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The impact of female education on fertility: a natural experiment from Egypt

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Abstract This paper presents new evidence on the impact of female education on fertility in Egypt using the change in the length of primary schooling as the source of exogenous variation in education. Beginning in 1988, the Egyptian government cut the number of primary school years from six to five, moving from a 12-year system of pre-university education to an 11-year system. This policy change affected all individuals born on or after October 1977. Using triennial pooled cross-section data from 1992 to 2014 and a nonparametric regression discontinuity approach, we compare education and fertility of women born just before and right after October 1977. Our analysis shows that female education significantly reduces the number of children born per woman. The reduction in fertility seems to result from delaying maternal age rather than changing women's fertility preferences. We also provide evidence that female education in Egypt does not boost women's labor force participation or affect their usages of contraceptive methods. Female education, however, does appear to increase women's age at marriage which might explain the delay of maternal age.

Keywords Fertility · Female education · DHS data · Regression discontinuity · Egypt

JEL classification J13 · I210 · I280

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1 Introduction

Standard economic theory suggests that enhancing women's employment opportunities is the most important mechanism through which female education reduces fertility (Becker 1960, 1993; Becker and Lewis 1973; Willis 1973). In particular, highly educated women are more likely to have high-paying jobs, which increases the opportunity cost of childbearing and, therefore, decreases fertility. This mechanism, however, does not appear to hold in the case of Arab countries, where the majority of women do not participate in the labor market despite the remarkable increase in female education over time. In fact, during the 1980s and the 1990s, fertility rate in Arab countries declined by more than two births per woman. Concurrently, female education in these countries has increased by more than threefold (Eltigani 2005). The increase in female education, however, hardly affected women's labor-force participation. Eltigani (2005) documents that labor force participation of women in the Arab countries is among the lowest in the world, and that the rate of their participation during the 1980s and 1990s changed very little, or may have even declined. This pattern also seems to exist in other non-Arab countries in the Middle East and North Africa (MENA) region such as Turkey (World Bank 2009).[1] A recent report by the World Bank (2013) has found that female labor force participation in the entire MENA region is only 25%—about half of the world average-despite the substantial increase in female education in this region.

The low rates of women's labor force participation in the Arab countries, and in the MENA region in general, raises some doubts on whether the large reduction in fertility rates that these countries witnessed over the last few decades was actually driven by improving female education. Put differently, both female education and fertility might be driven by other factors such as community background and women inherent ability. For example, many communities within the Arab countries, especially in rural areas, place a small value on female education and, at the same time, encourage big family sizes.[2] In this cultural context, a negative correlation between female education and fertility can be observed even if female education per se does not have a causal impact on fertility. Furthermore, even if the reduction in fertility rates was caused by the increase in female education, understanding the mechanisms through which female education reduces fertility, in spite of the low rates of women's labor force participation, has important implications for both economic theory and government policies targeted at reducing fertility rates. Although a substantial effort has been devoted by governments and international organizations to increase female education and promote women's empowerment in the Arab world (United Nations 2005), little is known about the mechanisms through which women's education

[1] During the past two decades, the proportion of women in Turkey with more than primary school education more than doubled. The number of children born per woman declined from 3 children in 1988 to 1.9 children in 2008. During the same time, female labor force participation has declined from 34% to 22% (World Bank 2009).

[2] In some communities in Arab countries, parents believe that their daughters will be better off learning household chores rather than going to school in order to prepare them for their future marriage lives. Women in these culture contexts also have a strong incentive to have many children for the purpose of securing marriage, helping out in household production, and discouraging husbands from having more than one wife.

reduces fertility given the unique culture and traditions of these countries. Understanding these intermediate mechanisms may provide great insights into the extent to which labor market opportunities play a role in household fertility decisions in the Arab countries. This paper attempts to address these issues.

In this paper, we use a natural experiment from Egypt to study the causal impact of female education on fertility and explore the mechanisms underlying this relationship. The majority of studies that examined the relationship between female education and fertility in developing countries did not adequately account for the endogeneity of female education, and hence, their results can be better regarded as correlations rather than casual relationships (Al-Qudsi 1998; Bhargava 2007; Cochrane et al. 1990; Handa 2000; Khawaja and Randall 2006; Lam and Duryea 1999; Martín and Juárez 1995). A recent and growing number of studies in developing countries have used sources of exogenous variations in female education to account for the endogeneity of education (Baird et al. 2010; Breierova and Duflo 2004; Chicoine 2012; Dinçer et al. 2014; Duflo et al. 2015; Güneş 2016; Osili and Long 2008; Ozier 2015; Tequame and Tirivayi 2015). These studies, however, did not focus on the Arab region, and only a few of them analyzed the mechanisms through which female education reduces fertility. One exception is Lavy and Zablotsky (2015), who examined the causal impact of female education on fertility among Arab citizens of Israel. Lavy and Zablotsky used the exposure to the end of the military rule, which restricted the mobility of Arabs in Israel, as the source of exogenous variation in education. The authors found that each year of female education reduces the number of children born per woman by 0.6 children. Although this study provides important insights into the effect of female education on fertility in the Arab region, there are two main concerns with its findings. First, the Arab citizens of Israel may not be representative of the majority of population in Arab countries given the unique position of Arabs in Israel and the historical context within which their communities have developed, which might put some constraints on their choice sets and influence their incentives to have children. Second, as acknowledged by the authors, it is possible that reducing mobility restrictions imposed on Arabs in Israel might have improved outcomes other than women's education that directly affected fertility. In particular, not only did the removal of travel restriction create access to schooling, but it also enabled access to better employment opportunities, health knowledge, and modern lifestyles. These factors are expected to have direct impacts on fertility in absence of female education.

Our paper uses the change in the length of primary schooling in Egypt in 1988–89 to examine the causal impact of female education on fertility in Arab countries and explore the mechanisms through which education affects fertility. To the best of our knowledge, this is the first study to use this natural experiment to create an exogenous variation in education in Egypt. Beginning in 1988, the Egyptian government cut the length of primary education from 6 to 5 years, moving from a 12-year system of pre-university education to an 11-year system (law No.233 of 1988).[3] The 5-year

[3] Before this change, the high school diploma in Egypt was composed of 6 years primary school, 3 years preparatory school, and 3 years secondary school, for a total of 12 years. After the policy change, the high school diploma was composed of 11 years: 5 years in primary school, 5 years in preparatory school, and 3 years in secondary school.

primary system was universal throughout the country, affecting all individuals born on or after October 1, 1977. Therefore, October 1, 1977, represents a cutoff such that individuals born before that date had to attend one more year of schooling than individuals born on or after that date. Assuming that women born immediately after and just before October 1, 1977, are similar in baseline characteristics, the differences in their completed years of schooling in adulthood are exogenous. In fact, even if some parents can plan when their children are born, they are less likely to have full control over the exact date of birth. Moreover, the reduction in the length of primary schooling occurred in 1988, and it applied immediately to children who were 11 years old at that time. Thus, it is unlikely that parents would have anticipated that such policy change would have happened 11 years in the future and changed their behaviors accordingly. Therefore, the policy change can be regarded as good as a random local experiment around the cutoff date, allowing us to compare fertility of women born right before and just after October 1, 1977, and relate the difference in their fertility outcomes to the difference in their education. Our identification strategy is in line with several recent studies in developed countries that use educational policy changes to create exogenous variations in education and explore its impact on fertility (Amin and Behrman 2014; Black et al. 2008; Currie and Moretti 2003; Cygan-Rehm and Maeder 2013; Grönqvist and Hall 2013; Lindeboom et al. 2009; McCrary and Royer 2011; Monstad et al. 2008).

We examine the effect of female education on three fertility outcomes: the number of children ever born per woman, ideal number of children, and age of women at first birth. We use a nonparametric regression discontinuity (RD) design to compare education and fertility of women around the cutoff date. We implement the RD regression using a fuzzy RD design to account for the possibility that women's educational attainment in adulthood is not solely explained by the length of primary schooling. In fact, a large portion of women in our study continued their education beyond the primary education level in addition to a considerable portion of women who did not enter school or dropped out of school before completing their primary education.[4] Therefore, other factors, beyond the length of primary schooling, also determine women's educational attainment in adulthood. The fuzzy RD design is more appropriate in this case as it allows for other factors, beyond the length of primary schooling, to influence female educational attainment in adulthood.[5] We estimate kernel-based local linear regression models for education and fertility. We also carry out a complementary analysis using local exponential mean regression models for count responses. The data for this study come from the recent seven waves of the Egyptian Demographic and Health Survey (EDHS) (1992, 1995, 2000, 2003, 2005, 2008, and 2014). The total sample size for the seven waves is 97,314 ever-married women of reproductive age. The EDHS survey is a nationally representative sample that provides rich information on fertility history and

[4] Despite the fact that primary schooling is mandatory in Egypt, the law is not strictly enforced and both failing to access formal education and school dropout before completing the primary education are prevalent problems in Egypt (Assaad and Barsoum 2009).

[5] As will be discussed in the methodology section, the fuzzy RD design consists of two steps. The first step involves extracting the part of variation in female education that is explained by the length of primary schooling. In the second step, we use this extracted variation in female education to explain fertility outcomes.

socioeconomic and demographics factors. Most importantly, for the purpose of our analysis, this survey provides information about the year and month of birth of each woman so that one can identify which woman attended which primary school system.

