Illusion of Gender Parity in Education: Intrahousehold Resource Allocation in Bangladesh*

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Abstract

Gender parity in education—an important global development goal—is often measured through school enrollment. However, this can be misleading as girls may lag behind boys in other measures. We investigate this with Bangladeshi survey data by decomposing households' education decisions into enrollment, education expenditure, and its share for the quality of education. We find a strong profemale bias in enrollment but promale bias in the other two decisions. This contradirectional gender bias is partly explained by conditional cash transfer programs, which promoted girls' secondary school enrollment but did not narrow the gaps in the intrahousehold allocation of education resources.

JEL Classification: D15, H52, I28, J16, O15

Keywords: education parity; conditional cash transfer; gender; Bangladesh

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1 Introduction

The last several decades have witnessed a significant progress around the globe in various aspects of education, particularly for girls who have been historically disadvantaged. As a result, the previous global targets of universal primary education and gender equality in all levels of education under the Millennium Development Goals (MDGs) were broadly attained in 2015, based on quantity indicators such as enrollment (United Nations, 2015). With this progress, the policy focus in the developing world has shifted from the quantity to the quality of education as epitomized by the fourth Sustainable Development Goal. Against this backdrop, we demonstrate that the gender gap in the quality of education may persist even when gender parity in enrollment is achieved—due to the gender bias in the intrahousehold allocation of education resources. While our analysis is based on data from Bangladesh, this finding has a global implication as current education policies implemented in developing countries are often unable to adequately address the gender gap in the quality of education.

The distinction between quantity and quality is important, because policies to increase quantity outcome measures do not necessarily lead to an improvement in the quality of education. To highlight this, take conditional cash transfer (CCT) programs as an example. These programs give cash to households, if children from eligible households fulfill certain conditions such as satisfactory school attendance. Therefore, CCT programs can simultaneously relax the budget constraint and lower the opportunity cost of education. Following the success of the pioneering CCT program, *Progresa* in Mexico, similar programs have been replicated around the world to help the disadvantaged groups (see Fiszbein and Schady (2009) for a review). In South Asia, studies in the Punjab province of Pakistan (Chaudhury and Parajuli, 2010) and eleven backward blocks in India (Sekher and Ram, 2015) suggest that gender-targeted CCT programs can narrow the gender gap in enrollment.

On the other hand, CCTs do not address the quality of education children receive. In various places, the supply of education services failed to keep pace with the massive increase in school attendance and enrollment. This, in turn, resulted in overcrowded classrooms—lowering the quality of school education (e.g., Kattan (2006)). Consequently, academic learning of children has often failed to improve (e.g., Saha and Saha (2018)). This is a particularly serious problem for economically and socially disadvantaged groups, since they are least likely to help children by hiring private tutors or supporting their learning at home.

Therefore, looking at gender parity in education simply through the narrow lens of school enrollment may only lead to an illusion of success. Girls may fall behind boys in other important educational outcomes, even when gender parity in school enrollment is achieved. For example, a strong gender preference for boys may bias the intrahousehold allocation of education resources. This, in turn, may lead to a systematic gender gap in education quality and performance. Hence, households' responses to programs—such as CCTs—and children's school performance must be considered holistically to assess the progress towards gender parity in education. For example, Alam et al. (2011) documented that a gender-targeted CCT program in the Punjab province of Pakistan may have contributed to a gender gap in learning as households responded to this program by sending boys to private schools. However, existing studies mostly neglected the (potentially negative) impact of gender-targeted CCTs on intrahousehold resource allocation and the quality of education children receive. Using household survey data from Bangladesh with detailed information on education expenditure, we show that a sizable and statistically significant gender gap—conditional on enrollment—has persisted in the intrahousehold allocation of education resources. This has apparently led to gender gaps in education quality, timely graduation, and school performance.

Bangladesh provides an interesting setup to study the impact of gender-targeted CCTs on the intrahousehold allocation of education resources. Despite being predominantly patriarchal, it has achieved remarkable progress in bringing girls and boys to school. The recent progress is especially pronounced in the education statistics at the secondary level. The gross secondary school enrollment rate for girls [boys] increased from 14 percent [27 percent] in 1990 to 72 percent [66 percent] in 2016. This noteworthy progress has been supported by several interventions implemented by government and nongovernment organizations (see Ahmed et al. (2007) for a review). In particular, interventions targeted at promoting girls' education have helped eliminate or even reverse the gender gap in some measures of education in Bangladesh

(Ahmed et al., 2007; Chowdhury et al., 2002; Shafiq, 2009). At the secondary level, the Female Stipend Programs (FSPs)—CCT programs that provide girls with a stipend and tuition fee waiver—have been notably credited for narrowing the gender gap in enrollment (Asadullah and Chaudhury, 2009; Behrman, 2015; Khandker et al., 2003; Mahmud, 2003).

Despite this progress, girls are lagging behind boys in various educational outcomes in the secondary and higher levels of education. As shown in Figure 1, girls have been underperforming boys in nearly all years between 1990 and 2017, both in terms of passing rates and the share of top students in the Secondary School Certificate (SSC) examination—a national exam for secondary school completion. As found by Schurmann (2009), girls also face higher rates of dropout and grade repetition than boys. These facts remain valid in the recently released education statistics of Bangladesh.[1] If we take the gender difference in the enrollment rate as a sufficient statistic for gender disparity in education, then this persistence of girls' under-performance in secondary education would be puzzling, given the reduction (and indeed reversal) of the gender gap in the secondary enrollment rate. This puzzle demonstrates the problem with focusing exclusively on gender parity in enrollment.

There are many factors that can potentially explain girls' under-performance in secondary education: the low female-male ratio among teachers, biased attitudes of educators (Lavy and Sand, 2018), and lacking gender-appropriate school curriculum and facilities (e.g., separate toilet arrangements for boys and girls). These supply-side factors are relevant and have been studied at length in the literature. In comparison, the demand-side constraints that would potentially limit education policies and programs are relatively understudied. With this broader perspective, we focus on the gender gap from the demand side by highlighting the allocation of education expenditure within the household.

One methodological challenge in addressing this question is the interdependence of the education decisions; whether to send a child to school, how much to spend on a child's education, and how to spend it. To tackle this challenge, we develop a three-part model consisting of three related education decisions made by the household: 1) enrollment, 2) total education expenditure conditional on enrollment, and 3) share of the total education expenditure on the "core" component—which would directly affect the quality of the child's education as elaborated

[1] For example, completion rate at the secondary level for boys increased from 43 percent to 67 percent and that for girls from 34 percent to 58 percent between 2008 and 2017.

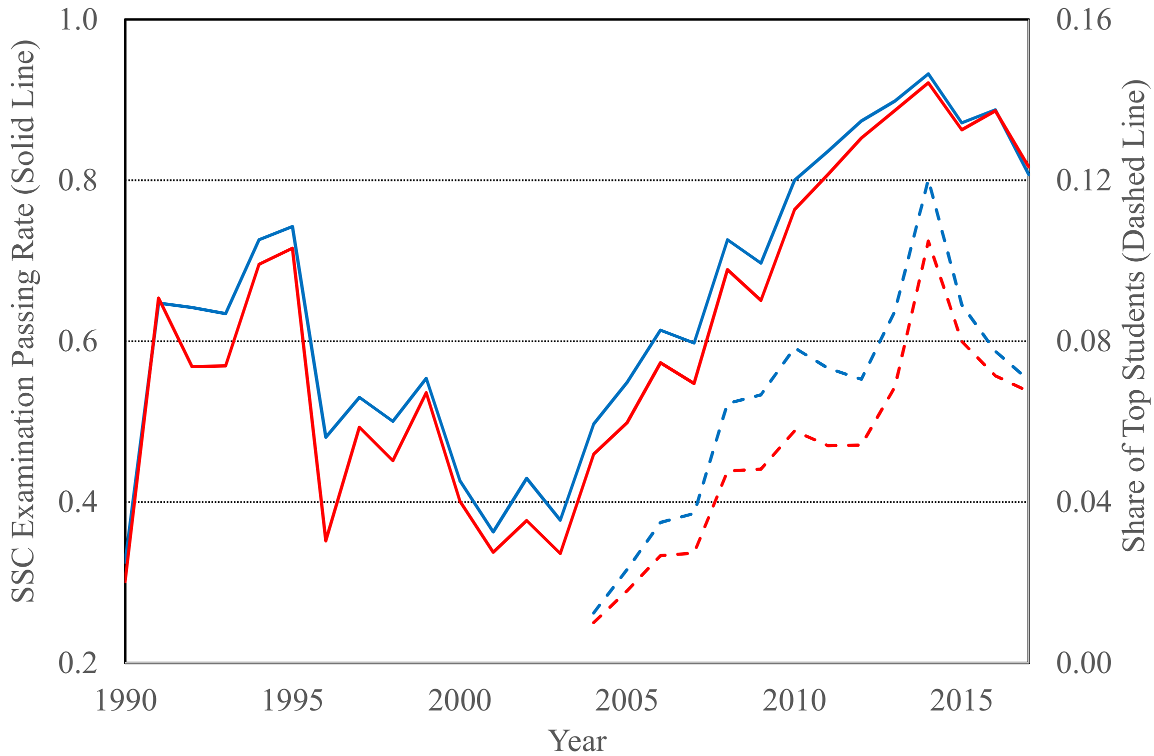

Figure 1: Performance in the Secondary School Certificate (SSC) examination by gender over time. The solid lines represent the proportions of boys (blue) and girls (red) who passed the SSC examination among those who took the examination and the dashed lines represent the share of top students who achieved the highest grade point average (locally known as "GPA 5"). *Source: BANBEIS-Education Database* (http://data.banbeis.gov.bd/) accessed on 29 October 2017.

in Section 4. We then apply this three-part model to four rounds of nationally representative household surveys. We find a clear profemale bias in the enrollment decision. On the other hand, the decisions on the total education expenditure and core share—conditional on enrollment—are significantly promale in the recent three survey rounds. For example, girls were 10 percentage points more likely to be enrolled in secondary school than boys in 2010. However, conditional on enrollment, the total education expenditure and the "core" component expenditure for girls in 2010 were lower than boys by 520 BDT and 542 BDT— which are about 7 and 10 percent of the total education expenditure and core expenditure on the boys, respectively.[2]

Our finding of *contradirectional* gender gap—profemale bias in enrollment decision and promale bias in the other two decisions in the three-part model—is unique to Bangladesh and noteworthy. In particular, existing studies in other South Asian countries such as India and Pakistan tend to find a promale gender gap as elaborated in the next section. Therefore, a natural question arises as to why a contradirectional gender gap is only found in Bangladesh and

[2]In 2010, the average official exchange rate was about 1 USD=70 BDT.

not in other South Asian countries that have broadly similar cultural, political, and economic backgrounds and share historical roots with Bangladesh. Clearly, gender discrimination alone fails to explain what is observed in Bangladesh, because it would lead to a *codirectional*—and not contradirectional—gender gap.

To better understand the observed contradirectionality of the gender gap in Bangladesh, we explore the relevance of the FSPs, because a comparable nationwide gender-targeted CCT program did not exist in India or Pakistan during our study period.[3] We find some evidence that the FSPs help explain this contradirectionality in gender gap. Specifically, we employed a treatment intensity measure as our identification strategy to understand the impact of gender targeted CCT program on intrahousehold educational resource allocation. Using a double-difference estimation strategy, we find that FSPs were successful in increasing enrollment but not in narrowing the gender gap in education expenditure and core share conditional on enrollment. This indicates the presence of a gender gap in the quality of education children receive among school enrollees. Therefore, while CCT programs like the FSPs can be effective in bringing girls to school and help improve or even reverse the gender gap in the quantity of education, they may be ineffective in narrowing the gender gap in the amount and kind of education resources given to children in their households. Hence, policy makers may also need to consider implementing complementary policies, such as school quality improvement programs and vouchers for free supplementary or remedial education to improve the quality of education for girls and narrow the gender gap in the quality of education.

The rest of this paper is organized as follows. We review related studies and discuss our paper's relevance and contributions to the body of existing studies in Section 2. We introduce the three-part model in Section 3, followed by the data description and key summary statistics in Section 4. In Section 5, we document the contradirectional gender gap using the three-part model. We then investigate the relevance of the FSPs to the contradirectionality of the gender gap in Section 6. Some discussions are provided in Section 7.

[3] Sekher and Ram (2015) and Chaudhury and Parajuli (2010) discussed earlier only cover a part of Pakistan and India, respectively.

2 Relevance and Contributions to Literature

This study contributes to the literature on intrahousehold allocation of resources for human capital investment in developing countries. Previous studies highlighted a gender bias whereby parents systematically invest more resources in sons' education (e.g., Deaton (1989), Li and Tsang (2003), Kaul (2018)). Employing a hurdle model, Kingdon (2005) finds a promale bias in the enrollment decision but no gender bias in education expenditure among enrolled children in rural India. Azam and Kingdon (2013) revisit this study with more comprehensive data from India and found the presence of pro-male bias in education expenditure. Besides India, the hurdle model has also been applied to Malaysia (Kenayathulla, 2016), Pakistan (Aslam and Kingdon, 2008), Paraguay (Masterson, 2012), and Sri Lanka (Himaz, 2010), among others. The main results of these studies using a hurdle model are summarized in Table 15 in Appendix F.

Table 15 shows that the promale bias is far from ubiquitous: Masterson (2012) finds a promale bias in rural areas but a profemale bias in urban areas in Paraguay. In Malaysia, no gender gap was found (Kenayathulla, 2016), whereas a profemale bias in education expenditure conditional on enrollment was detected in Sri Lanka (Himaz, 2010). Wongmonta and Glewwe (2017) also find a gender gap in favor of females in Thailand, though not based on a hurdle model. Table 15 also shows that the directions of gender biases in enrollment and conditional education expenditure decisions are never contradirectional (i.e., if one of them is significantly profemale [promale], then the other is never significantly promale [profemale]).

Therefore, the contradirectional gender bias documented in this paper is new. It is notable that the contradirectional gender bias in Bangladesh contrasts with a clear (codirectional) promale bias in India and Pakistan, particularly for the older age group. This contradirectional bias is also important because it has been clearly present since 2000 both in urban and rural areas. As elaborated later, the evidence for the presence of contradirectional bias is also robust.

This paper also makes a modest methodological contribution by extending the hurdle model to include a third equation for the core share in the total education expenditure. This additional equation enables us to detect the gender bias in the way education expenditure is used. Furthermore, we allow for correlations in the unobservable error terms across different decisions, which enables more efficient estimation than equation-by-equation estimation typically used in the literature.

This paper also contributes to the growing literature on the impact of CCT programs. These programs are found to be effective in promoting school enrollment for the targeted population (e.g., Khandker et al. (2003); Mahmud (2003); Glewwe and Kassouf (2012); Behrman et al. (2009)), though they may not help to improve education quality as shown in Mexico (Behrman et al., 2009), Bangladesh (Khandker et al., 2003), and Brazil (Glewwe and Kassouf, 2012). The impact of CCT programs on test scores, as a measure of educational performance, is weak at best (Saavedra and García, 2012). While there are some studies that examine the impact of CCT programs on the pattern of household expenditure (Maluccio and Flores, 2004; Edmonds and Shrestha, 2014; Abdoulayi et al., 2016), we offer a new angle in this literature by closely investigating the allocation of education resources within the household in the presence of a CCT program.[4]

In line with previous studies, we find that CCT programs were effective in bringing girls to schools. However, they did not attract a sufficient amount of complementary investment from households. The gap between enrolled boys and girls in school performance did not narrow as a result. While our analysis is based only on Bangladeshi data, the lack or inadequacy of complementary investment from households may be among the most important reasons why CCT programs did not achieve notable improvements in educational outcomes beyond attendance. Thus, this study offers a cautionary lesson to researchers and policy makers that simply increasing the enrollment of female students does not automatically narrow the gender gap in the quality of education that children receive.[5]

3 The Three-Part Model

We extend the hurdle model proposed in Kingdon (2005)—a model consisting of decisions on a child's school enrollment and the amount of education expenditure conditional on enrollment—in two directions. First, we extend the hurdle model to account for the gender difference in the way education expenditure is used, a point that is mostly neglected in the literature. To

[4]Note also that there are a number of studies that have examined the impact of CCT programs on noneducational outcomes such as health and cognitive abilities (Gertler, 2004; Fernald et al., 2008; Orazio et al., 2010; Paxson and Schady, 2010; Macours et al., 2012). While noneducational outcomes are also important, they are beyond the scope of this study.

[5]A related point was made in Shonchoy and Rabbani (2015). However, we provide more complete and coherent explanations of this phenomenon with more rounds of survey data and investigate the gender differences in educational performance.

see the relevance of this point, consider a household with a boy and a girl in which an equal amount is spent on their education. Suppose further that the education expenditure for the boy is mostly utilized to pay for private tutoring, whereas that for the girl is mostly used to buy better or more uniforms. This gender difference in the pattern of education expenditure would result in a gender difference in the quality of education. To address this point, we classify education expenditure items into core and peripheral components, where the former directly relates to the quality of education but the latter does not, as detailed in the next section. We then incorporate the core share of education expenditure as the third part of the model.

Second, we allow for correlations in unobservable error terms across all equations. This is important because there may be some unobservable characteristics, which may affect all three decisions simultaneously. Take unobserved innate ability as an example. Smarter children (with high innate intellectual abilities) are arguably more likely to be enrolled in school due to their high expected returns from education. On the one hand, they may require less education expenditure from the household than less smart children, because of a lower need for private tutoring or a higher chance of receiving merit-based scholarships. On the other hand, households may be encouraged to spend more money on education for children with high abilities to learn. Our model enables the data to indicate the sign and size of the correlations among the error terms arising from unobservable characteristics such as innate ability.

Formally, we consider the following three outcome variables: school enrollment $d \in \{0, 1\}$, education expenditure $y(> 0)$, and core share in education expenditure $s \in [0, 1]$, and our three-part model has the following structure:

$$d = \mathbf{1}(x'_d\beta_d + \epsilon_d > 0) \tag{1}$$

$$\log y = x'_y\beta_y + \epsilon_y \tag{2}$$

$$s = \max(0, \min(1, x'_s\beta_s + \epsilon_s)), \tag{3}$$

where $\mathbf{1}(\cdot)$ is an indicator function, and x, β, and ϵ in each equation are the vector of covariates, its coefficient vector, and the idiosyncratic error term, respectively. The covariates include, among others, a dummy variable for girl to identify the gender effect. The observed share s is related to its latent variable $s^* \equiv x'_s\beta_s + \epsilon_s$, and s is a truncated version of s^* from below at zero and from above at one. It should be noted that the education expenditure (y) and core share (s) are observable if and only if the child is enrolled in school (i.e., $d = 1$).

To allow for the dependency across the three equations, we assume that the error terms ϵ_d, ϵ_y, and ϵ_s have the following trivariate normal distribution:

$$\begin{bmatrix} \epsilon_d \\ \epsilon_y \\ \epsilon_s \end{bmatrix} \sim N\left(\mathbf{0}, \begin{bmatrix} 1 & \rho_{dy}\sigma_y & \rho_{ds}\sigma_s \\ \rho_{dy}\sigma_y & \sigma_y^2 & \rho_{ys}\sigma_y\sigma_s \\ \rho_{ds}\sigma_s & \rho_{ys}\sigma_y\sigma_s & \sigma_s^2 \end{bmatrix}\right), \tag{4}$$

where the variance of ϵ_d can be assumed to be unity without loss of generality.

