graduating from college. Based on regression kink designs, we find that having a college degree reduces the likelihood of childbirths by 23 percentage points and the total number of childbirths by 1.3. Analyses of possible mechanisms show that labor market-related factors are a significant channel driving the negative effects; female college graduates are more likely to be wage earners and more likely to have high-wage occupations.

Keywords Higher education · Female fertility · Regression kink designs · College degree

Introduction

Many industrialized countries are experiencing a decline in the total fertility rate, and finding ways to reverse this trend is considered one of the toughest challenges that many governments face. Research regarding whether a low fertility rate poses an issue has been mixed. For example, Lee et al. (2014) showed that a moderately low fertility rate and population level is favorable for the material standard of living. Nevertheless, many researchers have argued that a decline in the fertility rate to well below replacement level will be a serious threat to the sustained operation of government transfer

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programs, such as unemployment insurance (Bloom et al. 2010). Given that such programs are essential for the social welfare in any country, a country with a fertility rate well below replacement level will inevitably devote a high share of government spending toward raising the overall rate.

One of the many potential reasons for the ineffectiveness of government policies targeted at boosting fertility is that many of these policies have a limited effect in targeting factors that cause low fertility. Developing and implementing public policies directed at factors that drive low fertility is critical for increasing the effectiveness of such policies. Identifying the cause of low fertility, therefore, should precede any policy implementation.

One factor that most studies have explored is education (Skirbekk 2008). Education is widely believed to be a key determinant of the fertility rate. Yet, analyzing the causal impact of education on fertility is challenging because education is endogenously determined. That is, even if a correlation exists between education and fertility, it does not necessarily imply that the effect is driven by education per se. The observed association may be due to confounding factors, such as career aspirations, which influence both education and fertility. If the effect of education on fertility is driven mostly by the difference in career aspirations, policies targeted merely at one's education level will be limited in influencing fertility.

This study aims to answer the question, Is there a causal relationship between education and fertility? Although answering this question seems interesting from a research perspective, the answer itself is limited in providing policy implications. Suppose a study finds that an increase in education level reduces fertility. Should the government then engage in reducing the level of education in order to raise fertility rates? As a matter of course, developing and implementing policies to reduce education level is inappropriate; education may affect fertility negatively, but education entails many monetary and nonpecuniary benefits (Milligan et al. 2004; Oreopoulos and Salvanes 2011).

From a policy perspective, therefore, more important than determining whether a causal relationship exists between education and fertility is identifying the potential mechanisms that channel education and fertility. If certain causal channels are revealed and such channels are policy-relevant variables, then governments should put resources into targeting such mechanisms. In this study, therefore, we examine the potential policy-relevant mechanisms that can be tested statistically using data to help develop public policies that could ease any negative impact of education on fertility.

Identifying the causal impact of education on fertility requires researchers to exploit random or quasi-random variations in education level. In this study, we exploit Korea's higher education reform initiated in 1993. Prior to 1993, the Korean government controlled the level of college enrollment, and the trend of college enrollment during the 1980s and early 1990s was remarkably stable. In 1993, the newly elected Korean government, headed by President Young-Sam Kim, liberalized enrollment by allowing new universities that met certain minimum conditions to enter the higher education market. Consequently, the college enrollment and the number of institutions started to increase sharply after 1993, and a kink in the share of college graduates is observed. Exploiting such a kink (i.e., change in slope), we use regression kink design (Card et al. 2015) to causally estimate the impact of having a college degree on female fertility.

The results show that, on average, having a college degree reduces the likelihood of childbirth by 22.3 percentage points and the total number of childbirths by 1.3. An analysis of the possible mechanisms shows that the labor market–related factors are a significant channel driving the negative effects of having a college degree on fertility. We find that a college degree increases women's earning capacity; a woman with a college degree is more likely to be a wage earner with a professional occupation and is less likely to be unemployed.

Theoretical Background and Literature Review

Previous research has generated eight theories to explain the education-fertility relationship. The leading theory, proposed by Becker (1965), argues that education raises earning capacity, thereby affecting the opportunity cost of leaving the labor market. According to the theory, education influences fertility through substitution and income effects, and although substitution effects reduce fertility, income effects raise fertility.¹ Whether education reduces or raises fertility, therefore, depends on the relative magnitude of the two effects.

The second theory argues that education affects fertility through the marriage market (Whelan 2012). More education may make individuals relatively more or less attractive in the marriage market, which in turn will affect the likelihood of finding an appropriate spouse.

Third, the so-called assortative mating theory has been proposed to explain the relationship between education and fertility. This theory is based on the psychological notion that people tend to marry those who are similar to themselves. If an increase in education level induces people to marry someone with higher education level, this act may boost the income of one's partner. As can be inferred from the labor market theory, such behavior induces substitution and income effects. Behrman and Rosenzweig (2002) argued that the relative magnitude of these two effects depends on the partner's involvement in childcare activities. For example, if women are mostly responsible for child-rearing, the income effect will likely dominate the substitution effect, thereby raising fertility.

Fourth, education generates information effects. Education may affect knowledge and attitudes regarding the practice of contraception and consequently lead to a decrease in fertility (Buyinza and Hisali 2014).

Fifth, education also affects fertility through the so-called incarceration effect (or time effect). Education will likely raise the time spent in school, which in turn will reduce or delay opportunities to engage in fertility-related activities (Black et al. 2008).

The sixth theory argues that education affects fertility because higher education may provide bargaining power in decision-making. The increase in such power may affect the range of marriage-related activities, including fertility control (Dyson and Moore 1983).

The seventh theory states that education produces attitudinal effects (Basu 2002). If, for example, individuals with more education think that education is beneficial, they

¹ Regarding substitution and income effects, Becker and Lewis (1973) argue that income effects might be relatively weak because of a quality-quantity tradeoff when income increases.

may invest in the education level of their children. Because bringing up better-educated children is costly, these well-educated individuals may not have many children.

The last theory tries to explain the link between the two variables via peer effects. Sociological theories have examined the importance of social interaction and diffusion processes for child-rearing behavior (Bongaarts and Watkins 1996; Diaz et al. 2011; Kohler et al. 2001).

As can be inferred from the aforementioned theoretical propositions, education may either increase or decrease fertility. Whether the causal impact of education on fertility is positive or negative, therefore, is a matter of empirical investigations and may vary to a great extent depending on the context of the analysis sample. Many studies have analyzed the relationship between fertility and education empirically (for a review of these studies, see Skirbekk 2008). Determining causality between the two variables from the results provided by these studies, however, is difficult because of the endogeneity in education.

To overcome the endogeneity issue, most recent studies attempting to estimate the causal impact of education on fertility have exploited either a change in mandatory schooling law or educational reform within a country. In this section, we discuss only the published research analyzing the causal impact of education on fertility.²

The earliest work was conducted by Osili and Long (2008), who exploited the educational expansion program that was implemented in Nigeria to estimate the causal impact of education on fertility. Women who were treated because of the expansion program received about 1.5 more years of education than those who were not exposed to the program. The authors found a negative effect of education on fertility. Grönqvist and Hall (2013) also exploited educational reform implemented in Sweden, in which the two-year vocational track was extended to three years. Using this exogenous event, they found that education delayed women's childbearing.

