The Impact of a Female Targeted Educational Stipend on Children's Mortality in Bangladesh

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Abstract

This paper explores the causal impact of increased maternal education on children's mortality by investigating the introduction of the 1994 Female Secondary School Stipend Program (FSSSP) in Bangladesh. This program made secondary level education (grades 6-10) free for young girls residing in rural regions of Bangladesh. Eligibility differences in the FSSSP based on area of residence and duration of exposure are two sources of exogenous variation I use to employ difference-in-difference methods. Using data from the Bangladesh Demographic and Health Survey my results indicate higher secondary school level education improves infant and child health, as evidenced by a smaller but statistically significant reduction in the total number of children who died per woman by age 26. This research has important policy implications, as the link between better secondary school education for women and a reduction in children's death is another reason to provide widely accessible and affordable schooling for young girls in developing countries.

Introduction

Infant and child mortality remains high in least developed countries. In 2018 alone, infant and child mortality rates were six times higher in Bangladesh than in Canada (World Bank, 2020). Consequently, reducing children's mortality remains an important global objective, as mortality is a prominent indicator of socioeconomic status and quality of life.

In this paper, I examine the impact of a female-targeted educational stipend at the secondary school level in Bangladesh on children's mortality. I test whether increased maternal education reduces (i) on average the number of children who died per woman by age 26, (ii) on average the number of children who died before age 1 per woman by age 26, and (iii) on average the number of children who died before age 5 per woman by age 26. This research has important policy implications, as the link between better secondary school education for women and a reduction in infant and child deaths is another reason to provide widely accessible and affordable schooling for young girls in developing countries.

The Female Secondary School Stipend Program (FSSSP) began in 1994, providing free education for young women residing in rural regions of Bangladesh. The goal of the program is to increase female secondary school enrollment and retention. Indirectly, higher levels of education may reduce children's mortality along the intensive margin. For instance, parents may have less children to account for high child survival rates, leading to decreased fertility rates. Conversely, higher levels of education may reduce children's mortality along the extensive margin. For example, childless families may be influenced to have children, as they expect high child survival rates, leading to increased fertility rates. To analyze the impact of the educational stipend on children's mortality, I use micro-level data from the Bangladesh Demographic and Health Surveys (BDHS) from the years 2007, 2011, and 2014. To overcome the potential endogeneity of education, I use variation in program start dates and regional eligibility to employ the difference in difference methods. Using these methods allows me to investigate the causal effect of maternal education on children's mortality.

Hahn et al. (2018), exploiting the same program, study the impact of the FSSSP on education, marriage, fertility, occupational, and child outcomes. The authors study children's health by exploring anthropometric measures such as height-for-age and weight-for-age, as well as prevalence of anemia, and levels of hemoglobin. They find the stipend program reduces the number of desired children by 3% and adolescent fertility rates by 8%-12%. They propose this decline in fertility rates is through improved labour market outcomes for women, which aligns with the Becker and Lewis (1973) hypothesis that women desire and have less children when the opportunity cost of children increases.

This study differs from Hahn et al. (2018) as I examine children's health by using a more basic measure, children's mortality. In contrast, Hahn et al. (2018) measure children's health by studying various health outcomes related to the oldest child within five years of the surveys. Moreover, they use 2004, 2007, and 2011 BDHS data, whereas I use 2007, 2011, and 2014 BDHS data. Using the most recent waves of BDHS data provides an opportunity to explore the total number of children who died per woman by age 26. My results suggest that FSSSP increased total years of education by 0.527 to 1.065 years for stipend eligible women. Moreover, this increase in secondary school level education has a small but statistically significant impact on reducing children's mortality.

This paper contributes to the existing literature in two ways. First, this study investigates the causal link between maternal education and children's mortality, where mortality is measured at the micro level using difference-in-differences methods. To my best knowledge, this is the first paper to study this dependent variable in the Bangladeshi context. Secondly, my results suggest that a reduction in children's death is another potential mechanism through which fertility rates declines.

Literature Review

In the context of developed countries, a few studies have found a causal link between maternal education and children's mortality. Grytten et al. (2014) study the extension of compulsory schooling in Norway. They find an increase in maternal education reduces the probability of low-birth-weight infants, consequently reducing the risk of infant mortality. McCrary and Royer (2011) study an age-at-school-entry policy in the United States. They find an increase in maternal education has a small, but positive impact on improving infant health, measured by a decrease in infant mortality and an increase in birth weights. Further investigating this causal link, particularly in the context of developing countries, is important given the high prevalence of children's deaths, and gender inequality in educational attainment favouring males. These are therefore the key motivators for my study on education reforms and children's mortality in Bangladesh.

A few studies have explored the causal link between paternal education and children's mortality at the primary school level in developing countries. Breierova and Dulfo (2004) use a primary school construction program as exogenous variation in schooling to estimate the causal effect of education on child mortality and fertility outcomes in Indonesia. Their study shows that each school with a capacity of 1,000 students increased total years of education by 0.15 for women with full exposure to the program. The authors also find that an increase in average years of parental education causes a 0.0424 decrease in the total number of children who died per woman before age 25. Furthermore, they find no statistically significant evidence suggesting mothers' education causes a differential effect on infant mortality in comparison to that of fathers. I also estimate child mortality by using the total number of children who died per woman by age 26 when studying the FSSSP in Bangladesh. Makate and Makate (2016) use a universal primary education (UPE) policy as exogenous variation in schooling to investigate the causal link between maternal education and under-five mortality in Malawi. They find one additional year of primary school education lowers the probability of under-five mortality by 6.48 percentage points, which signifies a 36 percent decline in under-five fatality.

