The Journal of Development Studies, 2017
Vol. 53, No. 11, 1882–1898, https://doi.org/10.1080/00220388.2016.1277020

Girl Power: Stipend Programs and the Education of Younger Siblings

LUTFUNNAHAR BEGUM, ASAD ISLAM & RUSSELL SMYTH
Department of Economics, Monash University, Caulfield East, Australia

(Original version submitted June 2016; final version accepted December 2016)

ABSTRACT *We examine the effects of the schooling of girls on the education of their younger siblings. To examine the causal effect of the education of older children on their younger siblings, we use the introduction of a gender-targeted conditional cash transfer program – the Female Secondary School Stipend Program (FSSSP) in Bangladesh. We find that an additional year of schooling of older siblings increases schooling of their younger siblings by 0.13 years. The stipend program accounts for about a 10 per cent increase in the schooling of younger siblings. The results suggest that stipend programs can bring both short- and long-term gains, not only via direct benefits to affected children, but also via indirect benefits to their siblings.*

1. Introduction

Policies regarding the education of children have important long-term consequences for the welfare of the children involved. In households which face credit constraints, not all children have the same opportunities to attend school. In such households, siblings become rivals and their gender and birth order become important determinants of investment in human capital. In such situations, households often under-invest in daughters' education (Pande, 2003). In response, organisations such as UNICEF (2015) have called for policies to redress underinvestment in girls' human capital and improve female empowerment through education. Some of the benefits of investing in girls' education are well established. An important question that remains is: Do programs that target girls with the purpose of increasing female education have additional benefits in the form of increased education of siblings?

The main objective of the paper is to examine the effects of increased schooling of girls in rural Bangladesh on the education of their younger siblings. In order to examine the causal effect of the education of older children on their younger siblings, we use the introduction of a gender-targeted conditional cash transfer program – the Female Secondary School Stipend Program (FSSSP) in Bangladesh. The FSSSP was targeted towards girls (in rural area), but not boys from the same cohort. It generated exogenous variation in the schooling of a cohort of children in rural Bangladesh. We are, thus, able to estimate the effects of the older siblings' education on the education of their younger siblings using an instrumental variable (IV) approach. We use an interaction of gender and geographical targeting criteria, as well as the timing of the introduction of the FSSSP, to instrument for the education of older siblings. A secondary objective of the paper is to estimate the indirect effect of the FSSSP by examining its effects on younger siblings' education. We argue that the main channel

Correspondence Address: Asad Islam, Department of Economics, Monash University, PO Box 197, Caulfield East, VIC 3145, Australia. Email: Asadul.Islam@monash.edu
An Online Appendix is available for this article which can be accessed via the online version of this journal available at https://doi.org/10.1080/00220388.2016.1277020.

through which the FSSSP affects the education of younger siblings is through its effects on the older siblings' education.

The FSSSP is one of the earliest stipend programs targeted at girls in secondary school in developing countries. The long time period since its introduction in 1994 allows us to examine the effects on younger siblings of the girls who directly benefitted from the program. The FSSSP offered a stipend and tuition subsidy to girls in rural areas who were enrolled in secondary school (grades 6–10) and met certain condition.[1] Our identification strategy relies on comparing the education of older sisters who missed the program marginally to those who were eligible to receive the FSSSP. We consider girls who received the stipend in the first few years after the program was introduced. Specifically, a girl who was aged 11 or less (in grades 6 or less) in 1994 was eligible for the FSSSP as she reached secondary school age. However, her older sisters, aged 12 or older in 1994, were ineligible because they were already in (or dropped out of) secondary school before the program started. We compare educational outcomes of 6–11 year old older sisters to 12–17 year old older sisters in 1994. Since the effect of having, for example, a 15-year old older sister versus having a 7-year old older sister is likely to be different, we include, as a second comparison, boys (brothers) of the same age (all of whom are ineligible) to control for the age effect.

Our results indicate that the education of older siblings has a significant, and large, impact on the education of their younger siblings. Our results show that there is an additional channel through which stipend programs for girls can benefit younger siblings within the household. The instrumental variable (IV) estimates suggests that younger siblings' years of schooling increased by around 0.13 years for each additional year of schooling of their older siblings. The results suggest that the FSSSP increased the schooling of affected cohorts by about 2.6 years. This implies about a 10 per cent increase in the schooling of younger siblings if their older siblings were eligible for the stipend program. Overall, the results suggest that stipend programs, such as the FSSSP, could generate both short- and long-term gains, not only via direct benefits to the affected children, but importantly also via indirect benefits to their siblings.

The FSSSP in Bangladesh itself is of particular interest for at least three reasons. First, the program was introduced before many of the Latin American cash transfer programs that have been the focus of much investigation in the literature. This is important as it allows for an analysis of the longer-term effects of the program. Second, the FSSSP was implemented in a very poor setting, compared to many of the Latin American programs, which were implemented in settings in which households had higher per capita incomes. Third, relative to the Latin American programs, transfers made by the FSSSP were miniscule, not only in absolute levels (which makes sense, given the much lower income levels in Bangladesh than in, say, Mexico), but also as a proportion of household income. In most of the Latin American programs, transfer income was equivalent to 10–20 per cent of the average pre-transfer income of recipients. On the other hand, transfers made by the FSSSP were roughly equivalent to 1 per cent of the average rural household income at that time.

The paper complements to a recent study by Hahn, Islam, Nuzhat, Smyth, and Yang (2016) which examined the effects of the FSSSP on labour market and marital outcomes of the eligible girls. The authors find that the stipend recipient girls were more likely to get married later and have fewer children. These students also had more autonomy in making decisions about household purchases, health care and visiting relatives. They were more likely to work in the formal sector than the agricultural or informal sector. Stipend eligible women were likely to marry more educated husbands, who had better occupations and were closer in age to their own. Their children's health outcomes also improved. The present study focuses on siblings' outcomes using the same program. This study adds to the few studies in the literature that have examined the role of inter-sibling education externalities. However, no studies address the relative influence of older siblings' gender and education on younger siblings' schooling. Our study also adds to a growing number of recent studies examining the indirect effects of programs, such as PROGRESA in Mexico and Red de Proteccion Sol in Nicaragua. Our focus, though, is on a secondary school program of this sort, for which there has been relatively little research done in developing countries. As such, the findings of the study have a wider relevance for developing countries other than Bangladesh, which are implementing similar stipend programs.