Monstad et al. (2008) exploited the change brought about by compulsory education reform in Norway in which the mandatory years of education changed from seven to nine. They showed that education had little impact on fertility level. Cygan-Rehm and Maeder (2013) also exploited the compulsory education reform in Germany in which the mandatory years of education increased from eight to nine years. They found that education reduced fertility.

McCrary and Royer (2011) used regression discontinuity design to analyze the effect of education on fertility and child health. The results showed that education had no effects on fertility. Rather than exploiting some exogenous events, Amin and Behrman (2014) analyzed fertility behavior of U.S. twins. Exploiting educational differences observed within twins, they found that education reduced fertility.

As can be expected from theoretical predictions regarding the relationship between education and fertility, results from previous studies are not consistent. One possible reason for these inconsistent results is that each study examines a different country. Moreover, the educational reforms across studies differ with respect to time and educational level. Hence, more empirical studies are necessary for drawing a more complete picture of the relationship between education and fertility.

² We do not discuss the studies that examine the effect of education on teenage fertility because teenage fertility is not a focus of this study.

This study contributes to existing literature in four ways. First, no research has examined the relationship between education and fertility in Asian countries. East Asian countries, in particular, are suffering from a rapid decline in the fertility rate. In particular, South Korea has the lowest fertility rate in the world and is perhaps the first country in history to experience such low fertility. We therefore argue that Korea makes for a theoretically valuable case.

Second, the effect of education on fertility may not be homogeneous. The effect of finishing secondary education on fertility is unlikely the same as the effect of completing tertiary education. The samples studied in most of the previous studies are predominantly concentrated at the elementary or secondary school level. Because this study analyzes the impact of higher education, it complements previous studies in providing a more complete picture of the education-fertility relationship.

Third, few studies consider so-called sheepskin effects. The screening theory suggests that people with a diploma or a degree earn much more than those without, even if both parties received the same years of education (Belman and Heywood 1991). To our knowledge, this study is the first study to consider such effects: we compare those who have a four-year college degree with those who have a high school degree.

Fourth, a plausibly exogenous treatment variation observed in previous studies is typically less than one year. If increasing or decreasing returns to education exist with respect to fertility, exploiting this one-year treatment variation may not provide a complete picture of the effect of education on fertility. For example, Trostel (2004) found that the assumption of constant returns to scale is inappropriate for analyzing the relationship between years of education and the wage rate. The treatment variation exploited in this study is four years.

Institutional Background

This study exploits Korea's higher education reform, initiated in 1993.³ Prior to 1993, the Korean government controlled the capacity of college enrollment and allotted the enrollment quota across colleges. Accordingly, the trend in college enrollment during the 1980s and early 1990s was remarkably stable. Panel a of Fig. 1 shows that the college enrollment rate was around 0.35 in 1988, and the change in the rate was very stable until 1992. The stable trend observed for these periods was clearly driven by the Korean government's enrollment capacity control.

In 1993, the newly elected Korean government loosened the higher education-related regulations and liberalized enrollment capacity. New universities could enter the market if they met some minimum conditions. Consequently, college enrollment and institutions started to increase. As shown in panel a of Fig. 1, the college enrollment rate increased significantly and continuously beginning in 1993—by more than 25 percentage points over the period 1993 to 1997. The sudden increase in the rate was clearly driven by the higher education reform measures.

The abrupt increase in the supply of colleges and the size of enrollment capacity generated a kink in the likelihood of receiving a college degree. Exploiting such a kink,

³ Information regarding the higher education reform implemented in the 1990s is retrieved from Kim and Lee (2006) and Oh (2011).

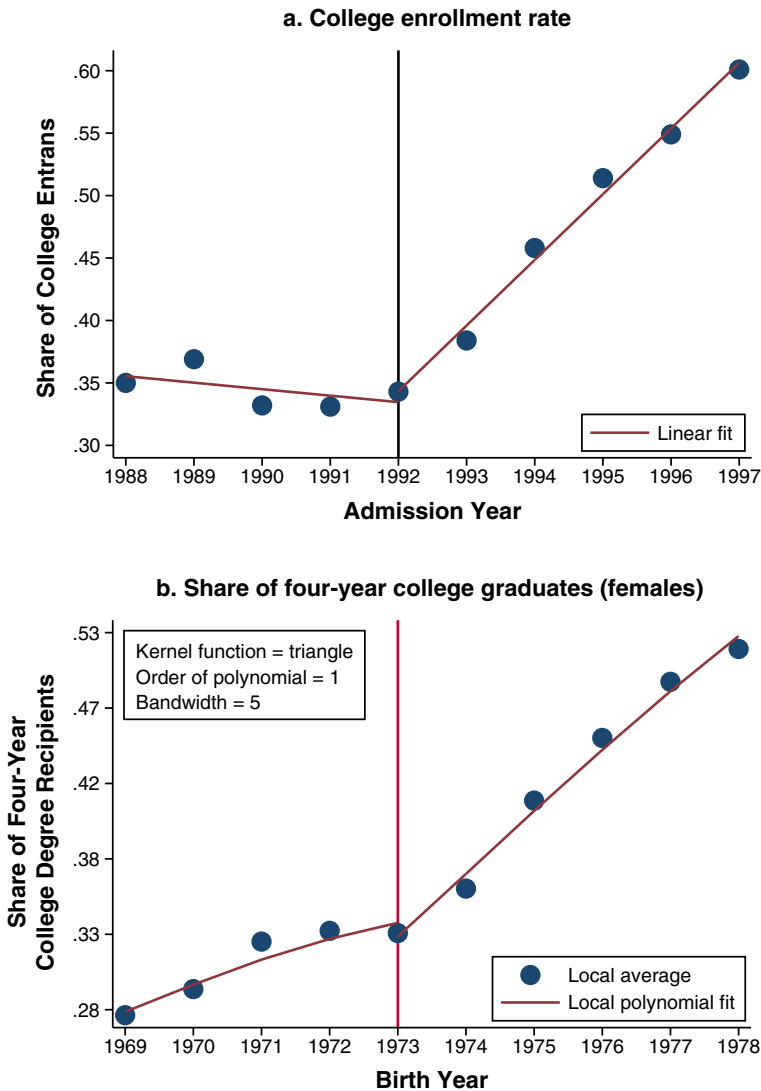


Fig. 1 College enrollment rate and share of four-year college graduates, by year.

we use regression kink design pioneered by Card et al. (2015) to causally estimate the effect of having a college degree on fertility. Note that the kink in 1992 is observed for the college *enrollment* rate. The fact that this rate increased doesn't necessarily imply that we would observe a kink in the probability of receiving a college degree. It is likely, however, that we would also observe a significant kink in the probability of obtaining a college degree because college completion rates among matriculants are high in Korea.⁴ Nevertheless, the validity of the regression kink design is contingent critically on the existence of the kink in college completion rates. In this study, we

⁴ To the best of our knowledge, no official statistics exist on college completion rates in Korea.

verify that the kink shown in panel a of Fig. 1 leads to a similar kink in college completion rates (see panel b of Fig. 1 and the Results section).

