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Effect of girls' secondary school stipend on completed schooling, age at marriage, and age at first birth

Evidence from Bangladesh

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Abstract: There are many studies on the effects of conditional cash transfer programmes on enrolment, productivity and poverty reduction but very few on causal effects on ages at marriage and first birth. And none of them considers the convergence effect. This paper provides new evidence on effects of the Female Secondary Stipend Programme in Bangladesh as an exogenous variation in time and region on schooling, ages at marriage and first birth outcomes with regression discontinuity and difference-in-difference approaches. The regression discontinuity results show that the programme increased completed years of schooling by at least 0.4 years, and delayed age at first marriage and age at first birth by at least 0.4 and 0.3 years respectively. We also show that the difference-in-difference method predicts biased results.

Keywords: secondary stipend, rural girls, schooling, age at marriage, age at first birth, regression discontinuity, difference-in-difference
JEL classification: D04, I21, I25, J12

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1 Introduction

Early marriage is still a concern for many developing countries. For girls particularly, it is associated with less schooling, lower social status and higher rates of maternal mortality and domestic violence (Field and Ambrus 2008). Also, early marriage is often associated with early child bearing as there exists strong social sanctions, especially in South Asia, against out-of-wedlock childbearing (Caldwell 2005). And early childbearing[1] is associated with maternal mortality, lower birth weight and higher rates of infant mortality. It also adversely affects women's economic conditions such as the level of schooling, labour market participation and earnings (Black et al. 2008). The education of girls and young women is considered to be a highly effective means to increase marriage age and delay age at first birth, and is expected to lead to women's empowerment, better child health and nutrition, and a reduced rate of total fertility (Adhikari and Sawangdee 2011; Breierova and Duflo 2004; Osili and Long 2008; Schultz 2002). Conditional cash transfer (CCT) programmes have been applied in many developing countries as supply side incentives to increase school enrolment, reduce poverty and delay the age of marriage under the system of social protection for disadvantaged and vulnerable groups. If implemented well, these programmes are considered to have impact on human capital formation by increasing households' current investment (Ferre and Sharif 2014). Investment in education, firstly, raises the permanent income of households through earnings; it provides the opportunity to reduce the optimal fertility choices. Secondly, education provides individuals with knowledge and information on better health (McCrary and Royer 2011). CCTs conditional on schooling, health, etc. are supposed to have direct income effects and other price effects. They also increase the bargaining power of the beneficiaries' receiving cash transfers. As the CCTs increase households' income, this increased income should lead to consumption demand for conditional schooling.

The Female Secondary Stipend Programme (FSSP), introduced in Bangladesh in 1994, was one of the pioneers of such programmes. Since one of the conditions for receiving the cash transfer under the FSSP is to remain unmarried, the hypothesis is that households receiving the transfer would invest more to send girls to school, thereby postponing girls' marriage which would also delay the age for first birth. The expectations are that this will encourage more schooling and better fit for employability, etc. As an indirect effect, education is supposed to improve awareness of negative impacts of early marriage and early birth among beneficiaries as it increases girls' ability to absorb and process information (McCrary and Royer 2011).

Khandker et al. (2003) found that the FSSP in Bangladesh increased girls' enrolment in grades 6 to 8 by 8 to 12 per cent. Later on, Brazil, Mexico, Nicaragua, Paraguay and Pakistan initiated similar interventions. Evaluations of these programmes include studies on both the immediate and long-term effects on time spent on schooling and work, enrolments, visits to health centres, gender gap, divorce and women's labour market participation (Attanasio et al. 2011; Bobonis 2011; Baird et al. 2009; Fuwa 2001; Khandker et al. 2003; Shamsuddin 2015; Schurmann 2009; Schultz 2004; Ullah 2013).

Several studies have analysed the immediate effects of the Mexican *Progresa* conditional cash transfer programme that gave educational grants on school enrolment and poverty reduction among other outcomes (Attanasio et al. 2011; Schultz 2004). Some studies have focused on the long-term effects of conditional cash transfers. For example, Shamsuddin (2015) studied the FSSP's impact on women's empowerment through labour market participation in Bangladesh.

[1] In general, literature defines birth age less or equal to eighteen as early child bearing or adolescent birth. Some literature even defines the age before twenty as early age for childbearing (Maitra and Pal 2007).

Amongst the huge number of studies on the impact of CCTs, very few have focused on the effects of stipend programmes on age at marriage and age at first birth. Alam et al. (2011) found that the girls who participated in the Punjab Female School Stipend Programme in Pakistan delayed marriage. Ullah (2013) estimated the impact of the FSSP on early marriage incidence by applying propensity score matching (PSM). He used Household Income and Expenditure Survey (HIES) data for Bangladesh, with questions on marital status, education level, enrolment and whether or not a stipend was received, among others. However, since remaining unmarried is one of the conditions for obtaining a stipend, the HIES data source might not reveal true information about marital status. In a parallel study, Hahn et al. (2015) showed that the FSSP had a significant impact on education, age at marriage and fertility, by applying a difference-in-difference (DiD) approach. While studying the effect on fertility, the result might be biased when the study includes respondents who have not finished their fertility phase yet. Therefore, I chose to study the effect on age at first birth.

While studying the programme effect, the existing literature, however, suggests a need for caution in interpreting the relations as causal. Negative/positive associations might arise due to omitted variables; schooling and cash transfer programmes may not always be randomly placed (Osili and Long 2008). When early marriage is one of the conditions for not receiving a stipend, endogeneity issues might arise. Moreover, if there is a tendency for convergence between the treatment and control groups over time, an approach that does not control for this might estimate a biased treatment effect.

Identifying and evaluating the causal effects of a programme requires valid counterfactuals. This study, by using the FSSP as an exogenous variation in female secondary education, provides new evidence on whether girls' schooling causes a delay in the marriage and first birth ages.[2] We utilized the pooled cross-sectional Bangladesh Demographic and Health Survey (BDHS) data for 1993–94, 1999–2000 and 2011, from which it was possible to retrieve information about the treatment group (girls living in rural regions) and the control group (girls living in urban regions). Our study is based on the regression discontinuity (RD) approach, which is a study design that shares some features with randomized controlled trials (RCTs) (Lee and Lemieux 2010) and can be used to capture the causal effect of a treatment at the local level. Also, as in previous studies, a quasi-experimental fixed-effect DiD approach was applied. It is commonly argued that the full impact of education—such as delayed marriage, delayed first birth, lower fertility, lower child mortality, increased employment, increased mobility, etc.—can take time to manifest and may only be demonstrated after a generation. In this paper, we focus on the short-run effects of schooling/CCT on teenage marital and giving-birth decisions identified through variations in exposure by region and time. The programme could have additional positive welfare effects not illustrated here. However, as will be illustrated by this study, if there is convergence between treatment and control groups over time due to other factors, estimation by the DiD approach might produce biased results compared to the RD design.

The main contribution of this paper is that we show that the RD design predicts a different causal effect of the FSSP on girls' years of completed schooling, ages at first marriage and first birth compared to the DiD approach. This suggests that the estimation result predicted by DiD might be biased when it does not control for time-variant convergence effects. The RD study indicates that the programme resulted in an increase of approximately 0.4 years of completed schooling for eligible girls living in the programme regions. Age at first marriage is estimated to have increased by at least 0.4 years and age at first birth increased by 0.3 years. The DiD

[2] Our approach of using an exogenous variation is similar to approaches followed by Duflo (2001) and Osili and Long (2008).

approach estimated completed schooling to have increased by 1.1 years, age at first marriage by 0.4 years and age at first birth by 0.2 years for the eligible girls fully exposed to the treatment.