2. Gender and Education of Older Siblings

2.1. Conceptual Arguments

Neoclassical models of intra-household human capital investment (Becker & Tomes, 1976) presume that, if markets are complete, with no credit constraints, and there are no parental preferences for equality of earnings, parents will 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 will not play any role in the educational attainments of a child. However, if parents have an aversion to earnings inequality among children, the sibling gender composition can affect the children's educational attainments even if there is no borrowing constraint (Behrman, Pollak, & Taubman, 1982). Hence, the sibling gender composition influences a child's education, and this influence is likely to be greater in more resource-constrained households. Under such circumstances, each child's education depends not only on his or her own rate of return, but also on the number, and gender composition, of the siblings who share the family's limited resources.

Birth order can also be important in influencing the education opportunities available to siblings. In particular, the education of a child might be affected by the education of their older siblings, although the effects are not clear-cut. There are a number of possible ways this could work. One positive effect could result from a spillover or demonstration effect from the older sibling to the younger siblings. A second positive effect might result from the better-educated older siblings having more labor market opportunities, meaning that they would be 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, the existence of older siblings might reduce the education of younger siblings if they are competing for very limited resources and the household is credit constrained.

2.2. Empirical Evidence

There is extensive empirical evidence that conditional cash transfer programs have a positive effect on the schooling outcomes of treatment groups (Schultz, 2004; De Janvry, Finan, Sadoulet, & Vakis, 2006; Filmer & Schady, 2008; among others). Some studies have found that such programs have a greater impact on girls than boys (for example, Meng & Ryan, 2010), although other studies have suggested the opposite (for example Ravallion & Wodon, 2000). Behrman, Sengupta, and Todd (2005) suggest that such programs have more long-term effects on boys, but more immediate effects on girls. Female-targeted programs have been found to be effective in increasing girls' school enrolment and attendance in a number of countries (Filmer & Schady, 2008). Using a unique experiment focused on adolescent girls in Malawi, Baird, McIntosh, and Özler (2011) show that making financial transfers conditional on school attendance significantly increased school enrolment and attendance. Importantly, for our purposes, these studies show that conditional cash transfer programs may cause a reallocation of responsibilities within the household, which could potentially have important implications for the school enrolment or work of other siblings (Barrera-Osorio, Bertrand, Linden, & Perez-Calle, 2011; Behrman, Parker, & Todd, 2011; Ferreira, Filmer, & Schady, 2009).

Similarly, there is a vast body of empirical literature focusing on the effects of older siblings on younger siblings' schooling and the relative roles of older brothers and older sisters. Studies have shown that older girls share significant childcare responsibilities in many developing countries, which has an adverse effect on their schooling. For example, in a study of Nicaragua and Guatemala, Dammert (2010) finds that older boys spend more time in domestic and market work than younger siblings, and older girls spend more time in domestic work; this result is found to be independent of the younger siblings' gender. Parish and Willis (1993), on the other hand, find that in Taiwan older sisters increase the educational attainments of younger siblings through taking care of them, 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 tend to help mitigate the household resource constraints by marrying earlier and leaving the household. The child labor literature suggests that younger siblings

are less likely to work (see, for example, Chesnokova & Vaithianathan, 2008). This result might be explained by the higher wage potential of older children or the comparative advantage of older girls in performing household work (Dammert, 2010).

A number of studies have found evidence of a pro-male bias in investment in the education and health of children in households facing financial and time constraints (see Parish & Willis, 1993 among others). However, other studies have not found any evidence of an effect of sibling gender composition (for example, Hauser & Kuo, 1998), or have found mixed evidence for different samples (Kaestner, 1997). Very few studies have examined the effect of a child's education on his/her siblings' education. Qureshi (2015) exploited the existence of gender segregation in schools in Pakistan, and found that the oldest sister's schooling has beneficial impacts on the younger brothers' schooling, enrolment, literacy, and numeracy. In a study for Nepal, Shrestha (2011) found that a change in the educational requirements for British Gurkha army recruitment generated an exogenous increase in the education of Gurkha boys, resulting in a decrease in the education of their female siblings. Lindskog (2013) uses within-household variation to estimate the effects of siblings' education on child schooling. She finds negative immediate effects of siblings' school attendance on school entry of other siblings, but positive long-term effects when older siblings (brother) have left school.

3. Background, the Stipend Program and the Data

3.1. Background and the Stipend Program

Bangladesh has made rapid progress toward realising gender equity in education, an outcome which many other developing countries are seeking to emulate. 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 Bangladesh in 1990, with the objective of realising universal primary enrolment. Similar to other developing countries in South Asia, Bangladesh has traditionally been characterised by low enrolment rates and gender disparity in educational achievements. In the mid-1990s, despite government and NGO efforts to improve primary school enrolments, more than half of all primary school students dropped out before completing grade five. In secondary schools, the dropout rate in the early 1990s was more than 60 per cent with girls faring worse than boys (World Bank, 2002b). Therefore, government intervention in secondary school education was necessary to improve the educational attainments of girls.

With support from donor agencies, the Bangladesh government introduced a nationwide FSSSP in January 1994, covering all rural areas in 460 sub-districts, and targeting girls at secondary schools (grades 6–10, ages 11–15). The objectives of this program were: (i) to increase the school enrolment among secondary school aged girls; (ii) to improve the secondary schooling completion rate for girls; and (iii) to increase the female age at marriage. The program provided a uniform stipend and a tuition subsidy to any girl enrolled in a secondary school in a rural area who met the following eligibility criteria: (i) at least 75 per cent school attendance; (ii) at least 45 per cent marks in the final exam; and (iii) remained unmarried.[2] In 1994, only students in grades six and nine were covered, while in 1995, the program covered all grades except grade eight. Starting from 1996, girls in all grades who met the eligibility criteria were covered by the stipend program (Supplementary Material Table A1.1). The monthly stipend was 25–60 Bangladesh Taka (≈ US$0.62 to US$1.50 based on the 1994 exchange rate[3]), depending on the grade level, covering grades six to ten, and with a book allowance of 250 Bangladesh Taka (≈ US$6.25) for grades nine and ten students (Supplementary Material Table A1.2). The average total stipend, about 1.5 per cent of average rural household consumption, was modest compared to other stipend programs in developing countries.

The stipend FSSSP were expected to cover about half of the costs of textbooks, uniforms, stationery, transportation, examination fees and other educational expenses for eligible students. The stipend and allowances were paid annually in two installments directly into the students' accounts in *upazila* (sub-district) branches of a nationalised bank, while the tuition was paid directly to the school for each girl receiving the stipend. In 1998–99, the total expenditure for the stipend program was 2847.6 million

Taka (≈ US$59.2 million), of which the Bangladesh government paid 59 per cent. This accounted for 14.5 per cent of the secondary education budget and six per cent of the overall education budget (World Bank, 2002a). The stipend component comprised the majority of the total cost of the program.