There are four distinct cases to consider in this setup: 1) the child is not enrolled in school ($d = 0$), 2) the child is enrolled in school with all education expenditure going to the peripheral component ($d = 1$ and $s = 0$), 3) the child is enrolled in school with education expenditure going to both the core and peripheral components ($d = 1$ and $0 < s < 1$), and 4) the child is enrolled in school with all education expenditure going to the core component ($d = 1$ and $s = 1$).[6]

The sample log-likelihood function $l(\theta)$ can be written as:

$$\begin{aligned} l(\theta) = \sum_{i=1}^{N} l_i(\theta) &= \sum_{i=1}^{N} \{\mathbf{1}[d_i = 0] \cdot l_i^1 + \mathbf{1}[d_i = 1, s_i = 0] \cdot l_i^2 \\ &\quad + \mathbf{1}[d_i = 1, 0 < s_i < 1] \cdot l_i^3 + \mathbf{1}[d_i = 1, s_i = 1] \cdot l_i^4\}, \end{aligned}$$

where l_i^j is the log-likelihood function for child $i \in \{1, \cdots, N\}$ under case $j \in \{1, 2, 3, 4\}$ and θ is a parameter vector that includes all βs, ρs, and σs. The maximum-likelihood (ML) estimator $\hat{\theta}_{ML}$ for the three-part model can be written as $\hat{\theta}_{ML} = \arg\max_\theta l(\theta)$. We relegate the detailed derivation of the log-likelihood function l_i^j for each case to Appendix A.

The primary coefficients of interest are those on the girl dummy in β_d, β_y, and β_s. If these coefficients have positive [negative] signs, they indicate a profemale [promale] bias. An important identification assumption is that the girl dummy is exogenous. While this treatment is common in studies on gender gap, it is potentially problematic as the child's gender may be correlated with unobservable characteristics such as household's gender preference. As elaborated in Section 5, we attempt to partially address this issue through fixed-effects regressions

[6] Cases 2) and 4) are relatively rare in our data, accounting for 0.42 percent and 0.25 percent of all observations across years, respectively.

for some subsamples.

It should be noted here that the size of the coefficient does not necessarily equate with the size of the effect, because the model is nonlinear. Therefore, using the ML estimates, we calculate the marginal effects of being a girl on the probability of enrollment as well as conditional and unconditional levels of the total education expenditure and core expenditure. Because we cannot obtain a simple closed-form solution for the marginal effect due to the correlation across error terms, we need to use numerical integration to calculate marginal effects. The girl effects on d, y, and s are computed as the change in the expected value of the outcome of interest when the value of the girl dummy variable changes from zero to one. The following expressions are used for the conditional and unconditional expectations:

$$\begin{aligned}
E(d) &= P(d=1) = \Phi\left(x_d'\beta_d\right) \quad \text{(Expected enrollment)} \\
E(y|d=1) &= \int_0^\infty yf(y|d=1)dy \quad \text{(Conditional expected education expenditure)} \\
E(y) &= P(d=1)E(y|d=1) \quad \text{(Unconditional expected education expenditure)} \\
E(ys) &= \int_0^1 \int_0^\infty ysf(y,s)dyds \quad \text{(Unconditional expected core expenditure)} \\
E(ys|d=1) &= \frac{E(ys)}{P(d=1)} = \frac{E(ys)}{\Phi\left(x_d'\beta_d\right)} \quad \text{(Conditional expected core expenditure),}
\end{aligned}$$

where Φ is the cumulative density function (CDF) for a standard normal distribution and f is the probability density function. We use simulations to compute the standard errors for the equations above and evaluate only at the sample means to reduce the computational burden of numerical integrations. The details of the mathematical expressions used for numerical integrations and the simulation method for computing the marginal effects are described in Appendix B.

4 Data

We primarily use the nationally representative Household Expenditure Survey (HES) for the year 1995 and Household Income Expenditure Survey (HIES) for the years 2000, 2005, and 2010, all of which were conducted by the Bangladesh Bureau of Statistics. These data sets provide demographic and socioeconomic characteristics of households and detailed information on education expenditure for each child in a household.[7]

We report the average education expenditure conditional on enrollment for boys and girls and the difference between them for each grade, including both the primary (grades 1-5, officially ages 6-10) and secondary (grades 6-10, officially ages 11-15) levels in Figure 2 in Appendix F.[8] We note three points from this figure. First, across all survey years, the education expenditure increases with grade, particularly from the secondary level. Second, boys receive a larger investment in education than girls conditional on enrollment. Third, except for the year 1995, the gender gap in education expenditure tends to widen as the grade progresses, especially at the secondary level.

Therefore, secondary education appears to be particularly important for analysis of the gender gap. It is also worth noting that gender-based education intervention by the government existed at the secondary level but not at the primary level in our study period. The FSPs were targeted only at girls in secondary schools, whereas the Food for Education program, started in 1993, and its successor, the Primary Education Stipend program, started in 2002, were not related to the child's gender. Furthermore, passing the SSC examination, which is held at the end of the secondary education phase, is a major milestone in the Bangladeshi education system.[9] For these reasons, we choose to focus on secondary education.

We include the following basic covariates in each of the three equations (eqs. (1)-(3)) in all reported three-part regressions: the age and gender of the child, the age and gender of the household head, logarithmic household size, logarithmic expenditure per capita, the number of children in the household, the head's working status and religion, and parental education in years. In addition, we also include the urban dummy to capture the geographical heterogeneity

[7]The top 1 percent of observations with the highest total educational expenditure are dropped as outliers. Further, to apply the three-part model to the data, we choose to drop from our sample around 0.39 percent of children who were enrolled in secondary school with no education expenditure. As a result, the education expenditure for a child in our sample is always positive (i.e., $y > 0$) whenever the child is enrolled in school (i.e., $d = 1$).

[8]Secondary education is sometimes subdivided into junior secondary (grades 6-8, officially ages 11-13) and secondary (grades 9-10, officially ages 14-15) levels in Bangladesh. We do not make this distinction.

[9]Analysis of older age groups, including the higher secondary and tertiary levels, is beyond the scope of this paper, because the analysis gets more complicated for three reasons. First, early marriage and pregnancy can result in grade repetition and dropout for girls, but we have only limited information about each child beyond gender and age. As a result, our three-part model cannot adequately address these issues and our estimates are likely to be confounded with early marriage and pregnancy. Second, the passing rate of the SSC examination was historically low: below 60 percent for most years before 2007 as Figure 1 shows. This makes it difficult to see whether a child is not in school because of not being able to pass the SSC or for some other reason. Finally, the proportion of girls in higher education is very small in earlier years, making it difficult to attain reliable estimates.

in parental investment in children's education. The choice of these covariates is broadly consistent with existing studies such as Kingdon (2005), Aslam and Kingdon (2008), Masterson (2012), and Azam and Kingdon (2013).

Some covariates are assumed to affect some but not all outcomes. In eq. (1), the numbers of secondary schools and madrasas per thousand people in an area of residence are included in the set of covariates as measures of school accessibility in addition to the basic covariates discussed above. We argue that this is reasonable, because school accessibility will primarily affect the enrollment decision, particularly in developing countries where school infrastructure is inadequate. On the other hand, it will not heavily affect education expenditure once the type of school that a child goes to is controlled for.

To construct the accessibility measures, we compile the number of schools and madrasas at the district or subdivision level (district-level data from BANBEIS (1995), BANBEIS (2006), and BANBEIS (2010) for the years 1995, 2005, and 2010 and subdivision-level data from the Bangladesh Bureau of Statistics (2002) for the year 2000) and divide by the population at that level using the population figures taken from the Population and Housing Census for the year 2001.[10]

In eq. (2), we add two school-type variables (public and private) as different types of schools may affect tuition, uniform, and other education expenditure items differently.[11] The logarithmic education expenditure is separately added to control for the education expenditure in the core share equation (eq. (3)).

The upper part of Table 1 reports some descriptive summary statistics for secondary school enrollment and its covariates for children in the secondary school age group, disaggregated by children's gender for the years 1995 and 2010. It shows impressive gains in a variety of development indicators between 1995 and 2010, including the enrollment rate, nominal household

[10]In 1991, there were 5 divisions, 64 districts, and 486 subdistricts in Bangladesh (Bangladesh Bureau of Statistics, 1994, Table 2.7). While subdivision is not a commonly used unit, the Bangladesh Bureau of Statistics (2002) divides Bangladesh into 22 subdivisions.

[11]The base school type in the regressions reported in Section 5 is all schools other than public and private schools, which include NGO schools and madrasas. While the choice of school type is potentially important, we choose not to model it for two reasons. First, public secondary schools are rare in Bangladesh, accounting for less than 5 percent of all secondary schools (BANBEIS, 1995, 2006, 2010). Second, there is a significant mismatch in the type distribution of secondary schools between the HIES data and other sources. The proportion of children in public schools in our data is around 20 percent, which is much higher than 5 percent or less reported by BANBEIS (1995, 2006, 2010) and Nath et al. (2008). This discrepancy may in part stem from the public nature of private schools in Bangladesh, where private school teachers are often paid by the government under the Monthly Pay Order scheme. It should also be noted that our results remain qualitatively similar even when the school-type variables are dropped from the regression.

Table 1: Summary statistics of basic covariates by gender for 1995 and 2010 (secondary school age group)

	1995				2010			
	Boy (B)	**Girl (G)**	**G-B**	**All**	**Boy (B)**	**Girl (G)**	**G-B**	**All**
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
All children aged 11-15								
Enrolled in secondary school	0.355	0.375	0.020	0.365	0.483	0.572	0.089	0.526
	(0.479)	(0.484)		(0.481)	(0.500)	(0.495)	***	(0.499)
Child's age (yrs)	13.032	12.906	-0.126	12.972	12.999	12.922	-0.077	12.962
	(1.370)	(1.353)	***	(1.363)	(1.394)	(1.379)	**	(1.388)
HH per capita expenditure	10.529	11.836	1.307	11.146	29.195	29.663	0.468	29.420
(thousand BDT/year)	(9.241)	(11.958)	***	(10.630)	(22.244)	(24.128)		(23.169)
Household size	6.638	6.802	0.164	6.716	5.526	5.599	0.073	5.561
	(2.519)	(2.512)	**	(2.517)	(2.011)	(1.869)		(1.944)
Father's education (yrs)	3.771	4.021	0.250	3.889	2.883	2.990	0.107	2.934
	(4.495)	(4.637)	*	(4.564)	(4.223)	(4.305)		(4.263)
Mother's education (yrs)	2.027	2.317	0.290	2.164	2.568	2.699	0.131	2.631
	(3.174)	(3.409)	***	(3.290)	(3.663)	(3.775)		(3.718)
Number of children	3.649	3.790	0.141	3.716	2.932	3.024	0.092	2.976
	(1.861)	(1.909)	***	(1.885)	(1.440)	(1.442)	**	(1.442)
Urban	0.318	0.371	0.053	0.343	0.349	0.343	-0.006	0.346
	(0.466)	(0.483)	***	(0.475)	(0.477)	(0.475)		(0.476)
Female head	0.085	0.091	0.006	0.088	0.131	0.138	0.007	0.134
	(0.279)	(0.287)		(0.283)	(0.337)	(0.345)		(0.341)
Head is a wage worker	0.354	0.366	0.012	0.360	0.447	0.445	-0.002	0.446
	(0.478)	(0.482)		(0.480)	(0.497)	(0.497)		(0.497)
Head's age (yrs)	46.488	46.525	0.037	46.505	47.167	46.925	-0.242	47.051
	(11.198)	(11.101)		(11.151)	(10.568)	(10.604)		(10.586)
Muslim	0.897	0.891	-0.006	0.894	0.899	0.886	-0.013	0.893
	(0.304)	(0.312)		(0.308)	(0.301)	(0.318)	*	(0.309)
Hindu	0.094	0.100	0.006	0.097	0.091	0.104	0.013	0.097
	(0.293)	(0.300)		(0.296)	(0.288)	(0.305)	*	(0.297)
Father's education missing	0.003	0.001	-0.002	0.002	0.176	0.196	0.020	0.186
	(0.058)	(0.035)		(0.049)	(0.381)	(0.397)	**	(0.389)
Mother's education missing	0.003	0.001	-0.002	0.002	0.058	0.077	0.019	0.067
	(0.058)	(0.035)		(0.049)	(0.234)	(0.266)	***	(0.250)
Obs	2,667	2,386		5,053	3,323	3,079		6,402
Enrolled in secondary school children aged 11-15								
Govt school	0.17	0.18	0.01	0.18	0.22	0.21	-0.01	0.22
	(0.37)	(0.39)		(0.38)	(0.42)	(0.41)		(0.41)
Private school	0.79	0.80	0.01	0.80	0.68	0.69	0.01	0.69
	(0.41)	(0.40)		(0.40)	(0.47)	(0.46)		(0.46)
Other	0.05	0.01	-0.04	0.03	0.10	0.10	0.00	0.10
	(0.21)	(0.11)	***	(0.17)	(0.29)	(0.30)		(0.30)
Obs	947	895		1,842	1,605	1,760		3,365

Note: Standard deviations are reported in parentheses below the mean. ***,** , and * denote that the means for girls and boys are different at 1, 5, and 10 percent significance levels, respectively, by a t-test of equality of means. Other school includes all types of schools other than public and private schools, including religious schools (like madrasas) and NGO schools.

income, and mother's education. The bottom part of the table provides a breakdown of the school types among children who are enrolled in a secondary school.[12]

There are two important observations to make from Table 1. First, the first row shows that girls are on average more likely to be enrolled in secondary school than boys. The gender difference in enrollment was small and not significantly different from zero in 1995 even at a 10 percent level, but it has become larger and statistically significant since the year 2000. This is consistent with the common observation of the reversal of the gender gap from promale to profemale in school enrollment in Bangladesh in recent years (e.g., Asadullah and Chaudhury (2009)).

Second, Table 1 shows that there are some important differences between boys and girls in their households' demographic characteristics. In particular, girls tend to live in a larger household than boys. This difference is observed for all rounds of the survey and creates a potential endogeneity concern. We address this issue in Section 5 and Appendix C.

To apply the three-part model to data, we categorized the education expenditure items into core and peripheral components. We choose to include expenditures for tuition, private tutoring, and materials (e.g., textbooks, exercise books, and stationery) in the core component. The peripheral component includes all other items, including admission, examination, uniform, meals, transportation, and others, which would only have a marginal relevance to the quality of education at best.

Because the choice of items in the core component is not obvious, let us explain the reasons for including tuition, private tutoring, and materials in the core component. First, it is reasonable to include the tuition fee in the core component because it reflects, at least to some extent, the quality of education provided by schools in Bangladesh. If schools face some degree of competition, those schools that consistently provide only low-quality education for high tuition fees will exit the market such that a positive correlation between the quality of education and tuition will emerge. The force of competition is likely to be important in Bangladesh where a large majority of secondary schools are private.[13]

Second, private tutoring is also a key item of the core component. It is widely documented

[12]The summary statistics for the years 2000 and 2005 corresponding to Table 1 are reported in Table 16 in Appendix F.

[13]A preliminary analysis of a separate data set in a companion paper shows a positive relationship between the average tuition fee and test score at the primary level. This also serves as suggestive evidence that a higher tuition fee reflects a higher quality of education. Results are available upon request.

that private tutoring can be an important educational input (Bray, 1999, 2003), because it is associated with better learning achievements for the students (Nath, 2012; Asadullah et al., 2018). This is also the case in Bangladesh (Nath, 2008; Hamid et al., 2009); it is not uncommon in Bangladesh for public school teachers to serve as private tutors for their students. In some cases, teachers may deliberately teach less in the regular classes to gain more income from private tutoring. Thus, there are good reasons to include private tutoring in the core component.

Nevertheless, the spending on private tutoring must be interpreted with caution. On the one hand, private tutoring would raise the overall quality of education that the child receives. On the other hand, if private tutoring is given only to weaker students and boys are generally weaker than girls, the promale bias in the core share shown below may be driven by the relatively weak academic performance of boys. We argue that this latter possibility is unlikely to be important, given that girls have underperformed both in the passing rate and the share of top students in the SSC examination over the years as shown in Figure 1.

Finally, it is also reasonable to include materials in the core component, because reading more textbooks and doing more problems in exercise books also contribute directly to academic performance. However, one could argue that more expensive books are not necessarily of higher quality. Thus, the inclusion of materials in the core component is admittedly disputable. To address this concern, we also repeated the analysis excluding the materials from the core component. It turns out that the results are qualitatively similar. Thus, our results are not driven by the inclusion of the materials in the core component. In sum, our choice of the definition of the core component is reasonable, if not undisputable.

Table 2 reports summary statistics of education expenditure items in nominal terms for the years 1995 and 2010 using a subsample of children who were enrolled in secondary school at the time of the survey.[14] The italicized items below each of the Core and Peripheral rows represent the underlying items in these components, respectively. As the bottom of the table shows, the average total education expenditure increased rapidly between 1995 and 2010. Its annualized average growth rate in this period is 7.3 percent, which is substantially larger than the average annual inflation rate of 5.9 percent in consumer prices based on the World Development Indicators.

Table 2 also shows that the core component accounts for roughly two thirds of the total

[14]The same summary statistics for the years 2000 and 2005 are reported in Table 17 in Appendix F.

Table 2: Summary statistics of annual education expenditure in BDT by items for secondary school enrollees in 1995 and 2010

	1995				2010			
	Boy (B)	**Girl (G)**	**G-B**	**% Zeros**	**Boy (B)**	**Girl (G)**	**G-B**	**% Zeros**
Item	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Core	1,873	1,834	-39	1%	5,631	5,040	-591	1%
	(2,341)	(3,025)			(6,563)	(8,133)	**	
Tuition	322	212	-110	31%	601	408	-193	46%
	(770)	(344)	***		(1,169)	(2,044)	***	
Private Tutoring	938	1,012	74	44%	3,589	3,213	-376	27%
	(1,791)	(2,755)			(5,624)	(6,293)	*	
Material	613	609	-4	1%	1,442	1,419	-23	1%
	(464)	(441)			(1,052)	(1,086)		
Peripheral	775	826	51	1%	2,347	2,288	-59	0%
	(1,036)	(1,139)			(3,332)	(2,877)		
Admission	139	152	13	24%	479	406	-73	21%
	(249)	(233)			(1,327)	(1,142)	*	
Exam	120	127	7	5%	313	303	-10	6%
	(155)	(145)			(352)	(303)		
Uniform	222	255	33	45%	621	650	29	20%
	(298)	(280)	**		(545)	(792)		
Meal	49	29	-20	99%	426	394	-32	58%
	(553)	(616)			(837)	(806)		
Transportation	110	136	26	80%	251	392	141	84%
	(455)	(478)			(1,005)	(1,407)	***	
Others	135	127	-8	44%	257	142	-115	75%
	(285)	(350)			(1,745)	(899)	**	
Total	2,648	2,660	12		7,979	7,328	-651	
	(2,940)	(3,611)			(8,318)	(9,961)	**	
Core Share	0.68	0.65	-0.03		0.66	0.64	-0.02	
	(0.19)	(0.20)	***		(0.20)	(0.19)	***	
Obs	947	895		1,842	1,605	1,760		3,365

Note: Standard deviations are reported in parentheses below the mean. ***,** , and * denote that the means for girls and boys are different at the 1, 5, and 10 percent significance levels, respectively. The summary statistics are for the subsample of the children who were enrolled in school at the time of the survey. Core share stands for the ratio of core components to the total education expenditure. The annual session and registration fees are included in admission because they are not separately reported in HES 1995.

education expenditure and boys have a significantly higher core share than girls. Within the core component, private tutoring is the major expenditure item, but a considerable share of children spend nothing on private tutoring in both years. There is an obvious trend of increasing popularity in private tutoring over the years, particularly among higher grades. In 1995, 57 percent of male and 55 percent of female secondary school students reported having spent a positive amount on private tutoring, and these ratios respectively increased to 76 percent and 71 percent in 2010. Further, among those with positive spending on private tutoring, its share in the total education expenditure also went up slightly from 40 percent and 41 percent, respectively, for boys and girls in 1995 to 44 percent and 43 percent in 2010. Taken together, these show increasing dependency on private tutoring and an increasing gender gap in the use of private tutoring, both in the intensive and extensive margins. Hence, parents are willing to invest more in children's, particularly boys', education for a better quality of education beyond the basic educational costs like school fees.[15] It is also notable that girls on average spend less on tuition. Further, a significant share of children spend nothing on tuition (31 percent in 1995 and 46 percent in 2010), which can be explained by the tuition waiver provided by various programs including the FSPs as discussed in detail in Section 6.

5 Contradirectional Gender Gap

In this section, we document the persistent contradirectional gender gap using the three-part model developed in Section 3. We first present the ML estimates and then compute the marginal effects of being a girl, which have direct quantitative interpretations.