The outline of the study is as follows. Section 2 gives some background on early marriage and age at first birth incidences in Bangladesh and the FSSP. Section 3 describes data and identification strategies. Section 4 discusses the theory and methods that were chosen for the study. Section 5 presents the estimation results, including some sensitivity tests. Sections 6 and 7 discuss the results and make some concluding remarks.

2 Early marriage incidence and the Female Secondary Stipend Programme in Bangladesh

Early marriage prevalence is very high in Bangladesh. It had the world's second highest percentage of adolescent brides in the year 2004, with 68.4 per cent of girls married by the time they reached the age of 18 (Schuler et al. 2006). Another report (National Institute of Population Research and Training et al. 2009) revealed that, out of 3,019 women aged 15–19 who were surveyed, 45.6 per cent were married. Among them, 21.1 per cent had experienced their first marriage at the age of 15. The National Institute of Population Research and Training et al. (2013) reported that 44.7 per cent of the women in the 15–19 age group were married. Mahmud and Amin (2006) confirmed that the norm of early marriage had not changed dramatically and that girls were withdrawn from secondary school to marry. In Bangladesh, the legal age for marriage is 18 for girls and 21 for boys, and there is a law against child marriage. However, the law is not often practised in reality. Mahmud and Amin (2006) and Ullah (2013) found that a significant percentage of girls from non-poor households dropped out of secondary school to get married, implying that early marriage is still a social norm in Bangladesh.

The parents' decision to marry their daughters off at a young age is often dominated by the family's economic condition. Also, younger girls have a higher demand (attracting higher-status husbands) in the marriage market, which reduces the dowry burden (even though dowries are currently illegal). Girls who are expected to pay a dowry drop out of school at a higher rate than girls who are not expected to do so (Mahmud and Amin 2006). Higher education of girls can cause the price to increase, as it is very much the norm in Bangladesh for husbands to be better educated than their wives. This gives parents less incentive to invest in their daughters' schooling before marriage.

Girls are seen as vulnerable to men's predations when they reach menarche, and parents feel safer if they marry them off early (Schuler et al. 2006). A report by the ICDDR (2007) stated that parents felt their daughters would be safer from sexual abuse or illicit sexual contact if they were married. Another reason mentioned for early marriage was the wish to acquire a daughter-in-law who could be shaped by her in-laws before she became too independent and developed her own views or ways of doing things. However, nowadays, education can also be seen as a way to raise girls' status in the marriage market. Parents may therefore keep their daughters in school up to a certain grade as an investment to secure a good marriage (Mahmud and Amin 2006). Marriage and early birth are therefore two of the reasons for girls' high drop-out rate.

In these circumstances, the government of Bangladesh has taken many initiatives to reduce the rate of early marriage. One such initiative was the FSSP. The intervention was inspired by population literature suggesting that the education of girls would delay marriage and increase contraceptive use, thereby reducing age at first birth and fertility. The programme was supposed to cover as much as 50 per cent of the cost of textbooks, uniforms, stationery, transportation, exam fees and other direct educational expenses of unmarried rural girls.

The short-term objectives were to increase enrolment among female students in grades 6–10, to help them to pass their Secondary School Certificate examination so that they would qualify for employment as primary school teachers, agricultural extension agents, health and family planning workers and non-governmental organization (NGO) field workers, to keep them in their studies, and to discourage them from marrying early. The long-term objectives were to enlarge the number of educated women capable of participating in economic and social development of Bangladesh, to reduce the gender gap, to create a positive impact on population growth and to provide occupational skills training to school-leaving girls interested in entering the labour market as self-employed workers, semi-skilled and skilled workers (Raynor and Wesson 2006).

2.1 Background of the FSSP

The Bangladesh Association for Community Education (BACE), a national NGO, started the FSSP in the year 1982 with USAID financial assistance, under the supervision of the Asia Foundation at a local level (in a single *upazila*, or sub-district). A second *upazila* was included in 1984 and several more subsequently, totalling seven by 1992. In 1990, girls' secondary education was made free up to grade 8 nationwide. The pilot FSSP yielded increased secondary enrolment by girls, from an average of 7.9 per cent to 14 per cent in some project areas, and drop-out rates fell from 14.7 to 3.5 per cent (Raynor and Wesson 2006). Based on the success of the abovementioned pilot projects, in January 1994 the Bangladesh government launched a nationwide stipend programme for girls in secondary school (grades 6–10), covering 70 per cent of all 460 *upazilas*, and 57 out of 64 districts. Presently, stipends are also provided to those in grades 11 and 12.

The nationwide FSSP instigated in 1994 was initially planned to last for five years, funded by the government and various donors, including the World Bank, the Asian Development Bank and the government of Norway (Schurmann 2009). Under the programme all girls in programme areas who enter secondary school (about 50 per cent of possible enrolments) are eligible for a monthly sum ranging from *taka* 25 in grade 6 to *taka* 60 in grade 10 (equivalent to US$0.37–$0.88 in July 2006). Girls receive additional payments in grade 9 for new books and in grade 10 for exam fees. Conditions for eligibility include a minimum 75 per cent attendance rate, a score of at least 45 per cent in the annual school exams, and remaining unmarried until sitting the Secondary School Certificate or turning 18. These three criteria have remained constant during the lifetime of the FSSP (Raynor and Wesson 2006). The stipend is paid directly to an account in the girl's name in the nearest Agrani bank, a state agricultural bank with branches all over rural Bangladesh (Khandker et al. 2003). For each stipend student, the schools also receive a tuition subsidy from the project so that the recipient is not required to pay any tuition (Fuwa 2001). Moreover, these stipend programmes have been complemented with other components such as curriculum reforms and the development of instructional materials, teacher training, the recruitment of female teachers, the improvement of school infrastructure, awareness programmes at the community level, and the building of institutional capacity. These additional project components differ depending on the funding agencies, however (Fuwa 2001).

3 Data

To study the impact of the FSSP, the BDHS data is the ideal source. From the dataset, it is possible to retrieve information on the women who were not exposed to the FSSP (one of the control groups). Datasets include information on the women's age, place of residence, childhood place of residence, marital status, education, occupation, age at first birth, fertility, child mortality, child nutrition status, maternal and child health, household socioeconomic characteristics, access to health care services, access to water, sanitation and electricity, and knowledge and attitudes regarding HIV/AIDS. We combined repeated cross-sectional BDHS data for the waves of 1993–94, 1999–2000 and 2011. The combined dataset gave us 23,491 observations of women born between 1965 and 1990, and aged 21 to 46, but only the cohorts aged between 21 and 31 in the year 2011 were exposed to the secondary stipend, since the intervention was implemented in 1994. The dependent variables are years of completed education and age at first marriage. We also created a variable for early marriages, indicating those women whose age at first marriage was lower than 18.[3] The descriptive statistics in Table 1 show that 80 per cent of the girls were married by age 17 and had completed an average of 4.12 years of schooling.