Our analysis shows that women who were affected by the 6-year primary system had completed in adulthood, on average, 1 more year of schooling compared to women who were subject to the 5-year primary system. Using this variation in education, we find that each year of female education reduces the number of children ever born per woman by 0.079 children. That is, a woman with a high school diploma has about one less child than a woman with no formal education. We also analyzed the mechanisms through which female education reduces fertility. The analysis shows that the reduction in the number of children seems to result from postponing maternal age rather than changing women's attitudes and preferences. In particular, each year of female education postpones maternal age by 2.63 to 3.45 months, with no significant impact on women's ideal number of children. We also provide evidence that the delay of maternal age results from delaying marriage rather than increasing women's labor force participation or affecting their usages of contraceptive methods. Our results are quite robust to several robustness checks and restrictions on the sample.

The rest of the paper is organized as follows: Section 2 summarizes the existing literature on education and fertility and briefly discusses the change in the length of primary schooling in Egypt. Section 3 describes the EDHS data. Section 4 discusses the regression discontinuity design we use in this paper. In section 5, we present the results of this paper dividing them into baseline results ignoring the endogeneity and the main results of the RD analysis. Section 6 provides robustness checks. Section 7 examines the mechanisms through which female education affects fertility. Finally, section 8 provides the conclusion.

2 Background

We first summarize the theoretical literature on the relationship between female education and fertility before turning to the review of the empirical literature. We then provide further details about the change in the length of primary education in Egypt.

Standard economic theory has ambiguous predictions about the relationship between female education and fertility. According to the theory, education results in two different effects: substitution effect and income effect. The substitution effect of education results from the possibility that higher levels of education increase the opportunity cost of childbearing through enhancing women's labor market outcomes and thus lead to a decrease in fertility (Becker 1960). Becker and Lewis (1973) also highlighted that more educated parents tend to invest more in children's human capital, resulting in increases in the cost of fertility per child. Moreover, education may reduce the cost of avoiding pregnancy because more educated women tend to have better knowledge and be more efficient in using contraceptive methods (Rosenzweig and Schultz 1989). On the other hand, education may also have an income effect that can lead to the opposite conclusion. Highly educated women, for instance, tend to have more earnings and hence can afford to have more children as children are assumed to be normal goods. Another channel of the income effect, introduced by Behrman and Rosenzweig (2002), is that a highly educated woman tends to marry a highly educated husband, resulting in increased family income that allows having more children.

In addition to income and substitution effects, Jejeebhoy (1995) emphasizes the role of cultural context in the relationship between education and fertility. She argues that education improves a woman's autonomy through improving her social and economic self-reliance and hence enabling her to make her own decisions. The power of education, however, in enhancing a woman's autonomy is largely dependent on the contextual factors and woman's position in society. For instance, in societies with wide gender disparities, small amounts of education may have a negligible impact on a woman's autonomy and hence a weaker effect on fertility.

Whether the substitution effect dominates the income effect, and under which cultural context, has remained largely an empirical question. A considerable number of empirical studies have been published in the past three decades about the impact of education on fertility. Unobservable factors, such as women ability and preferences, which might affect both education and fertility, and yet cannot be controlled for, have remained a challenge in empirical studies. Studies in developed countries have exploited changes in education policy to create exogenous variations in education and establish a causal relationship between female education and fertility. There is no consensus, however, among these studies on the effect of female education on fertility. For instance, Monstad et al. (2008) exploited the extension of compulsory schooling in Norway in 1959 from 7 to 9 years and found no evidence that more education resulted in more women remaining childless or having fewer children. In contrast, Black et al. (2008) exploited the regional and time variations in compulsory schooling laws in both the US and Norway and found that increased compulsory schooling reduced the incidence of teenage childbearing in both countries. Using a similar policy change in West Germany between 1958 and 1969 which extended the compulsory schooling from 8 to 9 years, Cygan-Rehm and Maeder (2013) found that this policy increased women's education and reduced their fertilities. Grönqvist and Hall (2013) exploited the eductional reform in Sweden, which prolonged the vocational tracks in upper secondary school from 2 to 3 years. They found that the implmemetation of this reform has significnatly decreased teen pregnancies. Another study by McCrary and Royer (2011) used data from California and Texas to examine the effect of education on fertility for mothers born just before and after the school entry date. The authors found no significant effects of female education on fertility. Amin and Behrman (2014), however, found opposite results using data on twins from Minnesota. The lack of consistency among the studies of developed countries makes it difficult to generalize results from developed countries for the developing countries. Additionally, the socio-economic and cultural contexts differ dramatically between developed and developing countries, which are expected to play important roles in the relationship between female education and fertility (Jejeebhoy 1995).

Earlier empirical studies in developing countries have documented a negative association between education and fertility (Al-Qudsi 1998; Balley 1989; Bhargava 2007; Cochrane et al. 1990; Handa 2000; Khawaja and Randall 2006; Lam and Duryea 1999; Martin 1995). Nearly all of these studies did not adequately control for the endogeneity of female education. Several recent studies in developing countries have used governmental education reforms to create exogenous variations in education. The majority of this research, however, has focused on countries in Sub-Saharan Africa. In particular, Osili and Long (2008) created an exogenous variation in education using the Universal Primary Education program in Nigeria, which provided tuition-free primary education and constructed new schools. The authors found that each year of female education reduces fertility by 0.26 births. Similar methodology and results were also obtained by Breierova and Duflo (2004) using a massive school construction program in Indonesia during 1973–78. Several studies have investigated the causal impact of female education on fertility in Kenya. For instance, Chicoine (2012) exploited the 1985 policy change in Kenya that reclassified 8th grade from secondary to primary school to create an exogenous variation in female education. The author found that the increase in education led to a reduction in woman's fertility by 0.32 children. The author also found that postponement in marriage and child bearing, reduction in the marital education gap, and increased early use of contraception are the mechanisms thorough which female education reduces fertility. Ferré (2009) has also used the same policy change and found close results. Duflo et al. (2015) conducted a field experiment in Western Kenya, which provided girls in the sixth grade free uniforms. The authors found that the program reduced the likelihood of teenage pregnancy by 4.4 percentage points. Ozier (2015) found a bigger effect (12 percentage points) on teenage pregnancy in Kenya using the probability of admission to government secondary school in Kenya as an exogenous variation in education.

Other studies have also examined the causal impact of female education on fertility in Sub-Saharan Africa. Baird et al. (2010), for instance, used the ongoing randomized conditional cash transfers (CCT) in Malawi, which reduce schools fees and provide cash transfers to schoolgirls to stay in or return to school. The authors found that CCT led to significant declines in early marriage and teenage pregnancy. In contrast, Grant (2015) has failed to find an impact of female education on age at first birth in Malawi using the 1994 Free Primary Education policy, which constructed new secondary schools. Tequame and Tirivayi (2015) examined the effect of women's higher education on fertility in Ethiopia. They exploited the liberalization of education sector in 1994, which resulted in a large increase in the number of private universities in Ethiopia. The authors found that education lowers fertility by 0.2 child and increases the likelihood of never giving birth by 25%. They also showed that the reduction in age gap between partners and the postponement of marriage and motherhood are key mechanisms that explain these effects. Grépin and Bharadwaj (2015) used the expansion in secondary school in Zimbabwe induced by the Zimbabwean independence in 1980. They found that, among mothers who were influenced by the reform, an additional year of schooling delays age at first birth by 0.34 years and reduces the number of children born per woman by 0.16 children.

Very few studies have examined the causal impact of female education on fertility in the Arab countries and in the MENA region in general. In particular, two studies by Güneş (2016) and Dinçer et al. (2014) have both focused on Turkey exploiting the compulsory education reform in 1997, which extended compulsory schooling from 5 to 8 years, as the source of exogenous variation in female education. The authors found that primary school completion reduces the number of children born per woman. They also showed that the increase in fertility among more educated women was mainly driven by the usage of contraceptive methods and the delay in marriage and childbearing rather than changing women's attitudes. Almost no study has explored the causal impact of female education on fertility in the Arab world. Only one study by Lavy and Zablotsky (2015) has examined the impact of female education on the fertility of Arab citizens in Israel exploiting the removal of mobility restriction during 1966. The authors found that the effect of education on fertility is negative and large. In particular, each year of female education reduces the number of children by about 0.6 children. As discussed earlier, there are some concerns on the extent to which the removal of travel restriction had no direct impact on fertility beyond its indirect impact through women's education. In particular, the large effect found in this study could in fact be combining the effects of other excluded factors such as health knowledge and exposure to modern lifestyles. Furthermore, the unique position and the historical context of Arabs in Israel may not represent the majority of population in the Arab countries.

Our paper contribute to the limited literature on the Arab world by exploring the causal impact of female education on fertility in Egypt, using the elimination of grade 6 in 1989–88 as a source of exogenous variation in education[6] (law No.233 of 1988). Before this policy change, students in Egypt attended a total of 12 years toward completing their high school diploma: 6 years in primary school, 3 years in preparatory school (middle school), and 3 years in secondary school. After this policy change, students attended only 5 years in primary school with no change in the lengths of both preparatory and secondary schools.[7] The first school cohort subjected to the 5-year primary system was born on or after October 1977. We use a regression discontinuity approach to compare education and fertility of adult women born close to October 1977 but attended different primary school systems. The change in the length of primary schooling in Egypt provides a good opportunity to explore the causal impact of female education on fertility in the Arab countries for several reasons. First, the 5-year primary system was mandatory and universal all over the country, so households were not able to let their children attend different primary school systems. Another reason is that the switch to the 5-year primary system was announced by the government in June 1988 and was immediately applicable to children who were 11 years old at that time. Thus, it is unlikely that parents would have anticipated that change and adjusted their fertility behaviors accordingly.