Estimation of coefficients

Table 3 presents the ML estimates of the coefficient on the girl dummy—the covariate of primary interest—in the three-part model for each year and for each of the primary and secondary school age groups. All the reported estimates have standard errors clustered at the household level. Columns (1)-(3) are the estimates for the primary school age group and columns (4)-(6) for the secondary school age group. As the table shows, the significance of the gender gap for

[15] Alternative interpretations are also possible here. For example, the increasing popularity of private tutoring may reflect the deteriorating quality in school education because of the overcrowding of classrooms or teacher absenteeism (Banerjee and Duflo, 2006).

Table 3: ML estimation of the three-part model by years and age groups

	Primary school age (6-10)			Secondary school age (11-15)		
	d	Cond y	Cond s	d	Cond y	Cond s
Coef.	(1)	(2)	(3)	(4)	(5)	(6)
1995						
Girl	-0.039	-0.021	-0.016	-0.000	-0.088***	0.003
	(0.037)	(0.033)	(0.011)	(0.042)	(0.033)	(0.035)
Obs.		6,538			5,053	
2000						
Girl	0.065*	-0.090***	0.007	0.330***	-0.173***	-0.075***
	(0.039)	(0.035)	(0.010)	(0.039)	(0.049)	(0.015)
Obs.		5,651			4,951	
2005						
Girl	0.033	-0.062*	-0.022***	0.272***	-0.141***	-0.061***
	(0.037)	(0.032)	(0.009)	(0.035)	(0.028)	(0.012)
Obs.		6,556			5,723	
2010						
Girl	0.162***	-0.064**	0.002	0.256***	-0.119***	-0.050***
	(0.038)	(0.028)	(0.009)	(0.033)	(0.025)	(0.009)
Obs.		7,416			6,402	

Note: ***,** , and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. Standard errors clustered at the household level are reported in parentheses. The estimation is based on the three-part model constructed in Section 3. In all regressions, the following covariates are also included: logarithmic per capita expenditure, logarithmic household size, father's and mother's education in years, number of children, female head, wage worker head, head's age and religion (Muslim/Hindu), urban area, and dummy variables for the child's age and whether the father's and mother's education are missing. In addition, the school accessibility variables, school-type dummy variables (public/private), and logarithmic education expenditure are also included in the equations for x_d, x_y, and x_s, respectively. Detailed results for the secondary school age group are presented in Table 18 in Appendix F.

the primary school age group is smaller both economically and statistically than that for the secondary school age group, and thus we hereafter focus on the analysis of the secondary school age group. While we allow for dependence in error terms, equation-by-equation regressions under the assumption that ρ's are all zero yield similar results.[16]

Column (4) of Table 3 shows the presence of a clear and strong profemale bias in the enrollment decision from the year 2000 onwards, after controlling for the observables discussed in Section 4. In other words, other things being equal, parents are more likely to send girls to school than boys. Column (5) reveals that, conditional on enrollment, households spend significantly less on the secondary education of girls than that of boys in all four survey rounds.

[16] The results are presented in Table 19 in Appendix F.

Further, conditional on enrollment, the core component for girls tends to account for a lower share of the total education expenditure than that for boys as shown in column (6). Our analysis thus uncovers the presence of a persistent contradirectional gender gap.

Columns (4)-(6) of Table 3 also indicate that the gender gap in 1995 is different from the three recent rounds. While we still see a promale bias in the conditional education expenditure, the coefficient on the girl dummy in 1995 is substantially smaller in absolute value than those in other years. Furthermore, the estimated coefficients on the girl dummy in the enrollment and core share equations are insignificant. We attempt to explain this observation in Sections 6. Note that we only presented its estimated coefficients in Table 3, because the girl dummy is our main covariate of interest. The complete regression results for columns (4)-(6) of Table 3 together with some additional discussions are provided in Appendix F.

Marginal effects

Our regression coefficients from the three-part model do not provide readily interpretable quantities. Hence, we report the marginal effect of being a girl at the sample mean in Table 4, using the formula presented at the end of Section 3. Column (1) shows the presence of a significant profemale bias in the probability of enrollment except in 1995. For example, girls are 9.9 percentage points more likely to enroll in secondary schools than boys at the sample mean in 2010. The effects of being a girl on the total education expenditure and core expenditure conditional on enrollment are shown, respectively, in columns (3) and (5). If we focus on school enrollees, girls enjoy less total education expenditure and less core expenditures than boys.

For example, column (3) shows that the gender difference in the total education expenditure in 2005 was 465.2 BDT at the mean of the subsample of secondary school enrollees. Similarly, there exists a significant promale bias in the core expenditure from 2000 onwards. However, as shown in column (2), when we consider the combined effect of enrollment and conditional expenditure, girls actually have a higher unconditional education expenditure than boys except for the year 1995. Further, the gender gap in the unconditional core expenditure is negligible as column (4) shows. These observations highlight the importance of clearly distinguishing the conditional and unconditional expectations.

The results above consistently show that girls received less expenditure in the core compo-

Table 4: Marginal effects of the girl dummy at the sample mean

Marginal effects	E(d)	E(y)	E($y \mid d=1$)	E(ys)	E($ys \mid d=1$)
at the sample mean	(1)	(2)	(3)	(4)	(5)
1995	0.000	-38.7	-184.0**	-2.6	-110.9
	(0.017)	(27.2)	(74.1)	(29.3)	(107.8)
Obs.	5053	5053	1842	5053	1842
2000	0.126***	153.3***	-230.5***	3.7	-315.8***
	(0.016)	(37.5)	(83.8)	(16.4)	(58.2)
Obs.	4951	4951	1955	4951	1955
2005	0.106***	117.0**	-465.2***	-23.6	-408.4***
	(0.014)	(49.1)	(97.0)	(19.8)	(65.1)
Obs.	5723	5723	2659	5723	2659
2010	0.099***	284.4***	-519.7***	-20.2	-542.3***
	(0.014)	(86.8)	(167.7)	(36.0)	(120.0)
Obs.	6402	6402	3365	6402	3365

Note: ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. Standard errors in parentheses are obtained by simulation with 100 replications (see Appendix B for details). E($\cdot$) stands for the expectation operator. Estimates in column (1) are the marginal effect of the girl dummy on the expected enrollment in secondary school for the children in the secondary school age group. The marginal effects presented in columns (2) to (5) are in BDT in nominal terms. Unconditional [conditional] expectations are evaluated at the mean of the full sample [subsample of secondary school enrollees].

nent than boys conditional on enrollment, and this gender gap grew over time. To identify the source of this growing gap, we compute the marginal effect of being a girl at the sample mean for the secondary school enrollees using alternatively item-by-item Tobit regressions. The results of this analysis (Table 22 in Appendix F) show that girls receive significantly less investment in tuition than boys for all the survey years. Girls also receive less in private tutoring, though the differences are statistically insignificant at the conventional level. On the other hand, the only item for which girls somewhat consistently receive a higher amount is uniform, but this difference does not make up for the disadvantages in other expenditure items. Therefore, girls have overall lower education expenditure and lower core expenditure conditional on enrollment and this female disadvantage mainly comes from tuition.

Robustness of contradirectional gender gap

Our identification relies on the implicit assumption that the sex of the child is exogenous. However, this assumption would be violated when parents have an unobserved gender preference, which would be correlated with the sex of the child. For example, such a gender preference may lead to sex-selective abortion. However, sex-selective abortion is unlikely to be a relevant concern in Bangladesh, since sex ratios at birth did not change between 1993 and 2011 (Talukder et al., 2014). Gender preference may also lead to a fertility stopping rule, in which households stop having additional children when a desired number of boys has reached. Consequently, this makes girls to reside in larger households. This possibility is consistent with the summary statistics in Table 1. To address this potential endogeneity concern, we run linear regressions with household fixed effects, controlling for all time-invariant household characteristics. The gender difference in household composition also affects intrahousehold competition that girls and boys face. We address this by analyzing a subsample of households with only one child and a subsample of children living in households with one boy and one girl and consistently find a contradirectional gender gap. The details of these exercises and the discussion of other relevant results are provided in Appendix C.

6 Analyzing the Role of FSPs

The contradirectional gender gap reported in the previous section is unique to Bangladesh and deserves further investigation. We conjecture that the FSPs may have played a role here for two reasons. First, the FSPs would encourage girls' school enrollment but may not necessarily affect the total education expenditure and core share conditional on enrollment. Second, India and Pakistan, which did not have a nationwide program similar to the FSPs in Bangladesh, exhibit a clear codirectional promale bias.

We start with a brief background of the FSPs. Then, we provide supporting evidence for the relevance of the FSPs to the contradirectional gender gap in four different ways. First, we focus on the impact of the FSPs on the quantity measures of education using the double-difference approach as this analysis provides relatively clean identification. Then, we incorporate in the three-part model the individual status of being an FSP recipient and the girl recipient ratio

(GRR). The latter is defined as the number of FSP recipients over the total number of girls of the same age in the division of residence and interpreted as a measure of the FSP intensity. Third, because the core share may be directly affected by the tuition waiver awarded to the FSP recipients, we mute its effect either by excluding the tuition from the analysis or by imputing the tuition for FSP recipients. Finally, we analyze the gender gap in the educational outcome using timely graduation from the secondary school as an outcome indicator.

Background of FSPs

The FSPs, which started as a small pilot program in 1982 and were rolled out nationwide in 1994, consist of the following four projects: 1) the Female Secondary School Assistance Project, 2) the Female Secondary Stipend Project, 3) the Secondary Education Development Project, and 4) the Female Secondary Education Project. These projects are similar except that their funding agencies and the locations of operation differ. FSPs' target population is unmarried girls studying in secondary schools outside of the metropolitan areas that have signed a participation agreement. At the entry grades (grades 6 and 9), all female students in participating schools are eligible to benefit from the FSPs regardless of past attendance or performance. However, the following three conditions must be maintained to remain in the program: i) attending at least 75 percent of school days, ii) achieving minimum marks of 45 percent in the annual school examination, and iii) staying unmarried until the SSC examination. The stipends are disbursed in two equal installments per academic year and the amount increases as the grades progress. The FSP recipients are also entitled to enjoy free tuition and schools are paid directly by the FSPs. However, around 15 percent of the FSP recipients, including both private- and public-school children, pay a small amount for tuition fee in our data. The FSPs' financial assistance is designed to cover slightly less than half of the expenditure on secondary education.[17]

The nationwide rollout of FSPs took place rapidly between 1994 and 1995. According to BANBEIS (2006), the number of FSP recipients was only 70 thousand in 1994. The number jumped to 1.4 million in 1995 and more than doubled in the following two years. It continued

[17] The monthly stipend amount starts from 25 BDT for grade 6 and reaches 60 BDT for grade 10, The tuition fee paid under FSPs also increases from 10 BDT per month in grade 6 to 15 BDT per month in grade 10 for public schools, and the amount is higher for private schools by 5 BDT per month. In addition, the book allowance and examination fee are given to grade 9 and 10 recipients, respectively. See also Table 2 of the Bangladesh Ministry of Education (1996) for further details of the FSPs.

to increase rapidly until reaching its peak of 4.2 million in 2002, after which it dropped to 2.3 million in 2005. These numbers are sizable both in absolute terms and relative to the cohort size (17.3 million in 2005) and the total enrollment (7.4 million in 2005) for the secondary school age group.

However, with the intention of improving the quality of education and reaching out to the poor regardless of the gender, the FSPs were subsequently replaced by the Secondary Education Quality and Access Enhancement Program (SEQAEP) in 2008, which targeted the poor in remote subdistricts in Bangladesh. Thus, the FSPs are relevant only to the early three rounds of our analysis, namely 1995, 2000, and 2005, whereas the SEQAEP was in place by 2010.

Because of the lack of clarity in the way the resources for the FSPs were allocated in practice and because of the lack of information on the individual FSP eligibility in our data set, we use the FSP status—whether the individual is actually receiving the stipends—in our analysis. Along with this problem, it is also difficult to obtain a clean identification of the impacts of the FSPs for two additional reasons. First, the assignment of FSPs is nonrandom as there are some eligibility criteria as noted above. Second, we have limited data before the national roll-out of the FSPs. In particular, the individual-level information on education expenditure is only available from the year 1995 when the FSPs were already available nationwide. Therefore, we start the analysis of the FSPs with quantity measures of education to enable a (relatively) clean identification through a double-difference approach.

Impact of the FSPs on the quantity of education

In this subsection, we focus on the impact of the FSPs on two quantity measures of education. The first quantity measure of education is the completed years of education (YrEdu_{ih}) for each working-age individual i between 19 and 65 years of age in each household h for each HIES survey round. The second analysis of a quantity measure of education is based on the retrospective panel data on enrollment (Enroll_{iht}) for each child i in household h in calendar year t. The retrospective panel data are created under the assumptions that each child enters secondary school (grade 6) at the stipulated secondary school entry age of age 11 and that no child repeats a grade.[18] Then, we go back through the calendar year to determine whether the child was in school. As an example, consider a boy who is 17 years old in 2005. If he completed grade 8, the last age at which he was in school would be 13. Therefore, he was in a secondary school between 1999 and 2001 (ages 11-13) and out of school between 2002 and 2005 (ages 14-17). We do this for all individuals born in or after 1949 in each round of the HIES survey up to 2007 and focus on the records that correspond to the secondary school ages of 11-15, such that the calendar year for the analysis starts from 1960 (= 1949 + 11).[19]

We estimate the impacts of the FSPs on these quantity measures using double-difference regressions, where one difference is taken between the two genders and the other between those who are covered and not covered by the FSPs. Specifically, we obtain from Table 3 of Shamsuddin (2015, p. 432) the year in which each subdistrict was covered by an FSP and use it to determine the FSP coverage (FSPCover), or whether an individual is in a subdistrict covered by an FSP in the reference year. Here, the reference year is year t [the calendar year in which the child is aged 11] for the regression of Enroll [YrEdu]. The construction of FSPCover is based on the assumption that the location of individuals does not change over time and this is a reasonable approximation, because the migration rate is low, especially in early years, in Bangladesh. Since the rollout of the FSPs is plausibly exogenous and all unobservable time-invariant household effects are controlled for, the double-difference approach substantially reduces the endogeneity concerns. While the timing of the FSP rollout is potentially endogenous, we argue below that the endogeniety issue is unlikely to seriously affect our results.

We use the following double-difference specifications:

$$\begin{aligned}\mathrm{YrEdu}_{ih} &= \alpha_1 \mathrm{Girl}_{ih} + \alpha_2 \mathrm{FSPCover}_{ih} + \alpha_3 \mathrm{Girl}_{ih} \times \mathrm{FSPCover}_{ih} \\ &\quad + \sum_b \mu_b \times \mathbf{1}(\text{Birth year}_{ih} = b) + \omega_h + \varepsilon_{ih}, \qquad (5)\end{aligned}$$

[18] According to BANBEIS (1995, 2010), the repetition rate was around 5 percent and 4 percent in the years 1995 and 2010, respectively. Thus, our nonrepetition assumption serves as a reasonable approximation.

[19] We followed Heath and Mobarak (2015) to determine the starting year of our study period. The results remain similar even when we shift the starting year to 1980.

Table 5: Impacts of the FSPs on the quantity measures of education

	HES 1995	**HIES 2000**	**HIES 2005**	**HIES 2010**
Coef.	(1)	(2)	(3)	(4)
	Panel A: Years of education			
Girl	-1.979***	-1.742***	-1.783***	-1.809***
	(0.041)	(0.039)	(0.036)	(0.038)
FSPCover	0.177	-1.493***	-0.506	-0.513
	(1.167)	(0.552)	(0.485)	(0.470)
Girl × FSPCover	0.016	0.933	1.809***	1.836***
	(1.392)	(0.715)	(0.128)	(0.090)
Obs	18,303	18,823	24,912	29,519
Mean of dep. var.	3.460	3.607	4.193	4.410
	Panel B: Enrollment using retrospective data			
Girl	-0.134***	-0.143***	-0.158***	-0.161***
	(0.004)	(0.004)	(0.004)	(0.004)
FSPCover	-0.061	-0.141***	-0.071	-0.067
	(0.050)	(0.048)	(0.052)	(0.047)
Girl × FSPCover	0.106***	0.173***	0.192***	0.195***
	(0.016)	(0.010)	(0.008)	(0.008)
Obs	102,319	110,439	150,518	162,056
Mean of dep. var.	0.265	0.279	0.319	0.335

Note: ***,** , and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. Standard errors clustered at the household level are reported in parentheses. In Panel A, we additionally include the fixed-effects terms specific to the birth year and household. In Panel B, we additionally include the fixed-effects terms specific to the birth year, age at the time of observation, household, and year of observation.

and

$$\begin{aligned}\text{Enroll}_{iht} &= \alpha_1\text{Girl}_{ih} + \alpha_2\text{FSPCover}_{iht} + \alpha_3\text{Girl}_{ih} \times \text{FSPCover}_{iht} \\ &+ \sum_{a=11}^{15} \beta_a \times \mathbf{1}(\text{Age}_{iht} = a) + \sum_b \mu_b \times \mathbf{1}(\text{Birth year}_{ih} = b) + \lambda_t + \omega_h + \varepsilon_{iht}, \quad (6)\end{aligned}$$

where μ_b , β_a, λ_t, and ω_h represent, respectively, birth-year-, age-, time-, and household-specific fixed effects. ε is the idiosyncratic error term. Our main coefficient of interest is α_3 on Girl × FSPCover in both equations.

Table 5 shows the OLS regression results of the two equations above. Panel A reports the regressions of the FSP coverage on the completed years of education for working-age individuals for each survey round, where the mean of the dependent variable for a given round is reported in the last row. Because the overwhelming majority (99.7 percent) of the working-age adults in 1995 were not covered by the FSPs, it is not surprising that the impact of the FSPs on the years of completed education is insignificant (column (1)). In the later rounds when the FSPs started to rapidly roll out nationwide, the years of schooling increased significantly for girls who were eligible for the FSPs at the age of 11. Column (4) shows that the promale gender gap in the years of education narrowed by 1.836 years after the FSPs rolled out.

Panel B presents the regression of the enrollment status for secondary school children aged between 11 and 15. The first row indicates that girls are *less* likely to be in secondary school than boys by 13-16 percentage points across years, but the FSPs had a significantly positive impact and indeed more than offset this negative effect of being a girl after 2000 as the third row shows. For example, column (4) shows that the positive impact of the FSPs on enrollment was 19.5 percentage points, reversing a promale gap of 16.1 percentage points to a profemale gap of 3.4 (= 19.5 - 16.1) percentage points with a t-statistic of 31.1. This profemale gap is both statistically and economically significant.

The double-difference specification significantly reduces the endogeneity concerns, because it is immune to selection on time-invariant household characteristics. However, one might argue that the rollout of the FSPs is not random. That is, the government and donors may have chosen to start the program in places where the promale gender bias is most prevalent or these places are different in other dimensions which may have an impact on our estimations. Nevertheless, the selection of program areas is unlikely to be a serious threat to our identification, since the coverage of the FSPs was extremely limited before 1994[20] and it expanded rapidly in 1994. Put differently, our identification is primarily through the interaction between the girl dummy and cohorts born after 1983 (= 1994 - 11) and not through the differences in timing in the implementation of the FSPs across subdistricts. Further, we have conducted a falsification test to boost the credibility of the discussion above. In this test, we focus on the period in which FSPs were not introduced and re-estimate the impact of FSPs by hypothetically shifting the introduction of the FSPs in each subdistrict earlier by five years (thus, for a majority of subdistricts, we pretend that the FSP coverage started in 1989 instead of in 1994). As expected, the impact of FSP coverage in the falsification test was found to be small in absolute value and statistically insignificant. Further details of the falsification test is given in Appendix D.

[20]For example, among working adults aged between 19 and 65 in 2010, only 2 percent of the FSP coverage came from the pre-1994 period.

It should also be noted that our finding of the positive impact of the FSPs on enrollment is in line with existing studies (Khandker et al., 2003; Schurmann, 2009; Asadullah and Chaudhury, 2009; Shamsuddin, 2015). However, it is notably at odds with Heath and Mobarak (2015, hereafter HM), who found no positive impact of the FSPs on female enrollment. Instead, they found that what led to an improvement in female secondary education—in their study areas—was an increasing demand for female labor.