Empirical Strategy

The identification strategy we use is the fuzzy regression kink design (RKD). Identification in the fuzzy RKD relies on two assumptions. The first assumption requires that individuals cannot manipulate their birth year precisely in an effort to take advantage of the higher education reform initiated in 1993. This assumption is reasonable given that manipulating the birth year is virtually impossible. In addition, because parents were unaware of the possibility of the higher education reform, they were not likely to have postponed having a child in order to take advantage of the reform. Although manipulation is unlikely, we test for such behavior using a modified version of the density test proposed by McCrary (2008), often used for testing the manipulation of the assignment variable in the context of regression discontinuity design (RDD). In this study, we test for the kink in the density of the assignment variable because RKD requires that there is no kink, rather than no discontinuity, in the assignment variable.

The second assumption rules out any statistically significant kink in baseline characteristics around the cutoff point. This assumption is analogous to testing for the continuity in baseline covariates in the RDD setting. The intuition behind testing for no kink in baseline covariates is that if there is kink in baseline covariates, we cannot determine whether the observed kink in an outcome is driven by the treatment variable itself or other baseline characteristics. In the Validity Check for the RKD section, we show that there are no kinks in baseline covariates.

Provided that the two assumptions hold, the identification of the effect of having a college degree (E) on fertility (Y) is obtained by dividing the change in the slope observed for the conditional expectation function for the outcome variable Y , $E = e(Y|C = c)$, at the kink point by the change in the slope observed for the assignment function $E = e(C)$ at the kink point. Here, C denotes an assignment variable (i.e., birth year).

Formally, the RKD estimand (τ_{RKD}) is defined in the population as follows:

$$\tau_{RKD} \equiv \frac{\lim_{c \rightarrow 1974^+} \frac{dE(Y|C=c)}{dc} - \lim_{c \rightarrow 1974^-} \frac{dE(Y|C=c)}{dc}}{\lim_{c \rightarrow 1974^+} \frac{de(c)}{dc} - \lim_{c \rightarrow 1974^-} \frac{de(c)}{dc}} = \hat{\beta}_1. \quad (1)$$

In Eq. (1), the numerator indicates the change in the slope of the conditional expectation function of an outcome variable at the kink point. The denominator, in contrast, expresses the change in the slope of the assignment function. To put it simply, the RKD estimand is the slope change in the outcome variable (i.e., fertility) scaled by the slope change in the treatment variable (i.e., education).

Estimation of Eq. (1) can be accomplished in many ways, but literature on RDD and RKD recommends estimating the equation using the nonparametric local polynomial regression techniques (Card et al. 2016; Fan and Gijbels 1996; Imbens and Lemieux 2008). Formally, this amounts to solving the following minimization problems:

$$\min_{\{\gamma_j^l\}} \sum_{i=1}^{N^l} \left[E_i^l - \sum_{j=0}^p \gamma_j^l (C_i^l)^j \right] K\left(\frac{C_i^l}{h}\right), \quad \min_{\{\gamma_j^r\}} \sum_{i=1}^{N^r} \left[E_i^r - \sum_{j=0}^p \gamma_j^r (C_i^r)^j \right] K\left(\frac{C_i^r}{h}\right)$$

for the numerator in Eq. (1), and

$$\min_{\{\beta_j^l\}} \sum_{i=1}^{N^l} \left[e_i^l - \sum_{j=0}^p \beta_j^l (C_i^l)^j \right] K\left(\frac{C_i^l}{h}\right), \quad \min_{\{\beta_j^r\}} \sum_{i=1}^{N^r} \left[e_i^r - \sum_{j=0}^p \beta_j^r (C_i^r)^j \right] K\left(\frac{C_i^r}{h}\right)$$

for the denominator in Eq. (1), subject to

$$\beta_0^l = \beta_0^r.$$

Solving the minimization problems leads to the following RKD estimate:

$$\hat{\tau}_{RKD} = \frac{\hat{\beta}_1^r = \hat{\beta}_1^l}{\hat{\gamma}_1^r = \hat{\gamma}_1^l}.$$

Here, l and r denote left and right of the cutoff point, respectively; p indicates the order of the polynomial; K corresponds to the kernel function that determines the relative weight of each observation; and h is the bandwidth, or the effective analysis sample used for estimation.

As can be seen from the minimization problems, researchers have to make choices on three factors: K , p , and h . Despite the lack of consensus for making choices on these factors, the majority of the regression kink (RK) literature has estimated local linear regression (i.e., a uniform kernel function for K , and $p = 1$) because this estimator is known to have desirable properties for estimating the regression function at the boundary point. Thus, we also use a local linear regression estimator. For the bandwidth choice, we report the RKD estimate based on several bandwidth choices recommended by Lee and Lemieux (2010).

Local linear regressions are, in principle, weighted instrumental variable estimators. Accordingly, standard regression inferential procedures can be used for conducting statistical inference (Lee and Lemieux 2010). This study uses the birth year as an assignment variable, so the data have a grouping structure. In such a case, Lee and Card (2008) proposed clustering standard errors on the assignment variable. We therefore cluster standard errors at the level of the assignment variable.

Data and Sample

For this study, we use 2010 census data administered by Statistics Korea. Researchers interested in using these census data can apply for the sampled data (either 1 % or 2 %) from the Microdata Integrated Service system. This study exploits the higher education reform initiated in 1993, thus it is necessary that the sample should contain those who were born around 1974. The 2010 census data are suitable for exploiting the 1993 higher education reform.

The 2010 census data contain information necessary for our analysis of the causal impact of having a college degree on fertility, such as information on fertility, education level, gender, and age for each member of a household. The data also contain information on place of birth, which we can use to test whether the estimated effects of having a college degree on fertility differ by urban or rural area. Although information is available on the place of current residence, the data do not have information on a person's migration experience occurring prior to the realization of our treatment variable, so we do not test for the balance in migration experience. The data also offer useful information on post-treatment variables, such as marital status, whether a person is a wage earner, unemployment status, and type of occupation. Because these variables are not predetermined, we do not test for the balance in these variables; rather, we use these data for the mechanism analysis.

The data, however, are limited in the sense that they mostly consist of voluntary response variables to questions such as, "Do you have any physical constraints?" The limitation of the census data, in particular, is that the data are mostly post-determined variables and therefore do not allow us to test heterogeneous effects, such as by household income. Also, despite information on education level, the data do not contain information on specific names of higher education institutions and thus cannot be used for testing the heterogeneous effects by higher education institutions. Nevertheless, the data are valuable because they allow us to exploit the 1993 education reform, which we use for isolating the causal impact of having a college degree. We also have data on labor market-related factors, such as whether a woman is a wage earner as well as industry categories. We use these data to test for the potential mechanisms that channel the relationship between having a college degree and fertility.

