Girls' Education, Stipend Programs and the Effects on Younger Siblings' Education*

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Abstract:

This paper examines the link between the female secondary school stipend program in Bangladesh, its effects on schooling of girls and the consequent impact on their younger siblings' education. The stipend program, introduced nationwide in 1994, affected girls in rural areas who were of secondary school age (grades 6-10) in 1994 or later, but not boys of the same cohort. We examine the effect of educational attainment of older siblings on schooling outcomes of younger siblings. We also examine the role of the gender of older siblings on the schooling outcomes of younger siblings. We find that the education of older siblings has a positive effect on the schooling of younger siblings and that the effect is sometimes stronger on younger brothers than on younger sisters. After taking into account the endogeneity of education of older siblings, their gender composition generally has no effect on the schooling attainment of younger siblings. Thus, failing to control for this inter-sibling education externality and the endogeneity of education may result in a biased estimate of the effect of older siblings' gender composition. The instrumental variable estimation, using stipend program eligibility as an instrument, suggests that younger siblings' completed years of schooling would be increased by upto 0.22 years if the education of older siblings increased by one year. The intent-to-treat effect suggests stipend program increased schooling by 2.2 years. This implies about 15 per cent increase of schooling of younger siblings due to the program received by their elders. Our results suggest that school programs that benefit children's education could bring both short- and long-term gains not only to the affected children directly, but also to their siblings indirectly.

Key Words: Girls' education, Stipend Program, sibling's schooling, Bangladesh

JEL Classifications: J160, H520, I280, D130

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1. Introduction

Household decisions regarding investment in human capital have important long-term consequences for economic growth and the socioeconomic outcomes and welfare of the children involved (Shrestha, 2011). Glewwe (2002) posits that education affects not only labour market outcomes, but also has implications for fertility, health, migration and marriage prospects, which are important determinants of welfare. Economic models of human capital investment presume that, in the presence of an incomplete market and borrowing constraints, children with the highest return receive the highest level of education (Becker, 1991). However, in households faced by credit constraints, siblings become rivals and the relative gender and age of siblings become important determinants of human capital investment (Dunn and Plomin, 1990). In a resource constrained family, older siblings may support their younger siblings by supplementing parental resources through wage employment (financial transfers) or through taking care of younger siblings (time transfers). There is also the potential for inter-sibling education externalities, in which the education of older siblings has a demonstration effect on the schooling of younger siblings. If sibling gender composition and/or education of older siblings affect educational achievement of younger siblings, then differences in intra-household resource allocation, due to different household configurations, can potentially have long-lasting effects for all of the children in the household.

Previous studies on siblings have focused mainly on sibling rivalry, specifying the effects of the number, and gender composition, of siblings on children's education (see, e.g., Butcher and Case, 1994; Black *et al*, 2005). Only a few studies consider the potential for inter-sibling education externalities (see Qureshi, 2011; Shrestha, 2011). We contribute to the literature on the effect of siblings on schooling outcomes by simultaneously considering the effects of the gender composition of older siblings and the education of older siblings on the schooling outcomes of younger siblings. While most previous studies focus exclusively on the gender composition of siblings, if significant inter-sibling education externalities exist within the household, it may be that it is the education of older siblings, and not their gender *per se*, which has the greatest impact on schooling outcomes of younger siblings. To the best of our knowledge, this is the first study that isolates the effect of older siblings' education from their gender composition and focuses on the relative contribution of older siblings' gender composition and education to the schooling of younger siblings.

There are factors, both at the household and individual levels, which are unobservable but could affect the education of both older and younger siblings. For example, some households tend to invest highly in the education of all children, while some others tend to discriminate between children based on birth order and gender. Hence, to examine the causal impact of education of older children on their younger siblings, one must address the potential endogeneity of education of older and younger siblings. We address the endogeneity of education using the female secondary school stipend program (FSSSP) in Bangladesh as an instrument. A substantial improvement in girls' school enrollment in Bangladesh is often attributed to the FSSSP and it has been a model for conditional cash transfer programs in other developing countries. However, despite its policy importance, few studies have examined its effectiveness in a systematic manner (see, e.g., Heath and Mobarak, 2012; Khandker *et al*, 2003). Thus, a second contribution of this study is to add to the embryonic literature around the efficacy of the FSSSP and to the literature more generally on schooling outcomes in Bangladesh; a country, which has made accelerated progress toward realizing gender equity in education and which other countries are seeking to emulate.

The stipend program, introduced nationwide in 1994, generated exogenous variation in the schooling of a cohort of children in rural Bangladesh. Specifically, the stipend program affected girls in rural areas who were of secondary school age (grades 6-10) in 1994 or later, but not boys in the same cohort, nor boys or girls who were in the immediate older cohorts. We exploit these variations in schooling among older siblings to examine the (indirect) effects of schooling of older siblings on younger siblings' education.

Overall, our results indicate that the education of older siblings has a significant, and large, impact on the education of their younger siblings and that failing to control for this inter-sibling education externality may result in a biased estimate of the effect of older siblings' gender composition. The ordinary least squares (OLS) estimates suggest that younger siblings' completed years of schooling would be increased by 0.16-0.23 years if the education of older siblings increased by one year. We find some evidence that the gender composition of older siblings has an effect on the schooling of younger siblings. For some samples, OLS estimates, without controlling for older siblings' education, suggest that having an older brother, instead of an older sister, significantly reduces younger siblings' schooling by 0.04-0.08 years. Once we control for older siblings' education in these samples, the sign on the OLS coefficient for gender composition reverses, suggesting a 0.06-0.07 year increase in schooling of younger siblings if they have an older brother instead of an older sister. The effect of gender composition using OLS estimation, however, is not robust in all samples, suggesting no gender effect in some samples, irrespective of whether we control for education. However, instrumental variable (IV) results suggest that once we take into account of the endogeneity of education of older siblings' education, schooling of younger siblings is not affected by the gender of the older sibling and this result is robust across alternate samples. The IV estimation, using eligibility for the FSSSP as an instrument, suggests that younger siblings' completed years of schooling would be increased by 0.06-0.22 years if the education of older siblings increased by one year. The overall results indicate that the FSSSP which increased the education of the older siblings by about 2.2-2.7 years, subsequently increased the education of the younger siblings by about 0.16-0.5 years, indicating an increase of 5-15 per cent from their mean education.

2. Existing Literature

Conceptual arguments

Neoclassical models of intrahousehold human capital investment (Becker and Tomes, 1976) presume that, if markets are complete with no credit constraints and there are no parental preferences for equality of earnings, parents invest in their children's education until the expected marginal return to education is equal to the market rate of interest. In such a case, sibling gender composition does not play any role in educational attainment of a child. However, if parents have an aversion to inequality of earnings among children, sibling gender composition can affect educational attainment even if there is no borrowing constraint (Behrman *et al*, 1982). In that case, parents will invest more in the low-return child and less in the high-return child. Thus, if returns to education are higher for boys than girls, a child with brothers will receive more education than he/she would receive if he/she had sisters instead, and vice versa (Butcher and Case, 1994). On the other hand, parents wishing to maximize the sum of their children's income, invest more in high-return children in the face of credit constraints. For example, if the return for boys' education is higher than that for girls', sons receive more education than daughters. This implies that a child with sisters will receive more education than if he/she had brothers instead (Butcher and Case, 1994).

Hence, sibling gender composition influences a child's education and this influence is likely to be greater the more resource constrained the household. Under such circumstances, each child's education depends not only on his or her own rate of return, but on the number, and gender composition, of siblings who share limited resources in the family. In societies where a daughter leaves the household after marriage and a son stays with his parents, or has the responsibility for old-age support for parents, gender composition is particularly important given that returns to investing in sons will be higher than investing in daughters. An alternative channel regarding the effect of gender composition on educational outcomes focuses on psychological aspects of child development. Research in development psychology posits that sibling gender composition can affect the educational attainment of a child through its effect on the personality, interest and emotional development of the child. According to the development psychology literature, spillover effects arise when traits specific to one gender are adopted by a child from a sibling of the opposite gender. Girls with older brothers have been shown to have more masculine traits (Koch, 1955). Thus, some authors argue that if education is treated as a masculine trait, girls with older brothers tend to receive more education than girls who do not have older brothers (Butcher and Case, 1994).

Birth order can also be important in influencing the education opportunities available to siblings. In particular, education of a child might be affected by education of older siblings, although the effects are not clear cut. There are a number of possible channels. One positive effect results from a spillover or demonstration effect from the older sibling to the younger sibling. A second positive effect might result from the better educated older sibling having more labour market opportunities and, thus, being better placed to support younger siblings through financial transfers within the family, especially when the difference in age between siblings is large. A third positive effect could result from decreasing the per-child fixed cost of education. Conversely, existence of older siblings might reduce the education of a younger sibling if they are competing for very limited resources and the household is credit constrained. This is particularly true when the age gap between siblings is relatively small.

Empirical evidence

There is a vast empirical literature focusing on the effect of older siblings on younger siblings' schooling and the relative role of older brothers and older sisters. Studies show that older girls share significant child care responsibilities in many developing countries, which adversely affects their schooling (Levison and Moe, 1998). Dammert (2010), in a study for Nicaragua and Guatemala, finds that older boys spend more time in domestic and market work, and older girls spend more time in domestic work, compared to younger siblings and that this result is independent of younger siblings' gender. Parish and Willis (1993), however, find that older sisters increase the educational attainment of younger siblings through taking care of younger siblings or earning extra income through wage employment which can be used for younger siblings' school fees and other expenses. They also find that older sisters help mitigate the household resource constraint by marrying earlier and leaving the household. The child labour literature suggests that younger siblings are less likely to work (e.g., Patrinos and Psacharopoulos, 1997; Chesnokova and Vaithianathan, 2008; among others). This result might be explained by the higher wage potential of older children or comparative advantage of older girls in household work (Dammert, 2010). Edmonds (2007) finds that an additional younger sibling increases household work by older girls, and market work by older boys, compared to their younger siblings in the same household in Nepal.

A number of studies find evidence of pro-male bias in investment in the education and health of children in households facing financial and time constraints (Parish and Willis, 1993; Chen *et al*, 1981; Garg and Morduch, 1998; among others). However, other studies do not find any evidence of an effect of sibling gender composition (for example, Hauser and Kuo, 1998) or find mixed evidence for different samples (Kaestner, 1997; Morduch, 2000). There is extensive empirical evidence establishing that conditional cash transfer or subsidy programs have a positive effect on schooling outcomes of treatment groups (Schultz, 2004; de Janvry *et al*, 2006; Filmer and Schady, 2008; among others). Some studies have found that such programs have a greater impact on girls than boys (for example, Meng and Ryan, 2010), although other studies have reached the opposite conclusion (for example, Ravallion and Wodon, 2000). Behrman *et al* (2005) suggest that such programs have more long term effects on boys, but more immediate effect on girls. Ravallion and Wodon (2000) show that the food for education program, targeted at primary school aged poor children, increased child schooling and reduced child labour in Bangladesh. Female targeted subsidy programs have been found to be effective in increasing girls' school enrollment and attendance (Chaudhury and Parajuli, 2008; Filmer and Schady, 2008). Baird *et al* (2011), using a unique experiment focused on adolescent girls in Malawi, have shown that making financial transfers conditional on school attendance significantly increased school enrollment and attendance. Again, Studies show that subsidies under conditional cash transfer program may cause a reallocation of responsibilities within the household which could potentially have important implications for the school enrollment or work of other siblings (Barrera-Osorio et al 2011; Ferreira *et al*, 2009; Behrman *et al*, 2010). Theoretically, a female targeted subsidy program,increases girl's schooling through both substitution and income effects. But for boy's schooling, substitution effect has a negative and income effect has a positive effect. Khandker et. al (2003) find a significant positive effect of the female secondary stipend program in Bangladesh on girls' enrollment, but a mixed effect on boys' enrollment. They find no effect on boys' enrollment with cross-sectional household survey data, while partial evidence of decrease in boys' enrollment in co-education schools only with school-level data.