3.2. The Data

This study uses data from the Bangladesh Multiple Indicator Cluster Survey (MICS) 2009. The survey was conducted jointly by the Bangladesh Bureau of Statistics and UNICEF. MICS 2009 is an extensive, nationally representative, cross-sectional household survey in Bangladesh, covering about three hundred thousand households across the country. The survey was conducted in all 64 districts and 481 sub-districts in Bangladesh. The sampling procedure was stratified random sampling with upazila (sub-district) as the main sampling domain. Within each upazila, census enumeration areas (EA) were selected with probability proportional to size and within each EA, 20 households were drawn randomly for the survey (see Bangladesh Bureau of Statistics, MICS Report 2009). It constitutes the largest household-level nationally representative survey that has ever been administered in Bangladesh.

We restrict our sample to rural households with more than one child, and exclude households that contain adopted or foster children, or stepchildren of the household head. We focus on rural households, given that only girls in rural areas were directly affected by the FSSSP. We exclude urban households as they are not comparable. Urban educational institutions and set ups are completely different.[4] Our analysis contains children of the household head. As we are interested in the effects of the education of older siblings on the schooling outcomes of younger siblings, we include households with children aged 7–14 years who have at least one older sibling aged 21–32.[5] For the younger sibling sample, the lower age limit is selected by taking into account the fact that the official enrolment age in primary school is 6 years, and the upper age limit is chosen to reflect the age at which some variation in choice between schooling and work, or in the schooling of children of different genders, might occur.[6]

Table 1 provides the basic summary statistics of the children aged 7–14 years who have at least one older sibling aged 21–32 years. The mean age of children in the sample is 11.3 years, on average they

Table 1. Selected descriptive statistics for children aged 7–14 years old

	Full sample		Girl		Boy	
Variable	Mean	S.D.	Mean	S.D.	Mean	S.D.
I. Child's characteristics:						
Age (in years)	11.3	2.24	11.4	2.27	11.1	2.20
Gender (girl = 1)	0.509	–	–	–	–	–
Completed years of education	3.78	2.68	4.06	2.73	3.49	2.59
II. Siblings' characteristics:						
Number of siblings	3.46	1.65	3.45	1.66	3.47	1.65
Number of older siblings^{+}	1.28	0.575	1.29	0.587	1.27	0.561
Age gap from older siblings^{+}	12.5	3.20	12.4	3.21	12.6	3.18
Years of education of older siblings^{+}	5.39	4.09	5.41	4.07	5.36	4.11
Years of education of older brothers^{+}	5.36	4.05	5.38	4.03	5.34	4.07
Years of education of older sisters^{+}	5.65	4.47	5.70	4.43	5.59	4.52
III. Household characteristics:						
Gender of household head (female = 1)	0.081	–	0.081	–	0.081	–
Years of education of household head	3.00	3.83	3.00	3.83	3.00	3.84

Note: Descriptive statistics correspond to rural households with children aged 7–14 years who have at least one older sibling aged 21–32 years. + descriptive statistics for older siblings are for older siblings aged 21–32 years.

have completed 3.8 years of education and 50.1 per cent are girls. On average, children in the sample have 1.28 older siblings who have 5.4 years schooling.

4. Empirical Strategy

We run the following regression to estimate the effects of education of older siblings on the schooling outcomes of younger siblings:

$$Edu\_Young_{ij} = \alpha_0 + \alpha_1 Edu\_Old_{ij} + \alpha_2 B_{ij} + \alpha_3 S_{ij} + \alpha_4 C_{ij} + \alpha_5 X_{ij} + \alpha_6 H_j + \gamma_g + v_j + u_{ij} \quad (1)$$

where Edu_Young_{ij} is the educational attainment of child i in household j;$Edu_Old_{ij}$is the education of older siblings[7] of child i in household j; B_{ij} is the number of older brothers of child i in household j; S_{ij} is the 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_{ij} is a vector of variables representing siblings' characteristics; and H_j is a vector of household-level variables. Adding B_{ij} in addition to S_{ij} in the regression isolates the effects of having a brother as opposed to a sister. Child i-specific controls include a vector of age and gender dummies; sibling controls include the mean age gap with older siblings and a dummy variable indicating number of siblings; and household controls include the head's education and gender.[8] Given that the regressions control for number of total siblings and age gap between siblings, the identification also relies on comparing households that have children with closer age gaps versus households that have children with larger age gaps. Separate dummies for each age-year (age dummies) of child i control for the growth in educational attainment across cohorts and changes in national educational policies and other shocks in a particular year which may affect children's schooling. To control for geographic variation in schooling or educational attainment we use separate dummies for each district as denoted by γ_g. In Bangladesh, the district educational administration is in charge of monitoring schools within a district. We also include the interaction between age dummies and district dummies (district-specific linear trend) to control for district-specific time trends and to ensure that we are identifying a program effect separate from incidental district trends.

The error term is assumed to consist of two components:v_j, which is common to all siblings in the household, and u_{ij}, which varies independently across children in the household. Since children in the same household are likely to share a wide variety of characteristics, we estimate standard errors clustered at the household level. Such clustering allows correlations across siblings, as well as a common unmeasured family effect, as reflected in v_j. Our main interest is in the coefficients α_1 and α_2, which measure inter-sibling education and gender composition effects, respectively.

Estimating the effects of older siblings' education levels on younger siblings' schooling is complicated, due to the endogeneity of education of older siblings. There could 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 the education of older siblings. Thus, any observed correlations between older 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 IV strategy. We take advantage of the exogenous variation in the timing of the introduction of the FSSSP and its focus on girls in rural areas. The first-stage of the IV strategy involves examining the impact of the FSSSP on schooling of female students. As discussed above, the program was offered to female students in rural areas who were in grades 6–10 (aged 11–15) in 1994 or later. The official minimum age for children to commence compulsory primary school is 6 years. Thus, girls aged 11 years old or younger in 1994 were eligible to receive the program in 1994 or later. Hence, any females who were 26 years old or younger in 2009 (when the survey was administered) and lived in a rural area in 1994 were eligible to receive the FSSSP. As Supplementary Material Table A1.1 shows, the program did not cover students in all grades at the time of introduction. In 1994, when it was first

introduced, the program was only available to girls in grades six and nine. Beginning in 1996, however, girls in all secondary school grades were covered by the program. Thus, a girl in grade six or below in 1994 could participate in the program for 5 years, up to grade ten. A girl in grade seven in 1994 could not participate in the program either that year or the following year, because grade eight was not included in the program in 1995; however, she could participate in the program in 1996 if she remained in school. A girl in grade eight in 1994 could not participate in the program in that year, but could participate in the program in 1995 if she remained in school. A girl in grade nine in 1994 could participate in the program both that year and in 1995 if she remained in school. Hence, girls who were 12–14 years old in 1994 (grades 7–9) were only affected by the program for 2 years, and girls in these grades could only participate in the program if they progressed to secondary schools before 1994, and remained enrolled in school. Considering the high dropout rate of girls of that age at that time, they represent only a small fraction of the children of their age cohort.