Table 1: Descriptive statistics

Variables	Description	Observations	Mean	Std. Dev.	Min	Max
Age2011	Respondents' age in 2011	23,491	35.04	7.05	21	46
Education	Completed years of schooling	23,491	4.12	4.06	0	18
AgeFm	Age at first marriage	23,491	15.39	2.96	6	45
AgeFB	Age at first birth	21,381	17.64	3.13	10	40
EM	Married before age 18	23,491	0.80	0.40	0	1
Treat	Women born after 1979	23,491	0.33	0.47	0	1
Rural	Respondents lived in programme areas or not	23,491	0.70	0.46	0	1
Muslim	Respondents’ religion Islam or other	23,491	0.88	0.32	0	1
Asset	If households own some assets	23,491	0.54	0.50	0	1
Electricity	Household has electricity	23,491	0.49	0.50	0	1
Division	Country is divided into divisions	23,491	3.68	1.72	1	7

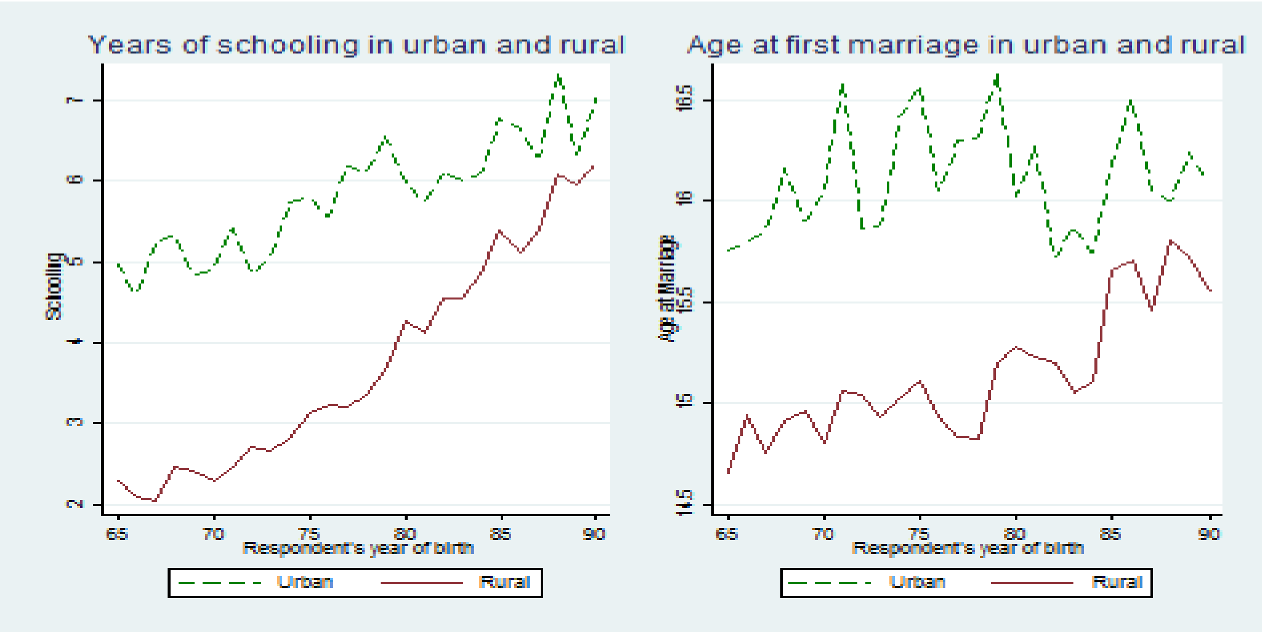
Source: Author’s analysis based on BDHS dataset.

Treat is the treatment variable that takes value 1 for girls born in and after 1979 and 0 for girls born before 1979. Similarly, variable rural takes value 1 for the respondents living in rural and 0 otherwise, and variable Muslim also takes value 1 for the girls being Muslim and 0 for other religions.

Figure 1a shows the trend in average years of completed schooling for urban and rural girls in different birth cohorts. The total years of education show upward trends for women from the younger birth cohorts in both the urban and rural samples. The mean age at first marriage varies between 14 and 17. For rural women, age at first marriage increases from around 14 for the cohort born in 1965 to approximately 16 for the cohort born in 1990. Figure 1b shows a slightly downward trend in age at first birth for both the urban and rural girls. From the visual inspection of both Figures 1a and 1b we see that the gap between urban and rural girls decreases in terms of schooling, marriage and birth-giving ages, indicating that the FSSP have had some impact.

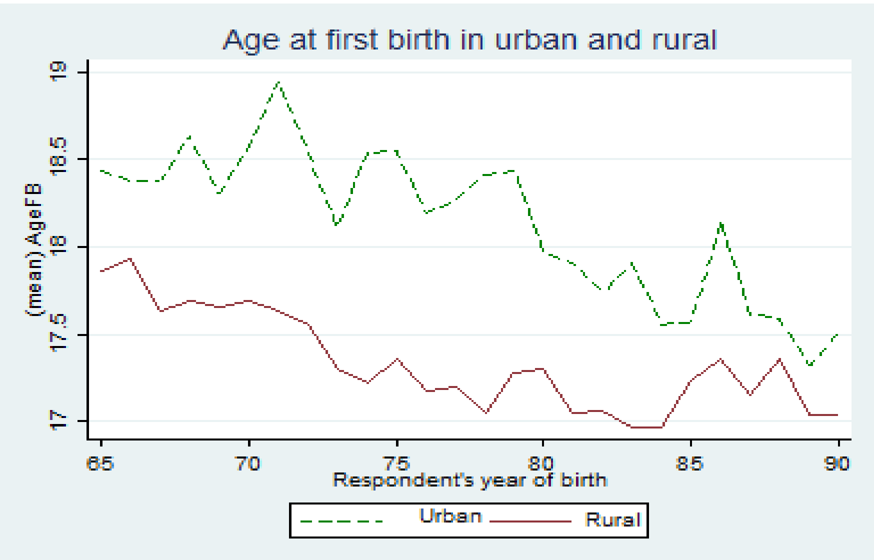
[3] The minimum legal age for marriage for a girl is 18.

Figure 1a: Average years of completed schooling and average age at first marriage for different birth cohorts



Source: Author's analysis based on BDHS dataset.

Figure 1b: Average age at first birth for different birth cohorts



Source: Author's analysis based on BDHS dataset.

3.1 Identification strategy

To evaluate whether the FSSP caused an increase in educational attainment, age at first marriage and age at first birth for the treatment group relative to the control group requires the identification of the treatment and valid counterfactual groups. The women's date of birth, place of childhood residence and level of education jointly identify their exposure to the Female

Secondary Stipend Programme.[4] The target group for the FSSP was girls enrolled in grades 6–10 in rural Bangladesh. The cohorts of girls eligible to attend secondary school since 1994 are considered the treatment group. The cohorts of women of corresponding ages before the introduction of the FSSP in 1994 are used as the control. Primary school officially starts at age 6 and lasts for five years in Bangladesh. We assume that most girls start secondary school at the age of 11, and secondary school also lasts for five years. However, in 1994, only girls enrolled in grades 6 and 9 received the stipend; in 1995 girls enrolled in all grades except grade 8 did so, and since 1996 girls enrolled in all grades have received the stipend (Shamsuddin 2015). Therefore, women born in 1980 or later are assumed to have received the stipend for at least one year of their secondary education.

To capture the causal effect by applying the DiD approach, we created two treatment groups and one control group as follows:

Treatment Group 1: Girls born between 1980 and 1982 and living in a rural area.

Treatment Group 2: Girls born between 1983 and 1990 and living in a rural area.

Control Group: Girls born between 1965 and 1979 and living in either a rural or an urban area, and girls born since 1980 but living in an urban area.

Treatment Group 1 was partially exposed to the programme. Treatment Group 2 is assumed to have been fully exposed to the programme for all five years of their secondary education. Accordingly, we constructed one counterfactual control group. Girls born in the year 1979 or earlier, and living in either urban or rural areas, are assumed not to have been exposed to the FSSP, and can therefore be considered valid counterfactuals. Even though the difference in time-invariant changes between periods might be captured by cohort fixed effects, to minimize the time-variant changes, we have excluded girls born before 1965.

Again, for the RD design, girls born since 1980 are taken to be the treatment group and girls born before 1980 and living in rural areas a valid control group.[5] The details of the RD design are explained later, in Section 4.4.

[4] This identification strategy follows strategies applied by Duflo (2001) and Osili and Long (2008).