In Table 1, we describe the series of sample restrictions conducted to analyze the research topic at hand. The sample size for the initial 2010 census data is 933,846. From these initial raw data, we keep only the observations whose value label for the *Relationship With the Household Head* variable is *household*, *spouse of household head*, *child*, and *spouse of child*. The reason for this restriction is that it is impossible to determine fertility level for other value labels. The resulting sample size is 864,412. As a second step, we exclude people with a two-year college degree for two main reasons. First, the higher education reform initiated in 1993 did not affect two-year colleges. Second, this sample restriction secures treatment variation in education level. The resulting sample size after excluding these observations is 767,974. The data also contain information on whether a person received a degree. To account for the sheepskin effect, this study focuses only on degree recipients. This third step reduces the sample size to 490,774. The fourth step drops observations whose value for the number of childbirths variable is 99 (not applicable), bringing the sample size to 404,369. Finally, we exclude those whose birth year is before 1967 and after 1979 for two reasons. First, including observations before and after these birth years results in unbalanced baseline characteristics. Second, we no longer observe a practically significant kink in the treatment variable and therefore cannot exploit RKD. Finally, we drop male observations because the analysis is based on the female sample. The final sample size used in the analysis is 57,547.

In Table 2, we provide descriptive statistics for some of the outcome variables and baseline covariates by treatment status, and we also provide ordinary least squares (OLS) regression estimates that test for the difference in these variables between those

Table 1 Step-by-step sample restrictions from the initial census data

Step	Description	Resulting Sample Size
Initial Data	Raw data: 2010 census sample (2 %)	933,846
Step 1	Keep observations that are recorded as <i>household head</i> , <i>spouse of household head</i> , <i>child</i> , and <i>spouse of child</i> , in the <i>Relationship With the Household</i> variable. For other labels, it is not possible to determine the fertility level.	864,412
Step 2	Exclude people who earned a two-year college degree in order to secure treatment variation.	767,974
Step 3	Restrict the sample to degree recipients in order to account for the sheepskin effect.	490,774
Step 4	Drop observations whose value for the <i>Number of Children</i> variable is 99 (not applicable).	404,369
Step 5	Keep only the observations with a birth year between 1967 and 1979. Including observations outside this range results in unbalanced predetermined covariates as well as insignificant kink estimates in the treatment variable.	102,185
Step 6	Drop male observations.	57,547

who have a college degree (treatment group) and those who do not (control group). Table 2 shows that, on average, the probability of having at least one child is .938 for female college graduates and .958 for those without a college degree, indicating a difference of 2 percentage points. The difference in the number of childbirths between the two groups is -0.231 . The differences are statistically significant. The fact that there are differences in the outcome variables does not imply that the causal effect of education on fertility is negative. Those who have a college degree and those who do not are likely to differ in many observable and unobservable ways. For example, as shown in Table 2, those with a degree and those without a degree differ in the share of people born in Seoul (by 6.4 percentage points).

The differences in these baseline characteristics imply that other unobservable confounding factors likely affect both fertility and education level. Hence, researchers need to control for these observable and unobservable differences between the treatment and control groups in order to estimate the causal impact of education on fertility.

Validity Check for the RKD

The use of an RKD in estimating the causal impact of higher education on fertility is conditional on the fact that we observe a statistically and practically significant kink in the probability of receiving a college degree at the cutoff. Panel a of Fig. 1 shows a kink that is visually clear, but the figure corresponds to the population data. Moreover, panel a shows the kink in the probability of entering—not graduating—college. To determine whether we can use the RKD, we therefore test for the kink in the treatment variable using the 2010 census data. Panel b of Fig. 1 shows the share of college degree recipients by year of birth. Note that those born after 1973 are likely to have been

Table 2 Differences in the baseline characteristics and outcome variables by treatment status

Variable	Untreated (A)	Treated (B)	Difference (B – A)	Sample Size
Have Children	0.958 [0.200]	0.938 [0.240]	–0.020 [†] (0.007)	57,547
Total Number of Births	1.882 [0.797]	1.651 [0.804]	–0.231** (0.039)	57,547
Age (in years)	37.764 [3.207]	36.509 [3.425]	–1.254** (0.349)	57,547
Born in Seoul	0.091 [0.288]	0.156 [0.362]	0.064** (0.003)	57,547
Korean Nationality	0.980 [0.138]	0.992 [0.088]	0.011** (0.001)	57,547

Notes: Tests of the difference were conducted using OLS regression; for the indicator outcome variables, a linear probability model was used. The explanatory variable is a dummy variable indicating college graduates. Observations with a birth year between 1968 and 1979 are used for the analysis sample. The results rarely change even if the comparison is based on a narrower period. The numbers in brackets are standard deviations. The numbers in parentheses are standard errors, clustered at the birth year level.

[†] $p < .10$; ** $p < .01$

affected by the reform initiated in 1993. As shown in the figure, a visually clear kink is observed at the cutoff. The share of college graduates increases from .275 to .325 (only 5 percentage points) during pretreatment periods, but the share is much higher during post-treatment periods. Since 1993, the share increased by more than 20 percentage points and increased rapidly and continually. As shown in panel A of Table 3, the RK estimates for the treatment variable are all statistically significant regardless of the bandwidth choice.

We conduct placebo regressions for the probability of receiving a college degree to validate the significance of the observed kink. Specifically, we derive RK estimates for other birth years, shown in Fig. 2. In the figure, the black dot indicates the true RK estimate at the 1973 cutoff. The figure presents other placebo RK estimates observed for the other cutoffs. Dashed lines indicate the 95 % confidence interval, and the solid line corresponds to each RK estimate at each cutoff. As shown in the figure, the true RK estimate is the highest in terms of its magnitude. None of the other RK estimates are larger than the true RK estimate. Furthermore, most of the other RK estimates are statistically insignificant at the 5 % level.

One of the identification assumptions required in the context of the RKD is that individuals cannot manipulate an assignment variable. If individuals could manipulate an assignment variable, then we would not likely be able to observe quasi-random variation in the treatment variable. We statistically test for this assumption using a modified version of the density test proposed by McCrary (2008) that derives RK estimates using the frequency observations as data points.

