# The Effect of Maternal Education on Sex Bias in Child Survival in Bangladesh[1]

**Aiko Hattori**
**Peter M. Lance**
**Gustavo Angeles**

Carolina Population Center
University of North Carolina at Chapel Hill
CB# 8120, University Square, Chapel Hill, NC 27516-39

**Abstract** In Bangladesh the child mortality is higher for girls than boys, likely due to differential parental behaviors favoring sons to daughters. Maternal education, on the other hand, is presumed to improve child health for both boys and girls, and often especially for girls by enhancing non-differential parental behaviors. This study estimates the causal effect of maternal education on sex bias in child survival between the first and fifths birthdays in rural Bangladesh. Applying instrumental variables (IV) constructed from education programs introduced nationwide in the1990s in Bangladesh and using integrated data from the 2007 Demographic and Health Survey, the 1981 population census, and the secondary school census, we find that a one-year increase in the highest grade achieved increases significantly the survival probability by .012. The IV estimate of the effect of maternal education is significantly larger than the OLS estimate of .002. However, we do not find any incremental effect of maternal education by gender of child, implying that girls do not benefit any more than boys from an educated mother. The education programs are found to have improved child survival for both genders by enhancing maternal education in Bangladesh, although, again, we do not find any incremental effect of the education programs by gender of child.

Globally, female mortality rates are typically lower, resulting in a higher life expectancy for women (Bhuiya and Streatfield 1991; D'Souza and Chen 1980). In Bangladesh and some countries, however, the reversal is observed for child mortality (defined as the number of deaths between the first and fifth birthdays per 1,000 children survived to their first birthday) (Balk 1994; Basu 1989; Bhuiya and Streatfield 1991; Chowdhury 1994; D'Souza and Chen 1980; El-Badry 1969; Fauveau et al. 1991; Filmer et al. 1998). The estimated child mortality of 20 among girls was higher than that of 16 among boys in 2007 (NIPORT et al. 2009), and the gap has remained almost unchanged over the last decade. Child deaths after the neonatal period are caused mainly by childhood diseases and accidents, which are likely to be associated with external factors including nutrition, hygienic practices, and prevention and treatment of illness (Bairagi 1986; Bhuiya et al. 1987; Chen et al. 1981; Chowdhury 1994; Das Gupta 1987; Miller 1997; Sen and Sengupta 1983). These external factors are largely determined by parental health seeking behaviors and access to services. The gap in child mortality between girls and boys therefore likely implies differential parental behaviors by gender of child (essentially parental

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behaviors favoring sons to daughters) (Chen et al. 1981; Chowdhury and Bairagi 1990; Chowdhury 1994; D'Souza and Chen 1980).

The differential parental behaviors in Bangladesh may be driven by son preference, that is parental perceptions favoring sons to daughters (Chen et al. 1981; D'Souza and Chen 1980; Das Gupta 1987; Filmer et al. 1998; NIPORT et al. 2009). Environmental and structural factors, including patriarchal structure, women's autonomy and marriage norms, may form the basis for parental son preference (Filmer et al. 1998). In the patriarchal social setting of Bangladesh family property is usually inherited by male members (Agarwal 1994; Quisumbing and Maluccio 2000). A daughter, on the other hand, is often considered a person who would be married off and a future economic burden, requiring a dowry upon marriage (Basu 1989; Das Gupta et al. 2003; Mason 1987). Women, who often marry men outside their community, need to gain standing in their husband's household and community by giving birth to a son (Basu 1989; Bhuiya and Streatfield 1991; Mason 1987). Therefore pressure is imposed on a family and a woman to have a son, leading to parental son preference and differential parental behaviors.

Household characteristics, including economic conditions and family structure, may shape differential parental behaviors as well. When households face a tightening of economic conditions, parents may concentrate the remaining resources on sons, resulting in disadvantageous situations for daughters (Bairagi 1986). Likewise, the number and gender of siblings may have differential effects between girls and boys. A study examining the effect of children's birth order and siblings in Northern India reports that female mortality exhibits a sharp increase when a girl has one or more surviving older sisters (Das Gupta 1987). This may indicate that differential parental behaviors concentrate on a subgroup of girls.

Maternal education is believed to improve child health for both genders and often especially for girls by enhancing non-differential parental behaviors (Bhuiya and Streatfield 1991; Bourne and Walker 1991; Caldwell 1979; Chowdhury 1994; Cleland and Van Ginneken 1988; Quisumbing and Maluccio 2000). In addition to equipping women with better parenting skills, education may change a woman's belief structure and her parental perceptions of gender values (Chowdhury 1994). Educated women may enhance their status within households and have a greater control over household resources to protect their daughters (Caldwell et al. 1983; Jeffery and Basu 1996). Also education may limit a family size through increased costs associated with childrearing, greater access to family planning, or delayed marriage, resulting in increasing resources available to each child (Jeffery and Basu 1996). Daughters born to educated women therefore may receive more equal treatment in terms of the proximate determinants of child mortality including nutrition, hygienic practices, and health care. The presumption has been the basis for policies promoting pro-female education environments to address sex bias in child mortality.

However, previous studies provide mixed findings regarding the effect of maternal education on sex bias in child mortality or its proximate determinants. For instance, Simmons *et al* find that girls born to educated parents have a significantly better postneonatal survival probability than those born to uneducated parents in Utter Pradesh, India, while the relationship is negligible among boys (Simmons et al. 1982). On the other hand, Das Gupta finds that improvements in socioeconomic status including maternal education are associated with improved child survivorship in general but not with sex bias in child mortality in Punjub, India (Das Gupta 1987). Bhuiya and Streatfield find that maternal education is associated with higher child survival in general, but the relative benefit of maternal education is higher for boys than for girls in Matlab, Bangladesh (Bhuiya and Streatfield 1991).

The chief obstacle in interpreting the results from previous research on the link between maternal education and sex bias in child mortality is the potential influence from relevant but omitted variables at the community and individual levels causing endogeneity (or what is called confounding in health science research). When individuals alter parental behaviors in response to factors observed by them but not by researchers and when the factors are related to an individual's decision regarding schooling, the estimated association may not represent the true causal effect of maternal education. For example, educated women may be more likely to have grown up in less patriarchal communities or households, with implications not only for education but also for parental behaviors. The magnitude and direction of the potential bias depend on the unobserved relationship between the omitted variables and parental behaviors and schooling decisions. The unobserved relationship cannot be determined as a priori knowledge and further complicates the interpretation of the estimated association between maternal education and sex bias in child mortality.