[5] There are two potential problems associated with our identification strategy. Firstly, even though the official age of starting secondary school is 11 (which is our threshold age), the data show a prevalence of both under-age and over-age enrolment (UNESCO Institute for Statistics 2013), with 5.6 per cent of girls aged 9, 19. 4 per cent of girls aged 10, 23.1 per cent of girls aged 12, 10.1 per cent of girls aged 13 and 7.5 per cent of girls aged between 14 and 17 enrolled in grade 6 in the year 2011. Due to under-age enrolment, there are some girls in Treatment Group 1 who might have missed out on the stipend. Similarly, due to over-age enrolment, some girls in the control group might have received the treatment, at least for one year. Therefore, on average, the treatment might produce a downward-biased effect. Secondly, migration between urban and rural areas might produce biased results. However, the Bangladesh Population and Housing Census 2011 reports that the migration rate from rural to urban areas was 4.29 per cent while that from urban to rural areas was 0.36 per cent.

4 Method

4.1 Theory and assumptions

To draw a causal inference about a programme's impact requires the answering of a counterfactual question such as 'how would individuals who participated in a programme have fared in the absence of the programme? How those who were not exposed to the programme would have fared in the presence of the programme?' (Duflo et al. 2008: 3,899). Since we cannot observe the outcomes for the same individual had they been treated and non-treated, we must estimate the average treatment effect of a programme by comparing a group of individuals being exposed to the programme to another group of individuals who were not exposed. To elaborate,[6] we denote by Y_i^T the outcome for a particular girl, had she received a stipend and by Y_i^C the outcome of the same girl had she not received a stipend. Further, we define Y_i as the outcome that is actually observed for each girl i. As we will not observe both Y_i^T and Y_i^C at the same time, we will not be able to estimate the effect of treatment (FSSP) individually. However, we will be able to estimate the average treatment effect of the stipend on a girl's education and age at marriage over an entire population:

$$E\left[Y_i^T - Y_i^C\right]$$

If we have access to data for girls with treatment status and for girls without treatment status, one approach is to take the average of each group and estimate the difference in the outcome variables. In a large sample, this will converge to

$$\mathrm{D} = \mathrm{E}[Y_i^T|FSSP\,] - \mathrm{E}\left[Y_i^C|Non - FSSP\right] \quad = \mathrm{E}[Y_i^T|T] - \mathrm{E}\left[Y_i^C|C\right]$$

Subtracting and adding $E\left[Y_i^C|T\right]$, i.e., the expected outcome for a subject in the treatment group had she not been treated, we obtain

$$\mathrm{D} = \mathrm{E}[Y_i^T|T] - \mathrm{E}\left[Y_i^C|T\right] - \mathrm{E}\left[Y_i^C|C\right] + \mathrm{E}\left[Y_i^C|T\right] \quad = \mathrm{E}\left[Y_i^T - Y_i^C|T\right] + \mathrm{E}\left[Y_i^C|T\right] - \mathrm{E}\left[Y_i^C|C\right]$$

The first term, $E\left[Y_i^T - Y_i^C|T\right]$, is the treatment effect that we are trying to isolate and the second term, $E\left[Y_i^C|T\right] - E\left[Y_i^C|C\right]$, is the selection bias. It captures the difference in potential untreated outcomes between the treatment and comparison groups.

When studying the effects of the FSSP on education and age at marriage, the following three possible phenomena should be considered:

A. The general assumption is that education and age at marriage will show an upward trend over time. However, for age at first birth we see the opposite trend both for urban and rural girls

B. The average education level, average age at marriage and average age at first birth are higher for urban than rural groups, and there is a tendency for convergence between urban and rural groups (see Figure 1a and 1b). Also, the results from Table 2 show that the time variable squared is positively significant for the rural girls.

[6] The elaboration and notational formats are drawn from Duflo et al. (2008).

C. Above all, the FSSP will have some effect as well.

Hence, to disentangle the treatment (FSSP) effect, we need to isolate the time trend and the convergence effect from the other factors. We apply the following three methods to study the treatment effect: ordinary least squares (OLS), DiD and RD. They are presented below.

4.2 Ordinary least squares

Even though the naïve OLS guides us about the trend and convergence effect (as shown in Table 2), it will not reveal the true causal effect as we have a self-selection issue. Decisions to attend higher education and not to marry at an early age will be determined by many other factors apart from the prevalence of the FSSP. As the FSSP is not allocated randomly, selection bias will also arise since the targeted rural areas would have been chosen due to lower enrolment by girls and a higher incidence of early marriage. A simple comparison between young and old cohorts or girls in rural and urban areas will not reveal the true causal effects on the outcomes of interest.

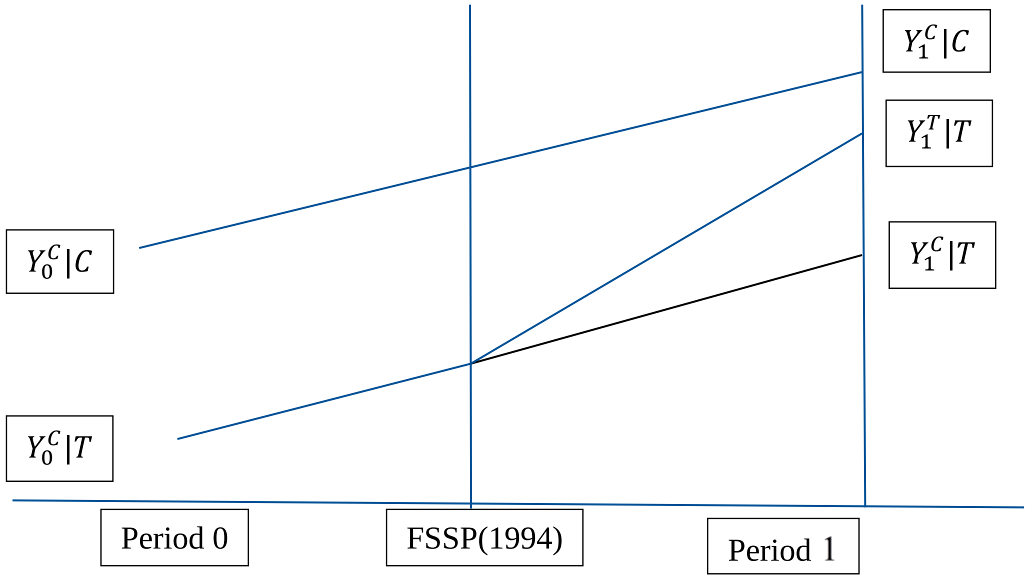
This selection bias can be fully removed when we have complete randomization. However, in the absence of complete randomization, we further apply the DiD approach and the RD design to evaluate the effect of treatment (FSSP).

4.3 Difference-in-difference approach

The DiD approach helps us to capture the time-invariant fixed effect between urban and rural areas and between old and young cohorts.[7]We denote by $Y_1^T\,(Y_1^C)$ the potential outcome if a girl received the FSSP (or did not: non-FSSP) in period 1 after the treatment occurred, and by $Y_0^T\,(Y_0^C)$the potential outcome if a girl received the FSSP (did not) in period 0, before the treatment occurred. Individuals belong to group T(FSSP) or group C (non-FSSP). Group T is treated in period 1 and untreated in period 0. Group C is never treated.

[7] Again, for the discussion and notational forms, we draw heavily on Duflo et al. (2008).

Figure 2: Depicting the underlying assumptions for the DiD



Source: Author's own illustration.