The control group consists of two sets of individuals. The first is older sisters who missed the program marginally as they 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.[9] However, comparing older sisters who were aged 11 years or less with those aged 12 years or more in 1994 could reflect an age effect. We address this issue by including boys of the same age as an additional control group, given that boys were not eligible to participate in the FSSSP.

To sum up, we use an identification strategy that relies on the timing of the introduction of the program in 1994 and compare the outcome of those exposed to the FSSSP with those who just marginally missed the program. We use individual, sibling and household level characteristics to address observable differences across groups. In addition, we control for macroeconomic factors by including a time-trend (age-year dummies). The latter also controls for confounding factors which potentially could have contributed to improvements in female education in Bangladesh or other concurrent factors over the period.[10]

Our sample contains households with children aged 7–14 in 2009 who have at least one older sibling aged 21–26 (6–11 years old in 1994). We drop households with siblings who were just above 14 years old (15–20 years old) in 2009 because an income effect associated with the stipend and tuition subsidy can affect the immediately younger siblings directly, thus violating our exclusion restriction; and in 2009 they were not old enough to influence the education of their immediate younger siblings. However, we keep these households in our sample if they have a child aged 21–32 years who has also a younger sibling aged 7–14 years.

We estimate the effects on education of older siblings using the equation:

$$\begin{aligned} Edu\_Old_{sij} = {} & \partial_0 + \partial_1 \, Eligible\,Age_{ij} + \partial_2 \, Girl_{ij} + \partial_3 \, Eligible\,Age_{ij} * Girl_{ij} + \partial_4 \, B_{sij} + \partial_5 \, S_{sij} \\ & + \partial_6 \, C_{sij} + \partial_7 \, X_{sij} + \partial_8 \, H_j + \gamma_g + \mu_j + \varphi_{ij} \end{aligned} \tag{2}$$

where Edu_Old_{sij} is the number of years of schooling completed by older sibling s of child i in household j. The control variables are defined analogously to those in Equation (1), with the subscript sij corresponding to older sibling s of child i in household j. Equation (2) is our first-stage of the IV estimates when we consider the effects of older siblings' education on the education of their younger siblings. For older siblings, we define, Eligible Age = 1 if the older sibling was aged 6–11 in 1994 (21–26 in 2009), and it is equal to zero if the older sibling was aged 12–17 in 1994 (27–32 in 2009)

Girl is a binary variable which is equal to 1 if the older sibling is female. The coefficient, $\partial_{3,}$ on the interaction between *Eligible age* and *girl*, is the effect of the FSSSP on the education of older girls in the household. It can be interpreted as the intent-to-treat (ITT) effect; that is, the effect of the program on those who were intended to receive the stipend. This estimate is likely to be smaller than the treatment effect on those treated (TOT), as there are age-eligible girls who did not attend school despite the existence of the program. However, the ITT effect provides an unbiased estimate of the causal effect of the program's intention to treat. In our context, ITT is the most policy relevant parameter. The estimate indicates the causal impact of being assigned to participate in the program.

Consequently, our analysis below of the effect on younger siblings' education is based on the ITT estimate of the stipend program on the education of older siblings.

The IV estimates of the effect on younger siblings' education are reported using a two-stage procedure, following Rivers and Vuong (1988). In the second stage, We consider two samples for younger siblings. Sample 1 included younger siblings aged 7–14 years who have at least one older sibling in the specified age group. It is possible that a child aged 7–14 years in 2009 may have older siblings who are not aged 21–32 years in 2009. Thus, we present an alternative set of results with a second sample (sample 2), which includes children aged 7–14 years in 2009, for whom *all* of their older siblings are in the 21–32 age categories at the time of the survey. Consequently, the number of observations in Sample 2 is significantly lower- about one quarter of the original sample . Thus, sample 2 is a subset of sample 1.

We address the robustness of our results in a number of ways. The FSSSP may have also affected program-ineligible siblings (boys and age-ineligible girls) from the same family. For example, the ineligible siblings could receive less education through a reallocation of family resources and/or a shift in family responsibilities, given that the cost of their education is relatively higher (substitution effect). However, offsetting this, it is possible that they could instead receive more education, through the easing of the pressure on family resources (income effect). In the case of any positive effect of the FSSSP on ineligible siblings, we will be estimating the lower bound of the effect of the program.

Furthermore, in robustness checks, we restrict our samples to households in which the older siblings (control group) were only girls. In separate estimates, we exclude older brothers of the same age as the eligible girls and instead use girls from urban areas as one of the control groups. We check the robustness of our main results using a much smaller age range, that is children having older siblings aged 24–29 years in 2009. Finally, we consider a subset of siblings whose family size was influenced by twins. In this case, we use twins as an instrument for the number of siblings. In all these cases, our results are similar.

5. Results

5.1. The Effects on Siblings: OLS Estimates

Table 2 provides OLS estimates of the effects of the gender composition and education of older siblings on the education of younger siblings. The columns in Table 2 differ in terms of the control variables used in the regressions. The results suggest the presence of an intra-household positive externality resulting from education in samples 1 and 2. The point estimates in column (3) indicate that an additional year of schooling is associated with an increase of about 0.16–0.21 years in the schooling of younger siblings.

5.2. The First-Stage of IV Strategy

Before presenting the IV results, we report the first-stage results which is the effects of the FSSSP on the education of older girls. Table 3 presents the 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 (sample 1) with a full set of controls, girls who were eligible for the program received 2.65 additional years of schooling. This indicates that the FSSSP had a very large effect, considering that the average education of older siblings increased to 5.39 years. The F-statistics are well above the rule-of-thumb value of 10 in each specification, supporting the relevance of the instrument.