Other studies have found a negative relationship between higher level education and child mortality in developing countries. Chou et al. (2013) analysis the impact of education on the child health outcomes using a junior high school construction program as exogenous variation for schooling in Taiwan. Grepin and Bharadwah (2015) explore the impact of maternal education on a number of child health outcomes using increased secondary school access in Zimbabwe as a natural experiment and exogenous variation in schooling. The former paper finds that maternal education reduces total infant deaths by 0.77 per 1000 live births, whereas the latter paper finds that maternal education reduces child mortality by 21%. I further contribute to the literature by investigating the link between higher-level education and child health outcomes in developing countries, as the target group for the FSSSP are girls in grades 6-10 in Bangladesh.

I extend a paper by Hahn et al. (2018) in which they explore the impact of the FSSSP on a number of outcomes using the difference in difference framework. Related to child outcomes, they find children of stipend eligible women weighed more and were taller compared to the mean, and that there is no statistically significant impact of the FSSSP on hemoglobin levels or the prevalence of anemia. The authors suggest the latter results are attributed to sample size issues, as only the 2011 survey contains information on these variables.

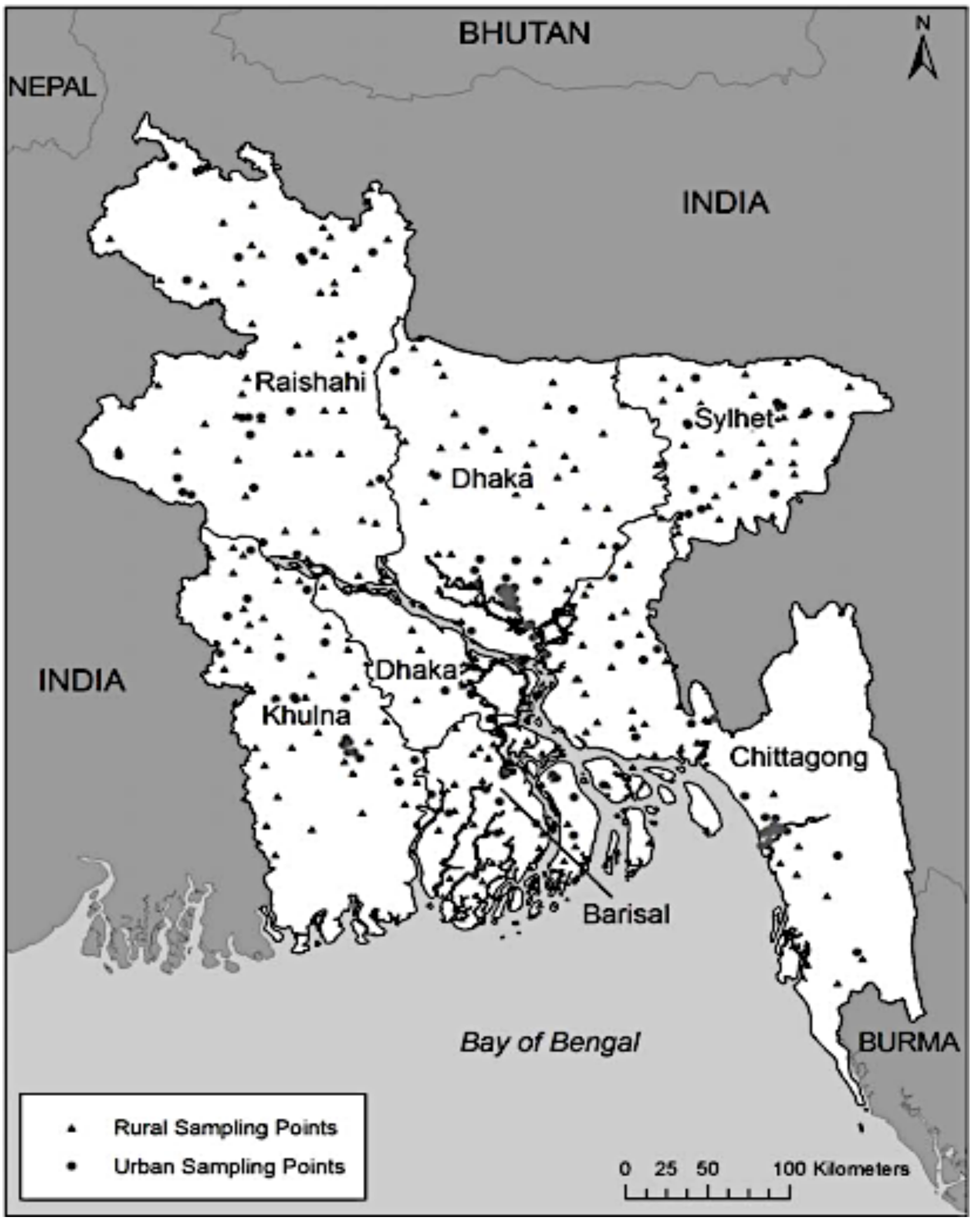
Data

I use data from the Bangladesh Demographic and Health Surveys (BDHS-2007, 2011, and 2014), a nationwide survey that collects information on the population of Bangladesh residing in non-institutional housing units. In particular, a representative subsample of women who are or were married (hereafter, ever-married) between the ages of 15-49 are asked questions related to demographic and socioeconomic characteristics, reproductive history, healthcare, family planning and family health.