Hence, the DiD estimator is

$$\widehat{DiD} = \left[\hat{E}[Y_1^T|T = FSSP] - \hat{E}[Y_0^C|T = FSSP]\right] - \left[\hat{E}[Y_1^C|C = Non - FSSP] - \hat{E}[Y_0^C|C = Non - FSSP]\right]$$

which provides an unbiased estimate of the treatment effect of being exposed to the FSSP under the assumption that $\left[\hat{E}[Y_1^C|T] - \hat{E}[Y_0^C|T]\right] = \left[\hat{E}[Y_1^C|C] - \hat{E}[Y_0^C|C]\right]$. That is, in the absence of the treatment, the outcomes of the two groups would have followed parallel trends (see Figure 2). In other words, since we do not know (observe) the trends that the outcome variables, schooling and age at marriage, of the individuals being treated would have followed had they not been treated, we can instead observe the outcomes of the control group (urban girls not exposed to the FSSP, line $Y_0^C|C, Y_1^C|C$) and assume that the outcomes of the rural girls (exposed to the FSSP) would have followed the same trend ($Y_0^C|T, Y_1^C|T$) had they not been treated. If this assumption holds, the treatment effect estimated by the DiD approach will give an unbiased result.

4.4 Regression discontinuity design

The idea of the RD design is to exploit an observable discontinuity in the level of treatment related to an assignment variable. The level of treatment jumps discontinuously at some cut-off point for the assignment variable. The discontinuous jump in the outcome variable in the neighbourhood of the cut-off point of the assignment variable can be attributed to the change in the level of treatment under some assumptions (Nichols 2007). The basis of the RD design estimate is that, near the cut-off, the level of treatment is assumed to be randomly assigned, and observations just below and just above the cut-off are fairly comparable. Hence, the selection bias will approximate to zero. Further assumptions that allow us to infer a causal effect on the outcome variables are as follows: the change in treatment at the cut-off is truly discontinuous; the assignment variable is observed without error; all other factors are continuous with respect to the assignment variable. Finally, another important assumption needed in order for the estimation result to be valid under the RD design is that the density of the assignment variable is continuous at the cut-off (Imbens and Lemieux 2008; Lee and Lemieux 2010; Nichols 2007). That is, individuals have very little scope to manipulate the assignment variable and treatment is locally randomized. Since our assignment variable is the respondent's year of birth, we assume that there is little scope for the girls to be able to manipulate their birth records.

5 Estimation results

5.1 Ordinary least squares

To start with a simple analysis of the treatment effect, we estimated the following equation by applying OLS:

$$\mathrm{YSC}_i = \alpha_0 + \alpha_1 \mathrm{YoB_i} + \alpha_2 (\mathrm{YoB} - 1965)^2{}_\mathrm{i} + \alpha_3 \mathrm{Treatment_i} + \varepsilon_\mathrm{i} \quad (1a)$$

$$\mathrm{AgeFM_i} = \beta_0 + \beta_1 \mathrm{YoB_i} + \beta_2 (\mathrm{YoB} - 1965)^2{}_\mathrm{i} + \beta_3 \mathrm{Treatment_i} + \mu_\mathrm{i} \quad (1b)$$

$$\mathrm{AgeFB_i} = \gamma_0 + \gamma_1 \mathrm{YoB_i} + \gamma_2 (\mathrm{YoB} - 1965)^2{}_\mathrm{i} + \gamma_3 \mathrm{Treatment_i} + \vartheta_\mathrm{i} \quad (1c)$$

where, in equation (1a), $\mathrm{YSC_i}$is completed years of schooling in (1b) AgeFM_i is age at first marriage and in (1c) $\mathrm{AgeFB_i}$ is age at first birth, for individual i;YoB is the respondent's year of birth;$(\mathrm{YoB} - 1965)^2$ captures the time trend in squared form; $Treatment_i$ is the treatment variable, assigned the value 1 for respondents born in 1980 or later and 0 otherwise; ε_i , μ_i and ϑ_i are the error terms. All three equations (1a), (1b) and (1c) are estimated separately for rural and urban girls, and the estimated results are shown in Table 2. The treatment effect is significant for schooling but not for marriage and birth ages. However, the square of the time variable is positively significant for rural girls for all three outcome variables. And negatively significant for the outcome variables age at marriage and age at birth for urban girls.

Table 2: OLS results for schooling and age at marriage

	Schooling		Marriage		First birth	
Variables	Rural	Urban	Rural	Urban	Rural	Urban
YoB	0.042***	0.084**	-0.002	0.056***	-0.093***	0.005
	(0.014)	(0.032)	(0.016)	(0.020)	(0.011)	(0.016)
(YoB-1965) sq.	0.004***	0.000	0.001**	-0.002**	0.002***	-0.002***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Treatment	0.348***		0.040		0.077	
	(0.106)		(0.118)		(0.113)	
Observations	16,468	7,042	16,468	7,042	14,996	6,419
R-squared	0.107	0.018	0.008	0.001	0.009	0.010

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: Author's analysis based on BDHS dataset.

The above diagnosis based on simple regression indicates (Table 2) that both the trend and the convergence effect should be taken into consideration when studying the effects of the FSSP.

5.2 Difference in difference

When estimating the average treatment effect by applying the DiD approach, the first difference is the exposure to the FSSP as measured by age cohort (year of birth), and the second difference is according to place of residence, as the FSSP was introduced only in rural areas and urban girls were not exposed to it.

To exploit the exogenous variation in the secondary stipend programme across regions and cohorts (by comparing the cohort groups defined in Section 3.1), we estimated the effects on schooling and age at marriage by adopting the following specifications:

$$\mathrm{YSC}_{icr} = \alpha_0 + \alpha_1 \mathrm{YoB}_{ic} + \alpha_2 \mathrm{Rural}_{ir} + \alpha_3 (\mathrm{YoB}_{ic} \cdot \mathrm{Rural}_{ir}) + \alpha_4 \mathrm{X}_{icr} + \varepsilon_{icr} \tag{2a}$$

$$\mathrm{AgeM}_{icr} = \beta_0 + \beta_1 \mathrm{YoB}_{ic} + \beta_2 \mathrm{Rural}_{ir} + \beta_3 (\mathrm{YoB}_{ic} \cdot \mathrm{Rural}_{ir}) + \beta_4 \mathrm{X}_{icr} + \mu_{icr} \tag{2b}$$

$$\mathrm{AgeFB}_{icr} = \gamma_0 + \gamma_1 \mathrm{YoB}_{ic} + \gamma_2 \mathrm{Rural}_{ir} + \gamma_3 (\mathrm{YoB}_{ic} \cdot \mathrm{Rural}_{ir}) + \gamma_4 \mathrm{X}_{icr} + \vartheta_{icr} \tag{2c}$$

where, in equations (2a), (2b) and (2c), i indexes individuals, c indexes cohorts (two age cohorts, before and after the programme) and r indexes regions (urban and rural) for the outcome variables, completed years of schooling, age at first marriage and age at first birth respectively. Variable YoB is a dummy variable taking the value 1 for women born in 1980 or later (the treatment group) and 0 for women born in 1979 or earlier (control group α_1, β_1 and γ_1 capture the cohort fixed effects, α_2 and β_2 and γ_2 capture regional fixed effects (taking the value 1 if the woman lived in a rural area and 0 if urban), and α_3, β_3 and γ_3 are the average treatment effects (for rural girls who had received the FSSP since 1994) respectively. X in both equations includes other covariates such as religion, asset ownership and access to electricity. We also controlled for fixed effects due to divisional variances. Since observations could be serially correlated within a birth cohort, the standard errors were clustered at the birth cohort level.

Table 3 shows the estimated results of equation (2a) for completed years of schooling. The variable YoB*Rural shows a positive, significant treatment effect for the girls both partially and fully exposed to the programme. Since Treatment Group 1 did not receive treatment during the whole five-year secondary period, the estimated treatment effect is lower than for those in Treatment Group 2 who received the treatment fully. When other covariates are included, the average treatment effect of the FSSP goes down by 0.8 and 1.1 years for Treatment Groups 1 and 2 respectively.