Their analysis is based on a triple-difference approach, where primary school children are used as a comparison group for the third difference in addition to the two differences in our double-difference estimation (i.e., the difference between the two genders and the difference between before and after the coverage by the FSPs). Thus, to understand the source of the difference from HM clearly, we also conducted a triple-difference analysis. We first replicated their results and progressively changed some elements of their analysis, including the data, the subdistricts studied, and the definitions of the FSP coverage and eligibility criterion. This exercise shows that the HM's findings are driven by a combination of the particular data they used, geographic coverage of their data, and the FSP eligibility criterion used in their study. In particular, their FSP eligibility criterion of at least six years of schooling appears to have led to an underestimation of the FSPs' impact on enrollment. Those girls who have completed a primary school are eligible for the FSPs if they go to a secondary school. This means that girls who are in grade 6 (and thus have not yet completed six years of schooling) are already able to benefit from the FSPs. Our preferred estimate of the FSPs' impact on enrollment within the framework of the triple-difference estimation, which uses the nationally representative HIES data and the completion of primary school as the eligibility criterion for the FSPs, shows that the FSPs' impact on enrollment is positive and statistically significant. Appendix E provides further details of this exercise and explain why we prefer the double-difference estimation discussed earlier over the triple-difference estimation discussed here.

Incorporating the FSPs in the three-part model

To obtain a more comprehensive understanding of the FSPs' impact on education expenditure, we now incorporate the FSPs in the three-part model using the HIES data for the years 2000

Table 6: Three-part model estimation with the FSP status

Year	*Coef.*	*d*	**Cond** *y*	**Cond** *s*	*d*	**Cond** *y*	**Cond** *s*
		(1)	(2)	(3)	(4)	(5)	(6)
2000	Girl	0.330***	-0.216***	-0.056***	0.193**	-0.191**	-0.004
		(0.039)	(0.056)	(0.018)	(0.090)	(0.086)	(0.027)
	FSP		0.077	-0.032**		0.107**	-0.033**
			(0.049)	(0.014)		(0.050)	(0.015)
	GRR				0.852**	-1.376***	0.266**
					(0.339)	(0.308)	(0.129)
	Girl × GRR				0.472*	-0.177	-0.161**
					(0.281)	(0.265)	(0.080)
	Obs.		4,951			4,951	
2005	Girl	0.270***	-0.155***	-0.049***	0.079	-0.112	-0.005
		(0.035)	(0.035)	(0.014)	(0.093)	(0.073)	(0.026)
	FSP		0.027	-0.024**		0.054	-0.025***
			(0.037)	(0.009)		(0.037)	(0.010)
	GRR				0.465	-1.068***	0.020
					(0.306)	(0.236)	(0.101)
	Girl × GRR				0.708**	-0.214	-0.158*
					(0.314)	(0.238)	(0.083)
	Obs.		5,723			5,723	

Note: ***,** , and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. Standard errors clustered at the household level are reported in parentheses. Girl recipient ratio (GRR) is the ratio of girl recipients to all girls for a given age group in a given division. The covariates discussed in Table 3 are also included in all regressions.

and 2005 as they contain information on the individual status of the receipt of FSPs.[21] This is important, because the education expenditure of the FSP recipients is affected by the tuition waiver and stipend provided by the FSPs. Thus, we include the dummy variable for the FSP recipients, who are all girls, in the conditional expenditure and core share equations.

The regression results are reported in columns (1)-(3) of Table 6. As the comparison with Table 3 shows, the inclusion of the FSP dummy makes the coefficients on the girl dummy in the conditional expenditure and core share equations even more negative. The point estimates on the FSP dummy are positive in the conditional expenditure equation, while they are significantly negative in the core share equation for both years.

[21]HES 1995 does not contain the information on FSP status. HIES 2010 was not used either because the FSPs had already been terminated by then. It should also be noted that the HIES 2000 data set appears to underrepresent the FSP recipients. Based on BANBEIS (2006), the ratio of the number of FSP recipients to the number of female enrolled secondary school students is 86 percent, while the figure directly derived from the HIES 2000 data is 58 percent. Therefore, the interpretation of the results for the year 2000 requires some caution. This issue does not exist for the year 2005.

To understand where this impact is coming from, we report in Table 23 in Appendix F the marginal effects by item-by-item Tobit regressions that include both the girl and FSP-recipient dummy variables. This analysis shows that the FSP recipients spend less on tuition as expected, because the tuition is waived for the FSP recipients. The FSP recipients receive more expenditure on private tutoring and materials compared with nonrecipients, but this positive effect of the FSPs does not offset the negative effect of being a girl. Thus, the recipients of the FSPs still do not enjoy as much core education expenditure as boys. For the peripheral items, FSP recipients get a higher expenditure in most items, especially in uniform, meals, and transportation with the notable exception of admission. Overall, this analysis indicates that the FSPs did not increase the core expenditure among school enrollees.

Next, we study the spillover effect of FSPs by exploiting the variations across regions and ages in the (treatment) intensity of FSPs as measured by the GRR. In columns (4)-(6) of Table 6, we report the results of the three-part model estimation that includes as covariates the GRR and its interaction with the girl dummy in addition to all the covariates used in columns (1)-(3) of the same table. These results show that girls living in more FSP-intensive divisions (for their age) are more likely to be enrolled in school. This indicates that FSPs may have a positive spillover effect on families living in the same area such that parents are more likely to enroll their children, particularly daughters, in school. However, there is no evidence that FSPs facilitate parental investment in the quality of education for girls. The coefficient on the interaction terms in the conditional education expenditure is negative for both 2000 and 2005, and the same coefficient in the conditional core share equation is significantly negative in both years. However, caution must be exercised when interpreting these estimates, because they are based on an assumption that the differences across divisions in the outcomes of interest for a given age group can be attributed to the differences in GRR conditional on other covariates.

We also investigate the spillover effect of FSPs on boys' education expenditure. Due to the nonrandom assignment of FSPs and the limited data of the pre-FSP period, clean identification is difficult. Nevertheless, we provide some supporting evidence of the spillover impact of the FSPs by comparing the education expenditure of boys from households with and without an FSP recipient. We estimate the three-part model with a subsample of boys (Panel A) and a subsample of boys in households with exactly one boy and one girl who are aged between 11

Table 7: Three-part model estimation with a subsample of boys

	HIES2000			HIES2005		
	d	Cond y	Cond s	d	Cond y	Cond s
Coef.	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: All boys					
FSP HH	0.179*	-0.236***	0.008	0.277***	-0.131*	-0.060*
	(0.099)	(0.089)	(0.028)	(0.101)	(0.070)	(0.033)
Obs		2,534			2,906	
	Panel B: Boys in one-boy-one-girl households					
FSP HH	0.341	-0.202	0.053	0.446***	-0.058	0.014
	(0.356)	(0.405)	(0.082)	(0.142)	(0.140)	(0.063)
Obs		591			609	

Note: ***,** , and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. Standard errors clustered at the household level are reported in parentheses. The estimates are obtained using the three-part model constructed in Section 3. The covariates discussed in Table 3 are also included in all regressions.

and 15 (Panel B) as reported in Table 7. The table shows that boys from an FSP-receiving household (FSP HH), or a household with at least one FSP recipient, are more likely to enroll in school than boys from a household without an FSP recipient. However, conditional on enrollment, they receive less education expenditure than boys from non-FSP households. This indicates that there are positive spillover effects on boys' enrollment status, even though we cannot exclude the possibility that this is driven by the unobserved heterogeneity between FSP-receiving and non-FSP-receiving households. The negative spillover effects of the FSPs on boys' education expenditure conditional on enrollment suggest that households with FSP recipients may shift education expenditure from boys to girls.

Muting the FSPs' tuition waiver

As mentioned above, the tuition waiver is an important component of the FSPs. The tuition waiver encourages enrollment but also tends to negatively affect the conditional expenditure and core share among the school enrollees. However, the latter negative effects may be spurious. This may be simply because the FSPs are replacing the household's tuition expenditure for girls through the tuition waiver; the FSPs might not have any impact on the conditional expenditure and core share once the tuition waiver is taken into consideration.

To see if this is a possible explanation, we attempt to mute the impact of the tuition waiver

Table 8: Three-part model estimation with the impact of the tuition waiver muted

Year	Model	d	Cond y	Cond s
	Baseline	0.330***	-0.173***	-0.075***
		(0.039)	(0.049)	(0.015)
2000	Exclusion	0.317***	-0.079*	-0.055***
		(0.039)	(0.046)	(0.013)
	Imputation	0.314***	-0.064	-0.047***
		(0.039)	(0.048)	(0.011)
	Baseline	0.272***	-0.141***	-0.061***
		(0.035)	(0.028)	(0.012)
2005	Exclusion	0.262***	-0.065**	-0.049***
		(0.035)	(0.028)	(0.011)
	Imputation	0.258***	-0.082***	-0.041***
		(0.035)	(0.029)	(0.010)

Note: ***,** , and * denote statistical significance at 1, 5, and 10 percent levels, respectively. Standard errors clustered at the household level are reported in parentheses. Additional covariates discussed in Table 3 are also included. The baseline results are taken from Table 3. In the exclusion exercise, tuition fee is excluded from both total education expenditure and core expenditures to compute s. In the imputation exercise, we instead impute the tuition fee for FSP recipients using the predicted value from a linear model estimated with the pooled sample that includes the fixed-effects terms for the following categorical variables: enrollment status, FSP-recipient status, district of residence, survey year, gender, and school type (private/public).

through two alternative empirical exercises: exclusion and imputation. In the exclusion exercise, we exclude the tuition fee from the calculations of both the total education expenditure and core expenditure. In the imputation exercise, we impute the tuition fee for the FSP recipients using a linear prediction model. Then, the imputed tuition fee is computed by predicting the fee with the estimated parameter values but omitting the term involving the FSP-recipient dummy. This predicted amount, which is truncated from below at zero, can be interpreted as the tuition fee parents would have to spend had their daughter not received a tuition waiver.

The results of these two exercises are presented in Table 8 together with the baseline estimates taken from Table 3 for ease of comparison. As the table shows, the absolute value of the coefficient on the girl dummy becomes smaller than the baseline results in each of the three equations after turning off the impact of the tuition waiver either by exclusion or imputation. This indicates that our finding is indeed driven in part by the spurious effect coming from the tuition waiver. However, as Table 8 shows, the sign and statistical significance of the coefficient on the girl dummy mostly remain the same. Therefore, the earlier finding of a contradirectional gender gap still remains valid even after muting the effects of the tuition waiver.

Since Table 8 does not distinguish girls by the FSP-recipient status, we also consider a model that incorporates the FSP status in the three-part model and mute the effects of the tuition waiver. In the top panel of Table 9, we present the baseline estimation of the three-part model with the FSP status reported in Table 6. Then, as with Table 8, we mute the tuition wavier effects by either exclusion or imputation.

As Table 9 shows, FSP girls tend to enjoy a higher total education expenditure than non-FSP girls, and the difference is significant, both economically and statistically, when the tuition waiver effects are muted. By comparing the signs and sizes of the coefficients on FSP and Girl, it can also be seen that the positive impacts of the FSPs can substantially mitigate the promale bias in the total education expenditure (conditional on enrollment). Nevertheless, the FSP did not remove the gender gap in the core share conditional on enrollment. Taken together, the FSPs do not appear to have removed the gender gap in the education expenditure on the core component conditional on enrollment.

Impact on Timely Secondary School Graduation

The results of the previous subsections suggest that the FSPs promoted girls' enrollment in secondary schools but fell short of reducing the gender gap in the investment in the quality of education. Indeed, the FSPs have been criticized for the lack of attention to the quality of education (Mahmud, 2003; Raynor and Wesson, 2006). Our analysis highlights the reason why the quality of education for girls lags behind that for boys among the school enrollees from the perspective of complementary investment in education from households.

Nevertheless, it is not evident from the preceding analysis how this has affected the performance of girls in school relative to boys. Unfortunately, our data do not contain standard education performance measures such as test scores. Therefore, we use completion of secondary school (roughly) on time as an indicator of education performance. Specifically, a child is regarded to have completed secondary school (roughly) on time if he/she has already passed at least grade 10 (SSC or equivalent) when he/she is in the age range 16-20. This is a reasonable indicator because the child has to pass the SSC exam to complete secondary education, which

Table 9: Three-part model estimation with FSP status after muting the tuition waiver

	HIES2000			HIES2005		
Coef.	d	cond y	cond s	d	cond y	cond s
Baseline						
Girl	0.330***	-0.216***	-0.056***	0.270***	-0.155***	-0.049***
	(0.039)	(0.056)	(0.018)	(0.035)	(0.035)	(0.014)
FSP		0.077	-0.032**		0.027	-0.024**
		(0.049)	(0.014)		(0.037)	(0.009)
Exclusion						
Girl	0.318***	-0.163***	-0.044***	0.261***	-0.104***	-0.040***
	(0.039)	(0.054)	(0.017)	(0.035)	(0.035)	(0.013)
FSP		0.149***	-0.018		0.079**	-0.018*
		(0.050)	(0.016)		(0.036)	(0.011)
Imputation						
Girl	0.317***	-0.198***	-0.049***	0.261***	-0.158***	-0.042***
	(0.040)	(0.056)	(0.016)	(0.035)	(0.035)	(0.013)
FSP		0.249***	0.003		0.161***	0.004
		(0.047)	(0.017)		(0.036)	(0.012)

Note: ***,** , and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. Standard errors clustered at the household level are reported in parentheses. The estimations are obtained using the three-part model constructed in Section 3. In all regressions, the following covariates are also included: logarithmic per capita expenditure, logarithmic household size, father's and mother's education in years, number of children, female head, wage worker head, head's age and religion (Muslim/Hindu), urban area, and dummy variables for the child's age and whether father's and mother's education are missing. In addition, the school accessibility variables, school-type dummy variables (private/public), and logarithmic education expenditure are also included in the equations for x_d, x_y, and x_s, respectively. Baseline results are taken from Table 6. See the table note for Table 8 for details on the exclusion and imputation exercises.

requires a certain level of mastery of the secondary-level curriculum.[22] For this exercise, we additionally use the HES 1991 data set as it contains information necessary to construct the indicator for completion on time.

In columns (1)-(5) of Panel A of Table 10, we report the estimated effects of being a girl on timely completion of secondary school for each survey year through OLS regressions. The effects have became less promale and the beginning of the narrowing of the gap roughly corresponds to the onset of the FSPs, which seems to indicate that FSPs helped close the gender gap in timely completion of secondary education.

However, if we restrict the sample to those who have already completed primary education,

[22]Because we do not observe the age at which the child passed the SSC examination, we derive the on-time secondary school completion from the age of the child and highest grade completed as described in the main text. As shown in Figure 1, the passing rate varies and may be as low as 40 percent depending on the year. Thus, passing the SSC examination is not a trivial matter.

Table 10: OLS regressions of on-time secondary school completion by year

	1991	1995	2000	2005	2010	2005	2010
Coef.	(1)	(2)	(3)	(4)	(5)	(6)	(7)
			Panel A: All individuals aged 16-20				
Girl	-0.043***	-0.054***	-0.044***	-0.014	-0.007	0.009	0.059**
	(0.012)	(0.012)	(0.012)	(0.010)	(0.011)	(0.020)	(0.027)
Lagged GRR						0.237***	0.712***
						(0.091)	(0.093)
Girl×Lagged GRR						-0.084	-0.247***
						(0.070)	(0.095)
Obs	3,043	3,721	3,988	5,056	5,316	5,056	5,316
			Panel B: All primary graduates aged 16-20				
Girl	-0.018	-0.084***	-0.064***	-0.022*	-0.026*	0.039	0.083**
	(0.027)	(0.019)	(0.017)	(0.013)	(0.014)	(0.027)	(0.033)
Lagged GRR						0.350***	0.858***
						(0.122)	(0.117)
Girl×Lagged GRR						-0.227**	-0.416***
						(0.093)	(0.117)
Obs	1,223	2,093	2,621	3,712	4,098	3,712	4,098

Note: ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. Standard errors clustered at household level are reported in parentheses. The dependent variable is a dummy variable for the completion of secondary school on time, and takes one if an individual aged between 16 and 20 at the time of the survey had already completed grade 10 or higher. Lagged GRR is the GRR at division-age level five years before the survey. In 2005 [2010], we use GRR for the year 2000 [2005]. In all regressions, the following covariates are also included: logarithmic expenditure per capita, logarithmic household size, the dummy variables for the household heads' education level (primary, secondary, and higher), female head, wage worker head, head's age and religion (Muslim/Hindu), urban area, and dummy variables for the child's age and whether father's and mother's education are missing. Panel A uses a sample of all individuals aged between 16 and 20 and Panel B uses a subsample of primary graduates among them.

the picture looks different as columns (1)-(5) in Panel B of Table 10 show. The gender gap in the timely completion of secondary education conditional on the completion of primary education is larger than that in the unconditional sample—except for the year 1991 when the FSPs were yet to be rolled out nationwide. This indicates that the narrowing of the gender gap observed in Panel A may be due to the improvement in girls' secondary enrollment. In other words, because more girls were enrolled, they had a higher unconditional probability of completion. However, the results of panel B indicate that the secondary school performance of girls among the potential school enrollees, or those who have completed primary school, was worse than that of boys. Assuming that the gender gap in the quality of education translates into the gender gap in school performance, the results above are consistent with our finding that the quality of education for girls conditional on enrollment consistently lagged behind that for boys.

Next, we attempt to understand the impact of the FSPs on the timely graduation from secondary school. This is challenging, because we do not have the history of the FSP-recipient status in the past. Instead, we include in the regressions the lagged FSP intensity—as measured by GRR five years prior to the survey—and its interaction with the girl dummy. That is, we use the GRR for the year 2000 [2005] and its interaction term in the analysis of timely graduation in the year 2005 [2010]. The lagged variable would arguably reflect the cumulative impact of the FSPs in the last five years. Note, however, that the results for the year 2010 suffer from the contamination of the sample because some of the individuals in the sample may have benefited from the SEQAEP.

The results of this analysis are presented in columns (6)-(7) of Table 10. For all children aged between 16 and 20, girls living in more FSP-intensive areas are less likely to graduate on time than boys. This can be seen from the negative point estimates on the interaction term (i.e., Girl$\times$Lagged GRR). When we look only at the subsample of those who have completed primary education, the promale gender gap is significant in more FSP-intensive areas. Thus, in line with our earlier findings, there is no evidence that the FSPs improved the quality of education for secondary school girls relative to boys. If anything, the girls in high FSP-intensive areas are less likely to graduate from secondary school on time than the girls in low FSP-intensive areas, indicating that the impact of the FSPs on the performance in secondary school was possibly negative. While the negative coefficient on the interaction term may be due to the selection of location for the FSPs, it is also possible that the FSPs directly lowered the quality of education as argued in the next seciton.

In sum, these preceding analyses collectively indicate two points. First, the FSPs increased the female secondary school enrollment and years of education. Second, despite the increase in these quantity measures of education, the FSPs did not attract sufficient complementary investment in the quality of education from households. As a result, the quality and performance of education for girls appear to have lagged behind those for boys among school enrollees.

Of course, the lack of investment in the quality of education for girls is not the only possible reason for their underperformance. For example, it is also possible that girls may receive less investment in health or pressured to spend more time on household chore than boys. To the extent that they are positively correlated with the investment in education quality, we can interpret the latter as a reflection of the opportunity given by the household to perform well in education.

7 Discussion

Gender parity in enrollment is a big achievement, but we would be merely indulging in illusions if we equated it to gender parity in education. The contradirectional gender bias in Bangladesh documented in this study—profemale bias in enrollment and promale bias in the total education expenditure and the core share in the total education expenditure among school enrollees—clearly illustrates that gender parity in education cannot be measured by the gender parity in enrollment alone.