[6] There is no single explanation as to why the Egyptian government had implemented this change. Most of the evidences on that reform are drawn from the newspapers at that time. One of the reasons highlighted by some government officials is that the purpose of the policy change was to reduce the burden on the average Egyptian household by reducing their out-of-pocket expenditures on primary education. Another reason that was mentioned is that the policy change was a step to cope up with educational systems in developed countries that were claimed at that time to follow an 11-year rather than a 12-year pre-university system. The most common explanation for the reduction in primary school years is that, by the mid-1980s, Egyptian elementary schools suffered from a shortage of classroom space because of large enrollments of children in the elementary schools. This problem led the government to shorten the length of the primary education from 6 to 5 years to save sixth-grade classrooms to absorb the large enrollment in school.

[7] It is worth mentioning that starting in year 2000 the length of the primary school was phased back to 6 years. Cohorts affected by this change have not shown up yet in the DHS survey and hence cannot be exploited in the current study. The reverse change in year 2000 was motivated by the government's desire to ensure that children spend enough time in primary school to get a better education.

In the section of robustness checks in this paper we tested for differences in baseline characteristics among women born right before and right after October 1977 and found no evidence of significant differences in baseline characteristics.

3 Data

This paper uses the seven recent waves of the Egyptian DHS survey: 1992, 1995, 2000, 2003, 2005, 2008, and 2014. The EDHS survey is a nationally-representative household survey that provide a wide range of information in the areas of population, health, and nutrition. In all households, ever-married women age 15–49 are interviewed to obtain more information on their health and fertility background. We focus our analysis on women age over 21 years to reduce the censoring in fertility variables, and also because it is more likely that women would have completed their education by that age.[8] The total sample size for the seven waves is 97,314 women. The questionnaires and other information about the survey are available in the recent EDHS report (Ministry of Health and Population [Egypt] El-Zanaty and Associates [Egypt] and ICF International 2015).

The survey provides information on female educational attainment in adulthood. In particular, the survey asked women about their completed educational levels and grades. For instance, if a woman answered that she left school after the 2nd year of her preparatory school (middle school), we compute her educational attainment in adulthood as follows: 6 years primary school if she was born before October 1977 (5 years if she was born on or after October 1977) + 2 years in preparatory school = 8 years of schooling (7 years if she was born on or after October 1977). The average years of women's education attained in adulthood in our sample is 6.24 years. This figure has almost been doubled during the period of study from 4.34 years in 1992 to 8.29 years in 2014. Figure 1 shows the distribution of women's year of education attained in adulthood during the entire period of study (1992–2014). As can be seen from the figure, about one third of the women in the sample has no formal schooling. This percentage has also been changed over time. Figure A1 in the supplementary appendix[9] shows that the percentage of women with no formal schooling in 2014 is half of what it was in 1992 (23.6% compared to 47% in 1992). Likewise, the percentage of women with secondary education has increased over time from 13.4% in 1992 to 35.6% in 2014.

We focus our analysis on three fertility outcomes: the number of children ever born per woman, the preferred (ideal) number of children, and the age of each woman when she delivered her first child. The sample size differs across these outcomes. All the women in the sample (97,314 women) answered the question about the total number of children ever born to them. Of these women, 6416 reported having no children at the time of the survey. The remaining 90,898 women in the

[8] Women in Egypt are less likely to return to school after leaving it, especially married women. The survey asked women whether they were attending school at the time of the survey. Less than 0.1% of the women have reported attending school at the time of the survey.

[9] The supplementary appendix including Tables A1–A4 and Figures A1–A3 is available upon request or at the web link at http://www.fatmaromeh.com/research

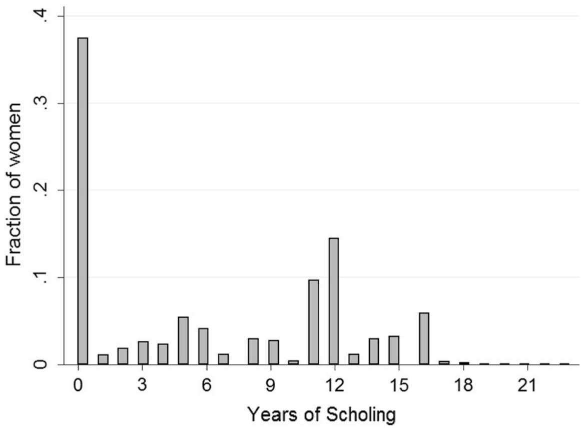

Fig. 1 Distribution of women's educational attainment in adulthood (1992–2014)

Table 1 Means and (Standard Deviations) for fertility and education variables

Variable	Number of children born	Age at birth (mothers' sample)	Ideal number of children
Fertility outcome	3.500 (2.333)	21.108 (4.120)	3.027 (1.441)
Education	6.245 (5.862)	6.132 (5.837)	6.689 (5.865)
Observations	97,314	90,898	84,849

EDHS data (1992–2014). The number of children born is based on the actual number of children born per woman at the time of the survey. Age at first birth is the age of a mother when she delivered her first child. The ideal number of children is a woman's preferred number of children as reported in the EDHS survey

sample who did report having children reported their ages at the time of their first birth. All the women in the sample were also asked about their preferred (ideal) number of children. There were 84,849 women able to provide a numerical response to this question. Table 1 provides means and standard deviations for the three fertility outcomes and the education variable. Women in the whole sample (first column) completed, on average, about 6.2 years of schooling and have 3.5 children. The sample of mothers in column 2 seems to be slightly less educated (6.1 years of schooling) compared to the full sample in the first column, but the difference in the means is statistically insignificant. The average age of a mother at the birth of her first child is about 21.1 years. The last column provides descriptive statistics for the third fertility outcome: the ideal number of children. The sample in this column includes women who were able to provide numerical responses for their ideal numbers of children. These women seem to be slightly more educated (6.7 years of schooling) compared to the full sample. The average ideal number of children is three kids (less than the actual number of children born per woman).

Figure 2 depicts the trends in the average number of children born per woman and the average years of women's educational attainment in adulthood during 1992–2014. As can be seen from the graph, the average number of children born per woman has been declining over time from 4.17 in 1992 to 2.88 in 2014. As discussed

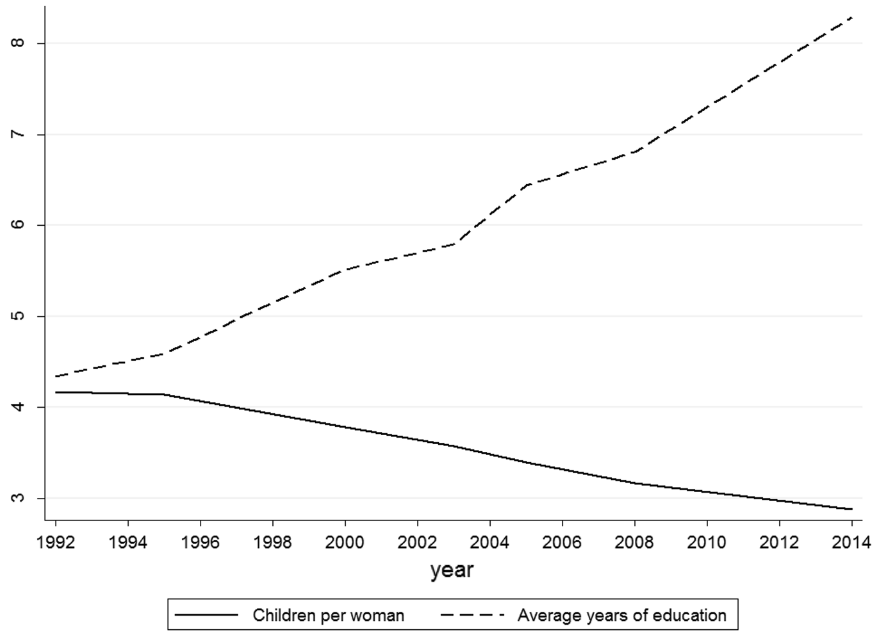

Fig. 2 Average numbers of children born per woman and average years of education

earlier and is shown in this graph, during this period, the average years of education of women has increased from 4.34 in 1992 to 8.29 in 2014. Thus, from a time series data perspective, there is a negative correlation between female education and fertility over time. Figure 3 depicts the same relationship using cross-sectional data for women for each year, separately. This paper examines whether this negative correlation represents a causal relationship.

4 Methodology

This section describes the RD design we use to estimate the impact of female education on fertility.[10] We employ a nonparametric RD design exploiting the reduction in the length of primary schooling in Egypt in 1988–89 from 6 to 5 years as the source of exogenous variation in education.

The first school cohort who was subject to the 5-year primary system were the children who were in their fifth grade during the academic year 1988–89 (September 1988–May 1989). Given that a child has to turn six before October 1 to enter school in Egypt, this implies that children who were in their fifth grade in 1988–89 were born between October 1, 1977 and September 30, 1978. Therefore, October 1977 represents a cutoff date such that individuals born before that date had to attend one more year of primary schooling. A standard linear RD model for the fertility

[10] See Imbens and Lemieux (2008) and Lee and Lemieux (2010) for a comprehensive review of the RD approach.