Very few studies have examined the effect of a child's education on his/her siblings' education. One such study is Qureshi (2011), who exploits the existence of gender segregation of schools in Pakistan. He finds that the oldest sister's schooling has beneficial impacts for younger brothers' schooling, enrolment, literacy and numeracy. Another study is Shrestha (2011), who finds that, in Nepal, a change in the educational requirement for British Gurkha Army recruitment, which generated an exogenous increase in the education of Gurkha boys, resulted in a decrease in the education of their female siblings. Relatedly, a number of studies find evidence that having a literate person in the household confers benefits on other members in the household, consistent with the existence of intrahousehold externalities or spillover effects of literacy and education (see e.g., Rosenzweig and Wolpin, 1994; Basu *et al*, 2001; Currie and Moretti, 2003; Pronzato, 2012).

To summarize, there is a substantial empirical literature examining the effect of the gender composition of older siblings on the schooling outcomes of younger brothers and sisters. There is a much smaller empirical literature examining the role of inter-sibling education externalities. There are, however, no studies which address the relative role of gender versus education of older siblings. This is a gap this study seeks to fill, using the FSSSP in Bangladesh as a natural experiment, to estimate the causal effect of the gender and education of older siblings on their younger siblings' education.

3. Background, the Stipend Program and the Data

Education in Bangladesh

Education in Bangladesh consists of primary (grades 1-5; ages 6-10), secondary (grades 6-10, ages 11-15) and tertiary (grades 11+, ages 16+) levels. The Primary Education (Compulsory) Act was introduced in 1990 with the objective of realizing universal primary enrollment.

Similar to other developing countries in South Asia, Bangladesh has traditionally been characterized by low enrollment and gender disparity in educational achievement. In 1991, the net enrollment in primary schools was 75 per cent for girls and 85 per cent for boys, while it was 14 per cent for girls and 25 per cent for boys in secondary school[1]. However, the country has achieved tremendous progress over recent decades, not only in improving educational attainment, but also in reducing gender disparity. The net enrollment rate in primary schools was 98 per cent for girls and 89 per cent for boys in 2009; in secondary schools, the rate was 54 per cent for girls and 45 per cent for boys. From 1990 to 2009, the gender parity index (ratio of girls to boys) increased from 0.83 to 1.01 in primary schools and from 0.51 to 1.07 in secondary schools.[2] Much of this progress could be attributable to mix of government and NGO initiatives in the primary education sector. A large number of NGOs, including the Bangladesh Rural Advancement committee (BRAC), started working in primary education in rural area. The government also introduced the food for education (FFE) Program in 1993 to support poor children in completing primary schooling. The primary education stipend project (PESP) replaced the FFE Program in 2002 and has been providing cash transfers to households of children in poor areas on the condition that children remain enrolled at primary school and maintain a minimum attendance level. In addition, a variety of policies - the elimination of official school fees, free textbooks and incentives to encourage the participation of vulnerable children - have been put in place to encourage school enrollment. However, despite government and NGO efforts in improving primary school enrollment, more than half of the primary school students dropped out before completing grade five in the mid to late 1990s (Ahmed *et al*, 2007). Less than 50 per cent of students proceeded to secondary school and the dropout rate in secondary schools was more than 60 per cent in the early 1990s, with girls faring worse than boys (WB, 2012, 2002a). Therefore, intervention in secondary education was desirable to improve the educational attainment of girls.

The Female Secondary School Stipend Program (FSSSP)

The FSSSP was introduced in January 1994 nationwide, covering all rural areas in 460 sub-districts, targeted to girls enrolled at secondary level (grades 6-10, ages 11-15). The objectives of this program were to: (i) increase school enrollment among secondary-aged girls; (ii) improve the secondary schooling completion rate for girls; and (iii) increase female

[1] 1991 Bangladesh Preliminary Census.
[2] Bangladesh Bureau of Educational Information and Statistics (BANBEIS).

age at marriage. The program provided a uniform stipend and tuition subsidy to any girl enrolled in a secondary school in rural areas, meeting the following eligibility criteria: i) 75 per cent school attendance; ii) 45 per cent marks in final exam; and iii) remaining unmarried. In 1994, only students in grade six and nine were covered and in 1995, all grades, except grade eight, were covered by the program. Starting from 1996, girls in all grades meeting the eligibility criteria were covered by the stipend program. The monthly stipend ranged from 25 to 60 Bangladesh Taka (≈ US$0.62 to 1.50 according to the 1994 exchange rate) depending on the grade level covering grades six to ten; with a book allowance of 250 Bangladesh Taka (≈US$6.25) for grade nine and ten students only. The stipend and allowances were expected to cover 50 per cent of the costs of textbooks, uniforms, stationary, transportation, examination fees and other educational expenses for eligible students. The stipend and allowances were paid in two installments annually directly through the students' accounts in *upazila* (sub-district) branches of a nationalized bank, while the tuition was paid directly to the school for each girl receiving the stipend. The stipend component comprised more than 75 per cent of the total cost of the program (WB, 2002a). In 1998-99, government expenditure for the stipend program accounted for 14.5 per cent of the secondary education budget and six percent of the overall education budget (WB, 2002).

Analyzing administrative data on school enrollment, Khandker *et al*. (2003) find a marked pattern of increased enrollment among girls, relative to boys, in secondary schools following the introduction of the program. For example, they find that at the onset of the program in 1994, the tenth grade had only 36 per cent of female students who had been enrolled in the sixth grade. In 1998, this proportion had increased to 59.2 per cent. They find that girls' school enrollment in each of grades six to ten was higher after 1994 than they were in 1994. The data did not show any such trend for boys' enrollment over the same period. However, Heath and Mobarak (2012) find that the growth in the ready-made garments industry in Bangladesh generated a quantitatively significant increase in the school enrollment of girls aged 5 to 10, while the FSSSP did not have a significant effect on girls' school enrollment.[3]

[3]They use a survey dataset of 1395 households from four sub-districts in Dhaka and Gazipur districts in Bangladesh. A large number of garment industries are located in these sub-districts or in surrounding areas. Therefore, it is not unusual that the girls from their survey areas were more concentrated in the garment sector. Our results support the findings of Khandker *et al* (2003). The impact of such supply side interventions in education has been proven to be highly effective in many other developing countries such as in Mexico, Africa and Latin America.

The data

The study uses data from the Bangladesh Multiple Indicator Cluster Survey (MICS) 2009. MICS is an international household survey conducted jointly by the Bangladesh Bureau of Statistics and UNICEF. MICS 2009 is a nationally representative extensive cross-sectional household survey in Bangladesh covering about three hundred thousand households throughout the country (MICS Report, 2009). It is also the largest household level nationally representative survey which has ever been administered in Bangladesh.

We restrict our sample to rural households with more than one child and exclude households with adopted, foster or step-children. We focus on rural households, given that the FSSSP only directly affected schooling of girls in rural areas. As we are interested in the effect of the education of older siblings on schooling outcomes of younger siblings, we include households with children aged 7-14 years who have at least one older sibling. The lower age limit is chosen given the official enrollment age in primary school (6 years) and the upper age limit is chosen to restrict children up to an age where variation in choice between schooling and work or in schooling of different gendered children might occur.[4] Our final data set contains 140,690 children aged 7-14 years old, who have at least one older sibling. The full sample size for siblings (including all children in the households with more than one child) is 496,370. Table 1 provides the basic summary statistics of the treatment and control group of older siblings aged 21-32 years who have at least one younger sibling aged 7-14 years.

[Insert Table 1 here]

Older brothers are more educated on average, compared to older sisters in control group, but the mean education of treatment girls are much higher than control boys' or girls' education. We also see a pattern similar to Ahmed et al (2003), that, the education of girls at the onset of of the program shows a tremendous increase, but there is no such trend in boys' education (Figure 1). There are slightly more younger sisters than younger brothers in the sample. Girls in control group have slightly more siblings and brothers, comared to boys in the same group. Edmonds (2007), in explaining a similar pattern for a Nepalese dataset, suggests that this might reflect parental preference for boys as parents continue to have children to get a desired number of sons. During the survey, 15-49 year old women in all households were interviewed

[4] Bangladesh ratified the Minimum Age Convention, 1973, which sets 15 years as the minimum age for child labour. Moreover, one can complete junior secondary school (grade eight) at the age of 14, assuming a timely completion.

separately and were asked about their fertility history. A total of 705,724 children were born to the women included in the survey; of which, 380,299 (53.9%) were boys and 325,425 (46.1%) were girls. Among them, 91.4 per cent of boys and 92.2 per cent of girls were still alive at the time of the survey. Thus, data on mortality rates does not indicate any sign of differential treatment for boys and girls. We also observe that the average number of older boys is greater than the average number of older girls. This might be due to the fact that our dataset does not contain full sibling history and it is likely that older sisters in many households have left the household upon marriage. However, it might also be the case that older brothers in a household live outside the household for work or education purposes.[5] The average schooling of children in the sample is about three years and the average level of education is higher for girls than boys.

[Insert Figure 1 here]

4. Empirical strategy

We estimate the following regression to account for the effects of education and gender composition of older siblings on schooling outcomes of younger siblings:

$$Edu\_Young_{ij} = \alpha_0 + \alpha_1 Edu\_Old_{sij} + \alpha_2 B_{ij} + \alpha_3 C_{ij} + \alpha_4 X_{sij} + \alpha_5 H_j + \gamma_l + v_j + \mu_{ij} \qquad (1)$$

where Edu_Young_{ij} is the educational attainment of child i in household j; Edu_Old_{sij} is the education of older sibling(s) of child i in household j; B_{ij} is the number of older brothers of child i in household j, controlling for number of older siblings of child i in household j; C_{ij} is a vector of child i-specific variables which vary across children in the household; X_{sij} is a vector of variables representing siblings' characteristics and H_j is a vector of household level variables which are the same for all siblings in the same household. Child i-specific controls include a vector of age and gender dummies; sibling controls include birth spacing and siblings fixed effects and household controls include household head's education and gender.[6] We also include a dummy variable indicating whether the child is the offspring or sibling of the household head. Separate dummies for each age-year of child i also control for growth in educational attainment across cohorts and changes in national educational policies as well as sociopolitical conditions or environmental shocks in a particular year affecting the schooling

[5] Using data of only those households in which we have full sibling history (i.e., all children of the mother are living in the household), we see a similar pattern in the data.