The surveys are conducted in two stages. In the first stage, the population is divided into enumerations areas based on information from the Bangladesh Census. Next, an in-person interview takes place in a subsample of approximately 30 households in each area. The interviews are conducted by individuals with relevant training and have past surveying experience. Figure 1 provides visual representation of the urban and rural sampling points used in the 2007 survey.

For the purpose of my analysis, I focus on respondents who are between the ages of 18-43. I select this age range because the respondents were of ages 6-23 in 1994, the year the FSSSP was implemented nationwide. Moreover, I use both the individual record and household files. From the individual record file, the variables of interest for my study are; area of residence, birth year, child's date of birth, whether the child is alive, and age at death if they are dead. From the household file the key variables are family structure and size. It should be noted that the extended family variable used as a control and all mortality related dependent variables were not directly available from the datasets they were instead carefully constructed, sometimes using multiple files. For example, to create the extended family variable I merged dwelling information from the household files with the individual recode file. Using the information on area of residence and birth year, I determine whether a woman is eligible to receive the stipend.

Figure 1
Map of Bangladesh with Urban and Rural Sampling Points



Source: Bangladesh DHS 2007 Final Report

My main dependent variable is the total number of children who died per woman before age 26, similar to Breierova and Duflo (2004). I chose age 26 because women of at least that age are represented in all three survey years, as shown in Table 1. This consists of women born between 1971-1981 in the 2007 survey, 1971-1985 in the 2011 survey, and 1971-1988 in the 2014 survey. From Table 2, the average age at first birth is 18 and the average number of children born is 2.6. Therefore, by age 26 women have most likely completed their fertility. The DHS recode manual defines missing observations for the woman's ith child as the ith child was not born, and therefore when constructing my main dependent variable all missing observations remain blank. Furthermore, women with no children ever born were excluded from my mortality analysis since my interest lies in exploring the impact of maternal education on the mortality of children. One limitation in my study is that never-married women who were eligible for the FSSSP are excluded from my analysis due to the nature of the data.

Table 1
Age at survey year based on Female Secondary School Stipend Program eligibility

Birth Year	1994	2007	2011	2014	Cohort
1988	6	19	23	26	Cohort 1
1987	7	20	24	27	
1986	8	21	25	28	
1985	9	22	26	29	
1984	10	23	27	30	
1983	11	24	28	31	
1982	12	25	29	32	Cohort 2
1981	13	26	30	33	
1980	14	27	31	34	
1979	15	28	32	34	Cohort 3
…	…	…	…	…	
1971	23	36	40	43	

Note: The FSSSP in 1994 was only available for women born between 1988-1980, therefore women in 1979-1971 were ineligible for the program. Cohort 1 and 2 are defined as the treatment group were as Cohort 3 is the control group. Source Hahn et al. (2018) Table 2

History of Education in Bangladesh/ Background on the FSSSP

In 1990, the Compulsory Primary Education Act was introduced. This act made primary school grades 1-5 mandatory for all children in Bangladesh. Within the first year of implementing this act, the total number of children enrolled in primary school increased by 600,000 (World Bank, 2000). The dropout rates, however, remained high. From 1991-1994, the average dropout rate was 56.5% (Ahmed et al., 2007, p. 13). Suggested by the literature, this may be because of the increased opportunity cost of remaining in school (see Bennell, 1996; Sarker, 2019; Government of Canada, 2017). In poorer regions of developing countries, parents tend to need their children to work in order to financial support the family. Therefore, children stop attending school and begin to work at an early age. This is particularly apparent when comparing rural-urban education levels. According to the Bangladesh DHS 1993-1994 Final Report, only 1.1% of females residing in rural areas had secondary school level or higher education, whereas in urban areas, 9.6% of females attained this level of education (Mitra et al. 1994). This large disparity between rural-urban educational attainment, in part, may be attributed to secondary and higher-level education being costly, unlike primary education which is free.

To counteract financial burdens, education programs began including financial support. The nationwide 1994 Female Secondary School Stipend program was introduced and funded by: the Bangladesh government, the Norwegian government, the Asian Development Bank, and the World Bank. The main objectives of the program are to increase female enrollment and retention, augment female employment opportunities, and delay early female marriage (Hahn et al. 2018). The FSSSP provides a sum of money to young girls residing in rural areas of Bangladesh enrolled in secondary school. In annual terms, the stipend ranges from approximately CAN $24 to CAN $59 (Hahn et. al, 2018). Receiving the stipend is conditional upon meeting the following requirements: i) remain unmarried for the duration of the program, ii) maintain a 75% school attendance rate, and iii) obtain at least a 45% grade on yearly school exams. Once met, the target group (girls in grades 6-10) receive the stipend for school related fees and any unused money is placed into a local bank near the girls' area of residence in her name. Moreover, the cash transfer came in waves in the first few years of nationwide implementation. In 1994, girls in grades 6 and 9 received the stipend. Next, in 1995, girls in grades 6,7,9,10 received the stipend. And then lastly, in 1996 all targeted grades received the stipend. By 2000, 22.1% of women had secondary level or higher education in rural areas of Bangladesh (BDHS 1999-2000, Final report, p. 13).