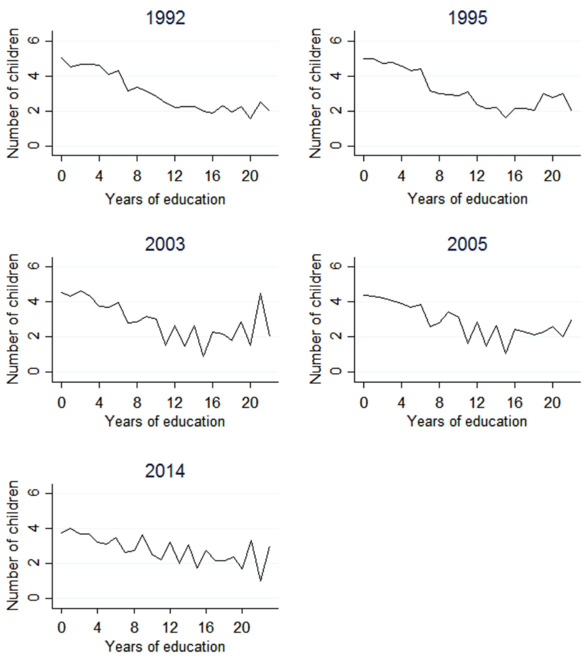

Fig. 3 Average number of children born per woman and years of education

outcomes for woman i can be specified as:

$$\begin{aligned} fertility_i &= \alpha_1 + \alpha_2 D_i + \alpha_3 (X_i - c) + \alpha_4 D_i * (X_i - c) \\ &\quad + \alpha_5 Age_i + \alpha_6 Age_i\_squared + \varepsilon_{i,} \end{aligned} \tag{1}$$

where $fertility_i$ refers to three outcomes: the number of children ever born to woman i, the ideal number of children of woman i, and the age of woman i at her first birth; X_i is the forcing variable, which is woman i's date of birth (expressed in year of birth and month of birth). Here c refers to the cutoff date at October 1977. Hence, $(X_i - C)$ denotes a woman's date of birth relative to October 1977. For instance, if woman i was born on October 1978, the value of her $(X_i - C)$ would be 12 months. In Eq. (1), D_i is an indicator variable for being born at or after the cutoff date $(X_i \geq C)$, Age_i is the age of woman i at the time of the survey, $Age_i\_squared$ is the age of woman i at the time of the survey squared, and ε_i is an idiosyncratic error term. We control for women's age at the time of the survey, in addition to controlling for the standardized dates of birth $(X_i - C)$, because we use seven waves of the EDHS survey from 1992 to 2014. Women with the same date of birth might appear in different survey rounds and hence be interviewed at different ages.[11] We also add an age squared term to

[11] We control for women's age at the time of the survey for the age at first birth outcome to account for any recall bias. In particular, even if the age of a woman when she delivered her first child is not influenced by her age at the time of the survey, younger women are more likely to remember their ages at first birth accurately compared to older women.

account for any nonlinearity in the relationship between women's age and fertility outcomes. Rather than controlling for women's age, we can instead include a set of year-of-survey dummies in the regression. Controlling for women's age is, however, expected to be more accurate. In the results section, we present estimates from both specifications and the results are very close.[12]

If female education is a binary variable that is determined solely by the length of primary schooling (5-year vs. 6-year systems), then the parameter α_2 in Eq. (1) would identify the causal impact of education on fertility, consistent with the case of the sharp regression discontinuity design. The sharp RD design would be appropriate if all women in our sample who were subject to the 5-year-primary system completed only 5 years of education and women who were subject to the 6-year-primary system completed only 6 years of education. This, however, is not the case in our data as only 10% of women has left school after completing their primary education. As indicated earlier, about 45% of the women in our sample continued their education after the primary education in addition to a similar percentage of the women who either did not enter school or dropped out of school before completing their primary schooling. Therefore, other factors, beyond the length of primary schooling, also determine women' educational attainment in adulthood. Hence, a fuzzy RD design is more appropriate in this case. The fuzzy design differs from the sharp design in that the endogenous variable (edu_i) is not a deterministic function of the length of primary schooling. Instead, the discontinuity in education (edu_i) is modeled similarly to Eq. (1) as follows:

$$\begin{aligned} edu_i = \quad & \beta_1 + \beta_2 D_i + \beta_3 (X_i - c) + \beta_4 D_i * (X_i - c) \\ & + \beta_5 Age_i + \beta_6 Age_i\_squared + \nu_i, \end{aligned} \tag{2}$$

where v_i is the equation error term.

In the fuzzy RD design, the impact of female education on fertility can be computed as the ratio of the reduced form estimate of discontinuity in fertility, α_2, to the reduced form estimate of discontinuity in education, β_2, provided that the same bandwidth (discussed below) is used to estimate Eqs. (1) and (2). Hahn et al. (2001) showed that the ratio of the two RD gaps, α_2/β_2, is numerically equivalent to the Instrumental Variable (IV)[13] estimate, δ_2, from the regression of $fertility_i$ on edu_i,

[12] We also show the results from an additional specification in the supplementary appendix (Table A1), where we control for both women's age and years of survey dummies. Note that only two out of the following three variables can be included in the regression equation: women's date of birth, women's age at the time of the survey, and year-of-survey dummies. The reason is that the excluded variable can be inferred from the other two variables (Bell and Jones 2013). For example, if women's date of birth and age at the time of the survey are both known, then the year of the survey can be easily computed. Therefore, adding the three variables in the regression may result in multicollinearity, which would be problematic in our case to the extent that these variables are correlated with women's education. However, the Variance Inflation Factor from regressing woman's education on these three variables is only 1.02, indicating that the multicollinearity may not pose a serious problem. In fact, our estimates from that additional specification are very close to Eq. (1) above.

[13] We implement the IV analysis using the Generalized Method of Moments estimator (GMM).

using the binary variable D_i as an instrument for education,[14] as follows

$$\begin{aligned} fertility_i = \quad & \delta_1 + \delta_2\, edu_i + \delta_3(X_i - c) + \delta_4 D_i * (X_i - c) \\ & + \delta_5 Age_i + \delta_6\, Age_i\_squared + \varepsilon_{2i}, \end{aligned} \tag{3}$$

$$\begin{aligned} edu_i = \quad & \gamma_1 + \gamma_2\, D_i + \gamma_3(X_i - c) + \gamma_4 D_i * (X_i - c) \\ & + \gamma_5\, Age_i + \gamma_6\, Age_i\_squared + \nu_{2i}. \end{aligned} \tag{4}$$

Following the literature on local RD approach (Hahn et al. 2001), we estimate these equations using kernel-based local linear regressions. This nonparametric setting uses a weighted linear regression with weights computed using a kernel function, such that observations far from the cutoff get smaller weights than observations near the cutoff. The weighting function we use is based on the triangle kernel $K(.) = \max\ \{0,\ 1-|(x-c)/h|\}$ similar to McCrary and Royer (2011), where h is the bandwidth. We used both the plug-in rule method (Imbens and Kalyanaraman 2012) and cross-validation procedure to compute the bandwidth, h. Each of these bandwidth selectors, however, chooses a bandwidth that is quite large (it ranges from $h = 80$ to $h = 100$). We chose a smaller bandwidth of 60 months throughout this study. A bandwidth of 60 months allows us, to some extent, to control for cohort effects by focusing the analysis within a 10-year birth cohort (5 years on each side of the cutoff date). As discussed below, the estimates are, however, quite robust to the bandwidth sizes within a reasonable interval of the chosen bandwidth.[15]

One concern about the previous setup is that the number of children outcomes (the number of children ever born and the ideal number of children) are count variables and hence the local linear model might not fit the data in this case. To account for the nonlinearity of these variables, we estimate local Poisson, more precisely local (nonlinear) exponential, regression models for these outcome variables. The expected outcome, conditional on observed and unobserved heterogeneity components, is specified as

$$\begin{aligned} E(fertility_i|.) = \quad & \exp[\lambda_1 + \lambda_2\, edu_i + \lambda_3(X_i - c) + \lambda_4 D_i * (X_i - c) \\ & + \lambda_5 Age_i + \lambda_6\, Age_i\_squared]\xi_i, \end{aligned} \tag{5}$$

[14] The equality between the IV estimate of δ_2 and the ratio of the two RD gaps, α_2/β_2, requires using the same bandwidth in estimating Eqs. (1)–(4). We follow the recommendation of Imbens and Lemieux (2008) to use the same bandwidth of the outcome equation.

[15] One possible concern with the local linear models is that the forcing variable, the normalized date of birth, is measured in months, which is a discrete variable. Lee and Card (2008) argued that the local linear regression is not suitable in this case as it is not possible to compare outcomes very close to the cutoff as the bandwidth can never be shrunk to zero. According to them, one should use global polynomial models with discrete forcing variables. Lee and Lemieux (2010), however, pointed out that even in the case of continuous variables researchers usually use data far from the cutoff. Therefore, in this paper we estimate local linear regression models following other studies (McCrary and Royer 2011).

where ξ_i is the unobserved heterogeneity component. The error function that is the basis of generalized method of moments estimation can be specified as[16]

$$\begin{aligned}\zeta_i = \; & fertility_i \cdot \exp[-\lambda_1 - \lambda_2\, edu_i - \lambda_3 (X_i - c) - \lambda_4 D_i \\ & *(X_i - c) - \lambda_5 Age_i - \lambda_6\, Age_i\_squared] - 1.\end{aligned} \tag{6}$$

Analogous to the case of local linear RD approach, we employ weighted local exponential mean regression with weights estimated using a triangle kernel function, described above. The variable edu_i is also modeled similar to Eq. (4).

5 Results

5.1 Baseline results

As the first step in our analysis, we estimate the impact of female education on fertility ignoring the endogeneity of education. We estimate Poisson models for the number of children outcomes and a linear model for the outcome of age at child-bearing. In this part of the analysis, we use six waves of the EDHS survey: 1995–2014, excluding the 1992 round because of the absence of the wealth index in this round.[17] The total sample of the six rounds is 88,426 women age between 22 and 49 (the sample for the outcome of age at first birth is 82,571 women; while, the sample for the outcome of the ideal number of children is 77,669 women).