[6] Mother's age and education level is highly correlated with that of the household head. We therefore control only for the head's characteristics. Our results remain robust adding controls for mother's characteristics.

of children. Geographic fixed effects are denoted by γ_l. We include district fixed effects in order to control for the geographic location of the educational administration which controls, and monitors, schools within a district.

The error term is assumed to consist of two components: v_j, which is common to all siblings in the household and μ_{ij}, which varies independently across children in the household. Since children of the same household are likely to be similar across a wide variety of characteristics, we estimate standard errors clustered at the household level. Such clustering allows correlation across siblings and a common unmeasured family effect as reflected in v_j. We are mainly interested in the coefficients α_1 and α_2—inter-sibling education and gender effects, respectively.

Estimating the effects of older siblings' education on younger siblings' schooling is not easy due to the endogeneity of education of older siblings. There might be omitted variables bias from unobserved factors, such as household characteristics or parental preferences, leading to a potential non-zero correlation between the error term and education of the older siblings. Thus, any observed correlation between older siblings' education and younger siblings' education might be due to such unobserved factors affecting both.

To estimate the causal impact of older siblings' education on younger siblings' education, we adopt an instrumental variable strategy. We take advantage of the FSSSP which was introduced in 1994 for rural girl students in Bangladesh. Whether older siblings benefited from the stipend program depended on their gender and age in 1994. We take advantage of this variation to use the timing of the introduction of the FSSSP as an instrument. The program was offered to female students in grades 6 to 10 enrolled in school in 1994 or later. The official age for children to attend compulsory primary school is six years old. Thus girls aged 11 years or younger in 1994 were eligible to receive the program in that year or later. Hence, a female who was 26 years old or younger, when the survey was administered in 2009, was eligible to receive the FSSSP.[7]

[7] A girl who was in grades seven to nine in 1994 could not anticipate that the stipend program would be introduced and, thus, the introduction of the stipend program could not induce her to remain at school. Hence, such girls are placed in the control group. We address the issue of partial versus full treatment in the robust analysis below.

The control group consists of two sets of individuals. The first is older sisters who were not eligible to participate in the FSSSP. Girls who were 12 years or older in 1994, or 27 years or older at the time of the survey in 2009, were not eligible to participate in the FSSSP. Comparing older sisters aged 11 years or less to those aged 12 years or more in 1994 could reflect an age effect. We address this issue by including older brothers, aged 11 or less and 12 or more in 1994, as the second set of individuals in the control group given that none of the older brothers were eligible to participate in the FSSSP. Specifically, we adopt a double difference (DD) strategy to control for any secular age effects. The difference in changes in outcomes of girls across eligible (aged 11 or less) and non-eligible (aged 12 or more) age cohorts is the single difference estimate for eligible girls or the target group. The difference in changes in outcomes of ineligible boys across two different age cohorts (aged 11 or less and 12 or more) is the single difference for boys. The DD estimate is the single-difference estimate for girls less the single difference estimate for boys. Any difference in schooling between eligible and non-eligible girls that is over and above the difference in (eligible) boys can be attributed to the program effect.

As mentioned in section 2, it is possible that the program affected the program-ineligible siblings (boys and age-ineligible girls) in the same family. The ineligible siblings might receive less education through a reallocation of family resources and/or shift in family responsibility, given that their cost of education is relatively higher (substitution effect). They might receive more education through easing of family resources (income effect). Again, parents might also be induced to continue schooling of a boy child (or an ineligible girl child) in order to accompany the eligible girl in her way to school. This is particularly applicable in the context of Bangladesh society where travelling alone to school is often troublesome for a girl[8]. Thus the potential spillover effect of the program on boys or girls of same age cohort depends on the relative strength of these counteracting forces. In case of any effect on ineligible siblings, conceptually, we will be estimating the lower bound of the effect of the program. However, we show below that the program did not have any significant effect on ineligible group in most of the samples we used in our estimation.

Below we present results defining treatment and control groups for two separate age groups of older siblings. The first is for older siblings aged 6-17 years in 1994 (21-32 years at the

[8] Qureshi (2011) mentions this in the context of Pakistan.

time of the survey in 2009). This uses a six year window and defines the treatment group as older sisters who were aged 6-11 years of age in 1994 (21-26 years of age at the time of the survey in 2009). The control group consists of older sisters who were aged 12-17 years in 1994 (27-32 years of age in 2009) and older brothers aged 6-11 years and 12-17 years in 1994 (21-32 years in 2009).

The second set of results is for older siblings aged 2-21 years in 1994 (17-36 years in 2009). This uses a broader window and defines the treatment group as older sisters aged 2-11 years in 1994 (17-26 years in 2009). The control group is older sisters aged 12-21 years in 1994 (27-36 years in 2009) and older brothers aged 2-11 years and 12-21 years in 1994 (17-36 years in 2009). We restrict the age of older siblings in the control group to those above 16 years in 2009 as children are expected to complete secondary school by this age and are potentially able to help younger siblings.

We run the following regression to estimate the effects of FSSSP on education:

$$Edu\_Old_{sij} = \partial_0 + \partial_1 B_{ij} + \partial_2 Eligible\ age + \partial_3 Girl + \partial_4 Eligible\ age * Girl + \partial_5 C_{ij} + \partial_6 X_{sij} + \partial_7 H_j + \gamma_l + u_j + \psi_{ij} \quad (2)$$

The dependent variable, $Edu\_Old_{sij,}$ is the years of schooling completed by older siblings *s* of child *i* in household *j*.

$$Eligible\ Age = \begin{matrix} 1 \text{ if he older sibling was aged } 6-11 \text{ in } 1994\ (21-26 \text{ in } 2009) \\ 0 \text{ if the individual was aged } 12-17 \text{ in } 1994\ (27-32 \text{ in } 2009) \end{matrix}$$

For 17-36 years old older sibling, we define eligible age analogously.

Girl is a binary variable set equal to 1 if the older sibling is female. The coefficient, $\partial_{4,}$ on the interaction of *Eligible age* and *girl*, estimates the effect of the stipend program on education of older girls in the household. It can be interpreted as the intent-to-treat (ITT) effect or difference-in-difference estimates of the effect of the stipend program on the education of the 1994 cohorts of primary and secondary school age children.

The IV results are reported using a two-stage procedure following Rivers and Quang (1988). The method involves obtaining residuals from the reduced form first-stage equation, and then uses the estimated residuals as an additional regressor in the second stage. Terza *et al* (2008) show that this two-stage residual inclusion method produces similar results to the two-stage predictor substitution method (i.e., using the predicted values of the endogenous variable from the first stage in the second-stage equation), but the former is generally consistent while the latter is not always.[9]

We present alternative sets of results for younger children aged 7-14 in 2009 who have one or more older siblings aged either 21-32 or 17-36 in 2009. It is possible that a child aged 7-14 in 2009 has one or more older siblings who are not aged either 21-32 or 17-36 in 2009 and that this might result in biased estimates. Thus, for each age grouping of older siblings we present results for two samples. The first sample is for children aged 7-14 in 2009 who have at least one older sibling in the 21-32 or 17-36 age groups ('sample 1'). The second sample is for children aged 7-14 in 2009, for whom all of their older siblings are in the 21-32 or 17-36 age categories at the time of the survey ('sample 2').

5. Main Results

OLS estimates

Table 2 provides OLS estimates for the relationship between gender composition and education of older siblings and education of younger siblings. The estimates reported in columns (1) to (8) in Table 2 differ in terms of control variables used in the regression- we successively add individual controls, sibling controls and household/area controls as well as geographic fixed effects. In order to examine the role of the gender of older siblings and their education separately, we report results with and without controlling for siblings' education. The odd number columns in Table 2 represent results without controlling for education, while the even number columns include education of older siblings as an additional control.

Panels A and B present the results where older siblings were in the age categories 21-32 years and 17-36 years respectively in 2009. The results suggest an intra-household positive externality resulting from education in both samples 1 and 2 in panels A and B. The point estimates in column (8) indicate that an additional year of schooling is associated with an increase of 0.16-0.23 years in the schooling of younger siblings. In panel A, the coefficients corresponding to gender composition of older siblings are generally statistically insignificant;

[9] We also estimated a Tobit model in the second stage to address concerns that some children aged 7-14 might not have gone to school at all. The results are similar to those which are presented and are available upon request.

however, in panel B they are statistically significant. If we do not control for the education of older siblings, having an older brother, instead of an older sister in the 17-36 age group has a negative effect on the education of the younger sibling. For example, based on the point estimates in column (7), having an older brother, instead of an older sister, aged 17-36, significantly reduces younger siblings' schooling by 0.04-0.08 years. But, when we control for the education of the older sibling, the sign on the number of older brothers is reversed. The estimates in column (8) suggest that having one additional older brother aged 17-36 instead of an older sister of that age group, while holding the number of older siblings in the 17-36 age group the same, would increase completed years of education by 0.06-0.07 years.

[Insert Table 2 here]

First-stage estimates and the validity of the instrument

Before presenting the IV results, we report the first-stage results and check the validity of the FSSSP as an instrument. Table 3 presents the first stage regression results with alternate specifications. The results show that FSSSP had a significant positive impact on the schooling of age-eligible girls. If we consider our preferred sample (Panel A, sample 1) with full set of controls, we find an increase of 2.24 years of schooling of girl children due to their eligibility to participate in the program. The corresponding estimates are higher if we consider 17-36 years old sample, with an increase of about 2.64 years of schooling for this age cohort. The F-statistics are well above the rule of thumb value of 10 in each specification, supporting the validity of the instrument. As we are exploiting the timing of the introduction of the program in 1994 and variation in its application between different age groups and gender, the exclusion restriction requires that introduction of the program has no effect on the education of younger siblings, aged 7-14 years in 2009. These children were not born at the time of the introduction of the stipend program in 1994. Note that our instrument is not the program *per se* that is still in place, but the timing of its introduction in 1994. As mentioned before, the program could have spillover effects on boys or older girls of the same households. While these do not invalidate our exclusion restriction, but in that case the results reported here would be conservative estimates of the effects of the program.

[Insert Table 3 here]

Effect of the program on Boys and Ineligible Girls

For 21-32 year old age group, we check if program has any effect on boys belonging to the same age group, and 27-32 years old girls (ineligible) separately using a dummy variable of whether any girl sibling in the household is eligible to receive the program (i.e., whether the household has any girl aged 21-26 years old) as treatment variable. The analysis is done for 17-36 year old age group analogously . The control group comprises children in the same age group who have no sibling receiving the program as they are all ineligibles. Controlling for siblings characteristics belonging to the age group (total number of siblings and number of borthers/sisters in the age group) together with other individual, sibling and household characterisitics, the treatment variable thereby shows the mean effect of the program on ineligible siblings. Appendix Table 1 presents the results for effect on boys and on ineligible girls in column (1) and (2), respectively. The results indicate that the program does not have any significant impact on ineligible older siblings' education except that we see a significant effect on boys at 10 per cent significance level in sample 2 for 17-36 age group. However, the magnitude of the coefficient is much lower compared to the effect on girls.