Methodology

I use the same estimation strategies employed by Hahn et. al (2018) to identify the impact of the FSSSP on education and mortality outcomes using the following equation:

$$Y_i = a_0 + \sum_{j=1}^{2} \beta_j Cohort_{ij} + \theta Rural_i + \sum_{j=1}^{2} \pi_j Cohort_{ij} \cdot Rural_i + \varphi X_i + u_i$$

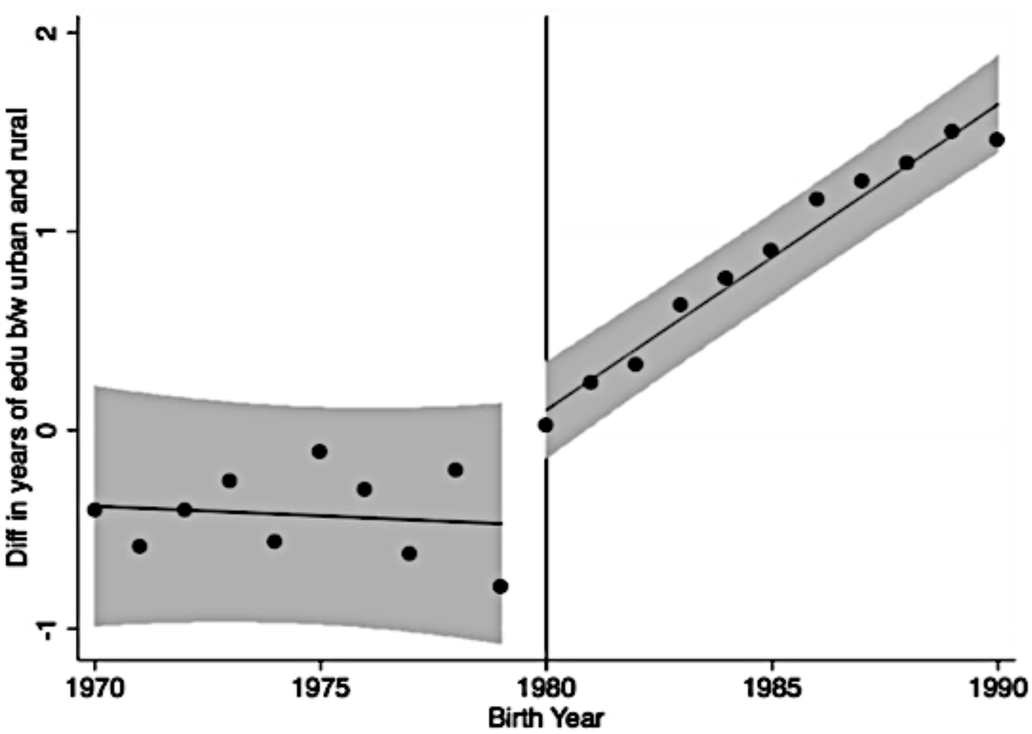
where Y_i indicates either the total years of education per woman i or the total number of children who died per woman i by age 26. The $Cohort_{ij}$ dummy variable captures whether a woman was exposed to the stipend and the duration of their exposure, where Cohort 3 is the base category. The $Rural_i$ dummy is valued 1 if woman i resides in a rural area and 0 if in an urban area. Moreover, X_i is a vector of control variables, including household type, wealth measure, religion, birth year, survey year, and division fixed effects. In particular, I control for dwelling effects by including a family type control variable. Household type is a dummy valued 1 if a woman lives in an extended family and 0 if in a nuclear family. I define extended family as individuals living with non-immediate family members, such as aunts, uncles, cousins, grandparents, etc. In addition, I control for family wealth, which is measured by a categorical variable valued 1 to 5, where 5 is the wealthiest quintile. I include this variable because respondents residing in wealthier homes may have better access to education and health care facilities. Furthermore, I control for religious fixed effects by including a dummy variable valued 1 if a woman is Muslim and 0 otherwise. In addition, within each regression, division fixed effects control for unobservable spatial heterogeneity. Furthermore, all standard errors are clustered at birth year*area of residence to account for within-district correlation.

Within the difference in difference framework two exogenous variations are necessary. The first variation is based on stipend eligibility via cohorts. The two-treatment groups consist of girls eligible for the stipend. Cohort 1 received the stipend for 5 years, aged 6-11 in 1994 and in rural districts; Cohort 2 received the stipend for 2 years, aged 12-14 in 1994 and in rural districts. The control group, Cohort 3, is older girls (ages 15-23) in rural districts who marginally missed the FSSSP. The second variation is area of residence as only individuals residing in rural areas were eligible for the stipend.

The coefficient of interest is the estimated value on the interaction term between cohort and rural, $\widehat{\pi_J}$, where $\widehat{\pi_1}$ is associated with Cohort 1, and $\widehat{\pi_2}$ is associated with Cohort 2. A priori, I expect $\widehat{\pi_1}$ and $\widehat{\pi_2}$ to be positive when examining the impact of the FSSSP on education outcomes and negative for mortality outcomes. Moreover, I would also expect the estimated coefficients will be larger for cohort 1 (in absolute value) because they were eligible for the full stipend.