At first glance, the contradirectional gender gap is puzzling, because it cannot be explained by gender discrimination and because it is not documented anywhere else. Our analysis, however, indicates that it is driven at least in part by the presence of the FSPs. Using a double-difference strategy, we show that FSPs helped to bring girls to school. However, the analysis of the three-part model suggests that the FSPs did not attract sufficient complementary investment from households in the quality of education, which appears to have resulted in the underperformance of girls relative to boys among primary school graduates. We further explored the possible explanations for the lack of investment in quality of education from households.

Because of the data limitations, at least four potentially important factors were not taken into account in this paper. First, it is possible that the FSPs *directly* lower the quality of education for girls by selectively attracting girls to schools and putting them in crowded classrooms as suggested in the introduction section. The teacher-student ratio in secondary schools was only 1:24 in 1990 but rose by 50 percent to 1:36 in 2010, indicating that classrooms have become overcrowded. Moreover, given the crowded classrooms, many school teachers capitalized on this opportunity by systematically exerting less effort in school teaching and promoting private tutoring to earn extra income (Mahmud, 2003).

The increase in the class size may also alter the class dynamics and affect the gender gap in education. While we are unable to directly observe the class dynamics, the change in the class dynamics may well depend on the gender of the teacher. To explore this possibility, we also estimated the three-part model that includes district-level female teacher ratio and its interaction with a girl dummy. The sign and size of the coefficient on the girl dummy remain similar to those in Table 3. Similarly, the inclusion of the district-level female teacher ratio and its interaction with a girl dummy do not qualitatively alter the results on graduation on time reported in Table 10. Hence, it seems unlikely that the class dynamics is an important causal channel.

Second, there may be a gender difference in the effective price of private tutoring, particularly if parents need to pay additional supporting costs, such as private transportation for an accompanying guardian.[23] Indeed, it is estimated that the cost of private tutoring for girls is 13 percent higher than that for boys (CAMPE, 2006, Table A4.1, p. 120). This observation is important, because first generation learners typically get no help with their study outside the classrooms. This in turn makes it difficult for children from disadvantaged backgrounds—particularly girls—to pass the SSC examination, because after-school tutoring is crucial for students struggling academically, particularly in mathematics and English (Nakata et al., 2018).

Third, a related factor is the supply-side constraint on female private tutors. While we are not aware of data on the availability of tutors, it seems likely that female private tutors were scarce, particularly in earlier years. Therefore, some parents with traditional social norms may choose not to hire a private tutor for their daughter, not because they are unwilling or unable to pay, but because there is no female tutor available. However, the supply-side constraint is

[23] Transportation can be an important barrier for girls to access education. In India, Muralidharan and Prakash (2017) found that conditional kind transfer of a bicycle to girls substantially narrowed the gender gap in enrollment.

unlikely to be of primary importance, because the contradirectionality of the gender gap has not changed much since the year 2000, even though women have been getting better educated.[24]

Fourth, we did not address the possibility that our results may be driven by the presence of gender difference in labor market returns to the quality of education. Such gender difference may arise not only because employers do not value the quality of education for males and females equally but because females have a lower probability of employment and lower average working hours conditional on employment than males. In a preliminary version of a companion paper, we show that our results are indeed consistent with the potential gender difference in labor market returns.

Our results highlight both the opportunities and challenges that a targeted CCT program like the FSPs is likely to face. On the one hand, the FSPs were successful as they substantially increased the secondary school enrollment rate. Although the secondary enrollment rate for girls historically lagged far behind that for boys, girls overtook boys soon after the nationwide rollout of the FSPs. This demonstrates that incentives work.

On the other hand, our results also suggest that the quality of education for girls continued to lag behind that for boys among school enrollees because of the lack of investment in quality. As a result, girls' observable educational outcomes have also been worse than those of boys. As shown in Figure 1, girls performed poorly in comparison with boys in terms of both the passing rate and the share of top students in the SSC examination. Further, conditional on completing primary school, girls are less likely to graduate from secondary school on time. Therefore, our results clearly show that narrowing the gender gap in the quantity of education does not narrow the gender gap in the quality of education. This, in turn, indicates that gender parity in enrollment may not translate into gender parity in learning, as a study of 43 countries by Psaki et al. (2018) also indicates.

The findings of this study offer three important policy implications. First, CCT programs have the potential to narrow the gender gap in enrollment, even in a traditionally patriarchal country like Bangladesh, by providing households with adequate incentives to send girls to schools. Second, despite the first point, the quantity of education as measured by enrollment or years of education does not tell the whole story about the gender gap in education, because the

[24] According to BANBEIS (2010, Table 2.1.0, p. 30), the proportion of female teachers in secondary schools was 13.88 percent in 1995. This figure reached 23.09 percent in 2010.

incentive to increase the quantity of education does not necessarily lead to an improvement in the quality of education. On the contrary, CCT programs like the FSPs may directly reduce the quality of school education if they make classrooms overcrowded. This may increase households' dependence on private tutoring and would exacerbate the female disadvantage because of the promale intrahousehold allocation of resources to perform well in education.[25] Therefore, policy makers must be aware of this limitation and consider implementing complementary policies.

Third, it would not be possible to truly achieve gender equality in education without addressing the gender gap in the investment in the quality of education by households, as is apparent from the underperformance of girls in secondary schools. Arguably, the quality is more difficult to address than the quantity, because the factors affecting the former—such as labor market returns and inherent gender bias among parents—may be beyond the control of those who make education policies. Nevertheless, interventions that are targeted at improving the access to education of better quality among disadvantaged groups (e.g., the voucher program in India (Muralidharan and Sundararaman, 2015)) or those that improve some supply factors for girls may narrow the gender gap in the quality of education.

There is indeed a piece of indicative evidence from a field experiment in Bangladesh. An impact assessment of the additional class teacher (ACT) program—in which teachers are hired to teach regular and additional supplementary classes in underserved and low-performing areas—demonstrates positive impacts on learning performance and the impact is particularly strong for girls (World Bank, 2018, Table 7.5). Further, anecdotal evidence suggests a significant reduction in the prevalence of private coaching practices at schools where ACTs are operating (World Bank, 2018, p. 33). Hence, it is possible to move towards gender equality in the quality of education if policies are implemented to ensure quality education, particularly for those who are disadvantaged.

[25] There is some suggestive evidence on the link between FSP intensity and private tutoring. Based on the regressions of the (binary) use and (continuous) spending amount of private tutoring on the FSP intensity as measured by GRR, we find i) both girls and boys are more likely to have private tutoring in more FSP-intensive areas, ii) the share of the expenditure on private tutoring in the total expenditure for girls tends to be lower than that for boys conditional on the use of private tutoring, and iii) this gender gap was larger in more FSP-intensive areas in 2000 and 2005 (see footnote 21 for the reason for the choice of these years). Although the sign is consistent between these two years, we refrain from drawing strong conclusions because the estimates are not always statistically significant and because we do not observe the teacher-student ratio in the schools children attend.

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Appendix

A Derivation of the log-likelihood function for the three-part model

We denote the probability density function and cumulative density function (CDF) for a standard normal distribution by ϕ and Φ, respectively, and the CDF for a standard bivariate normal distribution by Ψ. For brevity, we suppress subscript for child $i \in \{1, \cdots, N\}$ in the derivation of the log-likelihood function l_i^j for the three part model for case $j \in \{1, 2, 3, 4\}$. We denote the (unlogged) likelihood function by L^j and discuss each case separately below.

Case 1: $d = 0$.

$$L^1 = P(\epsilon_d \leq -x_d'\beta_d) = \Phi(-x_d'\beta_d).$$

Case 2: $d = 1, s = 0$.

$$L^2 = \frac{1}{y} P(-\epsilon_d \leq x_d'\beta_d,\ \epsilon_s \leq -x_s'\beta_s |\ \epsilon_y = \log(y) - x_y'\beta_y) \cdot f(\log(y) - x_y'\beta_y),$$

where $f(\cdot)$ is the density function of ϵ_y.

We rearrange the distribution of the error terms as follows:

$$\begin{bmatrix} -\epsilon_d \\ \epsilon_s \\ \epsilon_y \end{bmatrix} \sim N\left(\mathbf{0}, \begin{bmatrix} 1 & -\rho_{ds}\sigma_s & -\rho_{dy}\sigma_y \\ -\rho_{ds}\sigma_s & \sigma_s^2 & \rho_{ys}\sigma_y\sigma_s \\ -\rho_{dy}\sigma_y & \rho_{ys}\sigma_y\sigma_s & \sigma_y^2. \end{bmatrix}\right).$$

$(-\epsilon_d, \epsilon_s)'$ given ϵ_y follows a bivariate normal distribution with:

$$\mathbf{E}\left(\begin{pmatrix} -\epsilon_d \\ \epsilon_s \end{pmatrix} \middle| \epsilon_y\right) = \begin{pmatrix} 0 \\ 0 \end{pmatrix} + \begin{pmatrix} -\rho_{dy}\sigma_y \\ \rho_{ys}\sigma_y\sigma_s \end{pmatrix} \frac{1}{\sigma_y^2}(\epsilon_y - 0) = \begin{pmatrix} -\frac{\rho_{dy}}{\sigma_y}\epsilon_y \\ \frac{\rho_{ys}\sigma_s}{\sigma_y}\epsilon_y \end{pmatrix},$$

and

$$\mathbf{Var}\left(\left(\begin{array}{c} -\epsilon_d \\ \epsilon_s \end{array}\right)\middle| \epsilon_y\right) = \left(\begin{array}{cc} 1 & -\rho_{ds}\sigma_s \\ -\rho_{ds}\sigma_s & \sigma_s^2 \end{array}\right) - \left(\begin{array}{c} -\rho_{dy}\sigma_y \\ \rho_{ys}\sigma_y\sigma_s \end{array}\right)\frac{1}{\sigma_y^2}\left(\begin{array}{cc} -\rho_{dy}\sigma_y & \rho_{ys}\sigma_y\sigma_s \end{array}\right)$$
$$= \left(\begin{array}{cc} 1-\rho_{dy}^2 & (\rho_{dy}\rho_{ys}-\rho_{ds})\sigma_s \\ (\rho_{dy}\rho_{ys}-\rho_{ds})\sigma_s & (1-\rho_{ys}^2)\sigma_s^2 \end{array}\right).$$

Then, we have:

$$\begin{aligned} & P(-\epsilon_d \leq x_d'\beta_d,\ \epsilon_s \leq -x_s'\beta_s |\ \epsilon_y = \log(y) - x_y'\beta_y) \\ = & \ \Psi\left(\frac{x_d'\beta_d + \rho_{dy}\epsilon_y/\sigma_y}{\sqrt{1-\rho_{dy}^2}},\ -\frac{x_s'\beta_s + \rho_{ys}\sigma_s\epsilon_y/\sigma_y}{\sigma_s\sqrt{1-\rho_{ys}^2}},\ \frac{\rho_{dy}\rho_{ys}-\rho_{ds}}{\sqrt{(1-\rho_{dy}^2)(1-\rho_{ys}^2)}}\right), \end{aligned}$$

and

$$f(\log(y) - x_y'\beta_y) = \frac{1}{\sigma_y}\phi(\frac{\log(y) - x_y'\beta_y}{\sigma_y}).$$

Thus, the likelihood for this case is:

$$L^2 = \frac{\phi(e_y)}{y\sigma_y}\cdot\Psi\left(\frac{x_d'\beta_d + \rho_{dy}e_y}{\sqrt{1-\rho_{dy}^2}},\ -\frac{x_s'\beta_s + \rho_{ys}\sigma_s e_y}{\sigma_s\sqrt{1-\rho_{ys}^2}},\ \frac{\rho_{dy}\rho_{ys}-\rho_{ds}}{\sqrt{(1-\rho_{dy}^2)(1-\rho_{ys}^2)}}\right).$$

Case 3: $d = 1, s \in (0,1)$.

$$L^3 = \frac{1}{y}P(-\epsilon_d \leq x_d'\beta_d |\ \epsilon_y = \log(y) - x_y'\beta_y,\ \epsilon_s = s - x_s'\beta_s)\cdot g(\log(y) - x_y'\beta_y,\ s - x_s'\beta_s),$$

where $g(\cdot,\cdot)$ is the joint density function for ϵ_y and ϵ_s.

Let the submatrix Σ_{11} be

$$\Sigma_{11} = \left(\begin{array}{cc} \sigma_y^2 & \rho_{ys}\sigma_y\sigma_s \\ \rho_{ys}\sigma_y\sigma_s & \sigma_s^2 \end{array}\right).$$

Thus, we have

$$\Sigma_{11}^{-1} = \frac{1}{(1-\rho_{ys}^2)\sigma_y^2\sigma_s^2}\left(\begin{array}{cc} \sigma_s^2 & -\rho_{ys}\sigma_y\sigma_s \\ -\rho_{ys}\sigma_y\sigma_s & \sigma_y^2 \end{array}\right),$$

where the determinant of Σ_{11} is $|\Sigma_{11}| = (1-\rho_{ys}^2)\sigma_y^2\sigma_s^2$.

It can be shown that $-\epsilon_d$ given ϵ_y and ϵ_s follows a normal distribution with:

$$\begin{aligned}
\mathbf{E}\left(-\epsilon_d|\epsilon_y,\ \epsilon_s\right) &= 0+\frac{1}{|\Sigma_{11}|}\begin{pmatrix} -\rho_{dy}\sigma_y & -\rho_{ds}\sigma_s \end{pmatrix}\begin{pmatrix} \sigma_s^2 & -\rho_{ys}\sigma_y\sigma_s \\ -\rho_{ys}\sigma_y\sigma_s & \sigma_y^2 \end{pmatrix}\begin{pmatrix} \epsilon_y \\ \epsilon_s \end{pmatrix} \\
&= -\frac{1}{(1-\rho_{ys}^2)\sigma_y^2\sigma_s^2}\begin{pmatrix} (\rho_{dy}-\rho_{ds}\rho_{ys})\sigma_y\sigma_s^2 & (\rho_{ds}-\rho_{dy}\rho_{ys})\sigma_y^2\sigma_s) \end{pmatrix}\begin{pmatrix} \epsilon_y \\ \epsilon_s \end{pmatrix} \\
&= -\frac{(\rho_{dy}-\rho_{ds}\rho_{ys})\sigma_s\epsilon_y+(\rho_{ds}-\rho_{dy}\rho_{ys})\sigma_y\epsilon_s}{(1-\rho_{ys}^2)\sigma_y\sigma_s},
\end{aligned}$$

and

$$\begin{aligned}
\mathbf{Var}\left(-\epsilon_d|\epsilon_y,\ \epsilon_s\right) &= 1-\frac{1}{|\Sigma_{11}|}\begin{pmatrix} -\rho_{dy}\sigma_y & -\rho_{ds}\sigma_s \end{pmatrix}\begin{pmatrix} \sigma_s^2 & -\rho_{ys}\sigma_y\sigma_s \\ -\rho_{ys}\sigma_y\sigma_s & \sigma_y^2 \end{pmatrix}\begin{pmatrix} -\rho_{dy}\sigma_y \\ -\rho_{ds}\sigma_s \end{pmatrix} \\
&= 1-\frac{1}{(1-\rho_{ys}^2)\sigma_y^2\sigma_s^2}\begin{pmatrix} -(\rho_{dy}-\rho_{ds}\rho_{ys})\sigma_y\sigma_s^2 & -(\rho_{ds}-\rho_{dy}\rho_{ys})\sigma_y^2\sigma_s \end{pmatrix}\begin{pmatrix} -\rho_{dy}\sigma_y \\ -\rho_{ds}\sigma_s \end{pmatrix} \\
&= 1-\frac{(\rho_{dy}-\rho_{ds}\rho_{ys})\rho_{dy}+(\rho_{ds}-\rho_{dy}\rho_{ys})\rho_{ds}}{(1-\rho_{ys}^2)} \\
&= \frac{1-\rho_{ys}^2-\rho_{dy}^2-\rho_{ds}^2+2\rho_{dy}\rho_{ds}\rho_{ys}}{1-\rho_{ys}^2}.
\end{aligned}$$

We then have

$$\begin{aligned}
& P(-\epsilon_d \leq x_d'\beta_d|\ \epsilon_y=\log(y)-x_y'\beta_y,\ \epsilon_s=s-x_s'\beta_s) \\
= & \Phi\left(\frac{x_d'\beta_d(1-\rho_{ys}^2)+(\rho_{dy}-\rho_{ds}\rho_{ys})(\log(y)-x_y'\beta_y)/\sigma_y+(\rho_{ds}-\rho_{dy}\rho_{ys})(s-x_s'\beta_s)/\sigma_s}{\sqrt{(1-\rho_{ys}^2-\rho_{dy}^2-\rho_{ds}^2+2\rho_{dy}\rho_{ds}\rho_{ys})(1-\rho_{ys}^2)}}\right),
\end{aligned}$$

and

$$
\begin{aligned}
& g(\epsilon_y,\epsilon_s) = g(\log(y) - x_y'\beta_y,\ s - x_s'\beta_s) \\
= & \frac{1}{2\pi\sigma_y\sigma_s\sqrt{1-\rho_{ys}^2}} \exp\left[-\frac{1}{2}\begin{pmatrix}\epsilon_y & \epsilon_s\end{pmatrix}\frac{1}{|\Sigma_{11}|}\begin{pmatrix}\sigma_s^2 & -\rho_{ys}\sigma_y\sigma_s \\ -\rho_{ys}\sigma_y\sigma_s & \sigma_y^2\end{pmatrix}\begin{pmatrix}\epsilon_y \\ \epsilon_s\end{pmatrix}\right] \\
= & \frac{1}{2\pi\sigma_y\sigma_s\sqrt{1-\rho_{ys}^2}} \exp\left[-\frac{\epsilon_y^2\sigma_s^2 - 2\rho_{ys}\sigma_y\sigma_s\epsilon_y\epsilon_s + \epsilon_s^2\sigma_y^2}{2(1-\rho_{ys}^2)\sigma_y^2\sigma_s^2}\right] \\
= & \frac{1}{\sigma_y\sigma_s\sqrt{1-\rho_{ys}^2}}\phi\left(\frac{\epsilon_y}{\sigma_y\sqrt{1-\rho_{ys}^2}}\right)\phi\left(\frac{\epsilon_s}{\sigma_s\sqrt{1-\rho_{ys}^2}}\right)\exp\left(\rho_{ys}\frac{\epsilon_y\epsilon_s}{(1-\rho_{ys}^2)\sigma_y\sigma_s}\right) \\
= & \frac{1}{\sigma_y\sigma_s\sqrt{1-\rho_{ys}^2}}\phi\left(\frac{\log(y) - x_y'\beta_y}{\sigma_y\sqrt{1-\rho_{ys}^2}}\right)\phi\left(\frac{s - x_s'\beta_s}{\sigma_s\sqrt{1-\rho_{ys}^2}}\right)\exp\left(\rho_{ys}\frac{(\log(y) - x_y'\beta_y)(s - x_s'\beta_s)}{(1-\rho_{ys}^2)\sigma_y\sigma_s}\right).
\end{aligned}
$$

Thus, the likelihood for this case is:

$$
\begin{aligned}
L^3 = & \frac{1}{y\sigma_y\sigma_s\sqrt{1-\rho_{ys}^2}}\Phi\left(\frac{x_d'\beta_d(1-\rho_{ys}^2) + (\rho_{dy} - \rho_{ds}\rho_{ys})e_y + (\rho_{ds} - \rho_{dy}\rho_{ys})e_s}{\sqrt{(1-\rho_{ys}^2 - \rho_{dy}^2 - \rho_{ds}^2 + 2\rho_{dy}\rho_{ds}\rho_{ys})(1-\rho_{ys}^2)}}\right) \\
& \cdot\phi\left(\frac{e_y}{\sqrt{1-\rho_{ys}^2}}\right)\phi\left(\frac{e_s}{\sqrt{1-\rho_{ys}^2}}\right)\exp\left(\rho_{ys}\frac{e_ye_s}{1-\rho_{ys}^2}\right).
\end{aligned}
$$

Case 4: $d = 1, s = 1$.

$$
L^4 = \frac{1}{y}P(-\epsilon_d \le x_d'\beta_d,\ -\epsilon_s \le x_s'\beta_s - 1|\ \epsilon_y = \log(y) - x_y'\beta_y) \cdot f(\log(y) - x_y'\beta_y)
$$