This study aims to fill in the knowledge gap by estimating the causal effect of maternal education on sex bias in child survival between the first and fifth birthdays in Bangladesh, using the instrumental variable (IV) method to address specifically the sources of endogeneity. In particular, this study takes advantages of drastic changes in the education system introduced in non-municipal areas in the 1990s aimed at addressing persistently lower educational attainment among girls and a widening gap in the enrollment rate between girls and boys at the secondary education level (Ahmed and Sharmeen 2004; Ahmed et al. 2007; Arends-Kuenning and Amin 2000; Liang 1996; Raynor and Wesson 2006). The changes include abolishing tuition at the primary education level, providing food rations to poor households at the primary education level, providing financial assistance to female students at the secondary education levels, and constructing primary and secondary schools. At the primary and secondary education levels, the gap between boys and girls was eliminated. Between 1991 and 2000 the gross enrollment rates of both boys and girls at the primary education level increased to 97%, from 81% for boys and 70% for girls (Ahmed et al. 2007). Likewise at the secondary education level, the proportion of female students increased from 34% in 1990 to 52% in 2005, which suggests that more girls were enrolled than boys (BANBEIS 2006).

The changes in the education system provide a quasi-experimental setting that allows estimation of the causal effect of maternal education on sex bias in child survival between the first and fifth birthdays. Employing IVs constructed from the aforementioned education programs, this study links a woman's highest grade achieved and the survival status of her children in non-municipal areas in Bangladesh. The specific aims of the study are: (1) to estimate the effect of the education programs on maternal education and sex bias in child survival between the first and fifth birthdays; (2) to estimate the effect of maternal education on sex bias in child survival between the first and fifth birthdays; and (3) to compare the estimated coefficients of maternal education between the ordinary least squares (OLS) and two-stage least squares (2SLS) methods.

## 1. Empirical Framework

### *Data*

This study uses three data sets: (1) data on secondary schools, (2) data on population size, and (3) data on an individual's educational attainment and her children's survival status. First, the data on secondary schools come from the database managed by the Bangladesh Bureau of Educational Information and Statistics (BANBEIS). The database is based on a school census conducted in 2006 and includes each school's location and the year it opened. Second, the data on population size come from the 1981 Bangladesh population census (Bangladesh Bureau of Statistics 1983). The data provide the population size at the subdistrict level by age groups. We are interested in the population sizes of secondary school age groups, and the nearest found in the census data is the age group 10–17. Population sizes in other years are approximated under the assumption of an exponential growth at a rate $r$ of .026 (UNICEF 2010). Let $y_{ijt}$ denote the population size of age group 10–17 of woman $i$'s subdistrict of residence $j$ in year $t$. It is approximated as:

$$y_{ijt} = y_{ij,1981} \exp[0.026(t - 1981)].$$

Finally, the data on maternal education and the survival status of children come from the 2007 Bangladesh Demographic and Health Survey (BDHS 2007), which is one of the largest demographic surveys in Bangladesh. In the full survey, 10,400 households and 10,996 ever-married women ages 10-49 were interviewed between March 2007 and August 2007 (NIPORT et al. 2009). The survey collected various information, including background characteristics, educational attainment, and the survival status of children. For this study, 15,350 children who were born at least five years prior to the interview to ever-married women ages 20-44 and survived through their first year of life are analyzed with respect to their survival status between the first and fifth birthdays (Approximately, 8.9% of all children who were born at least five years prior to the interview to eligible women were dropped because they died before the first birth day).

The data on secondary schools and population size by year are aggregated at the subdistrict level to be matched with the data from BDHS 2007. The final dataset includes 230 subdistricits, approximately one half of the total subdistricts in Bangladesh. Sampling weights, based on BDHS2007, are assigned to women and their children throughout the analyses. To impute children's sampling weights, we divide their mother's sampling weight by the total number of eligible children born to her to correct for any over-representation of children by women's socioeconomic characteristics. For instance, it is possible that less-educated mothers have more children, which means that children born to less-educated women are over-represented in the children's sample. We address this potential problem by assigning the adjusted sampling weights to children. Descriptive statistics of the individuals and subdistricts are presented in Table 1. The proportion of girls survived between the first and fifth birthdays is 97.3%, which is slightly smaller than that of 98.3% among boys. The majority of women in the study sample do not have a high educational attainment, given that 64% of them have either no or incomplete primary education.

**Table 1.** Descriptive statistics

| Characteristics | Proportion |
|---|---|
| *Child Characteristics* | |
| Survived between 1st and 5th birthdays | |
| Girls (N=7,484) | 97.3 |
| Boys (N=7,866) | 98.3 |
| Total (N=15,350) | 97.8 |
| *Maternal Characteristics* (N=5,930) | |
| Age group | |
| 20-24 | 12.4 |
| 25-29 | 23.8 |
| 30-34 | 23.5 |
| 35-39 | 22.4 |
| 40-44 | 17.8 |
| Educational attainment | |
| No education | 40.1 |
| Incomplete primary | 23.9 |
| Complete primary | 8.4 |
| Incomplete secondary | 17.9 |
| Complete secondary or higher | 9.6 |
| *Sub-district Characteristics* (N=230) | |
| Women aged 5-29 attending school in 1981[a] | 17.0 |

[a] Data are from the 1981 population census

***Education Programs***

The persistently lower educational attainment among females than males until the early 1990s likely reflects constraints of demand and supply (Ahmed and Sharmeen 2004), namely affordability and accessibility. Parental perceptions of the returns on investments in daughters' education may be low in Bangladesh, where girls are expected to marry and subsequently belong to their husbands' households (Basu 1989; Das Gupta et al. 2003; Mason 1987). The dowry system, moreover, adds to the direct cost of raising daughters (Amin and Cain 1997) and may leave no financial resources for their schooling. Widespread poverty and limited job opportunities suitable for educated women also discourage parents from investing in a daughter's education (Liang 1996). In addition, adolescent girls in traditional Bangladeshi society are often allowed limited mobility, as their parents want to control their premarital sexual exposure because virginity is a critical condition of marriage (Ahmed and Sharmeen 2004; Begum 2003; Field and Ambrus 2008). Even though female education is considered a desirable attribute in the marriage market, the perceived risk of daughters' exposure to boys and men while traveling to school may outweigh the perceived benefits of education, resulting in parents withdrawing their daughters from school upon puberty.

During the 1990s the Bangladesh government launched a number of education programs to reduce the constraints to education on both the demand and the supply sides, especially in nonmunicipal areas at the primary and secondary education levels (Ahmed and Sharmeen 2004; Ahmed et al. 2007; Arends-Kuenning and Amin 2000; Raynor and Wesson 2006). In 1990 primary education (grades 1–5) became compulsory and free nationwide (Hossain 2004). In 1993 the government introduced the pilot program Food for Education (FFE) that provided food rations to poor households sending their children to primary school (Meng and Ryan 2007; Ryan and Meng 2004). In 1994, based on positive responses observed in the primary school enrollment rate, FFE expanded to all 460 nonmunicipal subdistricts in two stages: at the geographic and the individual levels (Ryan and Meng 2004). At the geographic level, two to three underdeveloped counties were selected in each of the nonmunicipal subdistricts based on their economic development and literacy rates. The program covered all of the registered primary schools and one religious school within each selected county (Ryan and Meng 2004). At the individual level, households sending their children to eligible primary schools were selected within each program county based on a set of four criteria.[2] Households meeting at least one of the criteria were entitled to food rations of 15 to 20 kilograms (kg) of wheat or 12 to 16 kg of rice per month (depending on the number of children attending primary school) on the condition that the children maintain an attendance rate of 85%. The estimated average monetary value of the food rations a household received was 120 taka (US$1.70)[3] per month (Ravallion and Wodon 2000). Nearly 27% of primary schools and 13% of pupils in the country were under FFE by 2000.