The parallel trend assumption is a crucial component of the difference in difference methodology. This implies that prior to the introduction of the FSSSP, we should see the underlying trend in total years of education as parallel for women residing in urban and rural areas. This is illustrated in Figure 2 as the constant difference in total years of education for women born before 1980. The graph also shows the difference in educational attainment only occurs for rural women born in 1980 or afterwards, as there is a clear increasing trend in the difference in years of education between urban and rural girls. Another interpretation is that individuals born before 1980 represent the control group, whereas individuals born after 1980 represent the treatment group. This implies the introduction of the FSSSP only had a differential impact on education for women young enough to benefit from the FSSSP in rural areas of Bangladesh. For further validation, I re-estimate equation (1) using non eligible cohorts exclusively in the sensitivity analysis section.

Figure 2:
Parallel Trend Assumption



Source: Hahn et al. (2014) Figure 2. Graphical representation of the estimated coefficient on the interaction term from the following equation with a 95% confidence interval and two best fit lines:

$$Y_i = a_0 + \beta_1 rural_i + \beta_2 birth\ year_i + \beta_3 rural_i * birth\ year_i + u_i$$

where Y_i is the total years of education per woman i and the standard errors are adjusted for clustering on birth year* rural-urban level

Results

Education Outcomes

I begin by performing a replication study of the education outcomes from the Hahn et al. (2018) paper. Demonstrating that the FSSSP is associated with an increase in total years of school is an important preliminary step to aid our discussion on how better secondary school level education impacts mortality outcomes.

Table 2 below provides a comparison of the summary statistics from my data set to those found in Hahn et al.'s (2018) paper. My study uses 2007, 2011, and 2014 BDHS data, whereas Hahn et al.'s study uses 2004, 2007, and 2011 BDHS data. It is important to note that my sample size is generally larger than theirs because women who were married for the first time between the 2011 and 2014 surveys would be included in my sample exclusively. In my study 39% of women are in cohort 1, whereas in their paper 37% of women are in cohort 1. Of these women 25% were eligible for the 5-year stipend in my study and 24% in Hahn et. al (2018) study. Also, in both studies 64% of women reside in rural areas of Bangladesh opposed to urban areas. Related to education, average years of education increase from 4.83 to 5.08 and average ideal number of children decrease from 2.25 to 2.22 when comparing the two studies. I therefore expect my results to be similar to Hahn et al.'s (2018) paper.

Table 2
Sample description

	Hahn et al. (2018)		Replication	
	Mean	SD	Mean	SD
Cohort 1	.37	.48	.39	.49
Cohort 2	.18	.38	.18	.38
Cohort 1 x Rural	.24	.43	.25	.43
Cohort 2 x Rural	.11	.32	.11	.32
Rural	.64	.48	.64	.48
Extended family	.49	.50	.50	.50
Wealth measure	3.17	1.45	3.16	1.44
Individual- level variables:				
Religion	.89	.31	.90	.31
Age	27.89	5.77	30.58	5.74
Years of Education	4.83	4.23	5.08	4.31
Fertility outcomes:				
Total number of children born	2.42	1.55	2.63	1.53
Ideal number of children	2.25	.69	2.22	.68
Age at oldest child's birth	17.74	3.13	18.00	3.33

Note: Hahn et al. (2018) results are from Table 1 of their paper using 2004, 2007, and 2011 BDHS data. Their observations are 24,329 except "age at the time of oldest child's birth" observations are 22,397 and "ideal number of children" observations are 23,958. My results are using 2007, 2011, and 2014 BDHS data. The observations are 26,928 except "age at oldest child's birth" observations are 25,645 and "ideal number of children" observations are 256,567. Standard deviation is abbreviated to SD.

Table 3 provides the estimated impact of the FSSSP on women's education using equation (1). Column (1) is from table 2 of Hahn et al.'s (2018) paper. Both columns include a full set of control variables; religion, wealth measure, household type, age fixed effects, survey year fixed effects, and division level fixed effects. The important regressors are Cohort 1 x Rural and Cohort 2 x Rural, as they determine whether a woman is eligible for the FSSSP and the duration of exposure. For rural women in Cohort 1, being eligible for the FSSSP increases total years of schooling by 1.065 years. This implies one additional year of exposure to the program is associated with a 0.21 increase in total years of education among women exposed to the FSSSP for 5 years. Moreover, for rural women in Cohort 2, being eligible for the FSSSP increases total years of schooling by 0.529 years. This implies one additional year of eligibility for the program is associated with a 0.26 increase in total years of education among women exposed to the

FSSSP for 2 years. In annual terms the FSSSP has a similar impact on additional years of education across cohort 1 and 2. The results also suggest that the FSSSP has a substantial impact on increasing total years of education, as the average years of education is 5.08 shown in Table 2.

When comparing the estimation results across the two studies, the signs on the coefficients are the same in both columns and all estimates are statistically significant. For example, the coefficient on rural is statistically significant and negative (-0.424 in column (1) and 0.398 in column (2)). This implies that on average, women residing in rural regions have fewer years of education in comparison to their urban counterparts. The coefficients on religion are also both statistically significant and negative (-0.581 in column (1) and -0.653 in column (2)). This implies that on average, Muslim women attend school less in comparison to women in other religions such as Christians, Buddhists, and Hindus. Both coefficients on the wealth measure are statistically significant and positive. This implies that on average, women residing in wealthier households are more likely to attend school, in comparison to poorer households. Generally speaking, the estimates are larger in magnitude in Hahn et al.'s (2018) paper in comparison to mine (excluding the religion and wealth measure). This could indicate variation in the datasets as our survey years are not the exact same.