[Insert Appendix Table 1 here]

IV estimates

Table 4 reports the IV estimates of the effects of the education, and gender composition, of older siblings on their younger siblings' education. Overall, the results suggest that it is the education of older siblings, rather than their gender composition, which is important for the education outcomes of younger siblings. The magnitude of the coefficient on older siblings' education is higher in panel A than panel B. In sample 1 of panel A, in column (4), an additional year of schooling of older siblings increases younger siblings' years of schooling by 0.22 years. Given that the average education of older siblings is 5.8 years in the sample, this figure translates into a 15 per cent increase in younger siblings' mean education (3.8 years in the sample) due to the FSSSP[10]. The corresponding estimate for sample 2 of panel A is 0.14 years, which would be reflected in a 8 per cent increase in the average education of younger siblings (4.6 years in the sample) due to the FSSSP. However, in panel B, the results in column (4) suggest that if the education of older siblings aged 17-36 increased one year, younger siblings' completed years of schooling would be increased by 0.06 years, which would be reflected in a 5 per cent increase in the average education of younger siblings (4.6 years in the sample) due to the FSSSP. The results are robust to the successive addition of controls. Thus, overall results indicate that the FSSSP which increased the education of the

[10] We use the following formula: Percentage change in younger siblings' education due to program= $\{(ITT*\alpha_1) / (meanEdu\_Young- ITT*\alpha_1)\}*100$.

older siblings by about 2.2-2.7 years, subsequently increased the education of the younger siblings by about 0.16-0.5 years.

A possible explanation for the finding that education by older siblings aged 21-32 years has a larger effect than the education of older siblings aged 17-36 years on the schooling of younger siblings is that older siblings belonging to the 17-36 age group were more likely to be still enrolled in school compared to the 21-32 year age group as the lower bound of the 17-36 age group includes a relatively younger cohort. Hence, sibling rivalry for limited resources between older siblings and younger siblings is potentially more acute with the 17-36 sample of older siblings than the 21-32 sample of older siblings. The data is consistent with this conjecture. In 2009, 21 per cent of older siblings aged 17-36 were still enrolled in school, compared with just eight per cent of older siblings aged 21-32.[11]

The existing literature suggests that each additional year of schooling by parents is associated with an increase in their children's schooling ranging between 0.01-0.65 years (Behrman, 1997). Our IV estimates of the effect of older siblings' education on younger siblings' schooling in panels A and B lie at the lower end of this range. The key difference between the effect of parental education and that of older siblings' education is that siblings compete for limited resources, reducing the positive spillover/demonstration effect. There is no such rivalry in the case of parental education. Therefore, it is quite expected that the impact of siblings' education should be less compared to that of parental education.

[Insert Table 4 here]

Overall, the IV estimates are generally lower than OLS estimates. Hausman tests indicate that OLS estimation is generally upward biased, consistent with the omitted variable bias problem in OLS estimates. When we instrument for education, the results are more consistent - the coefficient on the gender composition of older siblings is always insignificant and does not depend on the choice of the sample or use of controls. Our IV results can be interpreted as the local average treatment effect (LATE) since we are estimating the effects of older siblings' education who were induced by the program.

[11]The ratio is similar for sample 1 and sample 2.

6. Extensions and Robust Checks

Estimates by gender of younger siblings

We next investigate whether there is any differential impact of the education and gender composition of older siblings on schooling of younger sisters versus younger brothers. Table 5 reports the IV results separately for younger sisters and younger brothers, using a full set of controls.[12] In each sub-sample, the first column corresponds to the specification without older siblings' education, while the next column reports IV estimates of education.

The gender composition of older siblings is statistically significant for three of the four specifications in panel B, if we do not control for the education of older siblings. However, when we control for the education of older siblings, we do not find any effect of the gender of older siblings. This result signifies that it is the education of older siblings that drives the result for the gender composition of older siblings when we do not control for their education. Education of older siblings has a positive effect on the schooling of younger brothers and sisters in all the sub-samples, except for younger sisters in sample 2 with older siblings aged 17-36. The results in Panel A suggest that the effects of the education of older siblings on the schooling of younger brothers and sisters are very similar. Estimates for sample 1 suggest that one additional year of the education of older siblings aged 21-32 years causes an increase in schooling by 0.24 years for younger sisters and 0.22 years for younger brothers. This result implies an increase of 16 per cent for younger sisters from their mean education of 4.07 years (ITT is 2.332) and an increase of 15 per cent for younger brothers from their mean education of 3.5 years (ITT is 2.123) due to the FSSSP. Estimates for sample 1 in panel B suggest an increase of 4 per cent (ITT is 2.635) for younger sisters from their mean education (3.9 years) compared with an increase of 8 per cent (ITT is 2.664) for younger brothers from their mean education (3.4 years) due to the FSSSP.

We do not think that older siblings' education exerts a greater demonstration effect on younger brothers than on younger sisters. However, the different result for older siblings aged 21-32 versus 17-36 might reflect gender bias among the younger cohorts. We conjectured above that rivalry among older and younger cohorts night be greater for the 17-36 sample because there were a higher number of older siblings in this age group studying at the time of the survey and therefore competing for limited resources. In this situation, younger sisters

[12] The first stage results for this, and subsequent, estimations are quite similar to the results presented in Table 3 and, hence, are not reported. The results are available upon request.

might face greater rivalry than younger brothers while competing with older siblings for limited (financial and in-kind) resources available for investment in education. If this is the case, then the net benefit from older siblings' education will be less for younger sisters than for younger brothers. This argument also seems plausible from the perspective of selective discrimination (Das Gupta, 1987) in that if older daughters receive higher education due to the stipend program, parents might be less willing to educate younger daughters. Another plausible argument might be that if there exists strong gender division of labour in the household, younger sisters bear greater responsibilities for household work compared to younger brothers when older sisters receive more education.

[Insert Table 5 here]

Estimates by education of household head

We investigate how the impact of older siblings on younger siblings varies with the education of the household head. To the extent, education and income are correlated the results can be seen as the differences in effects by income groups. We do use household income as it may be endogenous due to children's contribution to household income and/or adverse home environment (Islam and Choe, 2012). We define three levels of education obtained by household head: *Low* refers to less than primary school completion (0-4 years of schooling); *Medium* refers to completion of primary school through to completion of high school (5-10 years of schooling), and *High* refers to more than a high school degree (11 or more years of schooling). Household income plays an important role in intra-household allocation of resources among children. Given that resource constraints is the main factor contributing to sibling rivalry, we expect that the net benefit from education of older siblings on schooling of younger siblings would be less in households with lower education/income.

We report the first stage results for different sub-samples according to household head's education level in Table 6(a), using eligibility for the stipend program as an instrument with a full set of controls. The results in the first stage indicate that the FSSSP had the greatest effect on older siblings' education in households with heads having a medium level of education. This result might be due to the fact that these households are more likely to send their children to school compared to low education households and are more likely to respond to incentives offered by the FSSSP. The impact of the FSSSP is the lowest in terms of magnitude, as well as the significance level, of the coefficient, in households with a high level of education. Households with high education place high value on the education of children in the household and are also expected to have higher income. Therefore, these households are less likely to rely on the stipend program for sending their children to school.

Table 6(b) presents the second stage results for how the education, and gender composition, of older siblings effect younger siblings' schooling according to the education level of the household head. A number of notable features emerge. First, the marginal effect of older siblings' education is consistently higher if the household head has low education compared to the medium or high education households. This result is similar to Qureshi (2011), who finds a consistently larger impact of oldest sisters' schooling in households with an uneducated mother compared with an educated mother. Second, the marginal effect of education is relatively low for medium-educated households.. The reason again might be that medium educated households are more concerned about children's education than low educated households which causes a lesser education externality. Again, these households have greater resource constraint than high education households and as such, sibling rivalry is greater.

Without controlling for older siblings' education, the gender composition of older siblings has a statistically significant effect in medium-educated households in sample 1. However, the coefficient becomes statistically insignificant once we control for education of older siblings and for endogeneity of education.

[Insert Table 6 here]

Full treatment versus partial treatment

As mentioned earlier, the program did not cover students in all grades at the time of introduction. In 1994, when first introduced, the program was only available to girls in grades six and nine. And girls in grade nine in 1994 could only participate in the program if they remained in school. Considering the high drop-out rate of girls of that age at that time period, they represent a small fraction of the children of their age cohort. Hence, girls who were 12-14 years old in 1994 could be partly affected by the program. Beginning in 1996, girls in all grades in secondary schools were covered under the program. To address this partial treatment for secondary school age girl cohorts in 1994 and 1995, we distinguish between a potential partial treatment effect and full treatment effect. Our alternative instruments are a set of dummy variables indicating whether a girl was in grades seven to nine in 1994 (partial treatment) and whether the girl was in grade six or below in 1994 (full treatment). The treatment groups for exposure to the program correspond to girls falling into cohorts 1-2 (grades nine or below in 1994) and the control group corresponds to girls in cohort 3 (grades 10 or above in 1994) as well as boys in any of cohorts 1 to 3 as defined as follows:

For older siblings aged 21-32:
Treatment 1 (cohort 1) = 1, if 12≤female age in 1994 ≤14 [27≤ female age in 2009≤29]
Treatment 2 (cohort 2) = 1, if 6≤female age in 1994 ≤11 [21≤female age in 2009≤26]
Control 1 (cohort 3) =1, if 15≤female age in 1994 ≤17 [30≤ female age in 2009≤32]
Control 2 (cohort 1-3) =1, if 6≤male age in 1994 ≤17 [21≤ male age in 2009≤32]

For older siblings aged 17-36, we define analogously:

Treatment group 1 corresponds to girls in cohort 1 with two years exposure to the program. A girl in grade seven in 1994 did not participate in the program in that year or in the following year because grade eight was not included in the program in 1995, but did participate in the program in 1996 if she remained in school. A girl in grade eight in 1994 did not participate in the program in that year, but did participate in the program in 1995 if she remained in school. A girl in grade nine in 1994 participated in the program in that year as well as in 1995 if she remained in school. Treatment group 2 corresponds to girls with five years exposure to the program (same as the treatment group in our main results in section 4). A girl in grade six or below in 1994 could participate in the program for five years up to grade ten. Control group 1 corresponds to girls with no exposure; covering girls in grade ten in 1994 as well as girls who were above secondary school age. Control group 2 includes boys from all these age groups who were not eligible to participate in the program.