We rearrange the distribution of the error terms as follows:

$$
\begin{bmatrix} -\epsilon_d \\ -\epsilon_s \\ \epsilon_y \end{bmatrix} \sim N\left(\mathbf{0}, \begin{bmatrix} 1 & \rho_{ds}\sigma_s & -\rho_{dy}\sigma_y \\ \rho_{ds}\sigma_s & \sigma_s^2 & -\rho_{ys}\sigma_y\sigma_s \\ -\rho_{dy}\sigma_y & -\rho_{ys}\sigma_y\sigma_s & \sigma_y^2 \end{bmatrix}\right).
$$

$(-\epsilon_d, -\epsilon_s)^T$ given ϵ_y follows bivariate normal distribution with:

$$
\mathbf{E}\left(\begin{pmatrix}-\epsilon_d \\ -\epsilon_s\end{pmatrix}\middle|\,\epsilon_y\right) = \begin{pmatrix}0 \\ 0\end{pmatrix} + \begin{pmatrix}-\rho_{dy}\sigma_y \\ -\rho_{ys}\sigma_y\sigma_s\end{pmatrix}\frac{1}{\sigma_y^2}(\epsilon_y - 0) = \begin{pmatrix}-\frac{\rho_{dy}}{\sigma_y}\epsilon_y \\ -\frac{\rho_{ys}\sigma_s}{\sigma_y}\epsilon_y\end{pmatrix},
$$

and

$$\mathbf{Var}\left(\left(\begin{array}{c} -\epsilon_d \\ -\epsilon_s \end{array}\right)\Bigg|\, \epsilon_y\right) = \begin{pmatrix} 1 & \rho_{ds}\sigma_s \\ \rho_{ds}\sigma_s & \sigma_s^2 \end{pmatrix} - \begin{pmatrix} -\rho_{dy}\sigma_y \\ -\rho_{ys}\sigma_y\sigma_s \end{pmatrix} \frac{1}{\sigma_y^2} \begin{pmatrix} -\rho_{dy}\sigma_y & -\rho_{ys}\sigma_y\sigma_s \end{pmatrix}$$

$$= \begin{pmatrix} 1 & \rho_{ds}\sigma_s \\ \rho_{ds}\sigma_s & \sigma_s^2 \end{pmatrix} - \begin{pmatrix} \rho_{dy}^2 & \rho_{dy}\rho_{ys}\sigma_s \\ \rho_{dy}\rho_{ys}\sigma_s & \rho_{ys}^2\sigma_s^2 \end{pmatrix}$$

$$= \begin{pmatrix} 1-\rho_{dy}^2 & (\rho_{ds}-\rho_{dy}\rho_{ys})\sigma_s \\ (\rho_{ds}-\rho_{dy}\rho_{ys})\sigma_s & (1-\rho_{ys}^2)\sigma_s^2 \end{pmatrix}.$$

Then, we have

$$\begin{aligned} & P(-\epsilon_d \le x_d'\beta_d,\ -\epsilon_s \le x_s'\beta_s - 1|\ \epsilon_y = \log(y) - x_y'\beta_y) \\ = \quad & \Psi\left(\frac{x_d'\beta_d + \rho_{dy}(\log(y) - x_y'\beta_y)/\sigma_y}{\sqrt{1-\rho_{dy}^2}},\ \frac{x_s'\beta_s - 1 + \rho_{ys}\sigma_s(\log(y) - x_y'\beta_y)/\sigma_y}{\sigma_s\sqrt{1-\rho_{ys}^2}},\ \frac{\rho_{ds}-\rho_{dy}\rho_{ys}}{\sqrt{(1-\rho_{dy}^2)(1-\rho_{ys}^2)}}\right), \end{aligned}$$

and

$$f(\log(y) - x_y'\beta_y) = \frac{1}{\sigma_y}\phi\left(\frac{\log(y) - x_y'\beta_y}{\sigma_y}\right).$$

Thus, the likelihood for this case is:

$$L^4 = \frac{\phi(e_y)}{y\sigma_y}\cdot\Psi\left(\frac{x_d'\beta_d + \rho_{dy}e_y}{\sqrt{1-\rho_{dy}^2}},\ \frac{x_s'\beta_s - 1 + \rho_{ys}\sigma_s e_y}{\sigma_s\sqrt{1-\rho_{ys}^2}},\ \frac{\rho_{ds}-\rho_{dy}\rho_{ys}}{\sqrt{(1-\rho_{dy}^2)(1-\rho_{ys}^2)}}\right),$$

where $e_y = \frac{\log(y) - x_y'\beta_y}{\sigma_y}$ and $e_s = \frac{s - x_s'\beta_s}{\sigma_s}$.

By taking the logarithm of L^j and putting back the subscript i, the log-likelihood function l_i^j for child $i \in \{1, \cdots, N\}$ under case $j \in \{1, 2, 3, 4\}$ is given as follows:

$$
\begin{cases}
l_i^1 &= \log\left[\Phi(-x'_{d_i}\beta_d)\right] \\
l_i^2 &= \log(\phi(e_{y_i})) - \log(y_i) - \log(\sigma_y) \\
&\quad + \log\left[\Psi\left(\frac{x'_{d_i}\beta_d+\rho_{dy}e_{y_i}}{\sqrt{1-\rho_{dy}^2}},\ -\frac{x'_{s_i}\beta_s+\rho_{ys}\sigma_s e_{y_i}}{\sigma_s\sqrt{1-\rho_{ys}^2}},\ \frac{\rho_{dy}\rho_{ys}-\rho_{ds}}{\sqrt{(1-\rho_{dy}^2)(1-\rho_{ys}^2)}}\right)\right] \\
l_i^3 &= \log\left(\phi\left(\frac{e_{y_i}}{\sqrt{1-\rho_{ys}^2}}\right)\right) + \log\left(\phi\left(\frac{e_{s_i}}{\sqrt{1-\rho_{ys}^2}}\right)\right) \\
&\quad + \left(\rho_{ys}\frac{e_{y_i}e_{s_i}}{1-\rho_{ys}^2}\right) - \log(y_i) - \log(\sigma_y) - \log(\sigma_s) - \log\left(\sqrt{1-\rho_{ys}^2}\right) \\
&\quad + \log\left[\Phi\left(\frac{x'_{d_i}\beta_d(1-\rho_{ys}^2)+(\rho_{dy}-\rho_{ds}\rho_{ys})e_{y_i}+(\rho_{ds}-\rho_{dy}\rho_{ys})e_{s_i}}{\sqrt{(1-\rho_{ys}^2-\rho_{dy}^2-\rho_{ds}^2+2\rho_{dy}\rho_{ds}\rho_{ys})(1-\rho_{ys}^2)}}\right)\right] \\
l_i^4 &= \log(\phi(e_{y_i})) - \log(y_i) - \log(\sigma_y) \\
&\quad + \log\left[\Psi\left(\frac{x'_{d_i}\beta_d+\rho_{dy}e_{y_i}}{\sqrt{1-\rho_{dy}^2}},\ \frac{x'_{s_i}\beta_s-1+\rho_{ys}\sigma_s e_{y_i}}{\sigma_s\sqrt{1-\rho_{ys}^2}},\ \frac{\rho_{ds}-\rho_{dy}\rho_{ys}}{\sqrt{(1-\rho_{dy}^2)(1-\rho_{ys}^2)}}\right)\right].
\end{cases}
$$

B Derivation of marginal effects

The equation for the expected enrollment is straightforward. The equation for the conditional expenditure can be derived as follows:

$$
\begin{aligned}
E(y|d=1) &= \int_0^\infty y f(y|d=1)dy = \int_0^\infty y f(y|\epsilon_d > -x'_d\beta_d)dy \\
&= \int_0^\infty y\frac{1}{y} f(\epsilon_y|\epsilon_d > -x'_d\beta_d)dy = \int_0^\infty \frac{f(\epsilon_y, \epsilon_d > -x'_d\beta_d)}{P(\epsilon_d > -x'_d\beta_d)}dy \\
&= \int_0^\infty \frac{f(\epsilon_d > -x'_d\beta_d|\epsilon_y)f(\epsilon_y)}{P(\epsilon_d > -x'_d\beta_d)}dy \\
&= \int_0^\infty \frac{\Phi\left(\frac{x'_d\beta_d+\rho_{dy}\epsilon_y/\sigma_y}{\sqrt{1-\rho_{dy}^2}}\right)\phi\left(\frac{\epsilon_y}{\sigma_y}\right)/\sigma_y}{\Phi(x'_d\beta_d)}dy,
\end{aligned}
$$

where $\epsilon_y = \log(y) - x'_y\beta_y$.

The unconditional expectation of y is:

$$
E(y) = P(d=1)E(y|d=1) = \int_0^\infty \frac{1}{\sigma_y}\Phi\left(\frac{x'_d\beta_d + \rho_{dy}\epsilon_y/\sigma_y}{\sqrt{1-\rho_{dy}^2}}\right)\phi\left(\frac{\epsilon_y}{\sigma_y}\right)dy.
$$

The unconditional expectation of the core expenditure ys is:

$$
\begin{aligned}
E(ys) &= \int_0^1 \int_0^\infty ysf(y,s)dyds \\
&= \int_0^\infty y \cdot 1 \cdot f(d=1,y,s=1)dy + \int_0^1 \int_0^\infty ysf(d=1,y,s)dyds \\
&= \int_0^\infty \frac{1}{\sigma_y}\phi(\frac{\epsilon_y}{\sigma_y})\Psi\left(\frac{x_d'\beta_d + \rho_{dy}\epsilon_y/\sigma_y}{\sqrt{1-\rho_{dy}^2}}, \frac{x_s'\beta_s - 1 + \rho_{ys}\sigma_s\epsilon_y/\sigma_y}{\sigma_s\sqrt{1-\rho_{ys}^2}}, \frac{\rho_{ds}-\rho_{dy}\rho_{ys}}{\sqrt{(1-\rho_{dy}^2)(1-\rho_{ys}^2)}}\right) dy \\
&\quad + \int_0^1 \int_0^\infty ys \frac{1}{y\sigma_y\sigma_s\sqrt{1-\rho_{ys}^2}} \Phi\left(\frac{x_d'\beta_d(1-\rho_{ys}^2) + (\rho_{dy}-\rho_{ds}\rho_{ys})\epsilon_y/\sigma_y + (\rho_{ds}-\rho_{dy}\rho_{ys})\epsilon_s/\sigma_s}{\sqrt{(1-\rho_{ys}^2-\rho_{dy}^2-\rho_{ds}^2+2\rho_{dy}\rho_{ds}\rho_{ys})(1-\rho_{ys}^2)}}\right) \\
&\quad \times\phi\left(\frac{\epsilon_y}{\sigma_y\sqrt{1-\rho_{ys}^2}}\right)\phi\left(\frac{\epsilon_s}{\sigma_s\sqrt{1-\rho_{ys}^2}}\right)\exp\left(\rho_{ys}\frac{\epsilon_y\epsilon_s}{\sigma_y\sigma_s(1-\rho_{ys}^2)}\right) dyds,
\end{aligned}
$$

where $\epsilon_s = s - x_s'\beta_s$.

The expectation of the core expenditure conditional on enrollment is:

$$
E(ys|d=1) = \frac{E(ys)}{P(d=1)} = \frac{E(ys)}{\Phi\left(x_d'\beta_d\right)}.
$$

We compute the conditional and unconditional expectations at the sample mean by replacing the parameters (θ) with the ML estimates ($\hat{\theta}_{ML}$) given covariates. The marginal effect of being a girl is computed by taking the difference in these expectations when the girl dummy is set equal to zero and when it is equal to one.

We obtain the standard errors for the marginal effects by the following simulation. We first draw the parameter θ from a multivariate normal distribution, where its mean and variance respectively follow the point estimate and its variance-covariance matrix from the ML estimation. We then calculate the marginal effects again with the drawn value of θ using the expressions above. By repeating this 100 times and taking the standard deviation of the estimates of the marginal effect across replications, we obtain a standard error.

In principle, we can calculate the marginal effect for each observation and then calculate the average marginal effect over all observations. However, we choose to calculate only the marginal effects at the sample mean, where the sample mean of the whole sample [subsample of secondary school enrollees] is used for the marginal effects on the probability of enrollment and unconditional quantities [conditional quantities] to reduce the computational burden.[26]

[26] Matlab was used for computation of the marginal effects and STATA was used in the rest of the analysis.

C Details of robustness checks and other relevant results

There is a potential endogeneity concern about the results in Table 3. To understand the endogeneity concern, recall from Table 1 that girls on average live in significantly larger households than boys. This may be explained by the fertility stopping rule with unobserved parental preference towards boys (Jensen, 2002). If parents would prefer to have a boy, they may continue to try to have more children until they have a boy. This will result in girls living in larger families than boys on average. Hence, the unobserved parental preference may simultaneously affect both the household's demographic composition and the education expenditure on children such that the unobserved error terms may be correlated with the covariates.

To partially address this concern, we include household size and number of children in the set of covariates to control for the differences in household composition in our regressions. However, these controls may not fully address the potential endogeneity concerns relating to household composition. Therefore, as an alternative, we run linear regressions with household fixed effects to control for all household-level observable and unobservable characteristics in addition to individual-level observable characteristics using a subsample of children from households with at least two children in the secondary school age group. The signs of the coefficient on the girl dummy from these estimations are broadly consistent as Table 11 shows, though some coefficients are no longer statistically significant. The lack of significance, however, can be attributed to the small size of the sample used in this analysis.

A related concern is that girls are likely to face stiffer competition from siblings than boys because the former have more siblings than the latter on average. Therefore, our main results may be driven by the difference in the intrahousehold competition between boys and girls. To address this concern, we also analyze a subsample of households in which there is only one child. This arguably mitigates the gender difference in the level of competition within the household. The results of this analysis are reported in Table 12. Because the sample size for this analysis is small, it is difficult to draw definitive conclusions. Nevertheless, columns (1)-(3) of this table indicate that the contradirectional gender gap remains, although it is weaker.

We also alternatively use a subsample of children living in households with one boy and one girl in the secondary school age group and run linear regressions with household-level fixed effects as reported in columns (4)-(6). Unlike in columns (1)-(3), we are able to control for the household-level observable and unobservable characteristics. Again, the statistical significance is weaker but broadly the signs are consistent with Table 3. Therefore, our results may be partly driven by intrahousehold competition, but this does not explain away all the contradirectional gender gap.

Table 11: Results of linear regressions with household-level fixed effects

Coef.	d (1)	Cond y (2)	Cond s (3)
1995			
Girl	-0.006	-0.139*	-0.014
	(0.028)	(0.076)	(0.027)
Obs	2,871	1,107	1,107
HHs	1,313	729	729
2000			
Girl	0.076***	-0.063	-0.043*
	(0.028)	(0.090)	(0.025)
Obs	2,776	1,074	1,074
HHs	1,295	730	730
2005			
Girl	0.098***	-0.032	-0.018
	(0.028)	(0.068)	(0.015)
Obs	2,661	1,138	1,138
HHs	1,254	768	768
2010			
Girl	0.081***	-0.067	-0.043**
	(0.030)	(0.065)	(0.021)
Obs	2,683	1,333	1,333
HHs	1,275	886	886

Note: ***,** , and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. Standard errors are reported in parentheses. Each point estimate corresponds to one linear regression. Household-specific and age-specific fixed-effects terms are included in all regressions. In addition, school-type variables (public/private) are included in the set of regressors in column (2) and the logarithmic education expenditure in column (3). All other covariates used in Table 3 are absorbed in the household-level fixed effects.

To understand the trend and pattern of the contradirectional gender gap, we also performed additional analyses. As detailed in Appendix F, we find that the contradirectional gender gap has persisted and strengthened, if anything, over time. We also find that the contradirectional gender gap exists both in urban and rural areas, where the gap is stronger in the latter.

Finally, we also addressed a potential concern about the definition of secondary school age. Because of grade repetition and delayed entry into school, some secondary school age children may be still in primary school and some post-secondary school age children may be still in secondary school. To see if the presence of these children affects our results, we re-estimate the same model with an alternative definition of age groups where primary and secondary school age groups are defined as 6-11 and 12-17, respectively. The results are quantitatively and qualitatively similar. In addition, we also analyzed

Table 12: Linear regressions by subsamples with different household compositions

	Only child			**One boy one girl**		
	d	Cond y	Cond s	d	Cond y	Cond s
Coef.	(1)	(2)	(3)	(4)	(5)	(6)
1995						
Girl	-0.028	-0.041	-0.054	0.009	-0.145	0.001
	(0.055)	(0.183)	(0.039)	(0.033)	(0.093)	(0.040)
Obs	258	96	96	1,090	436	436
2000						
Girl	0.112*	0.016	-0.003	0.065**	-0.141	-0.040
	(0.059)	(0.156)	(0.046)	(0.031)	(0.100)	(0.028)
Obs	229	83	83	1,182	473	473
2005						
Girl	0.030	-0.146	-0.075**	0.088***	-0.064	-0.013
	(0.050)	(0.100)	(0.032)	(0.032)	(0.078)	(0.020)
Obs	342	160	160	1,218	547	547
2010						
Girl	0.069*	-0.007	-0.045**	0.079**	-0.078	-0.049*
	(0.039)	(0.080)	(0.019)	(0.034)	(0.075)	(0.026)
Obs	536	288	288	1,146	566	566
Basic covariates	Y	Y	Y	Y[a]	Y[a]	Y[a]
HH fixed effects	N	N	N	Y	Y	Y

[a]: The age fixed effects are included in columns (4)-(6). In addition, the school-type dummy variables and logarithmic education expenditure are included, respectively, in columns (5) and (6). All other covariates are absorbed in the household-level fixed effects.

Note: ***,** , and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. Standard errors clustered at household levels are reported in parentheses. The estimations are obtained by equation-by-equation OLS estimations for each dependent variable. The only-child subsample contains children from households with only one child. The one-boy-one-girl subsample contains children from households with two secondary school age children, one boy and one girl.

the HIES 2016 data and found evidence that the contradirectional gender gap persisted.[27]

D Falsification test

To support the findings on the impact of the FSPs on the quantity measures of education in Section 6, we conduct a falsification test. Our strategy is to estimate the FSPs' impacts on the years of completed education and enrollment if the year of introduction of the FSPs were hypothetically moved earlier by five years. We chose five years to balance the number of observations before and after the hypothetical introduction of the FSPs without losing too many observations. To make a fair comparison between the estimates based on the actual year and hypothetical year (i.e., five years prior to the actual year) of introduction of the FSPs, we first construct two estimation samples, one for each of the actual and hypothetical years.

We choose the individuals aged 16-20 and compare them with those aged 21-25 in the actual year of introduction of the FSPs, where the former and latter groups serve as the treatment and comparison groups for the purpose of the falsification test. Neither group is likely to have benefited substantially from the FSPs as they are already past the official secondary school age, even though a small fraction of those in the treatment group may have benefited from the FSPs due to delayed entry into school and grade repetition. To conduct the falsification test, we move forward the year of introduction of the FSPs by five years so that the individuals in the (hypothetical) comparison [treatment] group are aged 16-20 [11-15] in the hypothetical year of the introduction of the FSPs.

Since the falsification sample is produced by restricting each of the treatment and comparison groups to a set of individuals who were born within a five-year band, we also re-estimate the impacts of the FSPs on the quantity measures of education by applying a similar sample restriction to make a fair comparison. Specifically, we choose the individuals aged 6-10 [16-20] for the treatment [comparison] group in the actual year of introduction of the FSPs to estimate the actual impact of the FSPs. Note that we chose not to use those aged 11-15 because they are not fully covered by the FSPs; this is an approach similar to that of Duflo (2001).

In this section, we focus on the analyses of HIES 2005 and 2010, because many of those aged 6-10 at the time of the nationwide rollout of the FSPs in 1994 had not completed their education in 1995 and 2000 as they were still aged, respectively, 7-11 and 12-16 in 1995 and 2000. For the analysis of

[27] The results discussed here are available upon request. Note that we omitted the results for HIES 2016 for two reasons. First, we do not have the identifier of the parents in HIES 2016. Therefore, the estimation sample is restricted to the children of the household head in the secondary age group. Second, this analysis does not help us understand the impact of FSPs.