Table 3
Impact of the Female Secondary School Stipend Program on education

	Hahn et al. (2018) Years of Education	**Replication** Years of Education
Cohort 1 x Rural	1.210***	1.065***
	(0.089)	(0.073)
Cohort 2 x Rural	0.666***	0.527***
	(0.078)	(0.043)
Cohort 1	-0.814***	-0.397**
	(0.129)	(0.254)
Cohort 2	-0.507***	-0.308*
	(0.096)	(0.114)
Rural	-0.424***	-0.398***
	(0.068)	(0.095)
Religion (1/0 Muslim)	-0.581***	-0.653***
	(0.097)	(0.189)
Wealth measure	1.527***	1.6154**
	(0.041)	(0.073)
Extended family	0.503***	0.348***
	(0.070)	(0.062)
Observations	24 329	25 738
R-squared	0.344	0.335
Age Fixed Effect	YES	YES
Survey Year Fixed Effect	YES	YES
Division Fixed Effect	YES	YES

Note: Hahn et al. (2018) results are from Table 2 column 5 of their paper using 2004, 2007, 2011 BDHS data, whereas the replication study used 2007, 2011, 2014 BDHS data. Standard errors (provided in parentheses) are adjusted for clustering on birth year*rural-urban level.
**** p<0.01, ** p<0.05, * p<0.10*

Mortality Outcomes

My main dependent variable as mentioned earlier is the total number of children who died per woman by age 26, shown as sample 1 in the summary description reported in Table 4. From this sample, 28% of women are in Cohort 1, of those women 19% were eligible to receive the 5-year stipend; 21% of women are in Cohort 2, of those women 14% were eligible to receive the 2- year stipend. As an unrestricted age group, I also include sample 2 which consists of women ages 6-23 in 1994 who had a child. In this sample, 39% of women are in Cohort 1, of those women 26% were eligible to receive the 5-year stipend; 19% of women are in Cohort 2, of those women 12% were eligible to receive the 2- year stipend. Furthermore, 90% of women in each sample identify as Muslim.

Table 4
Sample Description

	Sample 1		**Sample 2**	
	Mean	SD	Mean	SD
Cohort 1	.28	.45	.39	.49
Cohort 2	.21	.41	.19	.39
Cohort 1 x Rural	.19	.39	.26	.44
Cohort 2 x Rural	.14	.34	.12	.36
Rural	.65	.48	.64	.48
Extended family	.47	.50	.49	.50
Wealth measure	3.14	1.44	3.13	1.44
Individual characteristics:				
Religion (1/0 Muslim)	.90	.31	.90	.31
Age	31.83	4.27	30.26	5.23
Years of Education	4.89	4.32	5.04	.4.23
Mortality outcomes:				
Total children who died per women			.23	.55
Total children who died per women by age 26	.23	.54		
Observations	20 241		24 473	

Note: Column 1 and 2 describe the information for all mothers while Column 3 and 4 describe the information for mothers who had a child by age 26. Standard deviation is abbreviated to SD.

Table 5 reports the impact of the FSSSP on child mortality based on the total number of children who died per woman and by age 26. From the table it is clear that there is a larger reduction when no restriction is placed on the mother's age. Shown in column (1), being eligible for the five-year stipend reduced the total number of children who died by 0.095 per woman, and in column (2), the reduction is 0.062 per woman by age 26. Among women eligible for the two-year stipend the total number of children who died decreased by 0.064 per woman, and in column (2), the reduction is 0.044 per woman by age 26. The larger reduction in column (1) may be attributed to young adulthood mortality, as this sample includes deaths up to age 24. My interest lies in exploring child mortality at ages where the mother is more likely to be the primary caregiver. For example, the average age at first birth for a mother is, and 17 therefore, by age 26 their children's average age would be 9.

Table 5

Impact of the Female Secondary School Stipend Program on children's mortality

	(1) Total number of children who died per women	(2) Total number of children who died per women by age 26
Cohort 1 x Rural	-0.095***	-0.062***
	(0.011)	(0.011)
Cohort 2 x Rural	-0.064***	-0.044***
	(0.011)	(0.009)
Cohort 1	0.060	0.059*
	(0.037)	(0.029)
Cohort 2	0.020	0.020
	(0.021)	(0.016)
Rural	0.055***	0.034***
	(0.012)	(0.010)
Religion (1/0 Muslim)	0.045***	0.042***
	(0.012)	(0.013)
Wealth index	-0.061***	-0.060***
	(0.005)	(0.004)
Extended family	-0.010	-0.008
	(0.007)	(0.008)
Observations	25 645	21 413
R-squared	0.061	0.047
Age Fixed Effect	YES	YES
Survey Year Fixed Effect	YES	YES
Division Fixed Effect	YES	YES

Note: In Column 1 the sample is restricted to ever-married women born between 1971-1988 in the 2007, 2011, and 2014 BDHS data set. In Column 2 the sample is restricted to ever-married women born between 1971-1981 in the 2007 survey, 1971-1985 in the 2011 survey, and 1971-1988 in the 2014 survey. Standard errors (provided in parentheses) are adjusted for clustering on birth year*rural-urban level.