Table 3: Average treatment effect on years of completed schooling

Variables	Treatment Group 1	Treatment Group 1	Treatment Group 2	Treatment Group 2
YoB	0.546***	0.519***	1.134***	1.069***
	(0.170)	(0.158)	(0.214)	(0.202)
Rural	-2.688***	-1.125***	-2.688***	-1.190***
	(0.080)	(0.059)	(0.080)	(0.064)
YoB*Rural	1.066***	0.785***	1.505***	1.140***
	(0.087)	(0.070)	(0.156)	(0.126)
Muslim		-0.709***		-0.748***
		(0.127)		(0.100)
Electricity		1.706***		1.666***
		(0.084)		(0.078)
Asset		2.363***		2.234***
		(0.055)		(0.064)
Division FE	NO	YES	NO	YES
Observations	18,394	18,394	20,980	20,980
R-squared	0.096	0.262	0.131	0.285
Robust standard errors in parentheses				
*** p<0.01, ** p<0.05, * p<0.1				

Source: Author's analysis based on BDHS dataset.

Similarly, the estimated results for the indirect effects of schooling on age at first marriage (equation 2b) are shown in Table 4. Here again, the treatment effect is higher for the girls who received the treatment during the whole secondary period than for the girls who received it only partially. Moreover, the average treatment effect is lower when other covariates are included. Girls eligible to receive the stipend tend to delay marriage by 0.34 to 0.41 years.

Table 4: Average treatment effect on age at first marriage

Variables	Treatment Group 1	Treatment Group 1	Treatment Group 2	Treatment Group 2
YoB	-0.148	-0.215	-0.074	-0.162
	(0.157)	(0.128)	(0.114)	(0.105)
Rural	-1.210***	-0.613***	-1.210***	-0.630***
	(0.065)	(0.046)	(0.065)	(0.051)
YoB*Rural	0.448***	0.341**	0.607***	0.409***
	(0.143)	(0.123)	(0.098)	(0.091)
Muslim		-1.091***		-1.062***
		(0.080)		(0.072)
Electricity		0.749***		0.744***
		(0.048)		(0.050)
Asset		0.575***		0.534***
		(0.052)		(0.053)
Division FE	NO	YES	NO	YES
Observations	18,394	18,394	20,980	20,980
R-squared	0.030	0.099	0.031	0.097
Robust standard errors in parentheses				
*** p<0.01, ** p<0.05, * p<0.1				

Source: Author's analysis based on BDHS dataset.

Finally, from Table 5, we see that age at first birth increases for both the treatment groups after controlling for other covariates. And also the treatment effect is higher (0.25 years) for the girls who were fully exposed to the programme.

Table 5: Average treatment effect on age at first birth

Variables	Treatment Group 1	Treatment Group 1	Treatment Group 2	Treatment Group 2
YoB	-0.589***	-0.682***	-0.796***	-0.869***
	(0.084)	(0.082)	(0.102)	(0.102)
Rural	-0.953***	-0.457***	-0.953***	-0.462***
	(0.073)	(0.059)	(0.073)	(0.062)
YoB*Rural	0.225**	0.139*	0.426***	0.247**
	(0.092)	(0.071)	(0.115)	(0.117)
Muslim		-0.968***		-0.971***
		(0.086)		(0.077)
Electricity		0.713***		0.709***
		(0.065)		(0.064)
Asset		0.351***		0.347***
		(0.083)		(0.075)
Division FE	NO	YES	NO	YES
Observations	16,844	16,844	19,150	19,150
R-squared	0.018	0.063	0.020	0.060

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: Author's analysis based on BDHS dataset.

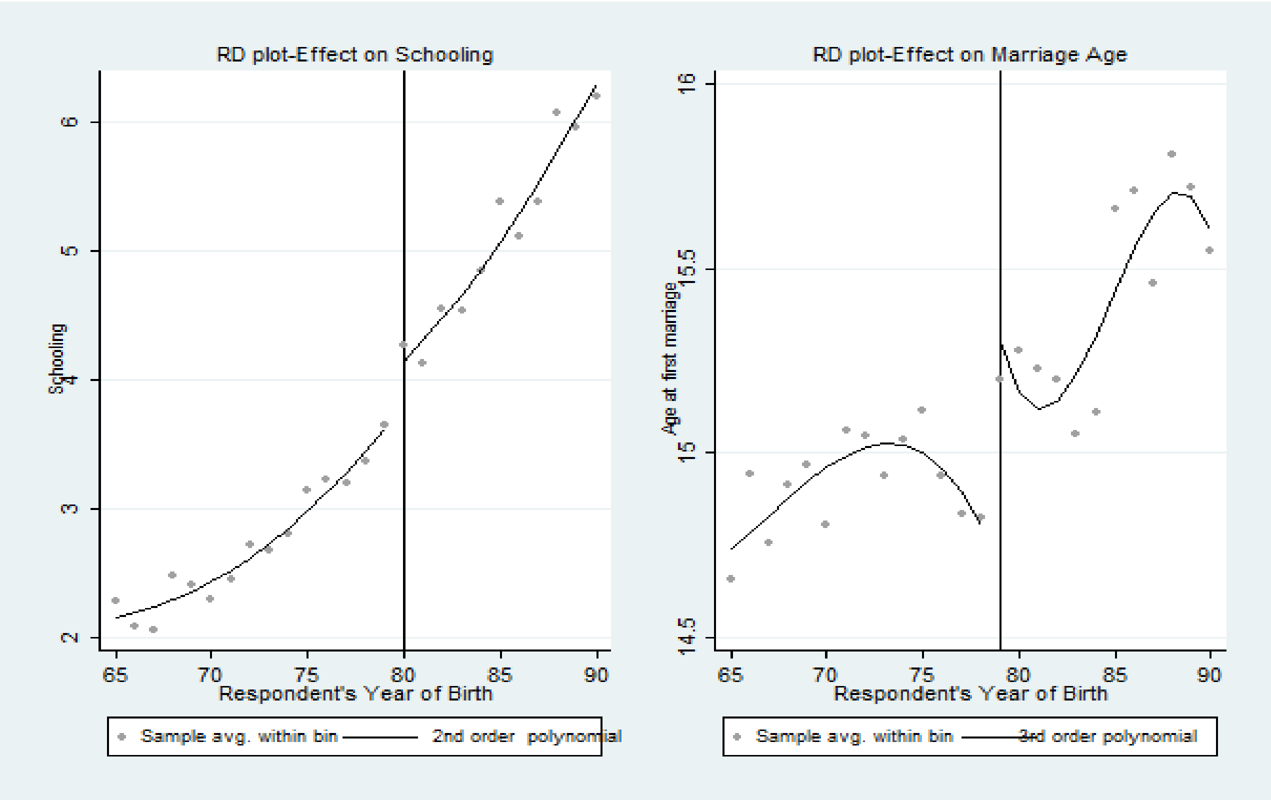
From the above estimation results obtained using the DiD approach, we find a positive significant impact of the stipend on schooling and age at marriage for the treatment groups compared to the control group. Even if the parallel trend assumption might not hold, the inclusion of other covariates captures part of the time and regional variant effects.

5.3 Regression discontinuity

A graphical presentation works as a preliminary guideline as to whether there is any discontinuity in an outcome variable for a treatment group. The visual inspection also guides us in choosing the functional form. Figures 4a and 4b show the relationship between the respondent's year of birth and the outcome variables of schooling, age at first marriage and age at first birth. These figures also show whether there is any visible treatment effect on the outcome variables for the girls who were supposed to receive the treatment (FSSP).