We estimate the first-stage regression using exposure to the program as an instrument:

$$Edu\_Old_{sij} = \theta_0 + \theta_1 B_{ij} + \theta_2 Treat\ Age + \theta_3\ Girl + \theta_4 Treat\ Age * Girl + \theta_5 C_{ij} + \theta_6 X_{sij} + \theta_7 H_j + \gamma_l + w_j + \varkappa_{ij} \quad (3)$$

Treat Age is a set of dummy variables for different age cohorts (base category is control 1 and control 2 together). The first stage regression results in all the specifications using exposure as an instrument, presented in Table 7, show that the instrument is highly significant with a positive coefficient in each specification. F-statistics of joint significance of the instruments are well above the rule of thumb value of 10 in all cases. Consistent with expectations, the magnitude of the coefficient for treatment group two, with five years of exposure, is much greater than that for treatment group one, with two years of exposure. Table 8 reports the IV estimates of the effects of the education of older siblings on their younger siblings' education. Compared with the main results in Table 4, we see that this alternative instrument gives similar results.

[Insert Tables 7 & 8 here]

Alternative measure of educational attainment

Next we examine the effect of older siblings on younger siblings' educational attainment using grade for age as an alternative measure of educational attainment of younger siblings. Following Islam and Choe (2012) and Patrinos and Psacharopoulos (1997), we define grade for age as follows:

Grade for Age= 100*[Actual grade/Expected education]

Expected education= {0 if age ≤ 6

Age-6 if 6< age ≤ 14}

Thus, if a child successfully starts and completes education, grade for age is 100. If a child experiences late entry, repeats a grade or drops out, grade for age is less than 100.

Table 9 reports the results of IV estimation for sample 1 using eligibility status as the instrument with a full set of controls.[13] The results are similar to those reported above. Education of older siblings has a positive effect on grade for age for both 21-32 and 17-36 samples. Without controlling for older siblings' education, having an older brother, instead of an older sister, has a negative effect on grade for age of younger siblings in the 17-36 sample. However, once we control for older siblings' education and address the endogeneity of education, the gender composition of older siblings is not statistically significant.

[Insert Table 9 here]

Estimates using highest education of an older sibling

We estimate regressions using the highest education attained among older siblings of the specified age, instead of the mean education of older siblings of the specified age. The

[13] For the remaining robust checks, the results for sample 1 and sample 2 are similar. Hence, we only report results for sample 1. A full set of results are available upon request.

rationale for using this alternative approach is that it is often the case, particularly in a resource-constrained household, that, the older sibling who has the highest education, bears primary responsibility for care of younger siblings and also acts as a role model for younger siblings. Column (1) of Table 10 reports the second stage results of IV estimation using eligibility status as the instrument with a full set of controls. The education of the most educated older sibling has a positive effect on the schooling of younger siblings for older siblings aged 21-32. The magnitude of the coefficient in this case is similar to that of older siblings' education presented in panel A of Table 4. The effect of older siblings' gender composition is similar to the main results presented in Table 4.

Estimates using education of older sisters and the oldest sister

As the stipend program affected older sisters, but not older brothers, we now consider the education of older sisters instead of the education of older siblings as a whole. In some households, it is the oldest sister that has the responsibility for taking care of younger siblings. Moreover, the oldest sister will have greater interaction with younger siblings if gender division of labour dictates that she is more likely to remain at home than the oldest brother because she is involved with completing household chores. Thus, some argue that the education of the oldest sister has a greater effect on the education of younger siblings than other older siblings (see, e.g., Qureshi, 2011).

Column (2) of Table 10 reports the second stage results for IV estimates using eligibility status as the instrument with a full set of controls. The results suggest that our above findings are robust with this alternative specification.

Column (3) of Table 10 reports the IV estimation for education of the oldest sister. In both the 21-32 and 17-36 samples, the oldest sister's education has a positive effect on younger siblings' schooling. Each additional year of schooling by the oldest sister increases younger siblings' schooling by 0.10 years for the 21-32 age group and 0.14 years for the 17-36 age group. Oldest sister's education has a greater effect on younger siblings' schooling in the 17-36 sample than older sister's education. This result is not surprising as there is a larger age gap between the oldest sister and her younger siblings than between older sisters and younger siblings and this results in less rivalry. The findings for education of older sister reported here are much lower than the estimates reported in Qureshi (2011), who finds that each additional year of schooling by the oldest sister increases younger brothers' schooling by 0.42 years. However, Qureshi (2011) measures the effect on younger brothers only.

[Insert Table 10 here]

Estimates with restricted samples

As mentioned earlier, our dataset contains information for members living in the household at the time of survey. This might be problematic if older siblings that are not living in the household are also affecting the education of their younger siblings. To address this issue, we restrict our sample to households where all children are living in the household. During the survey, 15-49 year old women in all households were interviewed and asked about their fertility history. We retain in the sample those households for which all living children of the mother are residing in the household and also the number of children in the household is the same as the number of children of the mother.[14] Our restricted sample contains 89,309 children. The results with this sample are reported in the first two columns of Table 11. The findings point to a much higher impact of older siblings' education on schooling of younger siblings in the 21-32 sample compared to the 17-36 sample. Older siblings' gender composition is negative and statistically insignificant in both samples once we control for education. Overall, the result is similar to that for the unrestricted sample.

Our full dataset contains a higher number of older brothers than older sisters. One might be concerned that this could potentially bias the result for older siblings' gender. In order to address this point, we restrict our sample to households where the proportion of older brothers to older sisters of a particular age group is not greater than one. As there are households with a higher number of older sisters than older brothers in this restricted sample, the sample now has at least as many older sisters as older brothers. The mean values of older brothers and older sisters for the 21-32 sample are 0.464 and 1.15, respectively. The results, reported in the third and fourth columns of Table 11, show that gender composition of older siblings is insignificant for older siblings aged 21-32 and 17-36 once older siblings' education is included. IV results suggest that older siblings' education has a positive effect on younger siblings' schooling for both 21-32 and 17-36 age groups. These results suggest that

[14] For some households, the number of siblings is greater than the number of living children of the mother. This implies that there might be siblings with the same father, but different mother. We omit those households as we cannot identify whether siblings with a different mother have any other siblings living outside the household.

our main findings were not being driven by the proportion of older brothers and sisters in the dataset.

We also construct an alternative sample with households having at least one sister of the specified age group. The sample again has a relatively higher proportion of older sisters. The mean values of older brothers and older sisters for the 21-32 sample are 0.800 and 1.13, respectively. The results, presented in the last two columns in Table 11, are similar to columns (3) and (4) and again suggest that our main findings are robust.

[Insert Table 11 here]

Omitting households with a mix of eligible and ineligible girls

One might be concerned about households with both types of older sisters coexisting in the group -eligible (21-26/17-26 years old) and ineligible (27-32/27-36 years old). Our sample indicates that only 0.3 per cent of households have both types of girl children co-existing. However, to deal with this issue, we restrict our sample omitting the households that have a mix of eligible and ineligible girls. The results in Table 12 are similar to the main results presented in Table 4.

[Insert Table 12 here]

Addressing endogeneity of sibling size

Family size is often chosen by parents and these unobserved parental preferences might also affect children's schooling. If parents have a preference for sons, they might continue to have children until they get the desired number of boys (Edmonds, 2007). Parental preferences, or greater resource competition, might also lead to a higher mortality rate for girls (Sen, 1992). The quantity-quality trade-off implies that parents who place a high premium on the education of their children are likely to have fewer children. Thus, there might be unobserved factors affecting both total number of siblings (explanatory variable) and children's schooling (dependent variable), leading to potential correlation between the error term and total number of siblings.. In order to address this issue, we use twins as an instrument for number of siblings, because having twins generates an exogenous variation in sibling size.[15]

[15] We also used an alternative instrument using the gender of the first two children (whether girls or not) considering that parents might have a preference for a son or different-gender children. We obtain similar results using this alternative instrument.

The first stage estimates for the sibling size regression includes all the children of a household that has at least one child in the sample of younger siblings. In this case, we have two endogenous regressors, sibling size and education of the older siblings, and two instruments, twins and eligibility status in the FSSSP. This first stage regression for sibling size includes all other controls used previously. Table 13(a) reports the first stage results for sibling size which suggest that twins significantly increase the size of the family. Table 13(b) reports the instrumental variable estimates for education of younger siblings. Older siblings' education has a positive effect on younger siblings' schooling, with the coefficient being slightly lower than in Table 4. The effect of gender composition is similar to that of the unrestricted sample. Overall, the findings presented in Table 13(b) suggest that our results remain robust after taking account of the endogeneity of sibling size.

[Insert Tables 13(a) and 13(b) here]

Addressing fertility effect of the program

One possibility is that the program might have exerted a direct impact on fertility. Parents might be encouraged to have more children anticipating that they would be supported for schooling. Theoretically, the effect is ambiguous. While the income effect should lead to reduction in children, the reduction of parents' opportunity cost for educating children (girls) might induce to have more children. We check whether the number of children born since 1995 (1 year after the program) is significantly different between the treatment and control group, using a dummy variable of whether any girl child in the household is eligible to receive the program (i.e., whether the household has any girl child aged 21-26/17-26 years old) as treatment variable. The control group comprises household who have no child receiving the program as they are all ineligibles. The child born in 1995 is aged 14 in 2009 when the survey was undertaken. Therefore, our outcome variable is number of children aged 14 year or below in the household. We Control for the number of children and number of boy/girl aged above 14 years (already born in 1994) together with other household characterisitics. The treatment variable, therefore, shows the mean effect of the program on fertility decision by the household.

7. Summary and Conclusion

This study has examined the role of the education and gender of older siblings on schooling of younger siblings. We use timing of the introduction of the female secondary school stipend program in Bangladesh as an instrument and compare children in households who receive the program to those who missed out as they were either boys or a bit older. We find consistent evidence that older siblings' education exerts a significant, and large, impact on younger siblings' education. When we address the endogeneity of education of older siblings, having an older brother rather than older sister, does not have any significant effect on younger siblings’ schooling. The results using IVs for education are consistent and they suggest that it is the education of older siblings, rather than their gender, that is the critical in influencing the education of younger siblings’ education. The gender does not have any independent role, rather the effect of gender is transmitted through their education. This calls for a cautious interpretation of the effect of sibling gender composition, without controlling for older siblings' education, and in particular without addressing the endogeneity of education of older siblings. The results also suggest that school programs that benefit children’s education could bring both short- and long-term gains for both affected children and their siblings. Having identified that the education of older siblings has an external effect on schooling of younger siblings, future research could focus on other external effects of older siblings education in the household, such as health. Research using datasets with full sibling history as well as for other countries are other possible avenues for future research.