Figure 3 shows the density of the assignment variable by birth year. The idea behind the density test is that if people can manipulate an assignment variable, we will see statistically and practically irregular patterns in the density of the variable, especially at the cutoff point, which casts doubt on the validity of the RKD identification

Table 3 Tests of kink in the treatment variable, density of the assignment variable, and baseline covariates

Outcome Variable	Bandwidth (h)		
	$h = 4$	$h = 5$	$h = 6$
A. Kink in the Treatment Variable			
College degree recipients	0.031** (0.004)	0.024** (0.004)	0.018** (0.003)
Number of observations	38,521	48,017	57,547
B. Kink in the Density of the Assignment Variable			
Birth year	-0.004 (0.003)	-0.003 (0.002)	-0.002† (0.001)
Number of observations	38,521	48,017	57,547
C. Kink in the Baseline Covariates			
Female	0.001 (0.004)	0.006 (0.004)	0.008* (0.003)
Korean nationality	-0.001 (0.001)	0.000 (0.001)	0.000 (0.001)
Born in Gyeonggi-do	0.001 (0.001)	0.000 (0.000)	0.001 (0.001)
Born in Seoul	0.006 (0.003)	0.000 (0.003)	-0.001 (0.002)
Degree holder	-0.002 (0.002)	-0.003* (0.001)	-0.002* (0.001)
College degree recipients	0.005 (0.003)	-0.002 (0.003)	0.002 (0.003)
Number of observations	38,521	48,017	57,547

Notes: For the analysis of the kink in the treatment variable, the outcome variable is an indicator for whether a person holds a college degree. For the assignment variable, the outcome variable is the density of the assignment variable. Each variable analyzed for the baseline covariates is an indicator variable. The number of observations used for analyzing the *Female*, *Degree*, and *College degree* variables are, respectively, 68,988, 40,873, and 13,146 for $h = 4$; 85,569, 50,976, and 16,418 for $h = 5$; and 109,721, 61,070, and 19,532 for $h = 6$. The numbers in parentheses are standard errors, clustered at the birth year level.

† $p < .10$; * $p < .05$; ** $p < .01$

assumption. Figure 3 shows no signs of such irregularity in the density of the assignment variable; thus, we do not observe any kink at the 1973 cutoff point.

Panel B of Table 3 shows the results obtained from the modified density test. For the bandwidth choice of four, five, and six, the estimated kink at the cutoff point is very small: 0.004, 0.003, and 0.002, respectively. Although the very small kink estimate under the bandwidth choice of six is somewhat statistically significant (i.e., at the 10 % level), the estimated effect of -0.002 implies practically no significant kink at the cutoff point.

The other important identifying assumption requires baseline characteristics to be balanced between the treatment and control groups. In the context of the RKD, this assumption rules out any statistically significant kink in the baseline characteristics at

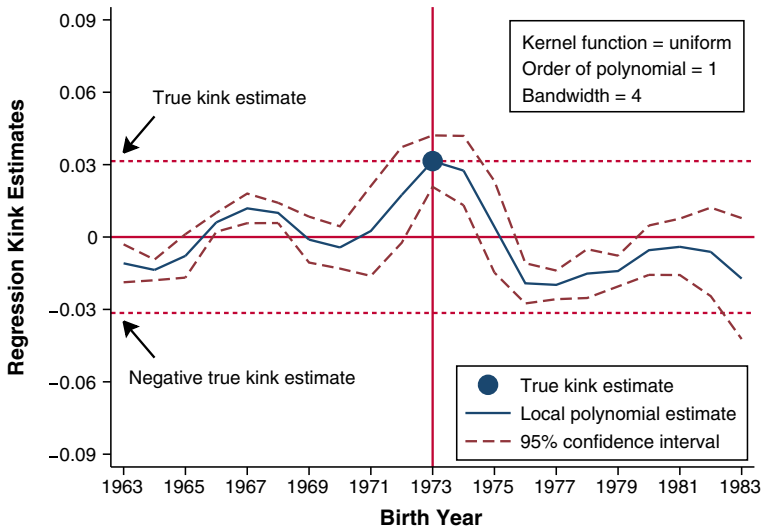


Fig. 2 Tests of statistical and practical significance of the kink in the treatment variable.

the cutoff point. We therefore test for the kink in baseline covariates available for use in the census data.

Panels a and b in Fig. 4 correspond to the shares of women and Koreans. The share of women and Koreans is smooth across birth years, and we do not observe any significant change in the slope at the 1973 cutoff. The share of women is approximately 57 % and is stable over the period displayed.⁵ The share of Koreans is more than 99 %, which is also stable across years. Panels c and d of Fig. 4 present two additional predetermined characteristics: whether the place of birth is Seoul or Gyeonggi-do. Examining the kink in these two variables is useful for testing differences in baseline characteristics because those who were born in Seoul or Gyeonggi-do likely come from a more advantageous socioeconomic environment. Panel c shows the share of individuals born in Seoul by the assignment variable. The share is approximately 10 %, and we do not observe any significant kinks at the cutoff. Furthermore, the magnitude of the share is relatively consistent across years. Panel d shows the share of individuals born in Gyeonggi-do. The overall share—approximately 6 %—is slightly lower than that for Seoul. Nevertheless, the share of individuals born in Gyeonggi-do is stable, the slope does not shift at the cutoff point.

Panels e and f of Fig. 4 present the share who received a degree and the share of college entrants who obtained a college degree. Obtaining a degree is influenced by many factors, such as motivation, effort, and other unobservable characteristics. If we see a difference in terms of these two variables, unobservable characteristics are unlikely to be similar between the two groups. Panel e displays the share of those who received a degree among all education levels. The mean share is approximately 94 % for those born before 1974 and approximately 93 % for those born in 1974 or later. No significant kink is observed at the cutoff point. Panel f shows the share of

⁵ For testing the balance in the share of women, we added male observations to the analysis sample.

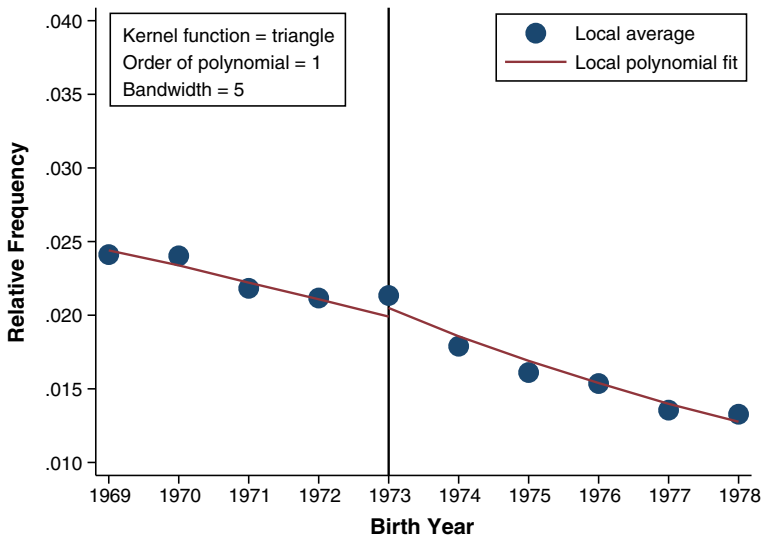


Fig. 3 Density of the assignment variable.

college matriculants who received a college degree, revealing little difference in the mean shares between the two groups and no visually salient kinks at the cutoff point.

In sum, all six panels of Fig. 4 indicate that no visually clear kinks occur at the cutoff point, indicating that the two groups are comparable in terms of predetermined characteristics. In panel C of Table 3, we present RK estimates for the variables examined earlier. Two results stand out. First, the estimated kinks at the cutoff point are statistically and practically negligible, which can be expected from the graphical analyses. Although some estimates are statistically significant, the magnitude of the estimates is extremely small, and the statistical significance is likely attributable to the precision driven by large sample size and small variance rather than to an imbalance in unobservable characteristics.