Table 2 provides means of key socioeconomic and demographic variables of this sample. The average number of children born per woman and the average years of education in this sample are very close to the full sample in Table 1 (3.4 children and 6.4 years compared to 3.5 and 6.2, respectively). Women with 7 or more children have as low as 1.38 years of education on average; whereas, women who have only 1 or 2 children have 8.91 years. More than 70% of women with 7 or more children has no formal education, and almost none of them has a college degree. On the other hand, 24% of women with one or two children has a college degree and only 20% has no formal education. Table 2 also shows that women with more children tend to live in rural areas and poor households in comparison to women with fewer children.

Table 3 reports average marginal effects (AMEs) of female education on three fertility outcomes: number of children born per woman, age at childbearing, and the number of children preferred. The first specification (Spec 1) controls for religion (Muslim vs. Christian) which shrinks the sample to 65,959 women because religion is missing in two survey rounds: 2000 and 2003. In the second specification (Spec 2) we exclude religion and run the regression on the same sample as Spec 1. In Specification 3 (Spec 3), we exclude religion and run the regression on the full sample (88,426 women). In all the specifications, we control for age at the time of the survey, age at the

[16] See Mullahy (1997) and Wooldridge (2010) (Section 18.5) for details on GMM estimation of the basic exponential model with endogeneity.

[17] The 1992 round will be included in the RD analysis. However, as explained in result section below, because the RD approach uses only a local sample within a chosen bandwidth. The 1992 round is not included in the local sample, and therefore, the findings from the RD analysis can be compared to the findings in this section.

Table 2 Means of main variables for all women by number of children born

Variables	Childless	1–2	3–4	5–6	7 and above	Full sample	Standard deviation
Current age	30.264	29.908	35.261	38.698	41.867	34.449	7.853
Number of children	0.000	1.649	3.410	5.393	8.266	3.433	2.281
Education years	7.981	8.906	6.833	3.159	1.382	6.436	5.881
Has no education = 1	0.280	0.203	0.328	0.574	0.729	0.483	0.500
Has primary education = 1	0.120	0.110	0.162	0.228	0.222	0.066	0.248
Has secondary education = 1	0.361	0.450	0.372	0.166	0.043	0.328	0.469
Has college and above = 1	0.239	0.237	0.137	0.032	0.005	0.124	0.329
Urban = 1	0.484	0.522	0.469	0.332	0.243	0.442	0.497
Use contraception = 1	0.763	0.940	0.961	0.935	0.875	0.929	0.257
First wealth quintile = 1	0.156	0.126	0.174	0.285	0.395	0.198	0.398
Second wealth quintile = 1	0.169	0.150	0.176	0.244	0.266	0.187	0.390
Third wealth quintile = 1	0.179	0.187	0.191	0.202	0.188	0.190	0.392
Fourth wealth quintile = 1	0.224	0.234	0.212	0.161	0.108	0.201	0.401
Fifth wealth quintile = 1	0.272	0.303	0.247	0.108	0.044	0.224	0.417
Number of observations	5855	27,926	31,874	13,717	9054	88,426	–

EDHS data (1995–2014). This table excludes the 1992 wave because of the absence of the wealth index

Table 3 Average marginal effects of female education on fertility outcomes: Baseline regressions

Variables	Spec 1	Spec 2	Spec 3
(a) Number of children			
Estimate	−0.072***	−0.072***	−0.078***
Standard error	(0.001)	(0.001)	(0.001)
Control for religion	Yes	No	No
Observations	65,959	65,959	88,426
(b) Age at first birth			
Estimate	0.265***	0.266***	0.272***
Standard error	(0.003)	(0.003)	(0.003)
Control for religion	Yes	No	No
Observations	61,499	61,499	82,571
(c) Ideal number of children			
Estimate	−0.018***	−0.018***	−0.019***
Standard error	(0.001)	(0.001)	(0.001)
Control for religion	Yes	No	No
Observations	60,150	60,150	77,669

This table is estimated using the EDHS data (six waves). Women age 22–49. Spec (1) controls for religion. Spec (2) excludes the religion variable and runs the regression on the same sample of Spec 1. Spec (3) excludes religion and runs the regression on the full sample. In all the specifications, we control for age at the time of the survey, age at the time of survey squared, a set of dummies for years of birth, marriage duration, a set of year-of-survey dummies, a dummy variable for contraceptive use, a dummy for the region, and a set of dummies for household wealth quintiles. Heteroskedasticity-robust standard errors are in parentheses. * refers to 90% confidence level, ** refers to 95% confidence level, and *** refers to 99% confidence level

time of survey squared, a set of dummies for years of birth, marital duration, a set of year-of-survey dummies, a dummy variable for contraceptive use, a dummy variable for the region, and a set of dummy variables for household wealth quintiles.

The results show that excluding religion has a slight effect on the results. This can be explained by the fact that almost 95% of the Egyptian population is Muslim. This percentage is almost the same across all the fertility groups described in Table 2, indicating that religion may have a little explanatory power in fertility behavior. We focus our discussion on the results given by Spec 3. Panel (a) in this column shows the results of the effect of education on the number of children born per woman. Other factors held constant, each year of female education reduces the number of children born per woman by 0.08 children. This estimate is statistically significant at 99% confidence level. This result is consistent with previous studies in developing countries that ignored endogeneity.[18] In particular, our estimate is comparable with Balley (1989), Al-Qudsi (1998), Handa (2000), and Bhargava (2007).

[18] The unadjusted data before including regressors is modestly overdispersed with variance-mean ratio of about 1.6. We have tested the model for overdispersion after inclusion of explanatory variables. This overdispersion is eliminated upon inclusion of regressors. We found some evidence of moderate under-dispersion and hence a negative binomial model is not appropriate in this case. We have also estimated a censored Poisson regression model allowing for right censoring in the number of children due to the age of the mother. The results are qualitatively similar to simply including quadratic in age in the regular Poisson regression model.

Panel (b) shows the impact of female education on age at childbearing. As can be seen from the table, each year of female education postpones the maternal age by 0.276 years (3.3 months), other factors held constant. The last panel shows that more educated women prefer to have fewer children than less educated women. In particular, each year of female education reduces the ideal number of children by 0.02 children. As can be seen, the effect of education on the actual number of children is four times larger than the effect of education on women's preferred number of children. These effects are all statistically significant at 99% confidence level.

5.2 Regression discontinuity results

5.2.1 Graphical representation

We start the RD analysis by a graphical representation for years of education and fertility outcomes over the support of the normalized forcing variable ($X_i - c$) (date of birth relative to October 1977). These graphs are shown in Fig. 4. The dots in these figures represent unconditional means outcomes within a one-month bandwidth; whereas, the solid lines represent fitted regression lines from 3rd order global polynomial regressions. Fig. 4 shows that there is a discontinuity in years of education completed at the cutoff. Particularly, there is a decrease in years of education completed by women born after October 1977 who attended 5 years in primary

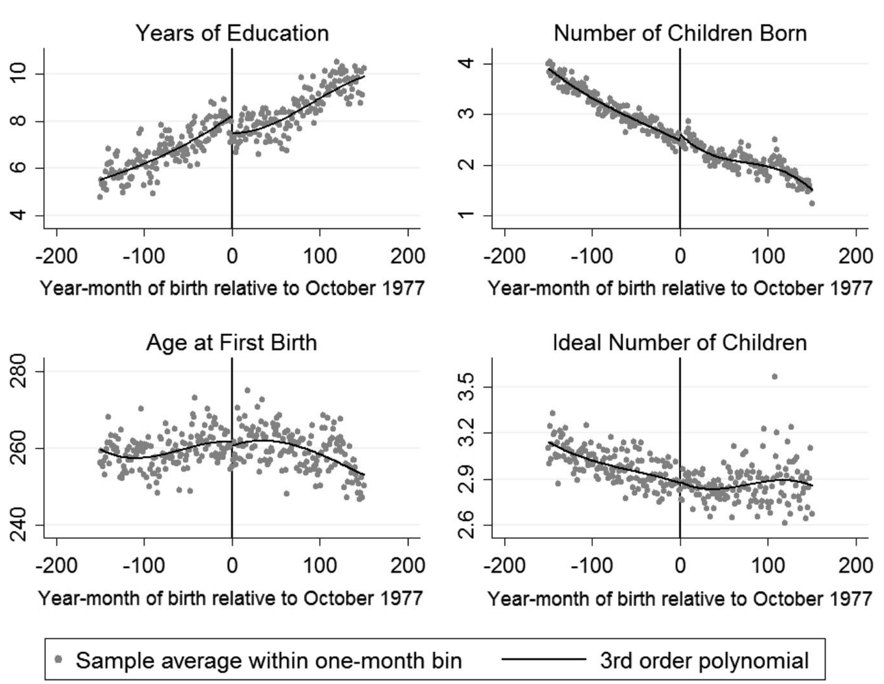

Fig. 4 Discontinuities in years of education and fertility outcomes

schooling compared to women born before October 1977 who attended the 6-year primary system. This figure also shows a discontinuous increase in the number of children born per woman and a discontinuous decrease in the average age at childbearing. Therefore, women who attended less time in primary school and have lower educational attainment in adulthood appear to start their childbearing age earlier and have more children compared to women who attended more time in primary school. Finally, Fig. 4 shows that there does not appear to be a discontinuity in ideal number of children.