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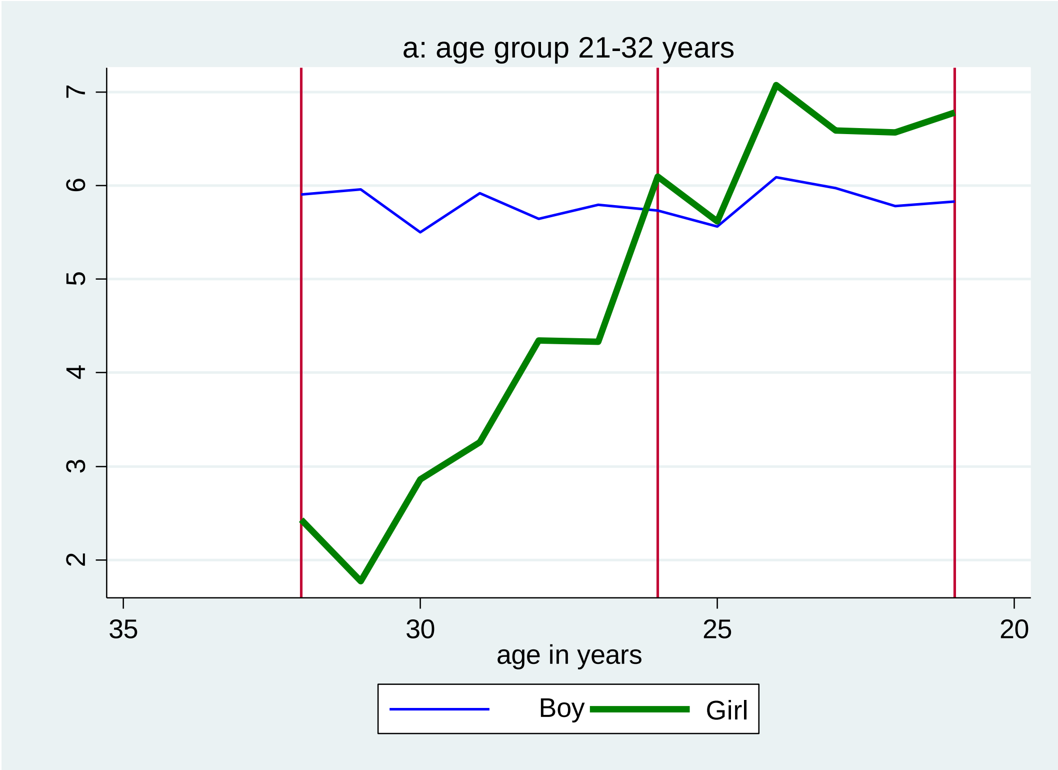
Figure 1: Older siblings' age and completed years of education



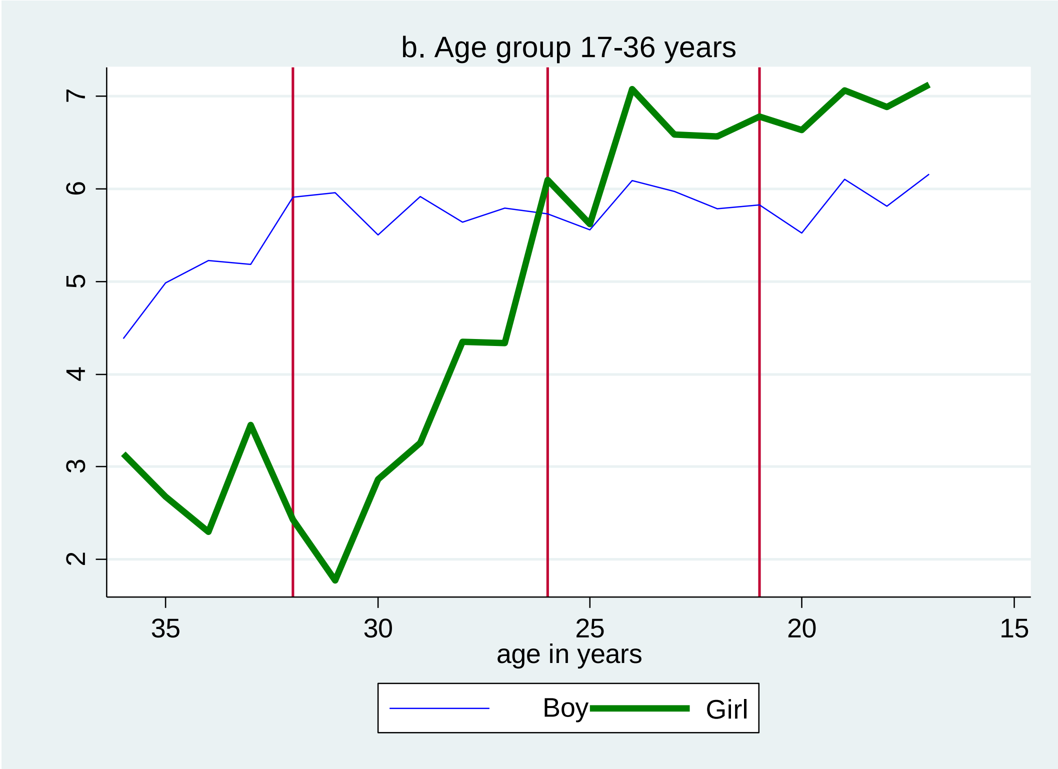


Table 1: Key Descriptive Statistics of Treatment and Control Group for Older Age Group 21-32

	Treatment		Control					
	Treatment		Total		Girl		Boy	
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
I. Individual Characteristics:								
Age (in years)	23.6	2.73	16.4	6.94	12.3	4.68	18.2	7.02
Gender (girl=1)	1.00	---	0.315	---	---	---	---	---
Years of education	6.09	4.50	4.65	3.66	4.30	3.27	4.81	3.81
II. Siblings Characteristics:								
Number of siblings	3.34	1.60	3.47	1.60	3.59	1.63	3.42	1.59
Number of brothers	1.92	1.31	2.25	1.32	2.38	1.27	2.20	1.34
Number of sisters	1.42	1.16	1.22	1.10	1.21	1.16	1.22	1.07
Number of younger siblings aged 7-14	1.55	0.781	1.76	0.915	1.88	0.962	1.70	0.887
Number of younger brothers aged 7-14	0.751	0.724	0.857	0.799	0.590	0.730	0.979	0.800
Number of younger sisters aged 7-14	0.794	0.754	0.899	0.813	1.29	0.800	0.717	0.752
Age of younger siblings aged 7-14	11.6	1.87	11.4	1.78	11.36	1.75	11.4	1.80
Age of younger brothers aged 7-14	11.3	2.02	11.1	1.97	10.90	2.02	11.2	1.95
Age of younger sisters aged 7-14	11.6	2.10	11.5	2.05	11.42	1.99	11.5	2.09
Years of education by younger siblings aged 7-14	11.6	1.87	11.4	1.78	11.36	1.75	11.4	1.80
Years of education by younger brothers aged 7-14	11.3	2.02	11.1	1.97	10.90	2.02	11.2	1.95
Years of education by younger sisters aged 7-14	11.6	2.10	11.5	2.05	11.4	1.99	11.5	2.09
III. Household/parental Characteristics:								
Gender of household head (female=1)	0.107	---	0.073	---	0.073	---	0.073	---
Years of education by household head	3.86	4.33	3.24	3.93	3.26	3.95	3.23	3.92

Note: The descriptive statistics correspond to rural households with children aged 7-14 years who have at least one older sibling aged 21-32 years.

Table 2: OLS estimates: the effects of gender and education of older siblings

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: 21-32 years old								
Sample 1								
Number of older brothers	-0.045	0.018	-0.063*	0.005	0.024	0.034	0.004	0.023
	(0.037)	(0.033)	(0.037)	(0.033)	(0.035)	(0.033)	(0.035)	(0.033)
Older siblings' education		0.206***		0.201***		0.170***		0.163***
		(0.003)		(0.003)		(0.004)		(0.004)
Observations			29,268	27,475	29,268	27,475	29,268	27,475
R-squared			0.401	0.493	0.450	0.500	0.463	0.506
Sample 2								
Number of older brothers	-0.072	-0.081	-0.106	-0.099	-0.013	-0.067	-0.034	-0.071
	(0.072)	(0.063)	(0.071)	(0.063)	(0.067)	(0.063)	(0.067)	(0.063)
Older siblings' education		0.246***		0.234***		0.198***		0.188***
		(0.006)		(0.006)		(0.007)		(0.007)
Observations	7,636	7,636	7,636	7,636	7,636	7,636	7,636	7,636
R-squared	0.265	0.395	0.291	0.405	0.353	0.417	0.377	0.432
Panel B: 17-36 years old								
Sample 1								
Number of older brothers	-0.102***	0.085***	-0.126***	0.065***	-0.034**	0.077***	-0.043**	0.067***
	(0.019)	(0.016)	(0.018)	(0.016)	(0.0172)	(0.016)	(0.017)	(0.016)
Older siblings' education		0.236***		0.227***		0.197***		0.190***
		(0.002)		(0.002)		(0.003)		(0.003)
Observations	63,919	61,432	63,919	61,432	63,919	61,432	63,919	61,432
R-squared	0.398	0.512	0.414	0.518	0.462	0.524	0.474	0.529
Sample 2								
Number of older brothers	-0.147***	0.082***	-0.175***	0.057**	-0.058**	0.070***	-0.075***	0.055**
	(0.027)	(0.023)	(0.027)	(0.024)	(0.025)	(0.023)	(0.025)	(0.024)
Older siblings' education		0.289***		0.277***		0.242***		0.232***
		(0.003)		(0.004)		(0.004)		(0.004)
Observations	28,678	28,678	28,678	28,678	28,678	28,678	28,678	28,678
R-squared	0.258	0.426	0.285	0.434	0.355	0.444	0.373	0.452
Control for education	No	Yes	No	Yes	No	Yes	No	Yes
Individual control	Yes		Yes		Yes		Yes	
Sibling control	No		Yes		Yes		Yes	
Household/area control	No		No		Yes		Yes	
District FE	No		No		No		Yes	

Notes: ***, **, * indicate significant at 1, 5, and 10 percent level, respectively. Standard errors in parentheses are corrected for clustering at the household level. The dependent variable is completed years of schooling of younger siblings (excluding oldest sibling) aged 7-14 years. Sample 1: Children aged 7-14 who have *any* older sibling in the 21-32 (17-36) years age group. These children may also have older siblings outside this age group. Sample 2: Children with *all* older siblings within the specified age groups. These children do not have older siblings outside this age group.

Table 3: First stage regression: eligibility for FSSSP and the education of older siblings

	(1)	(2)	(3)	(4)
Panel A: 21-32 years old				
Sample 1				
Eligible	2.880***	2.613***	2.231***	2.242***
	(0.224)	(0.224)	(0.192)	(0.191)
F Stat	165.98	136.06	135.10	137.88
Observations	23,642	23,642	23,642	23,642
R-squared	0.0105	0.030	0.257	0.278
Sample 2				
Eligible	2.895***	2.599***	2.232***	2.247***
	(0.233)	(0.232)	(0.200)	(0.199)
F Stat	154.78	125.02	124.54	127.49
Observations	22,732	22,732	22,732	22,732
R-squared	0.011	0.031	0.260	0.281
Panel B: 17-36 years old				
Sample 1				
Eligible	3.129***	2.819***	2.643***	2.639***
	(0.189)	(0.189)	(0.160)	(0.160)
F Stat	274.07	223.49	272.76	272.75
Observations	57,097	57,097	57,097	57,097
R-squared	0.020	0.0432	0.239	0.261
Sample 2				
Eligible	3.151***	2.840***	2.669***	2.667***
	(0.192)	(0.192)	(0.163)	(0.163)
F Stat	270.07	218.82	268.38	268.38
Observations	56,384	56,384	56,384	56,384
R-squared	0.0198	0.0436	0.240	0.261
Individual control	Yes	Yes	Yes	Yes
Sibling control	No	Yes	Yes	Yes
Household/area control	No	No	Yes	Yes
District FE	No	No	No	Yes

Notes: ***, **, * indicate significant at 1, 5, and 10 percent level, respectively. Standard errors in parentheses are corrected for clustering at household level. Eligible is the interaction of two binary variables: Eligible Age and girl. The dependent variable is completed years of schooling of older siblings with different age range having at least one younger sibling aged 7-14 years. Sample 1 and Sample 2 consist of older siblings corresponding to the children included in the younger samples : as defined in notes in Table 2.