Other covariates—such as demand for female university education, marriage patterns, and female labor force participation—may be changing at the same time as the endogenous variable in this study. Using the Korean registration system that records all the marriage records and the Economically Active Population Survey used for reporting the official employment rate in Korea, we checked that the average age at first marriage, female labor participation rate, crude marriage rates, and total fertility rates were all stable between 1993 and 2003, the period in which our analysis cohorts are likely to have completed their college education (results are available upon request). We do admit, however, that other unobservable characteristics might hinder internal validity of the analysis, and the findings of this article may be limited to the relatively short study period.

Note that the results from the density test produce a significant kink in the density of the assignment variable when the choice of bandwidth is six (see panel B of Table 3). Two estimates are statistically significant in panel C, although the value of these estimates is close to 0. Moreover, the estimated kink in the assignment variable is small (i.e., 0.018) under the bandwidth choice of six (see panel A of Table 3). When analyzing the effect of a college degree on fertility, therefore, we focus on the

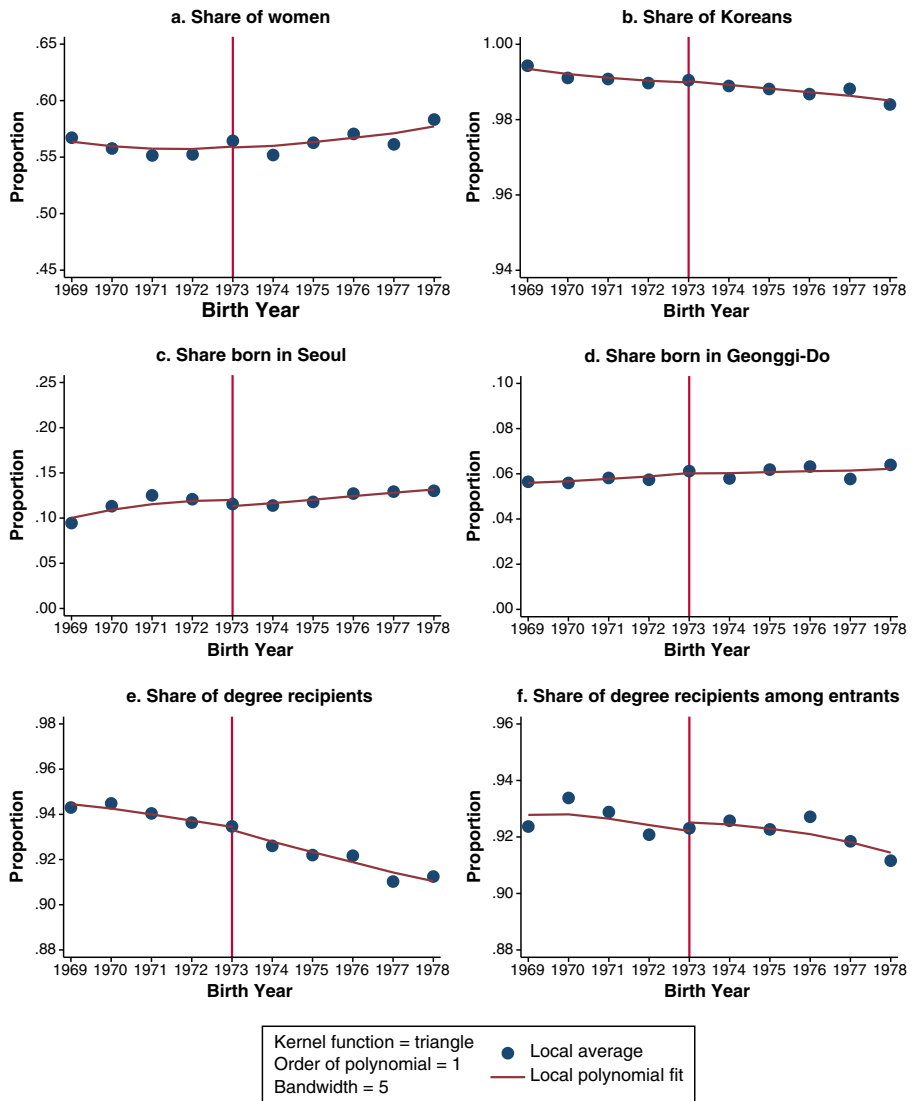


Fig. 4 Kink in predetermined covariates.

bandwidth choice of four or five and derive conclusions and policy implications from the results obtained from such a choice.

Results

Two outcome variables are analyzed for examining the causal impact of having a college degree on fertility. The first outcome is an indicator equal to 1 if a female has given birth and 0 otherwise. Panel a of Fig. 5 shows the graphical result. The share of women born in 1969 who have given birth is almost 98 %. The share continues to

decline until birth year 1978. One thing to emphasize regarding the observed trend is that this decline is driven by aging. That is, the younger generation is less likely to give birth than the older generation. Note, however, that the degree of the slope observed for both groups is quite different. The slope observed for the control group (i.e., those born in 1973 or earlier) is relatively flat and barely negative, but the slope observed for the treated group is relatively steep and more negative. Interestingly, the observed pattern in panel a of Fig. 5 is comparable with that observed in panel b of Fig. 1, in which the slope observed for the share of college graduates is relatively flat during the pre-treatment period but is steep and positive in the post-treatment period.