5.2.2 Main results from the RD Analysis

This section discusses the estimates from the nonparametric regression discontinuity regressions. We estimate local linear regression model for age at first birth outcome and local exponential mean regression models for number of children outcomes. Table 4 reports the average marginal effects of female education for the three fertility outcomes. As can be seen, the local sample sizes (numbers of women born within 60 months of October 1977) used in identifying the effects include more than 24,700 observations, which is quite reasonable to carry out our RD analysis. We report the results of two specifications. The first specification (Spec 1) in column 1 shows the results of Eqs. (3) and (5), where we control for women's age at the time of the survey and women's age at the time of the survey squared along with the other control variables of the RD design. The second specification (Spec 2) in column 2 is similar to Spec 1 except that we control for year-of-survey dummies instead of women' age.

Panel (a) of Table 4 shows that the estimate of discontinuity in education is negative and statistically significant. In particular, the first specification shows that women who attended five years in primary school have completed, on average, 0.913 less years of schooling than women who attended 6 years in primary school. This estimate slightly declines to 0.891 if we control for year-of-survey dummies instead of age (Spec 2). Panel (b) shows the causal impact of female education on fertility using this exogenous variation in education. In Panel (b.1), the effect of female education on the number of children born per woman is negative and statistically significant across the two specifications, with slight changes in magnitudes. Our preferred specification, shown by Spec 1, shows that each year of female education reduces the number of children born per woman by 0.079 children, other factors held constant. Panel (b.2) shows the impact of female education on age at childbearing. The AME is positive and statistically significant across the two specifications, with also slight changes in magnitudes. In particular, each year of female education increases maternal age by 0.219 years (2.63 months), other factors held constant. The result in Panel (b.3) shows that the increase in female education does not appear to change women' preferences for their ideal number of children. This finding is consistent across all the specifications. Table A1 in the supplementary appendix shows the results of the same set of regressions from an additional specification where we control for both women's age at the time of the survey and a set of year-of-survey dummies, along with the other control variables in Eq. (1). As can be seen from the table, the results from this specification are very close to the findings in Table 4. In particular, the first stage discontinuity in education equal to −0.874. The AME of female education is −0.77 for the number children born outcome, and 2.33 months

Table 4 The effects of female education on fertility: RD results

Outcomes	Spec 1	Spec 2
(a) Discontinuity in education		
Estimate	−0.913	−0.891
Standard error	(0.148)	(0.149)
P-value	[0.000]	[0.000]
Mean	{6.245}	{6.245}
Local Sample	26,673	26,673
(b) Effect of female education		
(b.1) Number of children born		
Estimate	−0.079	−0.081
Standard error	(0.040)	(0.040)
P-value	[0.048]	[0.045]
Mean	{3.500}	{3.500}
Local sample	26,673	26,673
(b.2) Age at first birth		
Estimate	0.219	0.199
Standard error	(0.098)	(0.101)
P-value	[0.025]	[0.049]
Mean	{21.108}	{21.108}
Local sample	24,480	24,480
(b.3) Ideal number of children		
Estimate	0.026	0.025
Standard error	(0.047)	(0.049)
P-value	[0.584]	[0.601]
Mean	{3.027}	{3.027}
Local sample	24,744	24,744

Notes: This table is estimated using the EDHS data (seven waves), women age 22–49. We estimate local linear regression models for Panels (a) and (b.2). Whereas, we estimate local Poisson models for Panels (b.1) and (b.3). In all the models, we use a triangular kernel and a bandwidth of 60 months. In Spec (1), we control for women's age at the time of the survey and women's age at the time of the survey squared. In Spec (2), we control for year-of-survey dummies rather than controlling for women's age. In all the specifications, we control for the standardized dates of birth (X_i−c), an indicator variable for being born at or after the cutoff date (D_i), and the interaction term between this indicator variable and the standardized dates of birth. Standard errors are clustered by primary sampling unit

for the age at first birth outcome. The effect of female education on the ideal number on children remain statistically insignificant.

To make our RD results comparable to the results from the baseline regression in Table 3, we run the RD analysis after excluding the 1992 survey. Consistent with our discussion in footnote 17, excluding the 1992 survey does not affect the RD results. The reason is that our RD analysis uses a 60-month bandwidth, which restricts the regression local sample to women born within five years interval from October 1977 on both sides. That is, the RD local sample includes only women born between October 1972 and October 1982. Women surveyed in 1992 were born before 1970 and hence are not included in the RD local sample. Therefore, the findings from this section can be compared to the baseline results in section 5.1. As can be seen, the effect of female education on the number of children born using the RD analysis is very close to estimated effect from the baseline Poisson in Table 3 (0.079 vs. 0.08). Our estimated effect using the RD analysis, however, is smaller than the effects in recent studies in Arabic countries. In particular, our estimated effect on the number of children born is smaller than the effect found by Lavy and Zablotsky (2015) for Arab citizens of Israel (0.60). As discussed earlier, the latter effect is quite large, which could be possibly explained by the fact that the removal of travel restriction in Palestine has enabled access to many facilities and knowledge that could also contribute to the reduction in fertility among more educated women. Therefore, the large estimated effect of education could in fact be combining the effect of these excluded factors.

6 Robustness checks

This section reports robustness checks to the main results in Table 4. Specifically, we examine whether our findings are sensitive to the choice of bandwidth, test for discontinuities in baseline characteristics, investigate whether husband education biases our RD results, check the sensitivity of our results to the exclusion of women born in 1977, and explore the effects of restricting the analysis to women with at least a primary schooling degree.

6.1 Sensitivity to bandwidth

As indicated earlier, we use a bandwidth of 60 months, which is smaller than the optimum bandwidth suggested by the plug-in and the cross-validation methods. There are still some concerns, however, about the extent to which our results are sensitive to the choice of the bandwidth. Figure 5 displays the 95% confidence intervals for the estimates of Table 4 for a broad range of bandwidths. Panel (a) in Fig. 5 shows that the estimate of discontinuity in education is quite stable over the bandwidth. The magnitude of this estimate ranges from −0.91 to −0.65. Increasing the bandwidth appears to enhance the efficiency of the estimate (through increasing the local sample size), but it does not seem to have a big effect on the magnitude. Panel (b) and panel (d) show the 95% confidence intervals for the estimated AME of female education on the number of children born and the ideal number of children, respectively. Likewise, these graphs show that increasing the bandwidth has a minor impact on the magnitude of the coefficients. As can be seen from panel (b), the efficiency of the estimate of female education on the number of children born is noticeably enhanced at higher bandwidths. Whereas, the effect on the ideal number of children remains statistically insignificant across a broad range of bandwidth. Panel (c) shows the confidence interval for the estimated impact of female education on age at childbearing. This estimated impact ranges from 0.11 years to 0.25 years (1.32 months to 2.99 months), which is statistically significant within a reasonable interval of the chosen bandwidth.

6.2 Discontinuities in baseline characteristics

One of our central assumptions of the RD approach is that women born around the cutoff date share similar baseline characteristics. A violation of this assumption

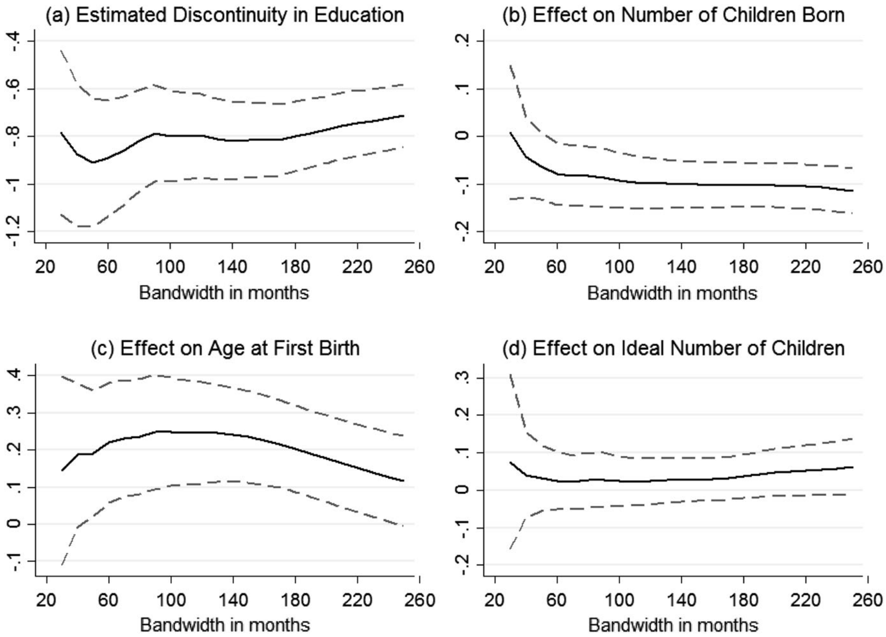

Fig. 5 Ninety-five percent confidence interval for the estimated coefficients in the main analysis. **a** Estimated discontinuity in education. **b** Effect on number of children born. **c** Effect on age at first birth. **d** Effect on ideal number of children

would imply that women near the cutoff did not randomly attend different primary schooling systems, and hence the results of the RD analysis would be biased. As was argued in the introduction, the first school cohort who was subject to the new 5-year primary system was born 11 years before the government announced the change in the system. Thus, it is unlikely that parents of those women would have anticipated this change 11 years before it happened and adjusted their behaviors accordingly.

