Journal of Health Economics 102 (2025) 103024

Contents lists available at ScienceDirect

Journal of Health Economics

journal homepage: www.elsevier.com/locate/jhealeco

The impact of the Female Secondary School Stipend Program on child health

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A R T I C L E I N F O

We are grateful to Bilge Erten, Jack Cavanagh, and Nishith Prakash for helpful comments and suggestions. We also benefitted from comments at the Southern Economic Association and Midwest International Economic Development conferences and at University of Kent, Wayne State University, Tilburg University, and University of South Florida seminars.

JEL codes:
H52
I12
J13
O12

Keywords:
Maternal education
School stipend program
Child health
Bangladesh

A B S T R A C T

In this study, we examine the inter-generational effects of the 1994 Female Secondary School Stipend Program (FSSSP) on child health inputs and child health outcomes in Bangladesh. Prior studies have shown that the FSSSP significantly increased secondary schooling among rural girls. Applying a difference-in-differences model based on differential exposure to FSSSP by birth cohort and rural residence, we find that full immunization rates increased by 4.2 percentage points among children of mothers eligible for a stipend for 5 years relative to children of mothers who were not eligible, but there were no significant effects for children of mothers eligible for a stipend for only 2 years. We also find improvements in other health inputs (e.g., antenatal care) and in child health outcomes (e.g., mortality). We also explore changes in marriage, fertility, autonomy, labor supply, and media exposure, which may contribute to the observed improvements in child health.

1. Introduction

A large literature has documented a positive correlation between maternal education and child health in low- and middle-income countries (LMICs). Recent studies have attempted to identify causal effects by relying on quasi-experimental variation in schooling access generated by policies such as compulsory schooling laws, tuition subsidies, or school construction programs. However, the evidence on such inter-generational spillovers is mixed.[1] While some studies find improvements in child health outcomes and inputs (Breierova and Duflo 2004; Chou et al., 2010; Grepin and Bharadwaj 2015; Hahn et al. 2018a; 2018b; Özer et al. 2018; Keats 2018; Wu 2022; Mazumder et al. 2023), others find no impacts or only find impacts on some measures of health (Zhang 2012; Ali and Elsayed 2018; Akresh et al. 2023). Further, it is unclear whether the difference in findings across these studies is due to differences in the

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[1] Studies based on high-income countries such as the US also find mixed results. For example, Currie and Moretti (2003) find that increasing the availability of colleges leads to significant improvements in infant health while McCrary and Royer (2011) find that school entry policies have small effects on infant health.

https://doi.org/10.1016/j.jhealeco.2025.103024
Received 4 September 2023; Received in revised form 28 May 2025; Accepted 10 June 2025
Available online 11 June 2025

measures of health used, the margin of education that was targeted (e.g., primary vs secondary schooling), or contextual factors that are specific to the policy or country being studied. Thus, there is a need for further research on this topic.

We contribute to this literature by examining the inter-generational impact of a stipend program on child health in Bangladesh. The Female Secondary School Stipend Program (FSSSP) was introduced in 1994 and provides a cash stipend to girls in rural areas enrolled in secondary school (grades 6–10). Several studies have shown that the FSSSP significantly increased schooling among girls eligible for the stipend (Khandker et al. 2003; Raynor 2006; Fuwa 2006; Schurmann 2009; Hong and Sarr 2012; Hahn et al. 2018a; 2018b; Khandker et al. 2021; Wu 2022; Sara and Priyanka 2022). There is also evidence of improvements in some measures of maternal and child health outcomes and inputs. Previous studies have found improvements in infant and child mortality, anthropometric measures, hemoglobin levels, antenatal care use, postnatal care use, and institutional delivery, but not in birthweight or anemia (Hahn et al. 2018a; 2018b; Wu 2022).[2] Our study complements this literature by examining a wider range of child health inputs and outcomes. We provide new evidence on the impact of the FSSSP on health inputs such as child immunizations, care provision by a doctor or nurse, and the use of supplements. We also examine the impact of the program on schooling, fertility, and other related outcomes and discuss how these outcomes may influence child health.