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

In Table 6, I present the impact of the FSSSP on age specific mortality before woman turned 26 years old. In terms of magnitude the results are robust to a diverse set of fixed effects: age, survey year, and division level. In order to determine infant (under age 1) and children (under age 5) mortality I separate the groups further. The estimates from column (3) suggest among women in Cohort 1 eligible for the FSSSP, the stipend program reduced the total number of children who died under age 1 by 0.044. Column (6) shows for the same group of women, the stipend program reduced total number of children who died under age 5 by 0.055. These results may be interpreted as a fractional reduction in the death of one child per women.

Table 6
Impact of Female Secondary School Stipend Program on Mortality under age 1 and under age 5

	(1) Total number of children who died before age 1 per woman by age 26	(2) Total number of children who died before age 1 per woman by age 26	(3) Total number of children who died before age 1 per woman by age 26	(4) Total number of children who died before age 5 per woman by age 26	(5) Total number of children who died before age 5 per woman by age 26	(6) Total number of children who died before age 5 per woman by age 26
Cohort 1 x Rural	-0.044**	-0.043**	-0.044***	-0.056**	-0.054**	-0.055***
	(0.017)	(0.017)	(0.009)	(0.020)	(0.021)	(0.010)
Cohort 2 x Rural	-0.025	-0.025	-0.026***	-0.040*	-0.040*	-0.041***
	(0.016)	(0.016)	(0.006)	(0.021)	(0.021)	(0.008)
Cohort 1	-0.055***	-0.058***	0.037	-0.081***	-0.084***	0.050*
	(0.000)	(0.000)	(0.023)	(0.000)	(0.000)	(0.026)
Cohort 2	-0.046***	-0.046***	0.011	-0.062***	-0.062***	0.016
	(0.000)	(0.000)	(0.012)	(0.000)	(0.000)	(0.014)
Rural	0.081***	0.022	0.022**	0.103***	0.028	0.028***
	(0.015)	(0.017)	(0.008)	(0.018)	(0.020)	(0.009)
Religion (1/0 Muslim)		0.016	0.018		0.030**	0.031**
		(0.011)	(0.011)		(0.013)	(0.013)
Wealth measure		-0.043***	-0.044***		-0.054***	-0.055***
		(0.003)	(0.003)		(0.004)	(0.004)
Extended family		-0.014**	-0.014**		-0.007	-0.008
		(0.007)	(0.007)		(0.007)	(0.007)
Observations	21413	21413	21413	21413	21413	21413
R-squared	0.011	0.025	0.029	0.016	0.033	0.040
Age Fixed Effects	NO	NO	YES	NO	NO	YES
Survey Year Fixed Effects	NO	NO	YES	NO	NO	YES
Division Fixed Effects	NO	NO	YES	NO	NO	YES

Note: The sample is restricted to ever-married women born between 1971-1981 in the 2007 survey, 1971-1985 in the 2011 survey, and 1971-1988 in the 2014 survey using BDHS data. Standard errors (provided in parentheses) are adjusted for clustering on birth year*rural-urban level.
**** p<0.01, ** p<0.05, * p<0.1*

While my mortality results are not numerically large, this still suggests that a reduction in infant and child mortality is a potential channel through which fertility declines. From Hahn et al. (2018) we know fertility decreased, and their suggested channel is through opportunity cost of the mother's time. However, women who expect their children to survive, may also have less children to account for their high survival rates (Todaro and Smith, 2015, p 307). This aligns with the theoretical treatment by Becker and Lewis (1972) which argues that women face a trade-off between child quantity and child quality. Highlighting how increased maternal education reduces children's mortality along the intensive margin.

Sensitivity Analysis

Placebo test

To further test the parallel trend assumption, I perform a placebo test using only FSSSP non-eligible women born between 1965-1978 from the appendix section of Hahn et al. (2017) paper. Placebo Cohort 1 has women born between 1973-1978 and Placebo Cohort 2 has women born between 1970-1972. The placebo cohorts were created to simulate the original cohort specifications shown in the methodology section. From Table 7, it is clear that there is no statistically significant impact of the FSSSP on non-eligible women's total years of education when looking at the estimated coefficient on Placebo Cohort 1 x Rural and Placebo Cohort 2 x Rural. Moreover, the signs are almost all negative. These are the expected results, because the introduction of the FSSSP should not have a statistically significant impact on total years of education for women not exposed to the program.