Table 4: IV estimates: eligibility for FSSSP, education of older siblings and its effects on their younger siblings' education

	(1)	(2)	(3)	(4)
Panel A: 21-32 years old				
Sample 1				
Number of older brothers	0.003	0.020	0.034	0.025
	(0.035)	(0.034)	(0.033)	(0.033)
Older siblings' education	0.143***	0.256***	0.228***	0.222***
	(0.035)	(0.023)	(0.034)	(0.036)
Observations	27,475	27,475	27,475	27,475
R-squared	0.487	0.493	0.500	0.508
Hausman Test p-value	0.034	0.006	0.0460	0.055
Sample 2				
Number of older brothers	-0.082	-0.099	-0.060	-0.068
	(0.064)	(0.064)	(0.064)	(0.064)
Older siblings' education	0.245***	0.239***	0.168***	0.143**
	(0.056)	(0.044)	(0.061)	(0.064)
Observations	7,347	7,347	7,347	7,347
R-squared	0.407	0.417	0.425	0.439
Hausman Test p-value	0.000	0.000	0.000	0.000
Panel B: 17-36 years old				
Sample 1				
Number of older brothers	-0.008	0.009	0.023	0.009
	(0.021)	(0.019)	(0.018)	(0.019)
Older siblings' education	0.091***	0.146***	0.073***	0.063***
	(0.023)	(0.016)	(0.022)	(0.023)
Observations	61,432	61,432	61,432	61,432
R-squared	0.512	0.518	0.525	0.530
Hausman Test p-value	0.000	0.000	0.000	0.000
Sample 2				
Number of older brothers	-0.019	-0.029	-0.012	-0.030
	(0.031)	(0.028)	(0.027)	(0.028)
Older siblings' education	0.158***	0.172***	0.077**	0.066*
	(0.032)	(0.024)	(0.033)	(0.035)
Observations	27,811	27,811	27,811	27,811
R-squared	0.438	0.445	0.452	0.461
Hausman Test p-value	0.000	0.000	0.000	0.000
Individual control	Yes	Yes	Yes	Yes
Sibling control	No	Yes	Yes	Yes
Household/area control	No	No	Yes	Yes
District FE	No	No	No	Yes

Notes: ***, **, * indicate significant at 1, 5, and 10 percent level, respectively. Standard errors in parentheses are corrected for clustering at household level. The dependent variable is completed years of schooling of younger siblings (excluding oldest sibling) aged 7-14 years. Sample 1: Children aged 7-14 who have *any* older sibling in the 21-32 (17-36) years age group. These children may also have older siblings outside this age group. Sample 2: Children with *all* older siblings within the specified age groups. These children do not have older siblings outside this age group.

Table 5: Estimates by gender of younger siblings

	Girl (7-14 years) Sample		Boy (7-14 years) Sample	
	(1)	(2)	(3)	(4)
nstrument for education	No	Yes	No	Yes
Panel A: 21-32 years old				
Sample 1				
Number of older brothers	-0.012	0.015	0.020	0.030
	(0.046)	(0.043)	(0.047)	(0.046)
Older siblings' education		0.236***		0.220***
		(0.045)		(0.046)
Observations	15,035	14,096	14,233	13,379
R-squared	0.490	0.536	0.426	0.467
Sample 2				
Number of older brothers	0.0260	-0.024	-0.124	-0.143
	(0.091)	(0.085)	(0.097)	(0.097)
Older siblings' education		0.149*		0.148*
		(0.086)		(0.089)
Observations	4,243	4,091	3,393	3,256
R-squared	0.385	0.449	0.358	0.417
Panel B: 17-36 years old				
Sample 1				
Number of older brothers	-0.039*	0.018	-0.048**	0.003
	(0.023)	(0.026)	(0.022)	(0.026)
Older siblings' education		0.050*		0.099***
		(0.030)		(0.029)
Observations	32,140	30,841	31,779	30,591
R-squared	0.505	0.561	0.434	0.490
Sample 2				
Number of older brothers	-0.046	-0.026	-0.100***	-0.023
	(0.035)	(0.039)	(0.036)	(0.040)
Older siblings' education		0.016		0.143***
		(0.049)		(0.046)
Observations	15,267	14,801	13,411	13,010
R-squared	0.385	0.474	0.346	0.434
Control for Education	No	Yes	No	Yes
Other controls	Yes	Yes	Yes	Yes

Notes: ***, **, * indicate significant at 1, 5, and 10 percent level, respectively. Standard errors in parentheses are corrected for clustering at household level. The dependent variable is completed years of schooling of younger siblings (excluding oldest sibling) aged 7-14 years. Education of older siblings is instrumented using FSSSP except for columns 1 and 4, where we do not control for older siblings' education. All specifications are estimated with a full set of controls, including controls for individual characteristics, siblings and household variables and district fixed effects. Sample 1: Children aged 7-14 who have *any* older sibling in the 21-32 (17-36) years age group. These children may also have older siblings outside this age group. Sample 2: Children with *all* older siblings within the specified age groups. These children do not have older siblings outside this age group.

Table 6a: First stage regression: by education of household head

	(1)	(2)	(3)
		Education of Household Head	
	Low Education	**Medium Education**	**High Education**
Panel A: 21-32 years old			
Sample 1			
Eligible	1.976***	2.844***	1.558*
	(0.219)	(0.405)	(0.899)
F Stat	81.01	49.27	3.00
Observations	14,646	7,880	1,033
R-squared	0.125	0.121	0.052
Sample 2			
Eligible	1.953***	2.891***	1.765*
	(0.231)	(0.415)	(0.975)
F Stat	71.49	48.44	3.28
Observations	14,091	7,573	990
R-squared	0.127	0.123	0.137
Other controls	Yes	Yes	Yes

Notes: ***, **, * indicate significant at 1, 5, and 10 percent level, respectively. Standard errors in parentheses are corrected for clustering at household level. The dependent variable is completed years of schooling of older siblings with different age range having at least one younger sibling aged 7-14 years. Sample 1 and Sample 2 consist of older siblings corresponding to the children included in the younger samples.*Low:* 0-4 years of schooling; *Medium:* 5-10 years of schooling; *High:* 11 or more years of schooling.

Table 6b: Estimates by education of household head

	Low Education		**Medium Education**		**High Education**	
	(1)	(2)	(3)	(4)	(5)	(6)
Older siblings: 21-32 years old						
Sample 1						
Number of older brothers	0.069	0.036	-0.098*	-0.036	0.033	0.056
	(0.048)	(0.045)	(0.053)	(0.055)	(0.111)	(0.107)
Older siblings' education		0.268***		0.121**		0.239*
		(0.047)		(0.054)		(0.130)
Observations	18,809	17,689	9,288	8,704	1,037	990
R-squared	0.373	0.435	0.561	0.585	0.673	0.666
Sample 2						
Number of older brothers	0.057	-0.047	-0.101	-0.0818	-0.244	-0.231
	(0.094)	(0.091)	(0.097)	(0.099)	(0.242)	(0.263)
Older siblings' education		0.221**		0.045		0.149
		(0.089)		(0.091)		(0.205)
Observations	4,860	4,679	2,457	2,361	283	281
R-squared	0.301	0.381	0.426	0.464	0.585	0.728
Control for Education	No	Yes	No	Yes	No	Yes
Other controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: ***, **, * indicate significant at 1, 5, and 10 percent level, respectively. Standard errors in parentheses are corrected for clustering at household level. The dependent variable is completed years of schooling of younger siblings (excluding oldest sibling) aged 7-14 years. All specifications are estimated with a full set of controls. Sample 1: Children aged 7-14 who have *any* older sibling in the 21-32 (17-36) years age group. These children may also have older siblings outside this age group. Sample 2: Children with *all* older siblings within the specified age groups. These children do not have older siblings outside this age group.

Table 7: First stage regression: Variation in exposure to FSSSP and the education of older siblings

	(1)	(2)	(3)	(4)
Panel A: 21-32 years old				
Sample 1				
Treat 1	1.428***	1.166***	1.430***	1.414***
	(0.396)	(0.399)	(0.339)	(0.340)
Treat 2	3.731***	3.311***	3.086***	3.088***
	(0.309)	(0.314)	(0.263)	(0.263)
F Stat	96.03	76.10	84.04	85.33
Observations	23,642	23,642	23,642	23,642
R-squared	0.012	0.030	0.256	0.279
Sample 2				
Treat 1	1.623***	1.304***	1.640***	1.604***
	(0.410)	(0.413)	(0.351)	(0.353)
Treat 2	3.889***	3.400***	3.239***	3.233***
	(0.320)	(0.327)	(0.272)	(0.275)
F Stat	94.16	72.27	83.47	83.40
Observations	22,732	22,732	22,732	22,732
R-squared	0.012	0.0299	0.260	0.282
Panel B: 17-36 years old				
Sample 1				
Treat 1	1.112***	0.794**	1.156***	1.172***
	(0.360)	(0.359)	(0.304)	(0.304)
Treat 2	3.673***	3.209***	3.211***	3.214***
	(0.250)	(0.249)	(0.204)	(0.204)
F Stat	147.27	117.71	156.90	156.66
Observations	57,097	57,097	57,097	57,097
R-squared	0.020	0.0433	0.240	0.261
Sample 2				
Treat 1	1.197***	0.855**	1.239***	1.227***
	(0.364)	(0.364)	(0.308)	(0.308)
Treat 2	3.746***	3.266***	3.287***	3.279***
	(0.252)	(0.254)	(0.206)	(0.208)
F Stat	147.95	116.44	158.48	156.77
Observations	56,384	56,384	56,384	56,384
R-squared	0.0200	0.044	0.240	0.261
Individual control	Yes	Yes	Yes	Yes
Sibling control	No	Yes	Yes	Yes
Household/area control	No	No	Yes	Yes
District FE	No	No	No	Yes

Notes: ***, **, * indicate significant at 1, 5, and 10 percent level, respectively. Standard errors in parentheses are corrected for clustering at household level. The dependent variable is completed years of schooling of older siblings with different age range having at least one younger sibling aged 7-14 years. Sample 1: Children aged 7-14 who have *any* older sibling in the 21-32 (17-36) years age group. These children may also have older siblings outside this age group. Sample 2: Children with *all* older siblings within the specified age groups. These children do not have older siblings outside this age group.