We test for evidence of non-randomness around the cutoff by estimating the discontinuities in baseline characteristics such as a woman's religion (Muslim or Christian), type of region (urban or rural), her mother's years of education, and her father's years of education. The data is not equally available for each of these variables. The region variable is available for all the women in the sample (97,314 women). It is based on the region of residence rather than the region where a woman obtained her primary schooling. However, the mobility rate in Egypt is quite small,[19]

[19] We used data from the Egypt Population, Housing, and Establishment Census 1996 and 2006 to compute the proportions of internal migrations from rural to urban areas and vice versa. The tabulations are shown in Table A2 in the supplementary appendix. We restrict our computation to individuals of the same age group of 22–49 consistent with the analysis in this paper. The data show that the percentage of rural-urban migration is very small, and varies from 1.6 percent in 1996 to 2.8 percent in 2006. Similarly, the percentage of urban-rural migration ranges from 2.5 percent in 1996 to 1.2 percent in 2006. These percentages are quite small in comparison to developed countries such as the United States. In fact, in the United States, among those who live in a different state than their birth state, roughly 35 percent of the 18–34 years-old have moved across states lines in the last 5 years (averaging across the 1980, 1990, and 2000 censuses) (Molloy et al. 2011).

and women are more likely to move within the same type of region. The religion variable is missing in two survey years: 2000 and 2003, so the sample size reduces to 74,847 women. Both mother's education and father's education contain many missing values, which shrinks the sample size to 5813 women and 3493 women, respectively. Table A3 in the supplementary appendix shows the estimation results from a set of separate local linear regressions in the form of Eq. (1) where each of the baseline covariates represents the dependent variable. If the assumption of the RD holds, these baseline covariates should evolve continuously around the cutoff date and hence the estimates of the discontinuity should not be statistically significant. The results in that table show that there are no significant discontinuities in these baseline covariates.[20]

6.3 Controlling for husband education

It is not a priori clear whether one should control for husband's education in the regression of female education on fertility. Without the inclusion of husband's education, the effect of women's education represents both the direct impact of women's education and the indirect impact of husband's education, which is due to assortative mating effects (Behrman and Rosenzweig 2002; Holmlund et al. 2011). Thus, the inclusion of the husband's education in the regression will exclude assortative mating effects.

The problem with our natural experiment setting is that the change in the length of primary schooling had simultaneous effects on both women and men. Isolating an exogenous variation in female education may not, therefore, be possible. The cultural context in Egypt, however, provides a natural setting for controlling for husband's education and isolating the exogenous variation in women's education. In particular, most men in Egypt marry women younger than they are. This is clearly shown in Table 5, which shows the joint empirical distribution of the lengths of primary schooling systems for both women and their husbands within a 60-month bandwidth. As can be seen, the majority of women in the sample are married to men who attended the old 6-year primary system (older men), regardless of women's types of primary schooling. Thus, in our analysis, we compare women on both sides of the cutoff (6-year primary vs. 5-year primary) where the type of primary schooling attended by husbands is almost the same on both sides. Therefore, the exogenous variation in women's education in our study is not quite associated with a similar exogenous variation in husband' education. In Table A4 in the supplementary appendix we reproduce the outputs of Table 4 where we add husband's education to the set of the control variables. Consistent with our prediction, the results of this analysis show that controlling for husband's education does not change the main findings of Table 4. Specifically, female education reduces the number of children born per woman and increases the age at childbearing with no significant impact on women's preferences toward the number of children.

[20] As pointed out by a reviewer, even if our analysis found no significant differences in observed baseline characteristics between the 6-year and the 5-year primary cohorts, it does not preclude the possibility that some unobserved differences between the two groups may account for part of the differences in their outcomes. This pitfall, common to almost all natural experiments studies, has been emphasized by Dunning (2008).

Table 5 The joint distribution of types of primary schooling systems for women and their husbands within a 60-month bandwidth

	Husbands attended 6 years	Husbands attended 5 years	Total
Women attended	13,961	521	14,482
6 years (%)	(96.4)	(3.6)	(100)
Women attended	10,074	2587	12,661
5 years (%)	(79.57)	(20.43)	(100)

Note: EDHS survey, seven waves (1992–2014). Row percentages are shown in parentheses

6.4 The exclusion of women born in 1977

Our analysis so far has used October 1977 as the cutoff date of birth. Women born before that date are classified among the 6-year primary cohort; whereas, women born after that date are classified among the 5-year primary cohort. Some parents, however, especially whose kids were born between October–December of 1977 might manipulate the system to enroll their children earlier in school rather than waiting a whole year. In this case, women who were born between October–December of 1977 would be misclassified in our analysis among the 5-year primary cohort rather than the 6-year cohort. To explore the potentiality of this measurement error we exclude all women born in 1977 and run our RD regression on the remaining sample. The results of this analysis are shown in Table A5 in the supplementary appendix. As can be seen from this table, our findings are less sensitive to the exclusion of women born in 1977, indicating a small effect of the measurement errors. In particular, the effect of women's education on the number of children born is negative and statistically significant (AME is equal to −0.087). Similarly, the results for the age at first birth and the ideal number of children outcomes are also consistent with our previous findings. In particular, the effect of female education on age at first birth is positive and statistically significant (AME is equal to 2.22 months); whereas, female education does not have a significant impact on ideal number of children.

6.5 Restricting the sample to women with at least a primary school diploma

So far, the RD analysis of this paper has included all women regardless of their education levels. Women born before October 1977 are considered the control group (attended the 6-year primary system); whereas, women born on or after October 1977 represent the treated group (attended the new 5-year primary system). As indicated earlier, a considerable portion of women in the sample has no formal education in addition to women who have dropped out before completing the primary school degree (37 and 10%, respectively). These women had neither attended the 6-year primary system nor did they attend the new five-primary system. They are however assigned to one of these systems based on their dates of birth. The reason we include them in the main analysis, however, is that we want to account for the possibility that the decisions of women (or their parents) not to go to school or drop out of school may have been affected by the length of primary schooling. For instance, parents who dropped their children out of the school or decided not to enroll them in school under the 6-year primary system might have decided differently if they knew that primary school would be five rather than 6 years. The fuzzy RD design we use allows us to extract only the part of variation in women's education that is explained by the length of primary schooling and use this variation to explain fertility outcomes.[21]

Restricting the analysis to women who were directly affected by the policy change (women with at least primary education) might create selection bias as women who stayed 6 years in primary school are expected, on average, to have high ability and motivation compared to women who attended the 5-year system. In other words, a portion of women who attended the 5-year primary system might have not completed their primary school had they faced the 6-year primary system. Adding women with less than primary education level in our analysis is expected to mitigate the effect of this selection bias. To illustrate, women with less than primary school education are, on average, older and thus are more likely to be considered among the 6-year primary cohort. Given that these women usually have low ability and motivation, adding them to the 6-year primary cohort is expected to reduce the average ability level of this cohort, which might balance the average ability between the 6-year and the 5-year primary cohorts. Furthermore, we believe that restricting our sample to women whose educational attainment is above the average level in Egypt (primary education) might cast some doubts about the external validity of our results. In particular, the culture context is expected to be correlated with both education and fertility. Excluding women with low education levels, where cultural barriers are expected to be much stronger, will cast some doubts about our ability to generalize our results to the whole society.

To explore the extent of the selection bias, we run our RD analysis in this section on women who had completed at least a primary school. This restricts the total sample to 49,283 women. These are the women who had actually faced one of the primary schooling systems. We re-estimate the average marginal effects of Table 4 in Table 6 using the restricted sample. Figure A2 in the supplementary appendix shows the discontinuities in years of education and fertility outcomes. We also draw the AME across a wide range of bandwidths in Figure A3 in that appendix. As can be seen from Table 6, restricting the sample to women with at least primary school diploma increases the exogenous variation in female education from 0.913 to 1.49 (using Spec 1). Figure A2 in the appendix shows that the exogenous variation is much higher for cohorts born close to the cutoff (small bandwidths), and it decreases for cohorts born far from the cutoff (bigger bandwidths). The exogenous variation in female education, however, remains above one even at large bandwidths.

Increasing the extracted exogenous variation in female education has not quite changed the main results in Table 4. In fact, the effect of female education on the number of children born is negative and statistically significant. The magnitude of the AME is slightly smaller than what we found in the full sample (0.061 compared to

[21] The downside from adding these women in the analysis, however, is that it tends to underestimate the exogenous variation in women's education. To illustrate, women with no formal schooling are, on average, older and thus are more likely to be considered among the 6-year primary cohort. Therefore, a considerable portion of women with no education is counted among the group who attended one extra year of primary schooling. This apparently underestimates the impact of that extra year of primary schooling on educational attainment.

Table 6 The effects of female education on fertility: RD Results for the restricted sample (cont'd)

Outcomes	Spec 1	Spec 2
(a) Discontinuity in education		
Estimate	−1.490	−1.478
Standard error	(0.097)	(0.098)
P-value	[0.000]	[0.000]
Mean	{11.497}	{11.497}
Local Sample	17,269	17,269
(b) Effect of female education		
(b.1) Number of children born		
Estimate	−0.061	−0.060
Standard error	(0.028)	(0.028)
P-value	[0.030]	[0.034]
Mean	{2.568}	{2.568}
Local sample	17,269	17,269
(b.2) Age at first birth		
Estimate	0.287	0.278
Standard error.	(0.072)	(0.073)
P-value	[0.000]	[0.000]
Mean	{28.402}	{28.402}
Local sample	15,716	15,716
(b.3) Ideal number of children		
Estimate	−0.010	−0.009
Standard error	(0.024)	(0.024)
P-value	[0.685]	[0.695]
Mean	{2.803}	{2.803}
Local sample	16,379	16,379

Notes: This table is estimated using the EDHS data (for only women with at least a primary diploma) (seven waves), women age 22–49. We estimate local linear regression models for Panels (a) and (b.2). Whereas, we estimate local Poisson models for Panels (b.1) and (b.3). In all the models, we use a triangular kernel and a bandwidth of 60 months. In Spec (1) we control for women's age at the time of the survey and women's age at the time of the survey squared. In Spec (2) we control for year-or-survey dummies. In all the specifications, we control for the standardized dates of birth (X_i−c), an indicator variable for being born at or after the cutoff date (D_i), and the interaction term between this indicator variable and the standardized dates of birth. Standard errors are clustered by primary sampling unit

0.079). Figure A3 in the appendix shows that this AME remains statistically significant and quite stable across a wide range of bandwidths. Likewise, consistent with the findings from the full sample in Table 4, female education has no effect on women's preferences for the ideal number of children. Finally, the impact of female education on age at childbearing in that restricted sample is quite bigger than the findings in Table 4, (3.44 months vs. 2.6 months in the full sample). Figure A3 in the appendix shows that the estimated effect remains statistically significant across a very broad range of bandwidths.