Table 13: Impacts of the FSPs on the quantity measures of education

	HIES 2005		**HIES 2010**	
	Actual	Hypothetical	Actual	Hypothetical
Coef.	(1)	(2)	(3)	(4)
	Panel A: Years of education			
Girl	-1.028***	-0.898***	-1.325***	-0.955***
	(0.259)	(0.195)	(0.310)	(0.242)
FSP Cover	-1.069	-2.809	-1.288	0.100
	(1.092)	(2.214)	(1.392)	(0.550)
Girl × FSPCover	1.295***	0.029	1.532***	-0.518
	(0.391)	(0.426)	(0.360)	(0.730)
Obs	5,669	6,963	8,898	7,324
Mean of dep. Var.	5.269	4.260	5.204	4.020
	Panel B: Enrollment using retrospective data			
Girl	-0.116***	-0.078***	-0.147***	-0.090***
	(0.025)	(0.019)	(0.031)	(0.023)
FSP Cover	-0.129	-0.168	-0.095	0.024
	(0.167)	(0.196)	(0.150)	(0.073)
Girl × FSPCover	0.153***	0.013	0.171***	-0.042
	(0.030)	(0.047)	(0.036)	(0.076)
Obs	38,985	34,815	44,490	36,620
Mean of dep. Var.	0.401	0.297	0.360	0.272

Note: ***,** , and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. Standard errors clustered at the household level are reported in parentheses. In Panel A, we additionally include the fixed-effects terms specific to the birth year and household. In Panel B, we additionally include the fixed-effects terms specific to the birth year, age at the time of observation, household, and year of observation.

the completed years of education, we simply take all individuals satisfying the age criteria discussed in Section 6. For the analysis of enrollment, we take the retrospectively constructed enrollment records corresponding to ages 11-15.

In the odd-numbered columns in Table 13, we report the estimation results based on the actual year of introduction of FSPs. They serve as our benchmarks and are quantitatively and qualitatively comparable to those reported in Table 5. While the point estimates appear to be somewhat attenuated and standard errors tend to be larger than those reported in Table 5, these are to be expected because those who are aged 16-20 may benefit from the FSPs and the sample size used in Table 13 is smaller.

In the even-numbered columns, we report the results of the falsification test, where the year of introduction of the FSPs is set at the hypothetical year, or five years prior to the actual year of introduction. As expected, none of the coefficients on Girl × FSPCover is positive and significant, and all coefficients are smaller in absolute value than the corresponding coefficients reported in the odd-numbered columns. Therefore, our falsification test provides suggestive evidence that the estimated positive effects of the FSPs on the quantity measures of education are not spurious. In particular, they are unlikely to be driven by subdistrict-specific time trends that are correlated with the rollout of the FSPs. Thus, the results in Table 5 do indeed appear to be driven by the rollout of FSPs.

E Comparison with Heath and Mobarak (2015)

As mentioned in Section 6, our finding of a positive impact of the FSPs on enrollment is notably at odds with Heath and Mobarak (2015, hereafter HM), who found no evidence that the FSPs have a positive impact on female enrollment. Therefore, we investigate the source of inconsistency between our results and theirs. To this end, we start with their data and specification and gradually change various elements of HM's analysis to arrive at our preferred estimate within the framework of the triple-difference estimation used by HM. We then argue that our preferred estimate is more suitable as an estimate of the impact of the FSPs on school enrollment in Bangladesh than HM's estimate.

The identification of the impact of the FSPs in HM's analysis relies on the triple-difference approach, which is somewhat similar to the double-difference specification in eq. (6). However, in addition to the differences between the two genders and between those who are in the subdistrict covered by an FSP at the time of observation and those who are not, the HM also includes the data for the primary school age group in the analysis and takes the third difference between the FSP-eligible and FSP-ineligible individuals, essentially using the fact that primary school children would not directly benefit from the FSPs. Therefore, the generic triple-difference specification we use for the comparison of our preferred specification with HM can be written as follows:

$$\begin{aligned}
\text{Enroll}_{iht} &= \alpha_1 \text{Girl}_{ih} + \alpha_2 \text{FSPCover}_{iht} + \alpha_3 \text{Eligible}_{iht} + \alpha_4 \text{FSPCover}_{iht} \times \text{Eligible}_{iht} \\
&+ \alpha_5 \text{Girl}_{ih} \times \text{FSPCover}_{iht} + \alpha_6 \text{Eligible}_{iht} \times \text{Girl}_{ih} \\
&+ \alpha_7 \text{FSPCover}_{iht} \times \text{Eligible}_{iht} \times \text{Girl}_{ih} + \lambda_t^0 + \lambda_t^1 \times \text{Girl}_{ih} + \sum_{a=5}^{a=18} \beta_a^0 \times \mathbf{1}(\text{Age} = a) \\
&+ \sum_{a=5}^{a=18} \beta_a^1 \times \mathbf{1}(\text{Age} = a) \times \text{Girl}_{ih} + \omega_h + \epsilon_{iht}, \qquad (7)
\end{aligned}$$

where Eligible_{iht} is the dummy variable for the FSP eligibility and βs, λs, and ω_h represent, respectively, age-gender-, time-gender-, and household-specific fixed effects. α_7 is the coefficient of our main interest.

Let us now highlight three major differences between HM's specification and our preferred specification in the triple-difference framework. First, the definition of FSPCover_{iht} is different. In HM, it is an indicator for the year 1994 or later ("P94"), which is a reasonable choice because the FSP was scaled up significantly in 1994 and all four subdistricts in the HM's data (see Appendix B of Heath and Mobarak (2015)) were indeed first covered by the FSPs in 1994. However, in our preferred specification, we take into account the full information ("Full") about the rollout of the FSPs to address the fact that some subdistricts were covered by the FSPs before 1994. Second, the definition of Eligible_{iht} is also different. In HM, it is an indicator of having completed at least six years of schooling at the time of observation, which means that the individual has already completed the first year of secondary school. In our preferred specification, we instead define Eligible_{iht} as an indicator of having completed primary school, or five years of education. We argue that this is a more suitable definition, because individuals will make an enrollment decisions taking into account whether they would benefit from the FSPs *if* they enrolled. Finally, the data are different. In particular, HM's data were collected in 2009 and only cover four subdistricts ("HM4"), but our preferred specification uses all districts ("All") included in the nationally representative HIES 2010 data set.

To ensure maximum comparability, we construct the retrospective panel data on enrollment both from HM's data and HIES 2010 using the same rule. As with the construction of the enrollment indicator for eq. (6), we construct past enrollment status using the age and maximum educational attainment at the time of observation under the assumption of no grade repetition. Because we also include observations corresponding to the primary school children in this section, we do this exercise under the assumption that all children start grade 1 at the age of six. As with HM, we take all observations for those individuals who are aged between 5 and 18 at the time of observation and do this for the period between 1960 and 2007.

Table 14 provides the estimation results for eq. (7) based on different choices of data and definitions of FSPCover_{iht} and Eligible_{iht}. In Panel A, we use at least six years of education as the FSP eligibility criterion to be consistent with HM. In Panel B, we use primary school completion as the FSP eligibility criterion. Columns (1)-(3) use only HM4 subdistricts, whereas columns (4)-(5) use all subdistricts. Columns (1)-(4) use P94 for the definition of FSPCover, whereas column (5) uses full information.

In column (1) of Panel A, we reproduce the estimate of the FSP impact reported in HM, which uses the actual age at which the individual entered school. This estimate in fact suggests that the FSPs had no impact on enrollment. Because we do not observe the actual age of school entry in HIES,

Table 14: Effects of FSPs on school enrollment: comparison with HM

Data source	HM[a]		HIES 2010		
Dep var: Enroll_{iht}	(1)	(2)	(3)	(4)	(5)
Panel A: Eligible is 6+ years of education					
FSPCover × Eligible ×Girl	-0.0097	-0.017	0.060	0.020**	0.022**
	(0.0609)	(0.059)	(0.056)	(0.009)	(0.009)
Panel B: Eligible is primary school completion (5+ years of education)					
FSPCover × Eligible × Girl	—	0.046	0.104*	0.078***	0.082***
		(0.050)	(0.053)	(0.008)	(0.009)
Definition of FSPCover[b]	P94	P94	P94	P94	Full
Subdistricts in the sample[c]	HM4	HM4	HM4	All	All
Observations	23,129	23,116	9,216	517,039	517,039
No. of Individuals	—	2,244	766	45,444	45,444
No. of Households	878	878	220	12,124	12,124

[a] Column (1) uses the enrollment data HM constructed (`JDE HM data -- enrollment.dta`). Column (2) uses educational attainment data (`JDE HM data -- educational attainment.dta`), which contain the age, gender, and highest educational attainment, but not the actual entry age in school. These two data cannot be merged because there is no individual identifier. As a result, we are unable to redefine Eligible_{iht} to obtain an estimate for column (1) of Panel B.
[b] In columns (1)–(4), the FSP coverage indicator (FSPCover_{iht}) is an indicator for the year 1994 or later ("P94"), whereas it uses full information ("Full") about the FSP rollout in column (5).
[c] The samples used in columns (1)–(3) cover the four districts in the HM data ("HM4"). Columns (4)–(5) use all subdistricts ("All") in HIES 2010.
Note: ***,** , and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. The estimation is based on the OLS estimation of eq. (7). Standard errors clustered at the household level are reported in parentheses. The fixed-effects terms by household, age-gender combination, and gender-year combination are included in all regressions. Further, FSPCover, Eligible, and Girl, and the interactions between any two of these three variables are also included.

we have to make the assumption that children enter primary school at the age of 6. Based on this assumption, we reconstructed retrospective panel data on enrollment using the HM data and ran the same regression. As reported in column (2) of Panel A, the estimate remains similar. Therefore, our entry age assumption does not appear to alter the results much.

Now, let us compare Panels A and B of column (2). While point estimates are both insignificant, it is worth noting that the point estimate is positive when the primary completion is used for the eligibility definition. Column (3) uses the HIES 2010 data instead of the HM data, but we focus on the HM4 subdistricts. While both data were randomly sampled and the difference between columns (2) and (3) is insignificant, it appears plausible that the sampling negatively affected the estimated FSP impact from the HM data relative to that from the HIES 2010 data.

In column (4), we expand the data to include all subdistricts. The larger sample size clearly allowed us to obtain a more accurate estimate. The point estimate is positive and significant whether we use HM's eligibility definition or ours. In column (5), we use the full information about the FSP rollout instead of P94. This change does not affect the results much as expected, because the FSP coverage started in 1994 for most individuals.

By comparing across the columns in Panel A, we see that both the choice of data and geographic coverage of the data (or sample size) appear to have affected HM's result. However, the primary difference between HM's estimate and our preferred estimate reported in column (5) of Panel B comes from the definition of the eligibility criterion.

As we argued earlier, our choice of eligibility criterion is more suitable, because the FSPs make it more attractive to keep girls enrolled in school after the completion of primary education. If we use at least six years of education, the FSPs' impact on grade-6 students is absorbed by the noneligible group. As a result, the FSPs' impact would be underestimated. It is therefore not surprising that there is a sizable difference between the estimates in Panels A and B in each column.

We also conducted a few robustness checks. First, we tested our results under the alternative school entry ages of 5 and 7, because not all children enter school at the age of 6. Second, instead of using the individuals aged between 5 and 18, we limit the sample to ages 6 to 15 to follow the stipulated primary and secondary school age groups. These analyses do not qualitatively change our results.

It is worth noting that the magnitude of the estimated impact of the FSPs on enrollment is quantitatively different between Tables 5 and 14. The most comparable estimate, which uses the HIES 2010 data with household fixed effects reported in column (4) of Panel B of Table 5, suggests that the FSPs had a positive impact on enrollment by more than 19 percentage points. On the other hand, our preferred estimate in Table 14 suggests only around 8 percentage points.

We argue that the latter estimate would serve as a lower bound of the impact for the FSPs' target age group for two reasons. First, by extending our earlier argument to use primary completion instead of at least six years of education as a more suitable eligibility criterion, it can be seen that the decision to enrol in a primary school is likely to be positively influenced by the FSPs that are available at the secondary level. This in turn means that the triple-difference estimate would underestimate the impact of the FSPs. Second, the double-difference estimate in Table 5 directly focuses on the secondary school students. On the other hand, the sample used in Table 14 includes relatively old individuals, aged 16 to 18, who are not the main target age of the FSPs. For these reasons, we prefer the double-difference estimates over triple-difference estimates and use the former in the main text.

F Supplementary tables and figures

Figure 2 depicts the average education expenditure conditional on enrollment by year, gender and grade. Table 15 summarizes the main results of the existing studies using a hurdle model. Tables 16 and 17 provide the same summary statistics as Tables 1 and 2 except that the former are for the years 2000 and 2005.

Table 18 reports the complete regression results for columns (4)-(6) of Table 3. Here, we briefly summarize some notable findings about the covariates other than the girl dummy. In general, children in richer households are more likely to be enrolled in school and receive a higher expenditure on education but a lower core share. Parental education, especially the mother's education, has a qualitatively similar effect in all three decisions except for the year 2000. The more educated parents are, the more likely children will enroll in school and the higher education expenditure they are likely to receive. These points suggest the presence of positive intergenerational transmission in education. In contrast, if the head is a wage worker, the child has a lower probability of enrollment.

Another notable finding is the relevance of the location of residence as well as school access and type. Children in urban areas are less likely to enroll in school but have a higher education expenditure conditional on enrollment, which may be a result of various aid programs targeted only at rural areas. Table 18 also shows that children are more likely to enroll when the number of secondary schools per thousand people in the area of residence is higher. The coefficients on the school-type variables show that children going to private schools spend more on education than those going to public or other types of schools.

Table 18 also shows that the estimated values of ρs are mostly highly statistically significant, indicating the relevance of allowing for the correlation in the error terms. The estimations for ρ_{dy} and ρ_{ds} are negative and almost all significant at a 1 percent level from 2000 onwards. One plausible explanation is that the unobserved academic capability affects the enrollment and the other two decisions in different directions, possibly because very smart students need little spending on education. Overall, these results are reasonable and broadly consistent with the literature.

Table 19 shows that the regression results are similar when the independence of error terms is assumed. The sign and significance of the coefficients remain similar, but the absolute value of the coefficients for the conditional education expenditure and core share equations appears to be somewhat larger when the dependence structure is allowed for.

To understand the time trend of the gender bias in education expenditure, we estimated the three-part model for all years simultaneously with the time fixed effects and their interaction terms with the girl dummy by pooling the four survey rounds. As the regression results in Table 20 show, the gender bias remains similar to the year-by-year results in Table 3. In other words, the enrollment decision is biased in favor of girls but the opposite is true for the conditional expenditure and core share decisions. Further, the coefficients on the interaction terms between the year and girl dummy variables show that the enrollment decision has become more profemale since the base year of 1995. In contrast, the core share has become more promale. The bias in the conditional total education expenditure did not change much over time and, if anything, became more promale. Therefore, Table 20 indicates that the apparent contradirectional gender gap did not change much after 1995 and, if anything, was strengthened by the fact that the profemale bias in the enrollment decision and the promale bias in the conditional core share decision became stronger.

It may be argued that rural and urban samples should be analyzed separately, because there are various important differences between the urban and rural areas as mentioned at the end of Section 5. Further, as detailed in Section 6, the FSPs only covered nonmetropolitan areas. Thus, we re-estimate the analysis of the three-part model separately for the urban and rural areas. As the results in Table 21 show, the directions of the gender gap in the three equations are essentially the same between the main results and rural subsample results. The comparison between the urban and rural areas shows that the contradirectional gender gap in rural areas is generally stronger than that in urban areas.

Table 22 reports the marginal effect of being a girl at the sample mean for the secondary school enrollees for each item in education expenditure by Tobit regressions. Finally, Table 23 provides the marginal effects of the girl and FSP dummy variables at the sample mean for each education expenditure item. We only present the results for the years 2000 and 2005, because the FSP recipient status is either unavailable or irrelevant in other years (see footnote 21).

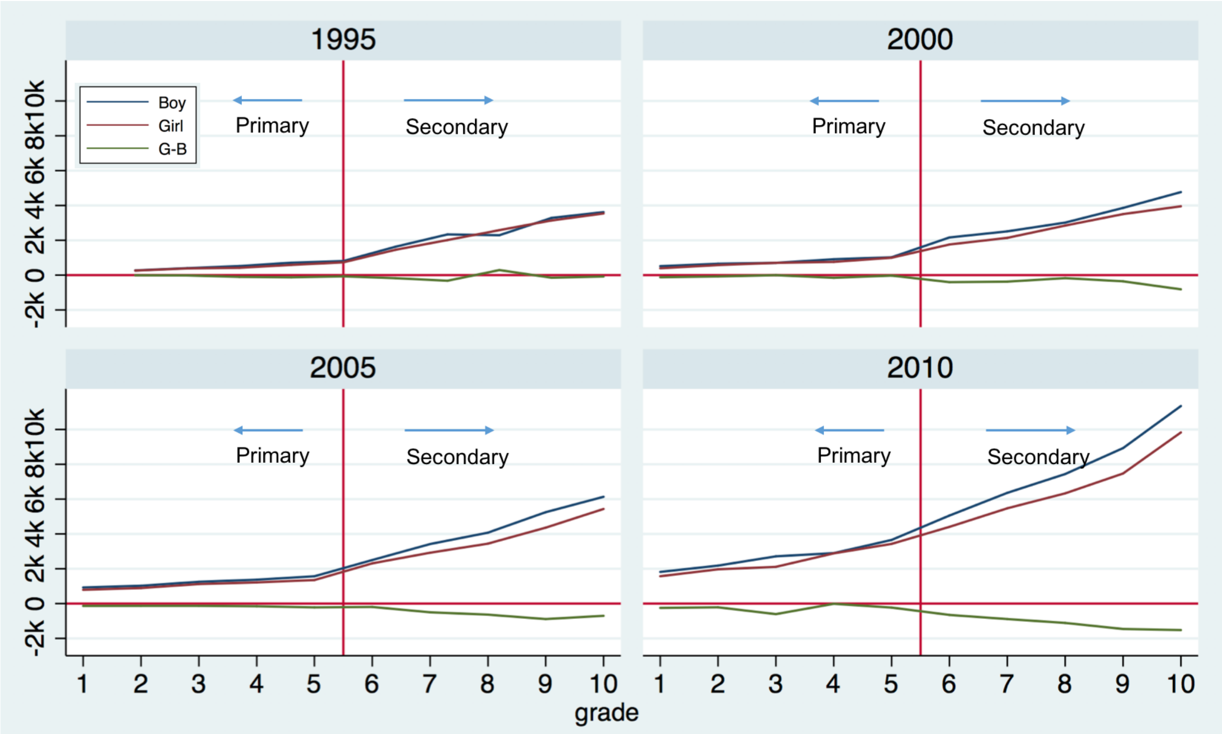

Figure 2: Nominal education expenditure in BDT by year, gender, and grade

Table 15: Existing studies on the gender gap in education expenditure using a hurdle model

Study	**Location & Year**	**Age Grp**	**Enroll.**	**Cond. Exp.**
Kingdon (2005)	16 states in rural India, 1994	5 to 14	−	≈
Aslam and Kingdon (2008)	Pakistan, 2001-02	5 to 9	−	≈
		10 to 14	−	−
Himaz (2010)	Sri Lanka, 1990-91, 1995-96, 2000-01	5 to 9	≈	+
		10 to 13	≈	≈
		14 to 16	≈	+
Masterson (2012)	Rural Paraguay, 2000-01	5 to 14	−	−
	Urban Paraguay, 2000-01	5 to 14	+	+
Azam and Kingdon (2013)	India, 2004-05	5 to 9	≈	−
		10 to 14	−	−
Kenayathulla (2016)	Malaysia, 2004-05	5 to 14	≈	≈

Note: −, +, and ≈ mean significant promale bias, profemale bias, and no bias, respectively.