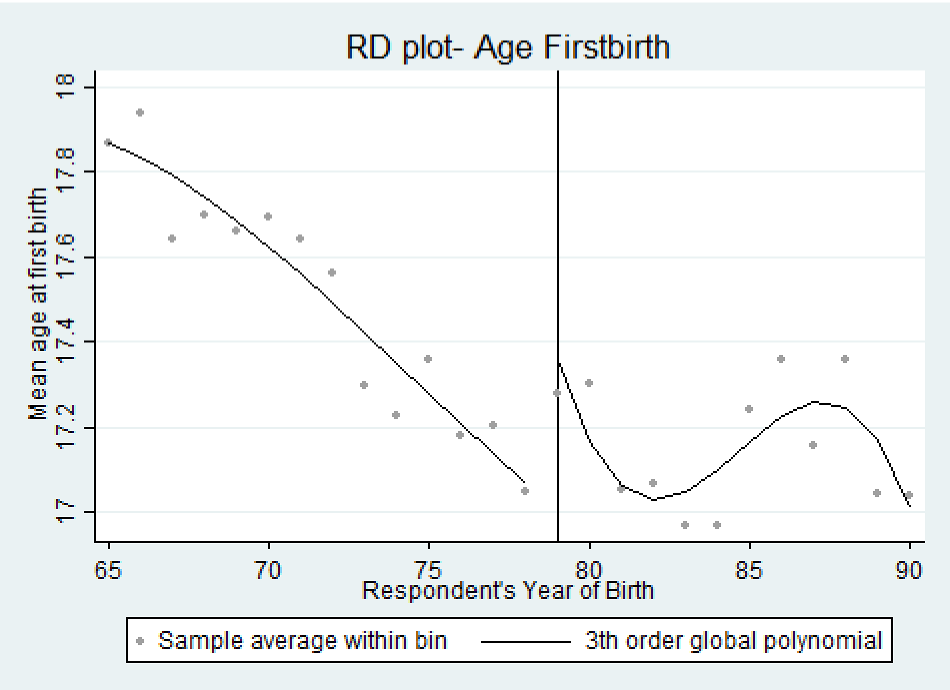
However, although there seems to be a jump for the schooling outcome variable, no such jump is visible for age at first marriage and age at first birth at the cut-off year of 1980 (not shown here). Rather, we see a jump at year of birth 1979 for the latter outcome variables. One possible explanation is that, as forthcoming expectation of the FSSP, respondents might have already decided not to get married early. Therefore, for the schooling outcome variable, we estimated the treatment effect at the cut-off year of 1980, and for ages at first marriage and first birth at a cut-off year of 1979.

Figure 4a: RD plot of schooling and age at first marriage for the rural girls



Source: Author's illustration based on BDHS dataset.

Figure 4b: RD plot of schooling and age at first marriage for the rural girls



Source: Author's illustration based on BDHS dataset.

For the outcome variables, schooling and age at first birth, there are level breaks in an upward trend. For age at first birth, there seems to be a slope break, going from a falling trend to no visible slope. The graphical inspection also reveals that the second-order polynomial fits the data well for the schooling variable, and for the marriage and first birth outcome variables the third-order polynomial does so.

The three models were estimated according to equations (4a), (4b) and (4c) by applying local, non-parametric regression:

$$\mathrm{YSC_i} = \alpha + \tau \mathrm{W_i} + \mathrm{f(Z_i)} + \varepsilon_\mathrm{i} \quad (4a)$$

$$\mathrm{AgeFM_i} = \beta + \theta \mathrm{W_i} + \mathrm{g(Z_i)} + \mu_\mathrm{i} \quad (4b)$$

$$\mathrm{AgeFB_i} = \gamma + \delta \mathrm{W_i} + \mathrm{h(Z_i)} + \epsilon_\mathrm{i} \quad (4c)$$

where equation (4a) is for the outcome variable completed years of schooling, (4b) is for age at first marriage and (4c) for age at first birth. W_i is the treatment variable, assigned the value 1 if respondents are born in 1980 or later for the variable schooling, and if they are born in 1979 or later for the variables marriage and first birth, and 0 otherwise.[8] τ, θ and δ predict the local average treatment effects. The terms $f(Z_i)$ $g(Z_i)$ and $\mathrm{h(Z_i)}$ can be linear or non-linear functions.

The simplest non-parametric estimation requires us to choose a small neighbourhood known as bandwidth to the left and right of the cut-off point. We chose the optimal bandwidth using Imbens and Kalayaranaman's (2012) approach and the triangular kernel density.[9] The RD results for completed years of schooling, estimated for rural and urban girls separately, are shown in Table 6. The average treatment effect is positive and significant for the rural girls, as expected, but for the urban girls (conducted as a placebo test) the effect is negative and significant for the polynomial of order one.

Table 6: Local non-parametric estimation results for years of completed schooling

	Rural		Urban	
	Polynomial (1)	Polynomial (2)	Polynomial (1)	Polynomial (2)
	0.386***	0.440*	-0.850**	-0.738
	(0.135)	(0.243)	(0.348)	(0.481)
Observations	13,504	11,447	2,977	3,991

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: Author's analysis based on BDHS dataset.

The RD results for both rural and urban girls' age at first marriage are shown in Table 7. The stipend has a significant effect on whether eligible rural girls delay getting married. As a placebo

[8] Due to the over-age and under-age enrolment issue, the probability of treatment might not jump from zero to one. Thus, the optimal method would be to run the estimation according to fuzzy RD design. However, as our data were not suitable for fuzzy RD, we estimated sharp RD.

[9] Triangular density is preferred by many for local non-parametric estimation as it gives more weight to the observations close to the cut-off and less to observations further away from the cut-off.

test, we assume that nothing should happen for the urban girls and as expected we find the estimated effect not to be significant.

Table 7: Local non-parametric estimation result for age at first marriage

	Rural		Urban	
	Polynomial (1)	Polynomial (2)	Polynomial (1)	Polynomial (2)
	0.440***	0.611***	0.200	0.207
	(0.114)	(0.168)	(0.274)	(0.528)
Observations	9,306	10,673	3,561	3,046

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Source: Author's analysis based on BDHS dataset.

Table 8 shows RD results for age at first birth both for rural and urban girls. The stipend has a positive treatment effect at the five per cent significance level implying eligible rural girls delay their first birth. As a placebo test, we assume that nothing should happen for the urban girls and as expected we find the estimated effect not to be significant.

Table 8: Local non-parametric estimation result for age at first birth

	Rural		Urban	
	Polynomial (1)	Polynomial (2)	Polynomial (1)	Polynomial (2)
	0.319**	0.440*	0.016	0.136
	(0.153)	(0.261)	(0.289)	(0.455)
Observations	6,097	6,097	3,645	3,645

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Source: Author's analysis based on BDHS dataset.

5.4 Sensitivity test

Due to the potential problem of under-age and over-age enrolment, as a sensitivity test we estimated the outcome variables of schooling and age at marriage for Treatment Group 2 with the control group being those cohorts born between 1965 and 1977 (see Table A1). The intuition behind choosing this control group was that it excluded girls born in 1978 and 1979 who might have received the treatment due to over-age enrolment. The treatment effects do not vary greatly from those for Treatment Group 2 shown in Tables 3, 4 and 5 respectively.

As mentioned earlier, one of the crucial aspects of local non-parametric estimation by RD is the choice of the optimal bandwidth. The standard practice is to show results with half and double the chosen bandwidth, along with the chosen bandwidth. If the standard errors and estimates are critically dependent on the bandwidth, then it will indicate that the results are dubious. Table A2 shows the results for completed schooling and age at marriage with three different bandwidths each. The estimated results for age at marriage do not vary much under different bandwidths. However, for the schooling variable the result is not significant when the bandwidth is 4. Finally, Table A3 shows the results for age at first birth with three different bandwidths and the estimated results still remain significant.