Table 7:
Impact of the Female Secondary School Stipend Program on non-eligible women's education

	(1) Education	(2) Education	(3) Education
Placebo Cohort 1 x Rural	-0.031	-0.026	-0.057
	(0.192)	(0.178)	(0.081)
Placebo Cohort 2 x Rural	0.155	-0.014	-0.043
	(0.124)	(0.115)	(0.067)
Placebo Cohort 1	1.142***	1.239***	0.325
	(0.000)	(0.007)	(0.201)
Placebo Cohort 2	0.208***	0.374***	0.051
	(0.000)	(0.012)	(0.102)
Rural	-2.629***	-0.397*	-0.331
	(0.105)	(0.205)	(0.218)
Observations	13357	13357	13357
Control Variables	NO	YES	YES
Age Fixed Effect	NO	NO	YES
Survey Year Fixed Effects	NO	NO	YES
Division Fixed Effect	NO	NO	YES

Note: The sample is restricted to only non-eligible women born between 1965-1978. Placebo Cohort 1 contains women born between 1973-1978 and Placebo Cohort 2 contains women born between 1970-1972. Column 2 and 3 include the following control variables: religion (1/0 Muslim), household type (1/0 extended family), wealth measure (scale from 1-5; 5 being wealthiest). Standard errors (provided in parentheses) are adjusted for clustering on birth year*rural-urban level.
**** p<0.01, ** p<0.05, * p<0.1*

Robustness Checks

I perform two robustness checks. First, I address the concern that the age difference between the treatment group and control group is too large for comparison. For example, the youngest member of Cohort 1 were 6 years old in 1994, while the oldest member of Cohort 3 were 23 years old. I test whether this impacts my results by re-estimating equation (1) with a narrower age group. I redefine Cohort 1 as girls aged 8-11 in 1994 and Cohort 3 girls aged 15-21. Table 7 presents the coefficient estimations from the original cohort specification shown in Column 1 and narrower subsample shown in Column 2 when the dependent variable is the total number of children who died by age 5 per women by age 26. The results show that the overall trend is the same in both cases, implying the main results are not age sensitive.

Table 7
Robustness check based on narrower age group

	(1) Total number of children who died before age 5 per woman by age 26	(2) Total number of children who died before age 5 per woman by age 26
Cohort 1 x Rural	-0.055***	-0.048***
	(0.010)	(0.009)
Cohort 2 x Rural	-0.041***	-0.038***
	(0.008)	(0.006)
Observations	21 413	17605
R-squared	0.034	0.034

Note: Column 1 is the original cohort specification where the sample is restricted to ever-married women born between 1971-1981 in the 2007 survey, 1971-1985 in the 2011 survey, and 1971-1988 in the 2014 survey. Column 2 is the narrower cohort specification where the sample is restricted to ever-married women born between 1973-1981 in the 2007 survey, 1973-1985 in the 2011 survey, and 1973-1986 in the 2014 survey. Moreover, the following control variables are included in both columns: religion (1/0 Muslim), household type (1/0 extended family), wealth measure (scale from 1-5; 5 being wealthiest), age fixed effects, survey year fixed effects, and division fixed effects. Standard errors are adjusted for clustering on birth year* rural-urban level.
**** p<0.01, ** p<0.05, * p<0.1*

Next, within the DHS data to my best knowledge there is no variable indicating the exact date women and their children are born. Therefore, I check the robustness of my age specific dependent variable by rounding the mother's age if she shares the same birth month as her children and was turning 26 years old that year. This applied to approximately 1000 cases. For example, if the mother's age was rounded down to 25 then she would be excluded from my sample, and if I rounded up to 26 then included. It is important to check whether the specification had a differential impact on the total number of children who died per woman as clustering could have occurred at one of the ages. I find that rounding the mothers age up and down has almost the exact same results. Throughout my estimations, I therefore chose to always round the mothers age down for consistency.

Table 8
Robustness check based on birth month

	(1) Total number of children who died per woman by age 26 with mothers age rounded down	(2) Total number of children who died per woman by age 26 with mothers age rounded up
Cohort 1 x Rural	-0.062***	-0.062***
	(0.011)	(0.010)
Cohort 2 x Rural	-0.044***	-0.043***
	(0.009)	(0.009)
Cohort 1	0.059*	0.059*
	(0.029)	(0.029)
Cohort 2	0.020	0.019
	(0.016)	(0.016)
Rural	0.034***	0.034***
	(0.010)	(0.009)
Muslim	0.042***	0.042***
	(0.013)	(0.012)
Wealth measure	-0.060***	-0.060***
	(0.004)	(0.004)
Extended family	-0.008	-0.007
	(0.008)	(0.008)
Observations	21413	21413
R-squared	0.047	0.047
Age Fixed Effect	YES	YES
Sample Year Fixed Effect	YES	YES
Division Fixed Effect	YES	YES

Notes: Sample is restricted to ever-married women born between 1988-1971 who had a child by age 26. Column (1) rounds the mother's age down if she shares the same birth month as her child, and column (2) rounds the mother's age up. Standard errors are adjusted for clustering on birth year* rural-urban level.
*** p<0.01, ** p<0.05, * p<0.1*

Conclusion

This paper provides empirical evidence that increased maternal education at the secondary school level reduces children's mortality. I use the female secondary school stipend program as a natural experiment. Variation across area of residence and cohort's eligibility is exploited in the difference in difference methodology. The OLS estimates suggest that better schooling for mother's improves infant health (0.044 reduction in the number of child death by age 1 per woman), and child health (0.055 reduction in the number of child deaths by age 5 per woman). While my results are focused on the intent to treat group, I believe they demonstrate that increased secondary school level education targeted at females has multigeneration benefits, aiding the discussion that women should have more affordable schooling in developing countries.

My main dependent variable is a categorical variable and it is clear from my findings that it takes the value 0 often. Therefore, OLS may not be the most accurate methodology. In future studies, I would like to explore the robustness of my findings by using a variety of estimation methods such as, count models and instrumental variables. Furthermore, studying children's mortality with child level data as opposed to mother level data would be an interesting extension of my work.

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