Table 8: IV estimates: Variation in exposure to FSSSP, education of older siblings and its effects on their younger siblings' education

	(1)	(2)	(3)	(4)
Panel A: 21-32 years old				
Sample 1				
Number of older brothers	0.073***	0.018	0.034	0.024
	(0.022)	(0.034)	(0.033)	(0.033)
Older siblings' education	0.154***	0.250***	0.215***	0.208***
	(0.033)	(0.023)	(0.034)	(0.035)
Observations	27,475	27,475	27,475	27,475
R-squared	0.487	0.493	0.501	0.508
Hausman Test p-value	0.0173	0.0127	0.1109	0.0547
Sample 2				
Number of older brothers	0.004	-0.100	-0.0571	-0.067
	(0.041)	(0.064)	(0.064)	(0.064)
Older siblings' education	0.236***	0.227***	0.152***	0.132**
	(0.052)	(0.042)	(0.056)	(0.058)
Observations	7,347	7,347	7,347	7,347
R-squared	0.407	0.417	0.425	0.439
Hausman Test p-value	0.000	0.000	0.000	0.000
Panel B: 17-36 years old				
Sample 1				
Number of older brothers	0.033***	0.009	0.024	0.010
	(0.013)	(0.019)	(0.018)	(0.019)
Older siblings' education	0.116***	0.146***	0.074***	0.064***
	(0.019)	(0.016)	(0.022)	(0.023)
Observations	61,432	61,432	61,432	61,432
R-squared	0.512	0.518	0.525	0.530
Hausman Test p-value	0.000	0.000	0.000	0.000
Sample 2				
Number of older brothers	0.023	-0.028	-0.010	-0.027
	(0.018)	(0.028)	(0.027)	(0.028)
Older siblings' education	0.179***	0.172***	0.081**	0.071**
	(0.027)	(0.024)	(0.033)	(0.034)
Observations	27,811	27,811	27,811	27,811
R-squared	0.438	0.445	0.452	0.461
Hausman Test p-value	0.000	0.000	0.000	0.000
Individual control	Yes	Yes	Yes	Yes
Sibling control	No	Yes	Yes	Yes
Household/area control	No	No	Yes	Yes
District FE	No	No	No	Yes

Notes: ***, **, * indicate significant at 1, 5, and 10 percent level, respectively. Standard errors in parentheses are corrected for clustering at household level. The dependent variable is completed years of schooling of younger siblings (excluding oldest sibling) aged 7-14 years. Sample 1: Children aged 7-14 who have *any* older sibling in the 21-32 (17-36) years age group. These children may also have older siblings outside this age group. Sample 2: Children with *all* older siblings within the specified age groups. These children do not have older siblings outside this age group.

Table 9: Alternative measure of education (Grade for Age) of younger siblings

	(1)	(2)
Panel A: 21-32 years old		
Number of older brothers	0.545	0.939
	(0.609)	(0.584)
Older siblings' education		3.331***
		(0.651)
Observations	29,268	27,475
R-squared	0.124	0.189
Panel B: 17-36 years old		
Number of older brothers	-0.719**	0.334
	(0.305)	(0.348)
Older siblings' education		1.410***
		(0.445)
Observations	63,919	61,432
R-squared	0.127	0.207
Control for Education	No	Yes
Other controls	Yes	Yes

Notes: ***, **, * indicate significant at 1, 5, and 10 percent level, respectively. Standard errors in parentheses are corrected for clustering at household level. The dependent variable is completed years of schooling of younger siblings (excluding oldest sibling) aged 7-14 years. Education of older siblings is instrumented using FSSSP in column 2, where we control for older siblings' education. All specifications are estimated with a full set of controls, The estimates are based on sample 1.

Table 10: IV estimates: using alternative measures of education of older siblings

	(1)	(2)	(3)
	Highest Education of an older sibling	**Older Sisters' Education**	**Oldest Sister's Education**
Panel A: 21-32 years old			
Number of older brothers	0.017	0.009	0.013
	(0.033)	(0.124)	(0.124)
Sibling's education	0.216***	0.100***	0.103***
	(0.035)	(0.037)	(0.033)
Observations	28,609	4,514	4,512
R-squared	0.505	0.541	0.541
Control for education	Yes	Yes	Yes
Other controls	Yes	Yes	Yes

Notes: ***, **, * indicate significant at 1, 5, and 10 percent level, respectively. Standard errors in parentheses are corrected for clustering at household level. The dependent variable is completed years of schooling of younger siblings (excluding oldest sibling) aged 7-14 years. Education of older siblings is instrumented using FSSSP. All specifications are estimated with a full set of controls. The estimates are based on sample 1.

Table 11: Estimates with restricted samples

	All siblings living in the household		Older brothers ≤ Older sisters		Older sisters>0	
	(1)	(2)	(3)	(4)	(5)	(6)
Instrument	No	Yes	No	Yes	No	Yes
Panel A: 21-32 years old						
Number of older brothers	-0.067	0.077	0.019	0.045	-0.027	-0.032
	(0.056)	(0.058)	(0.159)	(0.145)	(0.138)	(0.126)
Older siblings' education		0.323***		0.088**		0.102**
		(0.060)		(0.039)		(0.041)
Observations	11,168	10,712	4,203	3,960	4,605	4,303
R-squared	0.504	0.542	0.491	0.538	0.492	0.549
Control for education	No	Yes	No	Yes	No	Yes
Other controls	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: 17-36 years old						
Number of older brothers	-0.079***	-0.017	-0.107*	-0.031	-0.096*	-0.032
	(0.024)	(0.034)	(0.062)	(0.056)	(0.050)	(0.045)
Older siblings' education		0.062*		0.050*		0.051*
		(0.038)		(0.027)		(0.028)
Observations	32,240	31,305	16,941	16,420	19,272	18,559
R-squared	0.505	0.556	0.492	0.556	0.491	0.557
Control for education	No	Yes	No	Yes	No	Yes
Other controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: ***, **, * indicate significant at 1, 5, and 10 percent level, respectively. Standard errors in parentheses are corrected for clustering at household level. The dependent variable is completed years of schooling of younger siblings (excluding oldest sibling) aged 7-14 years. Education of older siblings is instrumented using FSSSP except for column 1, 3 and 5, where we do not control for older siblings' education. All specifications are estimated with a full set of controls,. The estimates are based on sample 1.

Table 12: IV estimates: Omitting households with a mix of eligible and ineligible girls

	(1)	(2)
Panel A: 21-32 years old		
Sample 1		
Number of older brothers	-0.008	0.022
	(0.035)	(0.033)
Older siblings' education		0.215***
		(0.035)
Observations	29,184	27,398
R-squared	0.462	0.508
Hausman Test p-value		
Sample 2		
Number of older brothers	-0.047	-0.077
	(0.067)	(0.065)
Older siblings' education		0.137**
		(0.063)
Observations	7,620	7,332
R-squared	0.378	0.439
Hausman Test p-value		
Panel B: 17-36 years old		
Sample 1		
Number of older brothers	-0.047***	0.005
	(0.017)	(0.019)
Older siblings' education		0.058**
		(0.022)
Observations	63,723	61,250
R-squared	0.474	0.530
Hausman Test p-value		
Sample 2		
Number of older brothers	-0.082***	-0.033
	(0.025)	(0.028)
Older siblings' education		0.063*
		(0.034)
Observations	28,593	27,730
R-squared	0.372	0.459
Hausman Test p-value		
Control for education	No	Yes
Other controls	Yes	Yes

Notes: ***, **, * indicate significant at 1, 5, and 10 percent level, respectively. Standard errors in parentheses are corrected for clustering at household level. The dependent variable is completed years of schooling of younger siblings (excluding oldest sibling) aged 7-14 years. Sample 1: Children aged 7-14 who have *any* older sibling in the 21-32 (17-36) years age group. These children may also have older siblings outside this age group. Sample 2: Children with *all* older siblings within the specified age groups. These children do not have older siblings outside this age group.

Table 13a: First stage regression for number of siblings and older siblings' education

	First stage I	First stage II
	(1)	(2)
Dependent variable (Instrument)	Sibling size (Twin)	Older siblings' education (FSSSP eligibility)
Panel A: 21-32 years old		
Coefficient	0.292***	2.249***
	(0.069)	(0.190)
F Stat		
Observations	73,513	23,642
R-squared	0.452	0.278
Panel B: 17-36 years old		
Coefficient	0.300***	2.656***
	(0.048)	(0.160)
F Stat		
Observations	145,346	57,097
R-squared	0.480	0.259
Other controls	Yes	Yes

Notes: ***, **, * indicate significant at 1, 5, and 10 percent level, respectively. Standard errors in parentheses are corrected for clustering at household level. The dependent variable is the number of siblings for households included in the older and younger sample, i.e., having at least one child aged 7-14 years and also at least one child aged 21-32 (17-36) years. The regression includes all the children of the household. Specifications correspond to the fullest set of controls. The estimates are based on sample 1. The sibling size regression includes all the children of a household.

Table 13b: IV estimates: the effects of sibling size and their education on younger siblings (instrumenting for sibling size and education)

	(1)	(2)
Panel A: 21-32 years old		
Number of older brothers	0.007	0.025
	(0.035)	(0.033)
Older siblings' education		0.183***
		(0.036)
Observations	29,268	27,475
R-squared	0.462	0.507
Panel B: 17-36 years old		
Number of older brothers	-0.041**	0.004
	(0.017)	(0.019)
Older siblings' education		0.045*
		(0.024)
Observations	63,919	61,432
R-squared	0.472	0.530
Control for Education	No	Yes
Other controls	Yes	Yes

Notes: ***, **, * indicate significant at 1, 5, and 10 percent level, respectively. Standard errors in parentheses are corrected for clustering at household level. The dependent variable is completed years of schooling of younger siblings (excluding oldest sibling) aged 7-14 years. Education of older siblings is instrumented using FSSSP except for column 1, where we do not control for older siblings' education. All specifications are estimated with a full set of controls. The estimates are based on sample 1.

Appendix Table 1: Effect of Stipend program on ineligible siblings

	Boy (21-32/17-36 years old)	Ineligible girl (27-32/27-36 years old)
	(1)	(2)
Panel A: 21-32 years old		
Sample 1		
Have age-eligible sister	0.302	1.151
	(0.257)	(1.123)
Observations	20,493	468
R-squared	0.265	0.301
Sample 2		
Have age-eligible sister	0.097	1.155
	(0.235)	(1.232)
Observations	19,726	426
R-squared	0.264	0.441
Panel B: 17-36 years old		
Sample 1		
Have age-eligible sister	0.216	-0.392
	(0.135)	(0.665)
Observations	44,242	575
R-squared	0.247	0.441
Sample 2		
Have age-eligible sister	0.256*	-0.231
	(0.136)	(0.676)
Observations	43,697	547
R-squared	0.246	0.454
Individual control	Yes	Yes
Sibling control	Yes	Yes
Household/area control	Yes	Yes
District FE	Yes	Yes

Notes: * indicate significant at 10 percent level. Standard errors in parentheses are corrected for clustering at household level. The dependent variable is completed years of schooling of ineligible older siblings mentioned in the respective columns. Sample 1 and Sample 2 consist of older siblings corresponding to the children included in the younger samples : as defined in notes in Table 4.