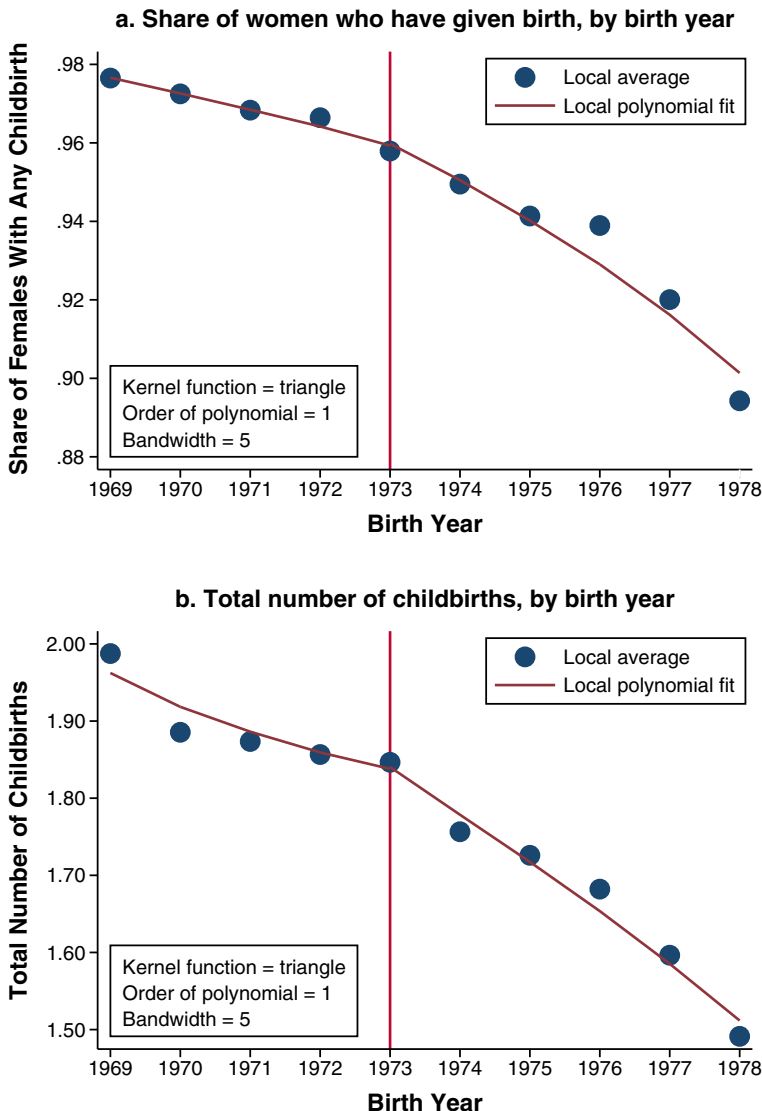


Fig. 5 Kink in outcome variables.

Table 4 Effects of having a college degree on fertility: Fuzzy RK estimates

	Bandwidth (h)		
Outcome Variable/Cutoff Year	$h = 4$	$h = 5$	Mean of Estimates
A. Outcome Analysis			
Given birth	-0.138* (0.056)	-0.319** (0.121)	-0.228 —
Total number of births	-1.347** (0.326)	-1.293** (0.478)	-1.320 —
Analysis sample	38,521	48,017	—
B. Mechanism Analysis			
Unemployed	-0.151* (0.067)	-0.220** (0.051)	-0.185 —
Wage earners	0.211* (0.090)	0.482** (0.186)	0.346 —
Professional occupation	0.143† (0.079)	0.226** (0.076)	0.184 —
Married	-0.026 (0.037)	-0.099* (0.044)	-0.062 —
Number of observations	38,521	48,017	—
C. Placebo Analysis			
Birth year = 1964	-0.070 (0.096) [53,770]	-0.178 (0.110) [68,034]	-0.124 — —
Birth year = 1965	0.067 (0.214) [52,693]	-0.045 (0.170) [66,779]	0.011 — —
Birth year = 1966	-0.053 (0.382) [51,792]	-11.028 (390.047) [65,803]	-5.540 — —
Birth year = 1967	-0.062 (0.196) [50,901]	-0.174 (0.139) [63,408]	-0.118 — —

Notes: The numbers in brackets are the sample size for the placebo analysis. The numbers in parentheses are standard errors, clustered at the birth year level.

† $p < .10$; * $p < .05$; ** $p < .01$

In panel b of Fig. 5, we present graphical results for the other outcome variable: the total number of female childbirths. The pattern observed for the number of childbirths is comparable with the pattern observed in Fig. 1. The graphical results in Fig. 5 indicate that the patterns of the treatment and outcome variables are closely and negatively related, suggesting that a college degree is negatively associated with the fertility rate. To get a sense of the extent to which a college degree influences fertility, we conduct a fuzzy RK analysis to estimate the causal impact.

Panel A of Table 4 presents the results for the outcome variables. The estimated effect of a college degree on the probability of giving birth is -0.138 under the bandwidth choice of four and -0.319 under the bandwidth choice of five. The estimates are statistically significant at the 5 % and 1 % levels. Compared with those who do not have a college degree, therefore, the probability of a female giving birth is, on average, 0.228 lower for those who have college degrees. The estimated effect on the total number of births under these bandwidths is, respectively, -1.347 and -1.293 , both of which are statistically significant at the 1 % level. Thus, on average, women with a college degree have approximately 1.3 children less than those without a college degree.

The magnitude of our findings coincides with previous studies (discussed earlier) estimating the causal impact of education on fertility. Although the magnitude of the estimated effects in the previous literature varies to some extent, most studies have found that, on average, one year of education reduces the total number of childbirths by 0.3. This study compares individuals who have a four-year college degree with those who have a high school degree or less. The estimated impact in this study is slightly higher than those presented in previous studies. This finding is reasonable given that this study takes the sheepskin effect into account. Moreover, this research examines the effect of higher education, so the size of the impact is likely to differ from that observed for the lower tail of the education levels.

Merely deriving the causal impact of a college degree on fertility provides few policy implications unless one speaks to the underlying mechanisms that induce the causal channel between a college degree and fertility. Although testing each of the aforementioned theories about why education is related to fertility would be difficult because of data availability, we examine some of the theories that can be tested using census data in an effort to shed light on the causal mechanisms.

Panel B of Table 4 presents the RK estimates for the potential mechanism variables. The estimation method used to derive such estimates is the same as the one we use for estimating the effect on fertility. The only difference here is that the outcome variable is replaced with other possible moderating variables related to labor market theory. To test whether education affects women's labor market-related status, we estimate the effect on three outcomes: unemployment status, whether a person is a wage earner, and whether a person has a high-wage professional occupation (e.g., lawyer).

According to the estimated results, a college degree reduces the likelihood of being unemployed. The estimated effect is -0.185 , on average, indicating that the unemployment rate is higher for women without a college degree. Further, female college graduates are, on average, 34.6 percentage points more likely to be wage earners. The result is reasonable because in Korea, female college graduates are more likely to enter the labor market than those without a degree. Having a college degree also affects women's occupation. Female college graduates are estimated to be 18.4 percentage points more likely to have a professional occupation than those without a college degree. Because the overall wage level of a professional occupation is relatively higher than that of other occupations in Korea, having a professional occupation is likely to increase women's earning capacity. It is difficult to draw conclusions from the estimated effects regarding the relative size of substitution and income effects. However, because the effect of having a college degree on

fertility is negative, and having a college degree raises the earning capacity, we argue that substitution effects are larger than income effects, which coincides with the conclusion provided by Becker and Lewis (1973).

We also analyze the heterogeneous impact of having a college degree on fertility. Specifically, we divide the analysis sample by place of birth (i.e., Seoul vs. non-Seoul). Seoul is the capital of Korea and is a highly urbanized area compared with other cities. Those who were born in Seoul are more likely to be from high-income households and to go to college in Seoul, where most of the high-ranked universities are located. As such, these women are more likely to be employed by high-paying firms. And if we believe that the opportunity costs of fertility are larger for those who were born in Seoul, then the effect of having a college degree on fertility should be larger in magnitude. Interestingly, the analysis by place of birth shows that the effect of having a college degree is roughly twice as large for women who were born in Seoul as for those who were not (e.g., -0.5 vs. -0.2 and -2 vs. -1). In our opinion, these results (available upon request) are consistent with our argument that the opportunity costs of fertility induced by women's earnings capacity drive our results. However, income may not be the only opportunity cost. Unfortunately, this study does not shed light on factors not related to income; future research should aim to identify other opportunity costs of fertility such that relevant policy alternatives can be developed to reduce these costs.