This estimate of the impact of the FSSSP is much larger than estimates of similar programs in other countries. For example, Schultz (2004) finds that Mexico's PROGRESA program increased years of schooling, on average, by 0.66 years above a baseline level of 6.80 years, representing a 9.7 per cent increase. Behrman et al. (2011) find that for the age group 9–12 before the program started, 5.5 years of exposure to PROGRESA generated a 0.9–1.0 year of additional schooling for boys, and 0.7–

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

Sample 1	(1)	(2)	(3)
Number of older brothers	0.03	0.014	0.016
	(0.032)	(0.033)	(0.033)
Older siblings' education	0.166***	0.160***	0.160***
	(0.004)	(0.004)	(0.004)
Observations	28,176	28,176	28,176
R-squared	0.506	0.514	0.528
Sample 2			
Number of older brothers	−0.09	−0.098	−0.095
	(0.064)	(0.064)	(0.067)
Older siblings' education	0.207***	0.198***	0.205***
	(0.007)	(0.007)	(0.008)
Observations	7,225	7,225	7,225
R-squared	0.436	0.45	0.476
Individual control	Yes	Yes	Yes
Sibling control	Yes	Yes	Yes
Household/area control	Yes	Yes	Yes
District dummy	No	Yes	Yes
District × Age dummy	No	No	Yes

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

Table 3. First stage regression- the effects of the FSSSP on the education of Girls

	(1)	(2)	(3)
Sample 1			
Eligible	2.75***	2.73***	2.65***
	(0.205)	(0.204)	(0.211)
F-Stat	179.5	179.4	158.0
Observations	22,815	22,815	22,815
R-squared	0.181	0.204	0.230
Sample 2			
Eligible	2.36***	2.38***	2.14***
	(0.302)	(0.301)	(0.324)
F-Stat	61.4	62.8	43.6
Observations	9,155	9,155	9,155
R-squared	0.199	0.228	0.287
Individual control	Yes	Yes	Yes
Sibling control	Yes	Yes	Yes
Household/area control	Yes	Yes	Yes
District dummy	No	Yes	Yes
District × Age dummy	No	No	Yes

Notes: *** indicates significance at the 1 per cent level. The standard errors (in parentheses) are corrected for clustering at the 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 aged 21–32 years, having at least one younger sibling aged 7–14 years. Samples 1 and 2 consist of older siblings corresponding to the children included in the younger sample, as per notes to Table 2.

0.8 years of additional schooling for girls, relative to children not exposed to the program. A potential explanation for our findings is that schooling attainment at the baseline is significantly lower in Bangladesh than Mexico, meaning there was much more potential for the FSSSP in Bangladesh to

have a larger effect. At the start of the FSSSP in 1994, education of boys aged 16 years was 4.8 years and that for girls was only 3.6 years. Meng and Ryan (2010) also find that the FFE program in Bangladesh had a larger impact than that of PROGRESA. Their estimates suggest an increase in the duration of schooling of the treated group by 0.7–1.05 years. Our estimates of the impact of the FSSSP are larger again than the FFE estimates suggested by Meng and Ryan (2010). This result likely reflects the fact that the FSSSP was targeted at girls in secondary schools only, while the FFE was targeted at both boys and girls in primary school. In 1994, females constituted 38 per cent of secondary school enrolments (Khandker, Pitt, & Fuwa, 2003). Baseline schooling of girls in secondary schools was lower than that for boys and there was more scope for intervention to have an impact at the secondary, than primary, level.

5.3. The Validity of the Instrument

The main identifying assumption for the IV estimates is that the timing of the introduction of the program had no direct 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. Hence, it is unlikely that the program had an effect on younger children in the family, other than that coming through their older siblings. However, it is plausible to conjecture that the program could have had spillover effects on either boys of the same cohort or older siblings in the same household (such as through income transfers). We run a placebo test that uses only boys in the first stage. The program should have no effect on boys belonging to the same age group. It should also have no effect on girls 27–32 years old who missed the program as they were not eligible,. The results, which are reported in Table 4, indicate that the program did not have any significant impact on the education of either boys of the same cohort or ineligible older sisters. The results confirm the validity of our instrument, suggesting that having an eligible sister has no effect on the education of other siblings who were not eligible to receive the stipend.

There might be a concern that the relative increase in girls' education at the onset of the FSSSP might have been a result of other socio-economic changes during that period, such as the growth in the garment industry and the Child Labor Deterrence Act in 1993. Although about 70 per cent of the

Table 4. Placebo test-Effect of the stipend program on ineligible siblings

	(1)	(2)
	Boy (21–32 years old)	**Ineligible girls (27–32 years old)**
Sample 1		
Has any age-eligible sister	0.256	0.647
	(0.284)	(1.69)
Observations	19,680	448
R-squared	0.220	0.620
Sample 2		
Has any age-eligible sister	−0.775	−3.44
	(0.479)	(4.07)
Observations	7,947	219
R-squared	0.284	0.807
Individual control	Yes	Yes
Sibling control	Yes	Yes
Household/area control	Yes	Yes
District and District × Age dummy	Yes	Yes

Notes: The standard errors (in parentheses) are corrected by clustering at the household level. The dependent variable is completed years of schooling of ineligible older siblings, as mentioned in the respective columns. The regression in column 1 includes older boys in the specified age group, while that in column 2 includes only ineligible older girls, that is girls in the specified age group who are older than 26 years. Samples 1 and 2 consist of older siblings corresponding to the children included in the younger sample, as per the notes to Table 2.

workforce in the garment sector in Bangladesh is female, studies suggest that female workers are mainly concentrated in low-paid unskilled jobs with little educational requirements, while the high-paid skilled jobs are mainly occupied by male workers (see, for example, Paul-Majumder & Begum, 2000). The garment sector in Bangladesh has experienced phenomenal growth since the early 1980s, as shown in Supplementary Material Figure 1A). The important distinguishing feature of the FSSSP is that the program was concentrated in rural areas and was available for girls only. As the enrolment data shows, the national level secondary enrolment for boys and girls has been growing at a similar pace, but girls' enrolment started growing at a higher pace in the mid-1990s. Also, enrolment in rural areas increased at a higher rate during the period, while the corresponding figure in urban areas does not exhibit this trend. Again, during this period there was a higher share of female enrolment in secondary schools (see Khandker et al., 2003). Thus, while the garment sector in Bangladesh has flourished since the late 1970s/early 1980s, the steeper growth in education among rural female students after the introduction of the FSSSP in the mid-1990s cannot be attributed to growth in the garment sector. The garment industry in Bangladesh is concentrated in particular areas and employs only a minuscule proportion of the girls enrolled in schools. There is also a very low migration rate in Bangladesh, especially for girls, According to The Bangladesh Household Income and Expenditure Survey 2010, only 3.8 per cent of household members migrated during the 5 years prior to the survey; of which, only 3.2 per cent were women. In general, historical migration from outside the greater Dhaka region is very low, with the exception of a few districts, even in the face of natural disaster or seasonal hunger/deprivation.