In 1999 FFE was supplemented by the Primary Education Stipend Project (PES Project), which was cash based and provided 25 taka (US$0.40) per month to eligible households in all rural non-FFE areas (Hossain 2004; Tietjen 2003). In 2002 both FFE and the PES Project were replaced by the Primary Education Stipend Program (PESP), which provided 100 to 125 taka (US$1.40–1.80) per month (depending on the number of children attending primary school) to qualifying households in all counties in nonmunicipal subdistricts (Ahmed and Sharmeen 2004). In 2003 the estimated average annual direct costs (fees and other payments) and indirect costs (textbooks, uniforms, private tutoring, and transportation) of primary education were 64 taka (US$0.90) and 892 taka (US$12.90), respectively. This suggests that the PES Project covered the direct costs and that FFE and PESP provided more than the total costs (Ahmed and Sharmeen 2004).

At the secondary level in 1994 girls in all nonmunicipal subdistricts were given free tuition and a stipend (Liang 1996). Under the program, female students were required to meet three conditions[4] (Asian Development Bank 1993; Asian Development Bank 1999; Asian Development Bank 2002; Asian Development Bank 2008; Uniconsult International Limited 2006; World Bank 2002a; World Bank 2002b; World Bank 2008). The yearly stipend increased

[2] The four criteria are (1) the household owns less than half an acre of land, (2) the household head is a day laborer, (3) the household head is female, or (4) the household has limited income.
[3] One U.S. dollar equaled 69.10 Bangladeshi taka on March 1, 2010.
[4] The conditions are (1) maintaining an attendance rate of 85% or higher, (2) passing the annual final exams with a score of 45% or higher, and (3) staying unmarried until the secondary school certificate examination (SSC) or age 18.

as girls proceeded to higher grades: 300 taka (US$4.30) for 6th graders, 360 taka (US$5.20) for 7th graders, 420 taka (US$6.10) for 8th graders, and 720 taka (US$10.40) for 9th and 10th graders. Also 9th graders were provided a book allowance of 250 taka (US$3.60), and 10th graders were provided examination fees of 730 taka (US$10.50). In 2003 the estimated average annual direct costs (fees and other payments) and indirect costs (textbooks, uniforms, private tutoring, and transportation) of girls' secondary education were 346 taka (US$5.00) and 3,191 taka (US$46.00), respectively. This suggests that the stipend covered the direct costs but not the full indirect costs (Ahmed and Sharmeen 2004).

At the same time, both external donors and nongovernmental organizations supported school construction throughout the country (BANBEIS 2006). As a result, between 1990 and 2000 the number of secondary schools increased by approximately 47%, from 10,448 to 15,403 (BANBEIS 2010). While the external donors and nongovernmental organizations did not establish priorities for school locations, the empirical evidence suggests that schools may have been constructed regressively with respect to the education levels of the subdistricts. The multiple regression model suggests that subdistricts with a lower female attendance rate (obtained from the 1981 population census) were allocated more secondary schools between 1990 and 1999 after controlling for the population size of ages 10–17 in 1981 (Table 2). While this evidence is crude, it suggests that the secondary school construction targeted the areas with greater needs.

**Table 2.** Regression of the number of secondary schools

| | Coefficients | | (SE) |
|---|---|---|---|
| Female attendance rate[a] | -0.28 | ** | (.099) |
| Population aged 10-17 (10,000)[a] | 1.04 | ** | (.369) |
| Constant | 13.76 | *** | (2.250) |
| Adjusted $R^2$ | 0.05 | | |
| F-statistics | 7.39 | | |
| N | 230 | | |

Note: Standard errors are presented in parentheses.
[a] Data are obtained from the 1981 population census
** p<.01; *** p<.001

### *Identification Strategy*

This study estimates the causal effect of maternal education on sex bias in child survival between the first and fifth birthdays of children born to ever-married women, who were born between 1963 and 1987 (i.e., ages 20 to 44 at the interview) and residing in non-municipal areas in Bangladesh. The survivorship is conditional on children's survival up to their first birth day. The effect of maternal education is assessed within a framework relating background factors, proximate determinants of child survival, and child survival. As one of the background factors, education is presumed to influence child survival through its effects on the proximate determinants.

To determine covariates to be included in the analysis, we refer to the framework suggested by Mosley and Chen. They propose a set of proximate determinants of child survival including: 1) "maternal factors", including maternal age, parity, and birth intervals, 2) "environmental contamination" including exposure to disease vectors through contaminated food, water or air, 3) "nutrient deficiency", 4) "injury" , and 5) "personal illness control" including both preventive and curative measures (Mosley and Chen 1984). In addition, in the context of sex bias in child survival specifically, Das Gupta suggests that the number of older siblings by gender influences survivorship when son preference is present, in which female mortality exhibits a sharp increase when a girl has one or more surviving older sisters (Das Gupta 1987).

In this study, we are interested in proximate determinants that are under the influence of parental behaviors after infancy, including "nutrient deficiency", "injury" and "personal illness control." The other proximate determinants, including maternal age, parity, birth intervals, and the number of older siblings by gender are included in the analysis as control variables. Environmental contamination is assumed to be independent of gender of child.

Maternal education is presumed to be endogenous in the context of its causal effect on the proximate determinants of child survival due to omitted variables at the individual and community levels. To control for omitted variables at the individual level, this study employs IVs constructed from the education programs described in the preceding section. The programs were introduced at different times in different nonmunicipal areas, which suggests that variations in individual women's exposures to the programs were determined by both the accessibility of a secondary school and an individual's year of birth.

The first IV is intended to capture the rapid expansion in the accessibility of secondary education. Bangladeshi children normally attend primary school between the ages of 6 and 10 and enroll in secondary school at age 11. Therefore the first IV is the number of secondary schools in the subdistrict when an individual reaches age 11, standardized per 10,000 population of ages 10–17 (hereafter referred to as the number of schools). For instance, if woman $i$ in subdistrict $j$ was born in 1985, the measure is:

$$\frac{P_{j,1996} \times 10{,}000}{y_{ij,1981} \times \exp[0.026(1996-1981)]}$$

where $P_{j,1996}$ is the number of secondary schools in the subdistrict in 1996 (when the woman was 11).

The second IV is intended to capture the increasing exposure to the financial incentives among younger cohorts. Given that the programs were introduced in the 1990s, women born around the early 1980s, specifically between 1979 and 1982, are more likely to have been partially exposed to free and compulsory primary education and stipend assistance at the secondary level. Women born in or after the mid-1980s, specifically after 1983, are more likely to have been fully exposed to free and compulsory primary education and stipend assistance at the secondary level and partially exposed to FFE. On the other hand, women born before the 1980s, specifically before 1979, are less likely to have benefited from any of the programs, because they reached age 16 or older (i.e., at least 1 year older than the expected age at grade 10) before any of the programs were introduced. Therefore a woman's year of birth is the second IV.