Table 16: Summary statistics of basic covariates by gender for 2000 and 2005 (secondary school age group)

	2000				2005			
	Boy (B)	**Girl (G)**	**G-B**	**All**	**Boy (B)**	**Girl (G)**	**G-B**	**All**
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
All children aged 11-15								
Enrolled in secondary school	0.342	0.450	0.108	0.395	0.417	0.513	0.096	0.465
	(0.475)	(0.498)	***	(0.489)	(0.493)	(0.500)	***	(0.499)
Child's age (yrs)	13.019	12.918	-0.101	12.970	13.079	13.005	-0.074	13.042
	(1.400)	(1.347)	***	(1.375)	(1.402)	(1.352)	**	(1.378)
HH per capita expenditure	10.890	11.592	0.702	11.232	14.604	14.955	0.351	14.777
(thousand BDT/year)	(8.080)	(9.302)	***	(8.704)	(11.070)	(12.022)		(11.549)
Household size	6.403	6.552	0.149	6.476	5.983	6.096	0.113	6.038
	(2.381)	(2.395)	**	(2.389)	(2.240)	(2.162)	*	(2.202)
Father's education (yrs)	2.891	3.161	0.270	3.023	3.111	3.238	0.127	3.174
	(4.177)	(4.239)	**	(4.209)	(4.236)	(4.256)		(4.246)
Mother's education (yrs)	1.773	1.974	0.201	1.871	2.287	2.372	0.085	2.329
	(3.161)	(3.246)	**	(3.204)	(3.565)	(3.591)		(3.578)
Number of children	3.514	3.626	0.112	3.569	3.233	3.331	0.098	3.281
	(1.739)	(1.756)	**	(1.748)	(1.567)	(1.582)	**	(1.575)
Urban	0.320	0.342	0.022	0.331	0.343	0.346	0.003	0.345
	(0.467)	(0.475)	*	(0.471)	(0.475)	(0.476)		(0.475)
Female head	0.073	0.079	0.006	0.076	0.094	0.092	-0.002	0.093
	(0.260)	(0.270)		(0.265)	(0.291)	(0.289)		(0.290)
Head is a wage worker	0.461	0.480	0.019	0.470	0.448	0.485	0.037	0.466
	(0.499)	(0.500)		(0.499)	(0.497)	(0.500)	***	(0.499)
Head's age (yrs)	47.000	46.854	-0.146	46.929	47.663	47.595	-0.068	47.630
	(10.736)	(10.951)		(10.841)	(10.625)	(10.431)		(10.529)
Muslim	0.920	0.923	0.003	0.922	0.892	0.895	0.003	0.893
	(0.271)	(0.267)		(0.269)	(0.310)	(0.307)		(0.309)
Hindu	0.075	0.070	-0.005	0.072	0.091	0.092	0.001	0.091
	(0.263)	(0.254)		(0.259)	(0.287)	(0.288)		(0.288)
Father's education missing	0.147	0.171	0.024	0.159	0.147	0.170	0.023	0.159
	(0.354)	(0.376)	**	(0.365)	(0.354)	(0.376)	**	(0.365)
Mother's education missing	0.069	0.082	0.013	0.076	0.062	0.079	0.017	0.070
	(0.254)	(0.275)	*	(0.264)	(0.240)	(0.270)	***	(0.256)
Obs	2,534	2,417		4,951	2,906	2,817		5,723
Enrolled in secondary school children aged 11-15								
Govt school	0.26	0.23	-0.03	0.24	0.25	0.23	-0.02	0.24
	(0.44)	(0.42)		(0.43)	(0.43)	(0.42)		(0.43)
Private school	0.67	0.70	0.03	0.68	0.64	0.69	0.05	0.67
	(0.47)	(0.46)		(0.46)	(0.48)	(0.46)	**	(0.47)
Other	0.08	0.07	-0.01	0.07	0.10	0.09	-0.01	0.10
	(0.27)	(0.25)		(0.26)	(0.31)	(0.28)		(0.29)
Obs	867	1,088		1,955	1,213	1,446		2,659

Note: Standard deviations are reported in parentheses below the mean. ***,** , and * denote that the means for girls and boys are different at the 1, 5, and 10 percent significance level, respectively. "Other" in school type includes all schools other than public and private schools, including religious (e.g., madrasa) and NGO schools.

Table 17: Summary statistics of annual education expenditure in taka by items for secondary school enrollees in 2000 and 2005

	2000				2005			
	Boy (B)	**Girl (G)**	**G-B**	**% Zeros**	**Boy (B)**	**Girl (G)**	**G-B**	**% Zeros**
BDT	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Core	2,332	1,924	-408	1%	3,019	2,637	-382	1%
	(2,808)	(3,537)	***		(3,690)	(3,883)	***	
Tuition	338	157	-181	48%	460	245	-215	50%
	(476)	(476)	***		(1,377)	(1,551)	***	
Private Tutoring	1,225	1,020	-205	49%	1,589	1,455	-134	42%
	(2,342)	(3,113)	*		(2,913)	(2,844)		
Material	769	747	-22	1%	970	936	-34	2%
	(550)	(523)			(629)	(597)		
Peripheral	1,050	955	-95	1%	1,345	1,177	-168	0%
	(1,652)	(1,188)			(2,100)	(1,572)	**	
Admission	200	181	-19	26%	244	232	-12	27%
	(527)	(502)			(672)	(729)		
Exam	155	149	-6	5%	179	186	7	5%
	(186)	(146)			(218)	(224)		
Uniform	241	259	18	46%	344	347	3	35%
	(324)	(295)			(463)	(390)		
Meal	191	178	-13	63%	193	155	-38	68%
	(422)	(353)			(418)	(360)	**	
Transportation	159	133	-26	83%	155	171	16	86%
	(587)	(475)			(701)	(777)		
Others	104	55	-49	75%	230	86	-144	65%
	(824)	(429)			(1,331)	(458)	***	
Total	3,382	2,879	-503		4,363	3,814	-549	
	(3,874)	(4,355)	***		(4,938)	(4,913)	***	
Core Share	0.68	0.63	-0.05		0.68	0.65	-0.03	
	(0.19)	(0.20)	***		(0.20)	(0.20)	***	
Obs	867	1,088		1,955	1,213	1,446		2,659

Note: Standard deviations are reported in parentheses below the mean. ***,** , and * denote that the means of girl and boy are different at the 1, 5, and 10 percent significance level, respectively. The summary statistics is for the subsample of children who were enrolled in secondary school at the time of the survey. Core share stands for the share of the core components in the total education expenditure. The annual session and registration fees are included in admission to maintain consistency with Table 2.

Table 18: ML estimation of three-part model with dependence for secondary school age group

	1995			2000			2005			2010		
Coef.	d	Cond y	Cond s	d	Cond y	Cond s	d	Cond y	Cond s	d	Cond y	Cond s
Girl	-0.000	-0.088***	0.003	0.330***	-0.173***	-0.075***	0.272***	-0.141***	-0.061***	0.256***	-0.119***	-0.050***
	(0.042)	(0.033)	(0.035)	(0.039)	(0.049)	(0.015)	(0.035)	(0.028)	(0.012)	(0.033)	(0.025)	(0.009)
Log(per capita expen)	0.506***	0.813***	-0.360	0.549***	0.817***	-0.146***	0.418***	0.665***	-0.114***	0.365***	0.744***	-0.085***
	(0.047)	(0.043)	(0.299)	(0.054)	(0.060)	(0.039)	(0.046)	(0.037)	(0.032)	(0.045)	(0.036)	(0.022)
Log(hh size)	0.100	0.146**	-0.043	0.313***	0.136	-0.054**	0.047	0.152***	0.004	0.213**	0.328***	-0.077***
	(0.085)	(0.070)	(0.063)	(0.099)	(0.084)	(0.026)	(0.080)	(0.055)	(0.020)	(0.083)	(0.074)	(0.021)
Father edu (yrs)	0.081***	0.019***	-0.009	0.063***	-0.003	-0.006***	0.051***	0.008	-0.008***	0.037***	0.007*	-0.005***
	(0.007)	(0.006)	(0.008)	(0.007)	(0.010)	(0.002)	(0.006)	(0.005)	(0.002)	(0.006)	(0.004)	(0.001)
Mother edu (yrs)	0.089***	0.031***	-0.014	0.057***	-0.003	-0.006***	0.060***	0.032***	-0.010***	0.064***	0.018***	-0.008***
	(0.010)	(0.007)	(0.012)	(0.009)	(0.009)	(0.002)	(0.008)	(0.006)	(0.002)	(0.007)	(0.005)	(0.002)
No. of children	0.006	-0.008	0.001	-0.071***	-0.004	0.012**	-0.057***	-0.030**	0.003	-0.057***	-0.024	0.014***
	(0.016)	(0.014)	(0.007)	(0.019)	(0.019)	(0.005)	(0.018)	(0.013)	(0.005)	(0.019)	(0.018)	(0.005)
Urban	-0.079	0.259***	-0.103	-0.128***	0.301***	-0.004	-0.084**	0.279***	-0.018	-0.118***	0.234***	0.039***
	(0.053)	(0.045)	(0.098)	(0.048)	(0.047)	(0.020)	(0.040)	(0.030)	(0.017)	(0.039)	(0.030)	(0.012)
Female head	-0.031	-0.028	-0.036	0.301**	-0.118	-0.063*	0.400***	0.125**	-0.060**	-0.019	0.159**	-0.016
	(0.084)	(0.074)	(0.039)	(0.119)	(0.119)	(0.035)	(0.103)	(0.061)	(0.028)	(0.091)	(0.081)	(0.022)
Head is a wage worker	-0.108**	0.014	-0.035	-0.173***	0.121**	0.017	-0.185***	0.044	0.029***	-0.151***	0.054*	0.005
	(0.049)	(0.041)	(0.022)	(0.044)	(0.049)	(0.014)	(0.038)	(0.031)	(0.011)	(0.037)	(0.030)	(0.009)
Head's age (yrs)	0.000	-0.003*	0.001	-0.000	-0.004**	-0.000	-0.006***	-0.000	0.000	-0.004**	-0.003**	0.000
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)	(0.000)
Muslim	-0.262	-0.150	0.091	0.216	0.029	0.129**	-0.065	-0.254***	-0.002	0.164	-0.313**	0.054
	(0.248)	(0.132)	(0.077)	(0.263)	(0.187)	(0.056)	(0.163)	(0.094)	(0.046)	(0.208)	(0.146)	(0.047)
Hindu	-0.087	-0.147	0.103	0.298	0.138	0.129**	-0.127	-0.194*	0.017	0.174	-0.298**	0.086*
	(0.257)	(0.139)	(0.079)	(0.270)	(0.197)	(0.059)	(0.173)	(0.102)	(0.047)	(0.214)	(0.151)	(0.048)
Father's edu is missing	-0.401	0.735	-0.400	-0.247**	0.130	0.038	-0.480***	-0.103	0.105***	-0.027	-0.056	-0.000
	(0.442)	(0.522)	(0.356)	(0.112)	(0.108)	(0.034)	(0.099)	(0.067)	(0.029)	(0.088)	(0.080)	(0.021)
Mother's edu is missing	-	-	-	-0.481***	0.101	0.124***	-0.124	0.165**	0.008	-0.361***	-0.057	0.079***
				(0.117)	(0.123)	(0.033)	(0.099)	(0.076)	(0.033)	(0.091)	(0.089)	(0.023)
Secondary school accessibility	2.673***			5.752***			1.449***			2.216***		
	(0.591)			(1.025)			(0.381)			(0.427)		
Madrassa school accessibility	-0.358			-5.940***			0.722			0.947*		
	(0.900)			(1.044)			(0.473)			(0.522)		
Government school		0.140			0.231***			0.100*			0.363***	
		(0.112)			(0.087)			(0.054)			(0.057)	
Private school		0.176			0.425***			0.264***			0.495***	
		(0.123)			(0.079)			(0.052)			(0.051)	
Log(education expend)			0.464			0.082*			0.100**			0.081***
			(0.367)			(0.047)			(0.049)			(0.029)
σ_y		0.701***			0.767***			0.666***			0.714***	
		(0.018)			(0.059)			(0.015)			(0.017)	
σ_s		0.339			0.244***			0.270***			0.240***	
		(0.214)			(0.015)			(0.009)			(0.008)	
ρ_{dy}		0.302***			-0.495***			-0.090			-0.364***	
		(0.080)			(0.186)			(0.086)			(0.065)	
ρ_{ds}		-0.286**			-0.801***			-0.923***			-0.866***	
		(0.132)			(0.107)			(0.031)			(0.039)	
ρ_{ys}		-0.829***			0.116			-0.075			0.112	
		(0.237)			(0.168)			(0.141)			(0.096)	
Observations		5,053			4,951			5,723			6,402	

Note: ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. Standard errors clustered at the household level are reported in parentheses. School accessibility variables are the number of secondary schools or madrasas per 1000 people, which is calculated at the subdivision level (for 2000) or district level (for all other years). Age-specific fixed-effects terms are also included in each regression (not reported).

Table 19: ML estimation of the three-part model with different error structure

	d	Cond y	Cond s
1995			
Independence	-0.003	-0.088***	-0.030***
	(0.042)	(0.032)	(0.009)
Dependence	-0.000	-0.088***	0.003
	(0.042)	(0.033)	(0.035)
2000			
Independence	0.334***	-0.101***	-0.040***
	(0.041)	(0.033)	(0.009)
Dependence	0.330***	-0.173***	-0.075***
	(0.039)	(0.049)	(0.015)
2005			
Independence	0.313***	-0.130***	-0.019**
	(0.037)	(0.026)	(0.008)
Dependence	0.272***	-0.141***	-0.061***
	(0.035)	(0.028)	(0.012)
2010			
Independence	0.285***	-0.078***	-0.019***
	(0.035)	(0.024)	(0.007)
Dependence	0.256***	-0.119***	-0.050***
	(0.033)	(0.025)	(0.009)

Note: ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. Standard errors clustered at household level are reported in parentheses. "Independence" rows are estimated under the assumption: $\rho_{dy} = \rho_{ds} = \rho_{ys} = 0$. "Dependence" rows are the same as those reported in columns (4)-(6) of Table 3.

Table 20: Results of the pooled regression using the three-part model

	d	Cond y	Cond s
Coef.	(1)	(2)	(3)
Girl	0.028	-0.102***	-0.029***
	(0.039)	(0.033)	(0.010)
Y_{00}	0.038	0.212***	-0.037***
	(0.039)	(0.036)	(0.012)
Y_{05}	0.024	0.393***	-0.067***
	(0.040)	(0.032)	(0.013)
Y_{10}	-0.093**	0.479***	-0.087***
	(0.045)	(0.036)	(0.016)
Girl $\times Y_{00}$	0.303***	-0.042	-0.044***
	(0.055)	(0.048)	(0.015)
Girl $\times Y_{05}$	0.241***	-0.070	-0.022
	(0.053)	(0.043)	(0.014)
Girl $\times Y_{10}$	0.228***	-0.010	-0.020
	(0.051)	(0.041)	(0.013)
Obs		22,129	

Note: ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. Standard errors clustered at the household level are reported in parentheses. Additional controls include the set of covariates discussed in Table 3 except that the school accessibility variables are constructed at subdivision level for all years to have a uniform definition across years. The year 1995 is the base year for comparison in these regressions.

Table 21: Estimation of the three-part model by urban and rural subsamples

	Urban			**Rural**		
Coef.	d	Cond y	Cond s	d	Cond y	Cond s
1995						
Girl	0.089	0.005	-0.032*	-0.044	-0.132***	0.010
	(0.071)	(0.047)	(0.019)	(0.053)	(0.043)	(0.045)
Obs		1,734			3,319	
2000						
Girl	0.333***	0.018	-0.044*	0.337***	-0.277***	-0.105***
	(0.077)	(0.066)	(0.024)	(0.047)	(0.056)	(0.019)
Obs		1,638			3,313	
2005						
Girl	0.278***	-0.068	-0.052***	0.284***	-0.174***	-0.070***
	(0.064)	(0.052)	(0.016)	(0.042)	(0.034)	(0.016)
Obs		1,972			3,751	
2010						
Girl	0.367***	0.002	-0.016	0.226***	-0.147***	-0.054***
	(0.062)	(0.041)	(0.011)	(0.042)	(0.032)	(0.011)
Obs		2,215			4,187	

Note: ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. Standard errors clustered at the household level are reported in parentheses. The same set of covariates is used as in Table 3 except that the urban dummy is dropped.

Table 22: Tobit marginal effect of the girl dummy on education expenditure by expenditure item among secondary school enrollees

Expenditure in BDT	**1995**	**2000**	**2005**	**2010**
Core	-167.6*	-239.7*	-206.4	-200.8
	(101.5)	(128.1)	(137.0)	(223.9)
Tuition	-410.3***	-578.9***	-1,644.4***	-1,023.2***
	(106.0)	(62.8)	(364.9)	(129.3)
Private Tutoring	-128.6	-229.6	-61.2	-247.6
	(139.3)	(176.6)	(153.3)	(215.8)
Material	-3.8	6.3	-9.6	20.4
	(20.1)	(21.7)	(20.9)	(34.5)
Peripheral	19.6	-17.4	-97.1	47.8
	(45.5)	(59.5)	(66.2)	(99.7)
Admission	6.6	-33.6	-12.0	-64.6
	(13.0)	(30.5)	(34.7)	(50.5)
Exam	6.3	2.8	13.6	-2.7
	(6.7)	(7.6)	(9.0)	(11.6)
Uniform	70.2***	82.4***	31.2	66.8**
	(22.4)	(22.3)	(23.7)	(29.4)
Meal	120.3	23.1	-47.8	-39.0
	(1,377.4)	(38.9)	(40.7)	(58.7)
Transport	13.1	-59.7	70.7	786.0***
	(78.2)	(105.8)	(140.8)	(209.8)
Obs	1,842	1,955	2,659	3,365

Note: ***,**, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. Standard errors clustered at the household level are reported in parentheses. Marginal effects using Tobit regressions of education expenditure items evaluated at the mean of the subsample of secondary school enrollees are reported. The covariates are the same as those used in columns (2) and (5) of Table 3. The annual session and registration fees are also included in admission because they are not separately reported in HES 1995.

Table 23: Tobit regressions of education expenditure items for secondary school enrollees with the FSP dummy

	Marginal effects	**Core**	Tuition	Private Tutoring	Material	**Peripheral**	Admission	Exam	Uniform	Meal	Transportation
	at the mean (BDT)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	FSP	-546.3**	-699.6***	-107.0	73.0**	-8.4	-143.9***	5.9	108.3***	138.7**	183.8
		(243.1)	(107.2)	(291.0)	(30.9)	(74.7)	(48.6)	(9.9)	(31.7)	(54.9)	(148.5)
2000 Sec	Girl	81.8	-213.6***	-165.1	-36.7	-12.4	50.7	-0.7	18.6	-61.1	-169.4
		(246.4)	(43.2)	(283.0)	(28.9)	(80.9)	(49.6)	(9.6)	(27.7)	(50.6)	(136.8)
	Obs	1,955	1,955	1,955	1,955	1,955	1,955	1,955	1,955	1,955	1,955
	FSP	-356.5*	-1,814.2***	230.9	63.4**	-87.2	-161.1***	-2.1	60.8**	76.9	177.3
		(213.7)	(493.2)	(229.0)	(28.4)	(82.1)	(61.0)	(12.5)	(30.5)	(55.8)	(202.1)
2005 Sec	Girl	-25.3	-889.1***	-180.8	-41.8*	-52.8	68.7	14.6	0.2	-87.6*	-18.6
		(210.0)	(199.4)	(205.1)	(25.2)	(87.1)	(57.6)	(13.4)	(29.1)	(51.6)	(172.8)
	Obs	2,659	2,659	2,659	2,659	2,659	2,659	2,659	2,659	2,659	2,659

Note: ***,** , and * denote statistical significance at the 1, 5, and 10 percent levels, respectively. Standard errors clustered at the household level are reported in parentheses. Marginal effects at the sample mean using Tobit regressions of each education expenditure item for the subsample of school enrollees are reported. The covariates are the same as those used in columns (2) and (5) of Table 3. The annual session and registration fees are included in admission to be consistent with Table 2.

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