7 Explaining the effect of female education on fertility

The findings of this paper show that increasing female education reduces the actual number of children born per woman with no significant impacts on women's fertility preferences. This finding gives rise to the possibility that female education reduces the number of children through postponing maternal age, rather than changing women's preferences and attitudes. In fact, we found that each year of female education delays maternal age by 2.6 to 3.4 months. This section further explores the reasons for postponing childbearing.

The delay of maternal age does not appear to result from enhancing women's job opportunities or affecting their usages of contraceptive methods, as suggested in the literature (Becker 1960, 1993; Becker and Lewis 1973). As discussed earlier, female labor force participation in Egypt has been historically low, as can be seen from Fig. 6, despite the remarkable increase in female educational attainment overtime (Fig. 1).[22] Furthermore, the usage of contraceptive methods in Egypt has been historically high since the government has expanded family planning programs and publicity campaigns to curtail population growth in the early 1990s. Figure 6 shows that the percentage of women using or intending to use contraceptive methods in the EDHS data remained above 80% during the period (1992–2014). To support this descriptive analysis, we run our RD analysis to investigate the effect of female education on the probability of work and the probability of using contraceptive methods.[23] The results are shown in Table 7. As can be seen from the first two columns, the increase in female education has no significant impacts on the probability of work or the probability of using contraceptive methods, respectively.

This finding raises the question as to what might have caused the delay in maternal age if both the probability of work and the probability of using contraceptive methods have not been affected. The delay of maternal age appears to result from an additional channel, which has not been quite emphasized in the theoretical literature. In particular, the increase in female education has led to a delay of marriage which resulted in a delay in maternal age.[24] As can be seen from the third column of Table 7, each year of female education delays marriage by 0.348 years (4.2 months).

[22] Explaining the low levels of female labor force participation in Egypt is out of the scope of this study. Assaad and Krafft (2013), Hendy (2015), and Sieverding (2012) provide a discussion of this issue. In particular, the authors highlight factors related to the supply of female labor such as family circumstances, women's preferences, and reservation wages, as well as factors related to labor demand such as shrinking public sector and discrimination in the private sector.

[23] We conduct the fuzzy RD analysis for these two outcomes using local Probit models, where each of these outcomes represent the dependent variable in Eq. (1).

[24] Other studies have also documented that more-educated women generally marry later than their less-educated counterparts in Arab countries (Rashad et al. 2005). While this could be partially explained by the fact that more educated women stay longer in the school (Black et al. 2008), a considerable portion of educated women in Egypt remains unmarried after leaving school, and some of them do not marry at all. Some studies indicated that returns in the marriage market, rather than labor market, provide a strong incentive for girls' schooling in Egypt (Lloyd et al. 2003; Mensch et al. 2003), in that more educated women are expected to marry highly educated and wealthy men. This increase in women's expectations about their future husbands combined with higher poverty levels in society and increasing the cost of marriage, which is born mostly by grooms, have all contributed to increasing age at marriage among more educated women.

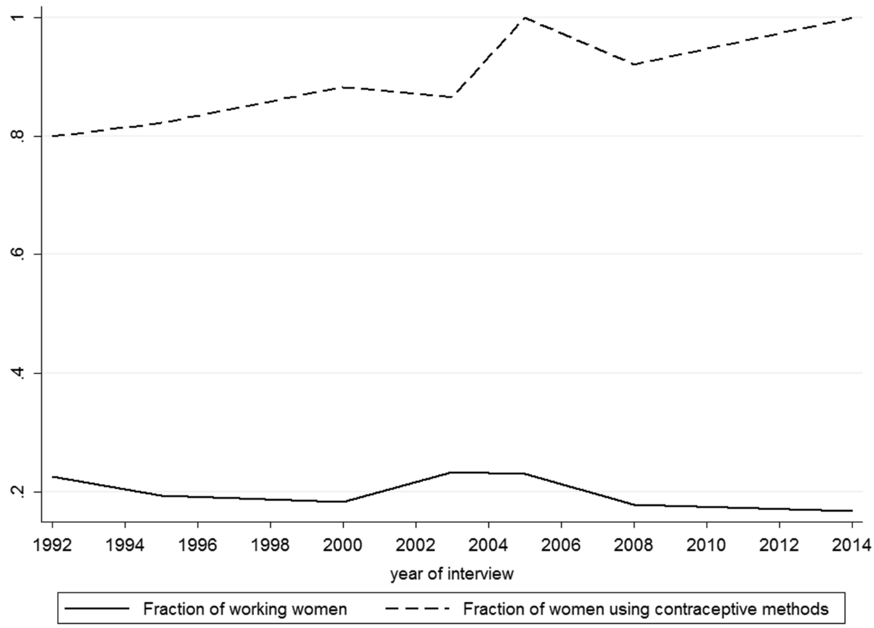

Fig. 6 Female labor force participation and contraceptive usage over time

This result is statistically significant at 99 confidence level. Given the nature of the Egyptian society, where out-of-wedlock births are socially prohibited and penalized by the law, only married women are allowed to have children. Thus, postponing marriage leads to postponing women's age at first birth (maternal age). In fact, the simple correlation coefficient between age at first marriage and age at first birth in our data is 0.9.

Our findings about the mechanisms through which female education reduces fertility in Egypt is consistent with some of the previous empirical studies in developing countries. In particular, our results are very similar to Tequame and Tirivayi (2015) findings in Ethiopia. Both our study and their study found that postponement of marriage and motherhood are the key mechanisms through which education reduces fertility, and that female labor force participation and contraceptive use do not play significant roles in this relationship. Our results are also consistent with Dinçer et al. (2014) who found that that the delay in marriage and childbearing rather than changing women's attitudes are responsible for the reduction in fertility among more educated women in Turkey. Our results also agree with Lavy and Zablotsky (2015)'s finding for the Arab citizens of Israel that women's labor force participation is not a mechanism for explaining the relationship between female education and fertility.

8 Conclusion

Does educating young girls reduce rapid population growth in Arab countries? The weak association between female education and the participation of women in the

Table 7 The effects of female education on intermediate outcomes: RD results

	Pr(work = 1)	Pr(use contraception = 1)	Age at first marriage
Woman education	0.010	0.002	0.348
Standard error	(0.010)	(0.005)	(0.098)
P-value	[0.348]	[0.746]	[0.000]
Mean	{0.197}	{0.917}	{19.525}
Local sample	26,640	26,640	26,640

Notes: This table is estimated using the EDHS data (seven waves), women age 22–49. We estimate local Probit models for the first two outcomes and a local linear model for the third outcome. All the regressions use a triangular kernel weighting function within a bandwidth of 60 months. In all the regressions, we control for women's age at the time of the survey, women's age at the time of the survey squared, the standardized dates of birth (X_i−c), an indicator variable for being born at or after the cutoff date (D_i), and the interaction term between this indicator variable and the standardized dates of birth. Standard errors are clustered by primary sampling unit

workforce in these countries casts doubts on a direct causal link between female education and fertility. Several empirical studies have examined this relationship but have mostly faced difficulties in addressing the endogeneity of female education

Our paper provide causal evidence on the impact of female education on fertility in an Arab country. In particular, we use data from the Egyptian Demographic and Health Survey (1992–2014) to examine the effect of female education on three fertility outcomes: the actual number of children born per woman, the preferred number of children per woman, and the age of women at first birth. We use the change in the length of primary schooling in Egypt in 1988, which reduced the length of primary education from 6 to 5 years, to create an exogenous variation in female education. The first cohort who was subject to this policy change was born on or after October 1977. We implement a nonparametric regression discontinuity design to compare adulthood educational attainment and fertility outcomes of women born just before and right after October 1977.

Our analysis shows that women who attended the 5-year primary system have completed, on average, 1 less year of schooling in adulthood as compared to women who attended the 6-year primary system. Using this exogenous variation in education, our RD results show that each year of female education reduces the number of children born per woman by 0.079 children. That is, a woman with a high school diploma (12 years of education) has about one less child than a woman with no formal education.

We also explore whether the estimated effect of education on fertility reflects a change in women's preferences towards the optimum number of children. Our analysis show that the increase in female education did not significantly change women's fertility preferences. We find, however, that the increase in female education has increased women's ages at their first birth. In particular, each year of female education postpones maternal age by 2.63 to 3.44 months. Thus, our findings suggest that the reduction in the number of children born per women among more educated women results from postponing maternal age rather than changing women's attitudes and preferences. We also provide evidence that the delay of maternal age results from delaying marriage rather than increasing women's labor force participation or affecting their usages of contraceptive methods. The results of this paper are quite robust to several robustness checks and sample restrictions, including varying the bandwidth, including husband education in the regression equation, excluding women born in 1977, and restricting the analysis to only women who were directly influenced by the policy change.

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Compliance with ethical standards

Conflict of interest The authors declare that they have no competing interests.

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