To control for omitted variables at the community level specifically, a set of dummy variables of subdistricts is introduced in each model. The subdistricts where women received their educations, however, may be measured with errors in this study, as information on natal or childhood/adolescence subdistricts was not collected by the BDHS 2007; the data were matched based on where women were located at the time of the interview. The only relevant measure in assessing the potential measurement error is the duration in years lived in the current place, while the geographic boundary of "current place" was not defined to the interviewees. Specifically, 83.7% of women in the study sample have ever migrated, and about 50% of women who have ever migrated did so at age 16 or later, which is approximately the average age at first marriage (14.8) in the study sample. This may reflect migration upon marriage, because women in Bangladesh often move to their husbands' households upon marriage (Agarwal 1994).

The measurement error, if any, could be problematic in two ways, depending on the structure of the measurement error. The first potential problem is underestimation of the relation between educational attainment and the education programs, which results from a random measurement error. The underestimation in turn could invoke the weak instrument problem. We assessed the significance of the set of IVs; the results are presented in Table 6.

The second potential problem is the endogeneity of the education programs in estimating their effects on maternal education and child survival, which result from a systematic measurement error (Duflo 2001). A systematic measurement error could arise from selective migration (Strauss and Thomas 1995), when the migration decision is a function of education and the destination of migration is based on factors related to the education programs and child survival (Cochrane 1979). For instance, women with a higher educational attainment may be more likely to migrate to communities with a better set of characteristics, such as more schools and health facilities, and child survival may be affected by access to health facilities, which may be correlated with the number of schools.

To address this potential problem, we employed the subdistrict as the unit of observation for the number of schools, based on previous studies reporting that the majority of marriages take place within the natal subdistricts in Bangladesh (Aziz 1979; Islam 1974; Kabeer 1985). In this situation, the number of schools in the resident subdistrict reflects that of the natal subdistrict. All the women in the study sample were born before any of the programs were introduced, which implies that the number of schools in the natal subdistrict is not endogenous (Duflo 2001). Also the set of dummies of subdistricts introduced in each model rids it of any time-invariant effect of unobserved factors at the subdistrict level. We performed the test of over-identifying restriction for each of the models; results are presented in Table 7. Overall, we are assured that the set of IVs is valid in terms of both its strength of correlation with maternal education and its collective exogeneity. This suggests that the measurement error in subdistricts, if any, does not pose a significant problem in this study.

The identification assumption is supported by preliminary evidence. Columns 1–4 of Table 3 show the average highest grade achieved stratified by quintiles of the number of schools and a woman's year of birth. They suggest that there is a substantial increase in the average highest grade for women born in the 1980s, as hypothesized. Likewise, the average highest grade achieved is higher where there are more secondary schools, which again supports the hypothesis that women in subdistricts with more schools have a higher educational attainment on average.

Columns 5-8 of Table 3 presents the proportion of children survived between the first and fifth birthdays, conditional on their survival until the first birth day, stratified by quintiles of the number of schools and a woman's year of birth, respectively. Again, the higher quintiles of the number of schools are associated with higher survivorship. As hypothesized, children born to women born after 1982 have the highest survivorship, followed by those born to women born between 1979 and 1982, and then by those born to women born before 1979.

In the next section, based on the supportive preliminary evidence, we apply a regression framework to estimate the causal effects of maternal education and the education programs on sex bias in child survival between the first and fifth birthdays. Similar methods are used by Duflo (2001), and Breierova and Duflo (2004).

**Table 3.** Maternal educational attainment and child survival between the first and fifth birthdays by a woman's year of birth and the number of schools

| Quintile of number of schools | Highest grade achieved | | | | Proportion of children survived between first and fifth birthdays | | | |
|---|---|---|---|---|---|---|---|---|
| | Year of birth | | | | Year of birth | | | |
| | Before 1979 | 1979-1982 | After 1982 | Total | Before 1979 | 1979-1982 | After 1982 | Total |
| Lowest | 2.84 | 3.10 | 4.60 | 3.42 | 96.39 | 99.83 | 100.00 | 96.49 |
| | (.201) | (.507) | (1.277) | (.205) | (.50) | (.179) | (.0) | (.487) |
| Low | 3.23 | 4.14 | 5.63 | 3.95 | 95.94 | 98.01 | 100.00 | 95.99 |
| | (.195) | (.560) | (.557) | (.224) | (.521) | (1.368) | (.0) | (.511) |
| Middle | 3.01 | 4.18 | 5.51 | 4.19 | 97.21 | 99.34 | 100.00 | 97.27 |
| | (.179) | (.405) | (.727) | (.225) | (.382) | (.648) | (.0) | (.374) |
| High | 3.80 | 4.28 | 4.67 | 4.70 | 97.47 | 97.47 | 100.00 | 97.49 |
| | (.248) | (.349) | (.664) | (.253) | (.396) | (1.119) | (.0) | (.372) |
| Highest | 4.03 | 4.59 | 5.12 | 5.41 | 97.49 | 98.09 | 100.00 | 97.62 |
| | (.242) | (.290) | (.380) | (.224) | (.361) | (.786) | (.0) | (.335) |
| Total | 3.31 | 4.28 | 5.05 | 3.46 | 96.86 | 98.20 | 100.00 | 96.96 |
| | (.107) | (.178) | (.291) | (.10) | (.198) | (.496) | (.0) | (.188) |

Note: Standard errors are presented in parentheses.

## 2. Reduced Form Results: Effect of Education Programs

***Effect of Education Programs on Maternal Education***

Two variables, the number of schools and a woman's year of birth, are used as measures of school accessibility and exposure to financial incentives to estimate the effect of the education programs on maternal educational attainment. To include the number of schools as a measure of the effect of school accessibility, we assume that the difference in educational attainment across the number of schools is due to different levels of school accessibility within subdistricts. The assumption is violated if there is any unobserved time-varying variable correlated with the number of schools specifically at the subdistrict level. This suggests running a model that includes interaction terms between subdistrict and birth cohort dummies. Due to the limited sample size, however, we are unable to fit the full set of interaction dummies. Instead, we use an available indicator of socioeconomic development at the subdistrict level, namely, the female attendance rate of ages 5-29, obtained from the 1981 population census, and interact that rate with birth cohort dummies. We assess the significance of the interaction terms in the following model to test the hypothesis:

(1) $$E_{ijt} = \alpha + \boldsymbol{\beta}'\mathbf{C} + \delta P_{jt'} + \varphi'\mathbf{u} + \boldsymbol{\gamma}'\mathbf{I} + \varepsilon_{ijt},$$

where $E_{ijt}$ is the highest grade achieved by woman $i$ in sub-district $j$ born in year $t$, $\mathbf{C}$ is a vector of dummies of woman's year of birth, $P_{jt'}$ is the number of schools in subdistrict $j$ in year $t+11$, $\mathbf{u}$ is a vector of dummies of subdistrict, $\mathbf{I}$ is a vector of interactions between the female attendance rate of subdistrict $j$ in 1981 and birth cohort dummies, and $\varepsilon_{ijt}$ is the disturbance term. Specifically, we are interested in the collective significance of $\boldsymbol{\gamma}$, the coefficients of the interaction terms.