5.4. The Effects on Siblings: IV Estimates

Table 5 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, which is important for the education outcomes of younger siblings. In sample 1, column 3 indicates that an additional year of schooling by older siblings increases younger siblings' years of schooling by 0.13 years. Given that the ITT effect is 2.65, this figure translates into a 10 per cent increase, or 0.35–4 years, in the education of younger siblings' due

Table 5. IV estimates: education of older siblings, and its effects on their younger siblings' education

	(1)	(2)	(3)
Sample 1			
Number of older brothers	0.029	0.013	0.012
	(0.033)	(0.033)	(0.033)
Older siblings' education	0.152***	0.155***	0.128***
	(0.042)	(0.042)	(0.018)
Observations	28,176	28,176	28,176
R-squared	0.506	0.514	0.528
Sample 2			
Number of older brothers	−0.079	−0.093	−0.089
	(0.066)	(0.064)	(0.067)
Older siblings' education	0.159**	0.155**	0.136***
	(0.075)	(0.076)	(0.026)
Observations	7,225	7,225	7,225
R-squared	0.436	0.450	0.477
Individual control	Yes	Yes	Yes
Sibling control	Yes	Yes	Yes
Household/area control	Yes	Yes	Yes
District dummy	No	Yes	Yes
District × Age dummy	No	No	Yes

Notes: See notes to Table 2.

to the FSSSP (mean education for younger siblings is 3.78). The estimates are similar for sample 2.[11] The results are robust to successive addition of controls.[12]

Behrman (1997) provides a review of the literature, and suggests that the marginal effect of parental education on children's schooling ranges from 0.08 to 1.03, in the context of developing countries. Our IV estimates of the effects of older siblings' education on younger siblings' schooling 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 are generally competing for limited resources, reducing the positive spillover/demonstration effect, while there is no such rivalry in the case of parental education. Therefore, it is to be expected that the impact of siblings' education should be less than that of parental education.

5.5. Partial versus Full Treatment

The program might have some effects on siblings who were in secondary schools at the time of its introduction in 1994 and 1995. In this section, we address the potential *partial* treatment of secondary school aged girl cohorts in 1994 and 1995. We consider girls who were in grades seven to nine in 1994. We define the treatment and control groups as:

Treat1 (cohort 1- *Partial Treatment*) = 1, if 12 ≤ female age in 1994 ≤ 14 [27 ≤ female age in 2009 ≤ 29];

Treat2 (cohort 2- *Full Treatment*) = 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 (cohorts 1–3) = 1, if 6 ≤ male age in 1994 ≤ 17 [21 ≤ male age in 2009 ≤ 32].

The partial treatment group consists of girls in cohort 1 who had 2 years' exposure to the program. The full treatment group corresponds to girls with 5 years' exposure to the program (the same as the treatment group in the results above). Control group 1 corresponds to girls with no exposure, covering girls who were in grade ten in 1994, as well as girls who were above secondary school age. Control group 2 includes boys from all of these age groups, none of whom were eligible to participate in the stipend program.

The corresponding first-stage regression is:

$$Edu\_Old_{sij} = \theta_0 + \theta_1 Treat_{ij} + \theta_2 Girl_{ij} + \theta_3 EligibleAge_{ij} * Girl_{ij} + \theta_4 B_{sij} + \theta_5 S_{sij} + \theta_6 C_{sij} + \theta_7 X_{sij} + \theta_8 H_j + \gamma_g + \tau_j + \varepsilon_{ij} \quad (3)$$

Treat is a set of dummy variables for the three cohorts, as defined above. We are interested in the coefficient θ_3, where the base category is controls 1 and 2 together. The first stage regression results, presented in Table 6, show that the instrument is highly significant. F-statistics of the joint significance of the instruments are well above the rule-of-thumb value of 10 in all cases. Consistent with our expectations, the magnitude of the coefficient for treatment group 2, with 5 years' exposure, is much greater than that for treatment group 1, with only 2 years' exposure. The magnitude of the coefficients suggests that the effect of full exposure is more than double than that of partial exposure. For example, column (3) indicates that program exposure increased schooling by 1.5 years in the case of 2 years' exposure and by 3.6 years in the case of 5 years' exposure, compared to no exposure to the program. Behrman et al. (2011) also show that schooling achievement is higher with a greater exposure to the PROGRESA program. Table 7 reports the IV estimates of the effects of the education of older siblings on their younger siblings' education. The estimates for older siblings' education are of almost the same magnitude as the main results in Table 5. Thus, our instrument provides robust estimates for both the partial and full treatment.

Table 6. Variation in exposure to FSSSP and the education of older siblings

	(1)	(2)	(3)
Sample 1			
Treat 1	1.55***	1.50***	1.50***
	(0.363)	(0.364)	(0.387)
Treat 2	3.70***	3.64***	3.62***
	(0.279)	(0.280)	(0.308)
F-Stat	111.0	109.2	93.9
Observations	22,815	22,815	22,815
R-squared	0.181	0.204	0.231
Sample 2			
Treat 1	1.76***	1.79***	1.83***
	(0.513)	(0.515)	(0.560)
Treat 2	3.37***	3.41***	3.26***
	(0.377)	(0.386)	(0.450)
F-Stat	43.9	43.4	29.8
Observations	9,155	9,155	9,155
R-squared	0.200	0.229	0.287
Individual control	Yes	Yes	Yes
Sibling control	Yes	Yes	Yes
Household/area control	Yes	Yes	Yes
District dummy	No	Yes	Yes
District × Age dummy	No	No	Yes

Notes: See notes to Table 3. "Treat 1" corresponds to the partial exposure (2 years), and "Treat 2" corresponds to the full exposure (5 years) to the FSSSP.

Table 7. IV estimates: the effects of education of older siblings on their younger siblings' education (variation in exposure to FSSSP as instruments)

	(1)	(2)	(3)
Sample 1			
Number of older brothers	0.029	0.013	0.012
	(0.033)	(0.033)	(0.033)
Older siblings' education	0.145***	0.150***	0.127***
	(0.040)	(0.040)	(0.018)
Observations	28,176	28,176	28,176
R-squared	0.506	0.514	0.528
Sample 2			
Number of older brothers	−0.076	−0.092	−0.088
	(0.066)	(0.064)	(0.067)
Older siblings' education	0.145**	0.153**	0.136***
	(0.068)	(0.068)	(0.026)
Observations	7,225	7,225	7,225
R-squared	0.436	0.450	0.477
Individual control	Yes	Yes	Yes
Sibling control	Yes	Yes	Yes
Household/area control	Yes	Yes	Yes
District dummy	No	Yes	Yes
District × Age dummy	No	No	Yes

Notes: See notes to Table 2.

6. Robustness Checks

We checked the robustness of our main results in a number of ways. We discuss the main points here briefly. Full results are reported in Supplementary Material Tables A2–A5. First, our dataset contains information only on the individuals who were actually living in the household at the time of the

survey. To address these issues, we used a restricted sample which included only those households in which all living children of the mother are residing in the household. The results were similar to those for the unrestricted sample (see column 1, Table A2).[13]

We also restricted our sample to households that do not have a mix of eligible and ineligible girls in the same group and got results similar to the main results presented in Table 5 (see column 2 of Supplementary Material Table A2). We also performed this exercise using a narrower age group. We consider only households with 7–14 year old children (younger cohort) who have an older sister aged 9–11 years in 1994 as the treatment group. In that case, the control group consists of corresponding households with older siblings aged only 12–14 years in 1994. The results, presented in column 3 of Supplementary Material Table A2, using a full set of controls are qualitatively similar. The coefficients of the IV estimates are similar to those reported in Table . In this case, however, the sample size was drastically reduced as we needed at least one younger sibling (7–14 years old) who has an older sister aged 24–29 years to be in the sample (9–14 years old in 1994).