External validity of the research should also be discussed. Although the effect estimates based on quasi-random variation may provide an estimate that is internally valid, this does not mean that such an estimate is externally valid. This study uses the cohorts born between 1969 and 1978, and the results obtained for these cohorts may not be applicable to other cohorts, such as those born before 1969. Using earlier cohorts, however, is problematic for several reasons. South Korea experienced a rapid change in the total fertility rate from more than 5.0 in the early 1970s to 1.5 in the mid-1980s. We argue that the rapid decline in the fertility rate during these periods reflects rapid changes happening in the endogenous variables as well as other covariates. Thus, we believe that using earlier cohorts would not allow us to isolate the causal impact of having a college degree that is internally valid because many unobservable characteristics are likely to be correlated with our variable of interest. Nevertheless, the results of this study should be interpreted with this limitation in mind.⁶

Another limitation of this study is the use of cohorts who are not considered to have reached the end of their childbearing years: namely, women aged 45–49. The cohorts analyzed in this study are aged 32–41, and the estimated results may differ if older cohorts are used for the analysis. According to the Korean registration system that records all the childbirth records for each year, however, the share of childbirths to women aged 40–49 was approximately 2.5 % during the 1990s and 3.5 % during the 2000s. Accordingly, we argue that any possible bias from the cohort used is small. Nevertheless, the results should also be interpreted with this limitation in mind.

⁶ The educational reform that we exploit to isolate the causal impact of education on fertility happened in 1993. The analysis periods are from 1988 and 1997, with 1988 to 1992 being the pre-treatment periods and 1993 to 1997 being the post-treatment periods. The women in our sample experienced their childbirths during or after these periods. Thus, our analysis periods do not overlap with the periods in which the total fertility rate dropped very rapidly (i.e., 1970 to 1985). Because the total fertility rate was very stable near 1.5 and 1.6, respectively, for our two analysis periods, we believe that our analysis periods suffer less from confounding factors.

Robustness Check: Placebo Tests

We conduct placebo tests to examine the robustness of the findings. For the estimated effects observed at the 1973 cutoff to be convincingly attributed to the effect of a college degree on fertility, we should not observe such effects when we apply the same method to the pre-treatment periods. Observing similar effects from such placebo tests would call into question whether the true observed effects indeed reflect treatment effects. In Fig. 6, we create placebo cutoffs at 6 and 10 years before the true cutoff to examine whether similar kinks occur. The four panels in Fig. 6 show no visually significant kinks for either the share of college degree recipients or the share of women with childbirths. Hence, the graphical results for the placebo cutoffs support our conclusion.

Panel C of Table 4 presents statistical results obtained for placebo tests. In this panel of the table, we present the fuzzy RK estimates that reflect the effect of having a college degree on the likelihood of childbirth obtained from using other birth years as placebo cutoffs. The estimated results support the true observed estimates. One particularly large estimate is observed for the 1966 birth year cutoff under the bandwidth choice of five (i.e., -11.028), although the estimate is not statistically significant. Note that the RKD estimand is obtained by dividing the slope change in an outcome variable by the slope change in a treatment variable. Because the slope change in the treatment variable is almost 0 for the 1966 birth year cutoff under the bandwidth choice of five, the small

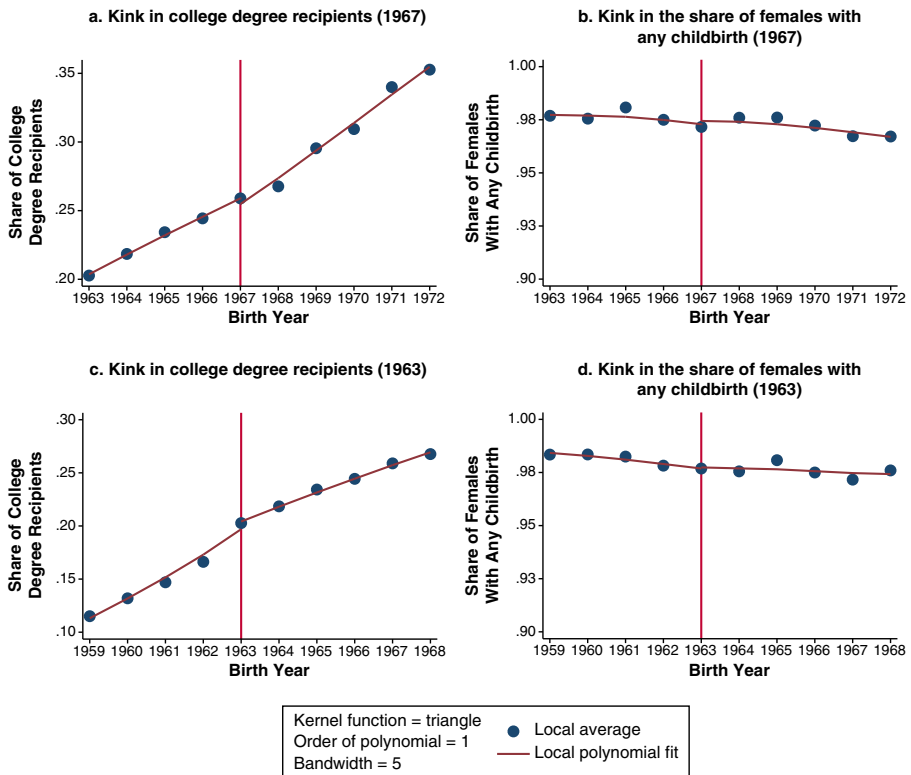


Fig. 6 Kink at placebo cutoffs of 6 and 10 years before the true cutoff.

slope change observed for the outcome variable results in a very large point estimate. We therefore argue that the placebo test results support the conclusion that having a college degree reduces fertility.

Conclusions

Using the higher education reform initiated in 1993 as an exogenous variation for the probability of holding a college degree, we apply the RKD to estimate the causal impact of having a college degree on fertility as well as to identify possible mechanisms that channel the relationship. The results show that, on average, having a college degree reduces the likelihood of childbirth by 23 percentage points and the total number of childbirths by 1.3. An analysis of the possible mechanisms shows that the labor market is one significant channel driving the negative effects of having a college degree on fertility. We estimate that having a college degree increases women's earning capacity; in particular, compared with a high school graduate, a female college graduate is more likely to be a wage earner, more likely to have a professional occupation, and less likely to be unemployed.

With today's high educational levels, a decline in the fertility rate may be inevitable. But a high educational level is extremely valuable for any society in general (e.g., leading to reductions in crime rates and governmental dependency). From a policy perspective, therefore, measures to decrease such high educational levels should never be adopted. Then, is the decline in the fertility rate a phenomenon that cannot be resolved? This study sheds light on possible policy measures that may be helpful for relieving such a trend, such as eliminating the opportunity cost of fertility induced by high educational levels. We also argue that future studies should investigate the possible opportunity costs inherent in the education-fertility relationship so that effective policy measures can be developed to target such opportunity costs.

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