The results are presented in column 1 of Table 4. None of the interaction terms is significant at the 5% level. While the model captures only limited characteristics at the subdistrict level, it is reassuring that there is no time-varying effect of a major socioeconomic development indicator, which is most likely to be correlated with maternal educational attainment.

Next, to include a woman's year of birth as a measure of the effect of the financial incentives, we assume that differences in educational attainment across cohorts are due to different levels of exposure to the financial incentives provided by the education programs. The assumption is violated if there is any systematic difference across cohorts that affects an individual's schooling decision. We examine the extent to which this assumption is supported by assessing educational attainment by birth cohorts. Because women who had reached age 16 or older in 1994 had left grade 10 before any of the programs were introduced, they are least likely to have benefited from any of the financial incentives. If maternal educational attainment differs significantly within this group of women, it may imply that there is a significant cohort effect besides exposure to the financial incentives, in which case the estimated effect of the financial incentives may be biased. This suggests running the following model:

(2) $$E_{ijt} = \alpha + \boldsymbol{\beta}'\mathbf{C} + \delta P_{jt'} + \varphi'\mathbf{u} + \varepsilon_{ijt}.$$

We are interested in $\boldsymbol{\beta}$, the coefficients of the vector of dummies of woman's year of birth, especially for women 16 or older in 1994, that is, for $t \leq 1978$.

Column 2 of Table 4 presents the results. The coefficients of birth cohorts are insignificant for women 18 or older in 1994, as hypothesized. However, the coefficients of birth cohorts for women 16 or 17 in 1994 are significant, which contradicts our assumption. This may reflect exposure to the financial incentives due to grade repetition or delayed entry into school. Indeed, the reported repetition rate among girls was 9.6% for grades 1-5 and ranged from 6.5% to 18.0% for grades 6-10. In addition, about 9.4% of girls in the first grade were 7 or older in 2004 (Ahmed et al. 2007), which is substantially older than the expected age of 6 in the first grade. Although corresponding figures for the 1980s are not available, it could be argued that girls older than expected were exposed to the financial incentives due to grade repetition or delayed entry. On the other hand, coefficients of birth cohorts for women 15 or younger in 1994 are significantly positive, as expected. Overall, the results support our assumptions that there is no substantial difference in educational attainment across birth cohorts among women who are least likely to have been exposed to the financial incentives and that educational attainment gradually increases for younger women who are likely to have been exposed to the financial incentives.

The results obtained from models (1) and (2) indicate that both the number of schools and a woman's year of birth are unlikely to be confounded by omitted variables. This suggests running the following model in estimating the effect of the education programs on a woman's highest grade achieved:

(3) $$E_{ijt} = \alpha + \beta_1 C_{1t} + \beta_2 C_{2t} + \delta P_{jt'} + \varphi' \mathbf{u} + \varepsilon_{ijt},$$

where $C_{1t}$ and $C_{2t}$ are dummies of woman's year of birth. The dummies indicate two cohort groups, women born between 1979 and 1982, and those born after 1982, respectively, so that they capture the effects of partial and full exposures relative to no exposure to the financial incentives. While some of the older women may have benefited from the financial incentives due to grade repetition or delayed entry into school, as shown in the previous analysis, they may differ in unobserved characteristics from women of the same birth cohorts who completed education at the expected age. Therefore we categorize birth cohorts by expected exposure to the financial incentives without any grade repetition or delayed entry into school.

The results presented in Table 4 suggest that a one-school increase per 10,000 population of ages 10-17 significantly increases the highest grade achieved by .236 years. Likewise, partial and full exposures to the financial incentives significantly increase the highest grade achieved by 1.021 and 1.807 years, respectively.

**Table 4.** Reduced form estimates of the effect of the education programs on maternal education

| Variable | Model (1) | | | Model (2) | | | Model (3) | | |
|---|---|---|---|---|---|---|---|---|---|
| | Coefficients | | (SE) | Coefficients | | (SE) | Coefficients | | (SE) |
| Number of schools | 0.114 | * | (.055) | 0.060 | | (.053) | 0.236 | *** | (.053) |
| Maternal age in 1994 | | | | | | | | | |
| 12 to 15 | - | | | - | | | 1.021 | *** | (.169) |
| 11 or younger | - | | | - | | | 1.807 | *** | (.358) |
| 7 | 3.667 | * | (1.786) | 1.810 | *** | (.443) | - | | |
| 8 | 3.122 | | (1.703) | 1.364 | ** | (.402) | - | | |
| 9 | 3.629 | ** | (1.368) | 1.192 | ** | (.372) | - | | |
| 10 | 3.060 | * | (1.181) | 1.175 | *** | (.337) | - | | |
| 11 | 3.164 | ** | (1.097) | 1.429 | *** | (.322) | - | | |
| 12 | 0.838 | | (1.001) | 1.117 | *** | (.317) | - | | |
| 13 | 0.676 | | (1.090) | 1.180 | *** | (.319) | - | | |
| 14 | 2.786 | * | (1.127) | 1.235 | *** | (.345) | - | | |
| 15 | 1.602 | | (1.036) | 1.449 | *** | (.330) | - | | |
| 16 | 2.136 | | (1.095) | 1.117 | ** | (.337) | - | | |
| 17 | 1.574 | | (1.113) | 0.683 | * | (.324) | - | | |
| 18 | 1.329 | | (.963) | 0.440 | | (.328) | - | | |
| 19 | 1.996 | | (1.05) | 0.531 | | (.332) | - | | |
| 20 | 2.158 | | (1.218) | 0.514 | | (.328) | - | | |
| 21 | -0.094 | | (1.057) | 0.304 | | (.344) | - | | |
| 22 | 1.516 | | (1.142) | 0.452 | | (.330) | - | | |
| 23 | 1.011 | | (1.063) | Reference | | | - | | |
| 24 | 2.220 | | (.994) | -0.064 | | (.340) | - | | |
| 25 | 0.844 | | (1.331) | 0.351 | | (.347) | - | | |
| 26 | -0.058 | | (1.041) | -0.232 | | (.335) | - | | |
| 27 | 0.245 | | (1.048) | -0.517 | | (.33) | - | | |
| 28 | 0.257 | | (1.011) | -0.521 | | (.322) | - | | |
| 29 | 1.868 | | (1.083) | -0.505 | | (.323) | - | | |
| 30 | -0.005 | | (1.033) | -0.407 | | (.329) | - | | |
| 31 | Reference | | | -0.470 | | (.336) | - | | |
| Constant | -0.117 | | (1.434) | 1.081 | | (1.345) | -0.483 | | (1.341) |
| Year of birth*female attendance rate | Yes | | | No | | | No | | |
| F-statistics[a] | 1.39 | | | 4.16 | | | 9.73 | | |
| Adjusted $R^2$ | 0.200 | | | 0.192 | | | 0.160 | | |
| N | 5,839 | | | 5,893 | | | 5,893 | | |

Note: Subdistricts of residence are controlled for in all the models.
[a] F-tests assess the collective significance of the interaction terms, the year of birth (1978 or earlier), and all the independent variables for models (1), (2), and (3), respectively.
* p<.05; ** p<.01; *** p<.001

***Effect of Education Programs on Sex Bias in Child Survival***

The effect of the education programs on sex bias in child survival between the first and fifth birthdays (conditional on their survival to the first birthday) can be assessed in the same manner with some modifications, that is inclusion of interactions of variables of interests and gender of child (i.e., girl) in each model. Because we are interested in sex bias in child survival, the potential omitted variable problems need to be examined by gender of child. By introducing the interaction terms, any differential effect of the variables of interests by gender of child can be assessed.