As the stipend program affected older sisters but not older brothers, we also used a sample with households having older girls who were eligible to receive the program, but exclude households with older boys of the same age. We consider girls from urban areas as one of the control groups, instead of boys from rural areas. The results are similar to the main findings presented above, and are reported in Supplementary Material Table A3. Since we are interested in both the gender and education of the older siblings, and the program was available only for girls in rural areas and the fact that urban education system and trend are significantly different from rural areas, the main results reported in the paper include both boys and girls from rural areas where boys are always used as one of the control groups in our regression.

Family size can be considered a choice variable and unobserved parental preferences might also affect their children's schooling. 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 which affect both the total number of siblings (explanatory variable) and the children's schooling (dependent variable), leading to potential correlation between the error term and total number of siblings. Dizon-Ross (2016) find that in developing countries, the inaccurate beliefs of less educated parents distort investments in their children's education. We address this issue using twins as an instrument for the number of siblings, because having twins generates exogenous variation in the number of siblings. The IV estimates were similar to those reported in Table 5 (see Table A4).[14]

Finally, in Bangladesh, because of the stipend program, there is almost universal enrolment for children of primary school-age (6–11 years old). Hence, we did not use enrolment as the outcome variable given that the vast majority of our students would be in primary school. We checked our main results using enrolment status (see Supplementary Material Table A5). Overall, we see similar results using enrolment as the outcome of interest. We find positive and significant effects of enrolment of younger siblings due to older sibling being the recipient of the stipend.

7. Conclusion

This study has examined the influences of the education and gender composition of older siblings on the schooling of younger siblings. We use the timing of the introduction of the FSSSP, together with the fact that it was only available to girls in rural areas, as a natural experiment, allowing us to compare children who received the program to those who missed out because they were either boys or age-ineligible girls. Our main conclusions are twofold. The first point is that older siblings' education has a significant, and large, impact on younger siblings' education. The IV estimates suggest that for each additional year of schooling undertaken by older siblings, the number of years of schooling completed by younger siblings increases by about 0.13 years. The results suggest that the FSSSP increased the schooling of children in the affected cohorts by 2.6 years. Taken together, these results imply 0.35–4 years increase in the schooling of younger siblings if their older siblings benefitted from the program. This is significant considering that the program benefitted both the targeted children by

more than two and half years and then, subsequently, their younger siblings' education by more than one-third of a year.

One important implication of our findings is that school programs that benefit children's education can provide gains in both the short- and long-term, both for the targeted children and for their younger siblings. This finding contributes to the literature on the impact of secondary school stipend or cash transfer programs on educational outcomes, by showing that there is an additional channel through which school programs, such as the one that we study here, benefit others, and in particular, younger siblings within the household. Another implication is that in a poor setting with initial low level of education, even a modest transfer can lead to a large impact on the targeted group. These results should be of interest to countries considering similar programs or examining the impacts of those programs.

Acknowledgement

We thank Sarah Baird, Gaurav Datt, Youjin Hahn, Fahad Khalil, Paula Lorgelly, Wahiduddin Mahmud, Pushkar Maitra, Nigar Nargis, Berk Özler, Núria Rodríguez-Planas, Sommarat Chantarat, Robert Sparrow, Yuji Tamura, and Liang Choon Wang, as well as participants at the IZA/World Bank Conference on Employment and Development in New Delhi, the International Growth Centre Conference in Dhaka, EEA conference in Gothenburg, the Econometric Society Australasian Meeting in Melbourne, the Australian Conference of Economists in Melbourne, and seminar participants at the Australian National University and Monash University, for their valuable comments and suggestions on earlier versions of this paper.

Disclosure statement

No potential conflict of interest was reported by the authors.

Notes

1. The conditions include an attendance requirement, a minimum threshold for academic performance and that the girls remain unmarried.
2. Approximately 90 per cent of the secondary school girls were reported to satisfy the eligibility criteria (World Bank, 2003).
3. The exchange rate was 1 US$ ≈ 40 Bangladesh Taka in 1994.
4. In robustness checks, we also consider girls from the urban sample. The conclusion remains the same as those presented in the results section.
5. We choose the older siblings' age limit based on the FSSSP exposure. The sample selection is explained in detail in Section 4. Our results are robust to various cut-offs for age.
6. Bangladesh has ratified the Minimum Age Convention, 1973, which sets 15 years as the minimum age for child labor. Moreover, one can complete junior secondary school (grade 8) by the age of 14, assuming a timely completion.
7. We use average education of all older siblings in the main estimation results. In robustness checks, we also consider education of the oldest sibling and maximum education of an older sister/sibling. The results are very similar and are reported in the Supplementary Material.
8. The mother's education level is highly correlated with that of the household head. Therefore, we control only for the head's education. Our results remain robust when adding controls for the mother's education.
9. 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 results reported in Section 5. Girls 12 years and older in 1994 would have been eligible for the FSSSP if they had repeated grades. In such a case, the program effect would be an underestimate of the true effect, as some of the controls are considered as treated.
10. As far as we are aware, the FSSSP program was not introduced concurrently with any other major programs, such as improvements in teacher quality or improvements in school infrastructure. However, if these improvements in the supply of education were to result in changes in the expected returns to education, then parents will send their younger children to school independently of the gender of the sibling. Hence, our results are not affected by any other programs which did not target any particular gender.

11. OLS would be upward biased if there is omitted variable bias. However, OLS would be downward biased if there is measurement error in the variable of interest. In our case, the OLS and IV estimates are statistically not different, though the OLS estimates are a bit higher than the IV estimates. The results could be a combination of both- downward bias, due to measurement error, and upward bias due to omitted variable bias.
12. The IV strategy does not isolate the causal impact of the FSSP on younger siblings education. It potentially isolates the causal link between older siblings education and younger siblings education. Hence, the final connection between the FSSSP and younger sibling's education is itself not estimated but implied.
13. The restricted sample was about 40 per cent of the size of the original sample for the 21–32 sample, and half the size of the original sample for the 17–36 sample. These are relatively younger families and all the children are still residing with their parents.
14. There are 274 pairs of twin siblings sample when the sample is for 21–32 years old, and it increases to 519 pairs when the sample includes 17–36 years old siblings.