6 Discussion

From the estimation results obtained from simple OLS, we can see that years of completed schooling increase for the girls born since 1980 (Table 2). The estimation results obtained using the DiD method show that completed years of schooling increase by 1.51 and 1.14 (Table 3) without and with covariates respectively, for the girls fully exposed to the programme compared to the control group. The corresponding effect is lower for the girls partially exposed to the programme. The local non-parametric estimation by RD shows that the average years of schooling increased by 0.39 for the eligible rural girls at the discontinuity. As a placebo test, we ran a similar regression for the urban girls and found the FSSP to have had a negative significant effect.

From the OLS estimation, we did not find any significant treatment effect for the outcome variable age at first marriage. It increased by 0.61 and 0.41 years for the rural girls fully exposed to the programme, when we applied DiD without and with covariates respectively. Alam et al. (2011) found that age of marriage was delayed by about 1.2 to 1.5 years for girls receiving a secondary stipend in Pakistan. Our non-parametric RD design shows that rural girls' age at first marriage increased by 0.44 and 0.61 years for polynomial order one and two respectively at the cut-off (see Table 8). However, the coefficient for the age at first marriage for the urban girls, calculated as a placebo test, was found to be insignificant.

Similarly, estimation results by applying DiD method show that stipend caused the eligible girls to increase their age at first birth by 0.14 and 0.25 years respectively for Treatment Groups 1 and 2 respectively. According to the Hahn et al. (2015), the programme delayed age at first birth by 0.30 and 0.48 years respectively. And our RD result gave an effect of 0.32 and 0.44 years to delay the age at first birth for the polynomial order one and two respectively.

The coefficients for the YoB variable capture time-invariant cohort fixed effects (Tables 3, 4 and 5). The completed years of schooling are, on average, higher for the women born in 1980 or later than for the women born before 1980 when other covariates are included as well as controlled divisional fixed effects. The average age at first marriage is lower for the young cohort than for the old cohort but the difference is not statistically significant. On the other hand, the variable rural captures the time-invariant differences between rural and urban areas. The average years of completed schooling and age at first marriage are, on average, lower for the rural than the urban girls.

Inclusion of other covariates such as access to electricity might control for the time-variant changes that occurred in the rural areas overtime.

When comparing the estimation results from the above three methods, it should be noted that, due to endogeneity and selection bias, the OLS estimation results might not be considered to estimate true causal effects. On the other hand, the naïve estimation results provided by OLS gave insight about the trend and convergence effects.

As a second approach, the DiD estimation results provide higher positive results for completed years of schooling under the assumption of a common trend. From the OLS estimation we can see that the common trend assumption is violated. The inclusion of other covariates in the DiD controls for the violation of the common trend assumption to some extent. We can see that the estimated coefficients are smaller after the inclusion of other covariates and different fixed effects. Even though this finding is in line with the results of other studies that applied DiD, such as Hahn et al. (2015), our estimation results obtained by using RD show a smaller effect. Moreover, the placebo test run using RD shows that the estimated years of completed schooling decrease significantly for the urban girls, something that might not be captured when applying the DiD method.

For the outcome variables ages at first marriage and first birth, the estimated coefficients obtained by applying DiD are a little lower than those found by Hahn et al. (2015). However, our RD design using a slightly different identification strategy (girls born in 1979 but not in 1980) produced a bit of a higher effect than the DiD method. Moreover, a randomized RD design estimates a local average, whereas the DiD method estimates the total average treatment effect.

From the estimated results shown in Tables 2 to 8, it is evident that, since the inception of the girls' secondary stipend programme in 1994, the possibility for girls to complete more years of schooling has increased. When this stipend is exploited as an exogenous variation in schooling, the estimation result shows that girls' ages of first marriage and first birth are delayed as well.

7 Conclusion

Educating girls is considered to be a highly effective means for better child health and nutrition, reducing the rate of early fertility and reducing early marriage. Conditional cash transfer programmes have been applied in many developing countries as an incentive to increase school enrolment and delay marriage under the system of social protection for disadvantaged and vulnerable groups. The FSSP, introduced in Bangladesh in 1994, was one of the world's pioneering conditional cash transfer programmes. There are, however, very few studies focusing on the effect of such programmes on marriage age.

This paper studies the causal effects of the FSSP on education, age at marriage and age at first birth by applying OLS, DiD and the RD design. The estimated treatment effect of the FSSP on completed schooling is found to increase by between 0.4 and 1.1 years when one applies the RD and DiD methods. We show that the estimation result obtained through DiD predicts an upwardly biased result as it does not control for the convergence and negative effects on education for urban girls. Similarly, the approaches following the RD design reveal that the treatment effect for age at first marriage takes place one year earlier as a result of expectations about the forthcoming stipend programme and, as a consequence, effect on age at first birth takes place earlier as well. Moreover, the estimated effects are higher according to the RD method than DiD method.

The significant positive impact on average years of schooling has a long-term effect on the beneficiaries' productivity and wage earnings. Shamsuddin (2015) found that the Bangladesh FSSP increased the likelihood of women participating in the formal sector. Increased age at marriage should have impacts on maternal health and fertility, among other things.

As a further study, we would like to extend the analysis to the impacts on the percentage of early marriages and child mortality, by instrumenting the FSSP for mother's education. Moreover, a cost-benefit approach of the programme (FSSP) evaluation will further strengthen the causal-effect analysis.

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Appendix A

Table A1: Sensitivity test of DiD estimation using a different control group

	Schooling Treatment 2		Age at Marriage Treatment 2		Age at First Birth Treatment 2	
Variables	control 65-77		control 65-77		control 65-77	
YoB	1.268***	1.201***	-0.024	-0.108	-0.822***	-0.891***
	(0.204)	(0.193)	(0.118)	(0.110)	(0.108)	(0.107)
Rural	-2.656***	-1.181***	-1.172***	-0.601***	-0.923***	-0.430***
	(0.085)	(0.069)	(0.071)	(0.052)	(0.080)	(0.065)
YoB*Rural	1.472***	1.111***	0.567***	0.369***	0.416***	0.224*
	(0.160)	(0.128)	(0.103)	(0.093)	(0.127)	(0.127)
Muslim		-0.744***		-1.058***		-1.021***
		(0.107)		(0.074)		(0.092)
Electricity		1.610***		0.715***		0.702***
		(0.074)		(0.050)		(0.065)
Asset		2.214***		0.526***		0.325***
		(0.068)		(0.056)		(0.079)
Division FE	NO	YES	NO	YES	NO	YES
Observations	19,142	19,142	19,142	19,142	17,617	17,617
R-squared	0.138	0.287	0.029	0.093	0.019	0.059

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: Author's analysis based on BDHS dataset.

Table A2: Sensitivity test of RD results with different bandwidths

	Schooling			Age at first marriage		
	Bandwidth 16	Bandwidth 8	Bandwidth 4	Bandwidth 12	Bandwidth 6	Bandwidth 3
Polynomial (1)	0.449***	0.386***	0.316	0.257**	0.440***	0.438**
	(0.112)	(0.135)	(0.241)	(0.089)	(0.114)	(0.220)
Observations	16,774	13,504	4,501	14,009	9,306	3,161

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: Author's analysis based on BDHS dataset.

Table A3: Sensitivity test of RD results with different bandwidths

	Age at first birth		
	Bandwidth 12	Bandwidth 6	Bandwidth 3
Polynomial (1)	0.215**	0.319**	0.487*
	(0.105)	(0.153)	(0.272)
Observations	12,929	6,097	2,710

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: Author's analysis based on BDHS dataset.