First, to assess omitted time-varying variables at the subdistrict level, we again examine the coefficients of the interactions between birth cohorts and the female attendance rate, and the interactions among birth cohorts, the female attendance rate, and gender of child (i.e., girl) in the following model:

$$(4) \qquad Y_{hijt} = \alpha + \boldsymbol{\beta}'\mathbf{C} + \delta P_{jt'} + \varphi'\mathbf{u} + \boldsymbol{\gamma}'\mathbf{I} + \boldsymbol{\eta}'\mathbf{X} + \boldsymbol{\lambda}'\mathbf{S}_1 + \mathbf{v}'\mathbf{S}_2 + \varepsilon_{hijt},$$

where $Y_{hijt}$ is a binary variable and indicates whether child $h$ born to woman $i$ in subdistrict of residence $j$ survived between the first and fifth birthdays, conditional on that (s)he survived through the first birth day. A linear probability model is applied to the equation. $\mathbf{X}$ is a vector of maternal factors, including dummies of maternal age at birth (indicating age 18 or younger or age 35 or older), dummies of parity (indicating first child or fifth or higher order child), preceding birth interval, a dummy of gender of child (indicating girl), the number of older male and female siblings at birth. $\mathbf{X}$ also includes the interaction terms of the number of older male and female siblings at birth with gender of child to capture any incremental effects of the number of siblings by gender of child. $\mathbf{S}_1$ is a vector of interaction terms between cohorts and gender of child. $\mathbf{S}_2$ is a vector of interaction terms between $\mathbf{I}$ (i.e., a vector of interactions between the female attendance rate of subdistrict $j$ in 1981 and birth cohort dummies) and gender of child.

We are interested in the significance of $\boldsymbol{\gamma}$ and $\mathbf{v}$. The results are presented in column 1 of Table 5. Overall, the interaction terms are jointly insignificant. Again, it is reassuring that there is no time- or gender-varying effect of a major socioeconomic development indicator, which is likely to be correlated with child survival.

Next, to assess if there is a cohort effect and if it differs by gender of child among women who are not exposed or who are least exposed to the financial incentives, we examine the coefficients of cohort dummies in the following model:

$$(5) \qquad Y_{hijt} = \alpha + \boldsymbol{\beta}'\mathbf{C} + \delta P_{jt'} + \varphi'\mathbf{u} + \boldsymbol{\eta}'\mathbf{X} + \boldsymbol{\lambda}'\mathbf{S}_1 + \varepsilon_{hijt}.$$

Again, we are interested in $\boldsymbol{\beta}$, the coefficients of the vector of dummies of woman’s year of birth, and $\boldsymbol{\lambda}$, the coefficients of the interactions between cohorts and gender of child, especially for women 16 or older in 1994, that is, for $t \leq 1978$ .The results are presented in column 2 of Table 5. The cohort fixed effects for women 16 or older or their interactions with gender of child are insignificant.

The results again indicate that both the number of schools and a woman's year of birth are unlikely to be confounded by omitted variables. This suggests running the following model to estimate the effect of the education programs on sex bias in child survival:

$$(6) \qquad Y_{hijt} = \alpha + \beta_1 C_{1t} + \beta_2 C_{2t} + \delta P_{jt'} + \varphi'\mathbf{u} + \boldsymbol{\eta}'\mathbf{X} + \boldsymbol{\pi}'\mathbf{G} + \varepsilon_{hijt},$$

where $\mathbf{G}$ is a vector of interactions between the education program variables (i.e., $C_{1t}$, $C_{2t}$, and $P_{jt'}$) and gender of child. The results are presented in column 3 of Table 5. It is suggested that a one-school increase per 10,000 population of ages 10-17 significantly increases the survival probability between the first and fifth birthdays by .002. Likewise, partial and full exposures to the financial incentives, as captured by woman's year of birth between 1979 and 1982 and that after 1982, respectively, significantly increase the survival probability by .015 and .017. None of the interactions between the education program variables and gender of child is significant, suggesting that there is no incremental effect of the education programs by gender of child.

**Table 5.** Reduced form estimates of the effect of the education programs on sex bias in child survival

| | Model (1) | | | Model (2) | | | Model (3) | | |
|---|---|---|---|---|---|---|---|---|---|
| | Coefficients | | (SE) | Coefficients | | (SE) | Coefficients | | (SE) |
| Number of schools | 0.000 | | (.001) | 0.000 | | (.001) | 0.002 | * | (.001) |
| *Maternal age in 1994* | | | | | | | | | |
| 12 to 15 | - | | | - | | | 0.015 | ** | (.004) |
| 11 or younger | - | | | - | | | 0.017 | ** | (.006) |
| 7 | 0.028 | | (.023) | 0.026 | * | (.011) | - | | |
| 8 | 0.046 | | (.035) | 0.034 | ** | (.013) | - | | |
| 9 | 0.056 | | (.021) | 0.035 | ** | (.011) | - | | |
| 10 | 0.075 | * | (.031) | 0.031 | ** | (.012) | - | | |
| 11 | 0.025 | | (.025) | 0.021 | | (.013) | - | | |
| 12 | 0.026 | | (.023) | 0.022 | | (.012) | - | | |
| 13 | 0.027 | | (.033) | 0.013 | | (.014) | - | | |
| 14 | 0.024 | | (.023) | 0.017 | | (.011) | - | | |
| 15 | 0.037 | | (.020) | 0.023 | | (.010) | - | | |
| 16 | 0.022 | | (.029) | 0.014 | | (.013) | - | | |
| 17 | -0.001 | | (.028) | 0.010 | | (.012) | - | | |
| 18 | 0.030 | | (.022) | 0.016 | | (.012) | - | | |
| 19 | 0.032 | | (.020) | 0.017 | | (.011) | - | | |
| 20 | 0.027 | | (.022) | 0.007 | | (.013) | - | | |
| 21 | 0.002 | | (.027) | 0.000 | | (.013) | - | | |
| 22 | 0.033 | | (.028) | 0.006 | | (.013) | - | | |
| 23 | 0.024 | | (.021) | 0.018 | | (.011) | - | | |
| 24 | 0.018 | | (.031) | Reference | | | - | | |
| 25 | 0.027 | | (.026) | 0.011 | | (.013) | - | | |
| 26 | 0.044 | | (.030) | 0.005 | | (.013) | - | | |
| 27 | 0.036 | | (.021) | 0.019 | | (.011) | - | | |
| 28 | -0.025 | | (.031) | -0.018 | | (.015) | - | | |
| 29 | -0.011 | | (.025) | -0.007 | | (.014) | - | | |
| 30 | -0.010 | | (.027) | -0.008 | | (.013) | - | | |
| 31 | Reference | | | 0.000 | | (.013) | - | | |