References

Baird, S., McIntosh, C., & Özler, B. (2011). Cash or condition? Evidence from a cash transfer experiment. *The Quarterly Journal of Economics*, *126*, 1709–1753. doi:10.1093/qje/qjr032

Bangladesh Bureau of Statistics. (2009). *Multiple indicator cluster survey report*. Dhaka: Author.

Barrera-Osorio, F., Bertrand, M., Linden, L. L., & Perez-Calle, F. (2011). Improving the design of conditional transfer programs: Evidence from a randomized education experiment in Colombia. *American Economic Journal: Applied Economics*, *3*(2), 167–195. doi:10.1257/app.3.2.167

Becker, G. S., & Tomes, N. (1976). Child endowments and the quantity and quality of children. *The Journal of Political Economy*, *84*, S143–S162. doi:10.1086/260536

Behrman, J. R. (1997).*Mother's schooling and child education: A survey.* Penn Institute for Economic Research Department of Economics University of Pennsylvania, Working Paper 97-025.

Behrman, J., Pollak, R., & Taubman, P. (1982). Parental preferences and provision for progeny. *The Journal of Political Economy*, *90*, 52–73. doi:10.1086/261039

Behrman, J. R., Sengupta, P., & Todd, P. E. (2005). Progressing through PROGRESA: An impact assessment of a school subsidy experiment in rural Mexico. *Economic Development and Cultural Change*, *54*, 237–275. doi:10.1086/431263

Behrman, J. R., Parker, S., & Todd, P. E. (2011). Do conditional cash transfers for schooling generate lasting benefits? A five-year follow up of PROGRESA/oportunidades. *Journal of Human Resources*, *46*, 93–122. doi:10.3368/jhr.46.1.93

Chesnokova, T., & Vaithianathan, R. (2008). Lucky last? Intra-sibling allocation of child labor. *The B.E. Journal of Economic Analysis & Policy*, *8*, 1–28. doi:10.2202/1935-1682.1908

Dammert, A. C. (2010). Siblings, child labor and schooling in Nicaragua and Guatemala. *Journal of Population Economics*, *23*, 199–224. doi:10.1007/s00148-008-0237-0

De Janvry, A., Finan, F., Sadoulet, E., & Vakis, R. (2006). Can conditional cash transfer programs serve as safety nets in keeping children at school and from working when exposed to shocks?. *Journal of Development Economics*, *79*, 349–373. doi:10.1016/j.jdeveco.2006.01.013

Dizon-Ross, R. (2016). *Parents' beliefs and children's education: Experimental evidence from Malawi.* Unpublished Manuscript, Booth School of Business, University of Chicago.

Ferreira, F. H. G., Filmer, D., & Schady, N. (2009). *Own and sibling effects of conditional cash transfer programs: Theory and evidence from Cambodia* (Policy Research Working Paper 5001). Retrieved from http://datatopics.worldbank.org/hnp/files/edstats/KHMimp09a.pdf

Filmer, D., & Schady, N. (2008). Getting girls into school: Evidence from a scholarship program in Cambodia. *Economic Development and Cultural Change*, *56*, 581–617. doi:10.1086/533548

Hahn, Y., Islam, A., Nuzhat, K., Smyth, R., & Yang, H.-S. (2016). Education, marriage and fertility: Long-term evidence from a female stipend program in Bangladesh. *Economic Development and Cultural Change, Forthcoming*.

Hauser, R. M., & Kuo, H. D. (1998). Does the gender composition of sibships affect women's educational attainment? *The Journal of Human Resources*, *33*, 644–657. doi:10.2307/146336

Kaestner, R. (1997). Are brothers really better? Sibling sex composition and educational achievement revisited. *The Journal of Human Resources*, *32*, 250–284. doi:10.2307/146215

Khandker, S. R., Pitt, M. M., & Fuwa, N. (2003). *Subsidy to promote girls' secondary education: The female stipend program in Bangladesh* (MPRA Paper No.23688). Retrieved from http://mpra.ub.uni-muenchen.de/23688

Lindskog, A. (2013). The effect of siblings' education on school-entry in the Ethiopian highlands. *Economics of Education Review*, *34*, 45–68. doi:10.1016/j.econedurev.2013.01.012

Meng, X., & Ryan, J. (2010). Does a food for education program affect school outcomes? The Bangladesh case. *Journal of Population Economics*, *23*, 415–447. doi:10.1007/s00148-009-0240-0

Pande, R. P. (2003). Selective gender differences in childhood nutrition and immunization in rural India: The role of siblings. *Demography*, *40*, 395–418. doi:10.1353/dem.2003.0029

Parish, W. L., & Willis, R. J. (1993). Daughters, education, and family budgets Taiwan experiences. *The Journal of Human Resources*, *28*, 863–898. doi:10.2307/146296

Paul-Majumder, P., & Begum, A. (2000). *The gender imbalances in the export oriented garment industry in Bangladesh*. (Policy Research Report on Gender and Development: Working Paper Series. No. 12). Retrieved from http://siteresources.worldbank.org/INTGENDER/Resources/trademajumder.pdf

Qureshi, J. A. (2015). *Additional returns to investing in girls' education: Impact on younger sibling human capital* (working paper). Chicago: University of Illinois.

Rashid, M. A. (2006, November). *Rise of readymade garments industry in Bangladesh: Entrepreneurial ingenuity or public policy.* Paper presented at the Workshop on Governance and Development organized by the World Bank and BIDS, Dhaka.

Ravallion, M., & Wodon, Q. (2000). Does child labour displace schooling? Evidence on behavioural responses to an enrollment subsidy. *The Economic Journal*, *110*(462), 158–175. doi:10.1111/ecoj.2000.110.issue-462

Rivers, D., & Vuong, Q. H. (1988). Limited information estimators and exogeneity tests for simultaneous probit models. *Journal of Econometrics*, *39*, 347–366. doi:10.1016/0304-4076(88)90063-2

Schultz, T. P. (2004). School subsidies for the poor: Evaluating the Mexican Progresa poverty program. *Journal of Development Economics*, *74*, 199–250. doi:10.1016/j.jdeveco.2003.12.009

Shrestha, S. A. (2011). *Sibling rivalry in Education: Estimation of intra-household trade-offs in human capital investmen*. Unpublished Manuscript, Kellogg Graduate School Management, Northwestern University.

UNICEF. (2015). *The investment case for education and equity*. New York: Author.

World Bank. (2002a). *Project appraisal document on a proposed credit to the Peoples' Republic of Bangladesh for a female secondary school assistance project-II*. Dhaka: Author.

World Bank. (2002b). *Implementation completion report (IDA-24690) on a credit to the Peoples' Republic of Bangladesh for a female secondary school assistance project*. Dhaka: Author.

World Bank. (2003). *Project performance assessment report Bangladesh female secondary school assistance project (Credit 2469)*. Dhaka: Author.