**Table 5 (cont'd).** Reduced form estimates of the effect of the education programs on sex bias in child survival

| | Model (1) | | | Model (2) | | | Model (3) | | |
|---|---|---|---|---|---|---|---|---|---|
| | Coefficients | | (SE) | Coefficients | | (SE) | Coefficients | | (SE) |
| *Interaction with gender of child* | | | | | | | | | |
| Maternal age: 12 to 15 | - | | | - | | | -0.001 | | (.008) |
| Maternal age: 11 or younger | - | | | - | | | 0.015 | | (.008) |
| Number of schools | -0.001 | | (.001) | -0.001 | | (.001) | -0.002 | | (.001) |
| Constant | 0.977 | *** | (.017) | 0.981 | *** | (.016) | 0.973 | *** | (.012) |
| Attendance rate*cohort | Yes | | | No | | | No | | |
| F-statistics[a] | 0.78 | | | - | | | - | | |
| Attendance rate*cohort*girl | Yes | | | No | | | No | | |
| F-statistics[a] | 0.99 | | | - | | | - | | |
| Cohort*girl | Yes | | | Yes | | | No | | |
| F-statistics[a] | 0.94 | | | 1.62 | | | - | | |
| Adjusted $R^2$ | 0.037 | | | 0.034 | | | 0.027 | | |
| N | 15,350 | | | 15,350 | | | 15,350 | | |

Note: Subdistricts of residence are controlled for in all the models.
[a] F-tests assess the collective significance of the interaction terms.
* p<.05; ** p<.01; *** p<.001

## 3. Instrumental Variable Method Results: Effect of Maternal Education

***Effect of Maternal Education on Sex Bias in Child Survival***

We employ the 2SLS method to address the potential endogeneity of maternal education and to estimate the causal effect of maternal education on sex bias in child survival between the first and fifth birthdays, which calls for inclusion of an interaction term of maternal education and gender of child, and the control variables. The first-stage equation estimating maternal education and its interaction with gender of child therefore is specified as:

$$E_{lhijt} = \alpha + \beta_1 C_{1t} + \beta_2 C_{2t} + \delta P_{jt'} + \varphi' \mathbf{u} + \boldsymbol{\eta}' \mathbf{X} + \boldsymbol{\pi}' \mathbf{G} + \varepsilon_{hijt}, \quad l = 1,2, \tag{7}$$

where $E_{1hijt} = E_{ijt}$ and $E_{2hijt} = E_{ijt} S_{hijt}$. We are interested in $\beta_1$, $\beta_2$, $\delta$, and $\boldsymbol{\pi}$, the coefficients of the IVs and their interactions with gender of child. The results of model (7), presented in Table 6, suggest that both maternal education and its interaction with gender of child are significantly correlated with the IVs after controlling for the covariates in the model.

**Table 6.** First-stage equation results

| | Education | | Education*gender of child | |
|---|---|---|---|---|
| | Coefficients | (SE) | Coefficients | (SE) |
| Number of schools | 0.205 *** | (.044) | 0.104 *** | (.028) |
| *Maternal age in 1994* | | | | |
| 12 to 15 | 1.154 *** | (.215) | 0.256 *** | (.065) |
| 11 or younger | 1.860 *** | (.495) | 0.210 | (.137) |
| *Interaction with gender of child* | | | | |
| Maternal age: 12 to 15 | -0.198 | (.297) | 0.385 | (.217) |
| Maternal age: 11 or younger | -0.121 | (.66) | 1.078 * | (.455) |
| Number of schools | -0.045 | (.033) | 0.001 | (.025) |
| Constant | 0.192 | (.791) | -1.515 * | (.645) |
| F-statistics | 21.78 | | 16.43 | |
| Adjusted $R^2$ | 0.188 | | 0.354 | |
| N | 15,350 | | 15,350 | |

Note: Subdistricts of residence and other covariates are controlled for.
* p<.05; ** p<.01; *** p<.001

Then, the second-stage equation of the 2SLS model is specified to estimate the effect of maternal education on sex bias in child survival between the first and fifth birthdays as:

(8) $$Y_{hijt} = \kappa + \lambda \hat{E}_{ijt} + \varpi' \mathbf{u} + \boldsymbol{\theta}' \mathbf{X} + \rho(\hat{E}_{ijt} S_{hijt}) + \varepsilon_{hijt}.$$

We are interested in $\lambda$ , the coefficient of estimated highest grade achieved, and $\rho$ , the coefficient of interaction between estimated highest grade achieved and gender of child. Again, a linear probability model is applied to the equation.

The results are presented in column 1 of Table 7. The test of over-identifying restriction does not reject the collective orthogonality of the instrumental variables (p=.502). It is suggested that a one-year increase in the highest grade achieved significantly increases the survival probability by .012. However, the interaction term between maternal education and gender of child is not significant, suggesting that there is no differential effect of maternal education by gender of child.

The estimates are consistent with the reduced form results. Note that increasing the number of schools by one increases the highest grade achieved by .24 years. Then the direct effect of the number of schools on the survival probability should be .003 (=.012*.24). Also partial and full exposures to the financial incentives are estimated to increase the highest grade achieved by 1.02 and 1.81 years, respectively. Then the direct effect of partial and full exposure on the survival probability should be .012 (=.012*1.02) and .022 (=.012*1.81), respectively. These estimates are approximately equal to the results shown in Table 5.

***Difference between 2SLS and OLS Estimates***

Finally, the estimated coefficients of maternal education are compared between 2SLS and OLS, the latter replacing $\hat{E}_{ijt}$ with $E_{ijt}$ in model (7). The OLS estimates are presented in column 2 of Table 7. The Durbin-Wu-Hausman test suggests that the 2SLS estimates are significantly different from the OLS estimates. The 2SLS estimate for the effect of maternal education is larger than the corresponding OLS estimate. However, the estimated incremental effect of maternal education for girls is equivalent between the two methods.

**Table 7.** 2SLS and OLS estimates of the effect of maternal education on sex bias in child survival

| | 2SLS | | | OLS | | |
|---|---|---|---|---|---|---|
| | Coefficients | | (SE) | Coefficients | | (SE) |
| Education | 0.012 | * | (.005) | 0.002 | *** | (.0004) |
| Interaction (Education * gender of child) | 0.001 | | (.011) | 0.001 | | (.001) |
| *Maternal age at birth* | | | | | | |
| 18 or younger | 0.007 | | (.004) | -0.004 | | (.003) |
| 35 or older | 0.017 | | (.010) | 0.016 | | (.008) |
| *Birth order* | | | | | | |
| First | -0.002 | | (.007) | 0.014 | ** | (.005) |
| Fifth or higher | 0.004 | | (.008) | -0.001 | | (.008) |
| Preceding birth interval | 0.000 | *** | (.0001) | 0.000 | *** | (.0001) |
| Gender of child | -0.009 | | (.046) | -0.010 | * | (.005) |
| *Number of siblings* | | | | | | |
| Older male siblings | 0.001 | | (.004) | -0.002 | | (.003) |
| Older female siblings | 0.001 | | (.004) | -0.001 | | (.003) |
| *Interaction with gender of child* | | | | | | |
| Number of older male siblings | 0.000 | | (.010) | -0.001 | | (.004) |
| Number of older female siblings | -0.002 | | (.008) | -0.002 | | (.003) |
| Constant | 0.969 | *** | (.018) | 0.993 | *** | (.005) |
| *Test of over-identifying restriction* | | | | | | |
| Chi-square | 3.343 | | | | | |
| p-value | 0.502 | | | | | |
| *Durbin-Wu-Hausman test* | | | | | | |
| Chi-square | 23.523 | | | | | |
| p-value | 0.000 | | | | | |

Note: Subdistricts of residence are controlled for in all the models.
* p<.05; ** p<.01; *** p<.001

## 4. Conclusion

In this paper we examined the causal effect of maternal education on sex bias in child survival between the first and fifth birthdays in rural Bangladesh, where girls have been in a disadvantageous situation for their survival. We assumed that maternal education is endogenous due to unobserved variables at the individual and community levels. We employed IVs generated through the education programs to estimate the causal effect of maternal education on sex bias in child survival.

Our findings suggest that maternal education significantly increases child survival of both genders between the first and fifth birthdays, conditional on survival to the first birthday. Specifically, a one-year increase in the highest grade achieved increased significantly the survival probability by .012. The set of specification tests supports our assumptions and yields a causal interpretation from the estimate. The 2SLS estimate for the effect of maternal education on the survival probability is significantly larger than the corresponding OLS estimate, which may suggest that maternal education is endogenous in the context of child health and the OLS estimate is biased downward. This suggests that endogeneity needs to be addressed methodologically to examine the effect of maternal education on child survival. On the other hand, we did not find any incremental effect of maternal education by gender of child, implying that girls do not benefit any more than boys from an educated mother.

The study results agree with the findings from numerous studies observing a positive association between maternal education and child survival in general. Also the study results are consistent with the findings reported by Rosenzweig and Schultz in their study in India, which did not observe a link between maternal education and differential survival by gender of child (Rosenzweig and Schultz 1982). However, our finding does not support the hypothesis that girls born to educated mothers are at greater risks, as proposed by Das Gupta in her study in Punjab, India (Das Gupta 1987).

However, the relationship between maternal education and sex bias in child survival may vary in other settings and the estimated effect of maternal education may not be immune to differences in their contexts. Das Gupta *et al* assessed possible factors at the household and societal levels that can influence sex ratio and sex bias in mortality among Northwest India, China, and the Republic of Korea by reviewing research and examining descriptive statistics (Das Gupta et al. 2003). They find variations in son preference and differential parental behaviors within the countries and suggest that the rigid patrilinial kinship system observed in all the three regions may influence son preference and differential parental behaviors. Their findings therefore call for a cautious comparison of our study results across countries or regions.

In particular, it could be argued that son preference and differential parental behaviors in Bangladesh may not be as extreme as in North India, where many of the studies on this topic were conducted. The sex bias in child mortality between the first and fifth birthdays (expressed as a ratio of female to male child mortality) was 1.61 in 2005-06 in India (International Institute for Population Sciences (IIPS) and Macro International 2007), which is higher than that of 1.25 in 2007 in Bangladesh (NIPORT et al. 2009). This may suggest that son preference and differential parental behaviors in Bangladesh may not be as extreme as in India. The study results therefore may not be generalized to other countries.

We also examined the causal effect of the education programs on maternal education, as measured by the highest grade achieved. A one-school increase per 10,000 population of ages 10-17 when a woman was age 11 increased the highest grade achieved by.24 years. Likewise, partial and full exposures to the financial incentives, as captured by a woman’s year of birth, significantly increased the highest grade achieved by 1.01 and 1.81 years, respectively. The finding suggests that the education programs have been effective in enhancing maternal educational attainment. Correspondingly, a one-school increase per 10,000 population of ages 10-17 when a woman was age 11 significantly increased the probability of child survival between the first and fifth birthdays by .002. Partial and full exposures to the financial incentives significantly increased the probability of child survival by .015 and .017, respectively. However, we did not find any incremental effect of the education programs by gender of child, implying that girls have not benefited any more than boys from a mother with a greater exposure to the education programs.

However, this study does not identify the components of the programs that have been most effective in bringing about the improvements in child health or maternal education due to a lack of variations in the components across the country. For instance, women born after 1982, who are presumed to have been exposed to more financial incentives than those born earlier, were found to have a higher educational attainment. While this may indicate that larger financial incentives have enhanced maternal educational attainment, it could instead reflect a lagged response to the programs introduced previously. Because the program components have become more varied since the mid-2000s, further research on sex bias in child survival that addresses these variations will provide insight into education program designs that effectively promote maternal education and child survival, and eliminate the sex bias.

The study has several limitations. First, a number of the proximate determinants of interests to this study are not observed, including nutritional status, prevention and treatment of illness, and hygienic practices. Therefore, the estimated effect of maternal education should be interpreted as a collective effect on child survival.

Second, there may be selectivity of sample by maternal educational attainment because: 1) highly educated women may not be married and hence do not have a child; and 2) children born to those who have moved to municipal areas, to be employed for instance, are not in the sample. The estimated effect of education therefore may not fully account for a higher educational attainment.

Third, it could be argued that the effect of maternal education may take longer time to be observed. As mentioned above, women who are expected to have been exposed to the education programs may not have developed fully their motherhood. Given the cultural sensitivity associated with son preference and differential parental behaviors, the society may require a longer time to observe a significant effect of maternal education on their cultural norms and values to reduce son preference and differential parental behaviors.

Fourth, the difference in child survival between girls and boys was small in this study sample, suggesting that the study may have required a larger sample size to examine statistically the difference.

Finally, the estimated effect of maternal education on sex bias in child survival may depend on the types of education programs introduced. This is true when the effect of maternal education on sex bias in child survival varies among women. Consequently, the 2SLS estimates may reflect the weighted average effect of education of women affected by the education programs (Imbens and Angrist 1994). For instance, it could be argued that the stipend provided at the secondary education level affects those who have completed a primary education and can afford the partial cost of a secondary education. The very poor, who are less likely to complete a primary education or to afford any costs associated with education, may not be affected by the stipend. On the other hand, the food rations may have enticed poor households to send their children to primary school. Multiple programs targeting different populations were introduced simultaneously in Bangladesh, complicating efforts to identify a group of women who have been affected by the programs since the 1990s.

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