Using a difference-in-differences approach based on differential exposure to the FSSSP by birth cohort and rural residence, we find evidence of improvements in child health outcomes and inputs. We show that the rate of full immunization and any immunization increases among children born to mothers exposed to FSSSP for 5 years, relative to children of mothers who were not exposed. Consistent with previous literature, we also find improvements in mortality, anthropometric measures, and antenatal, postnatal, and delivery care. The stipend program affected secondary schooling levels and fertility among women exposed to the program. We explore the impact on other outcomes such as labor supply, female autonomy, and improved health knowledge that may influence child health. Our findings show that secondary schooling programs that are only targeted towards girls can have important inter-generational effects on child health.

2. The 1994 Female Secondary School Stipend Program (FSSSP) in Bangladesh

The Female Secondary School Stipend Project (FSSSP) was originally introduced by the Bangladesh Association for Community Education (BACE) with financial support from USAID and the Asia Foundation as a pilot project in 1982 (Schurmann 2009). The pilot project began in the Shahrasti upazila of Chandpur district and Kaharole upazila of Dinajpur district.[3] From 1987 to 1992, the Norwegian Agency for Development Cooperation (NORAD) expanded the stipend program to one additional upazila every year (Khandker et al. 2021). The pilot project was successful in increasing girls' secondary school enrollment from an average of 7.9 % to 14 % in some project areas and reducing dropout rates from 14.7 % to 3.5 % (Raynor et al. 2006). This pilot project's success was the basis for launching the nationwide FSSSP in 1994, which is the focus of our study.[4] The main goal of the FSSSP was to reduce gender inequality in secondary education (grades 6–10, ages 11–15).[5] Under this program, girls attending a secondary school in rural areas were eligible for a stipend and full tuition subsidy if they satisfied the following criteria: i) attended at least 75 % of all school days, ii) secured a score of at least 45 % in the annual exam, and iii) remained unmarried. Boys and urban residents were not eligible for the program. Hahn et al. (2018a) show that secondary school enrollment increased much faster among girls than boys after the introduction of the FSSSP at the national level.

In 1994, only girls enrolled in grades 6 and 9 were eligible for the stipends, while in 1995, girls enrolled in all grades except grade 8 were entitled to receive the stipends. From 1996 onward, girls enrolled in all secondary education grades (grades 6–10) qualified for the stipends (Khandker et al. 2003). This generates variations in the duration of exposure to the program for rural girls: those born in 1980–1982 (aged 12–14 and enrolled in grades 7–9 in 1994) received two years of stipends, those born in 1983 or later (aged 11 or younger and enrolled in grades 1–6 in 1994) received five years of stipends, and those born before 1980 (aged 15 or older in 1994) received no stipends as they were enrolled in grade 10 or had already exceeded secondary-school ages in 1994 (Hahn et al. 2018b). Fig. 1 illustrates the variation in exposure to the program by birth cohort and grade. For example, a girl born in 1983 would be in grade 6 in 1994 and eligible for five years of stipends whereas a girl born in 1980 would be in grade 9 in 1994 and only eligible for two years of stipends. A girl born in 1979 would be in grade 10 in 1994 and would not be eligible for FSSSP. Women born in 1983 or later who received stipends for 5 years are categorized as "Fully exposed", women born between 1980 and 1982 are categorized as "Partially exposed" and women born in 1979 or earlier are categorized as "Unexposed".

The stipend amounts varied by grade. In 1994, the annual stipends were equivalent to US $18 in grade 6 and US $36 in grade 9,

[2] Studies have also found that the stipend program led to increases in the age at marriage, assortative mating, greater female autonomy, increased contraceptive use, improved labor market outcomes for women, increased age at first birth, decreases in the number of children (Hahn et al. 2018a; 2018b; Hong and Sarr 2012), improvements in younger siblings' education (Begum et al. 2017), and decreases in intimate partner violence (Sara and Priyanka 2022).

[3] An upazila is a small administrative division in Bangladesh, similar to a county in the United States.

[4] Only eight upazilas out of about 500 upazilas in Bangladesh received the stipend program before 1994. Moreover, the stipend was not uniformly provided across all schools within these upazilas before 1994. Since our focus is on the nationwide program, we categorize girls exposed to the pilot projects but not the nationwide program as part of our "unexposed" group. To the extent that the pilot project may have led to better health among the children of some women in the unexposed group, our main specification underestimates the true effect of the stipend program.

[5] Other goals included increasing female age at marriage and improving employment capabilities. See Fuwa (2006) and Raynor et al. (2006) for a more detailed discussion of the background and goals of the FSSSP.

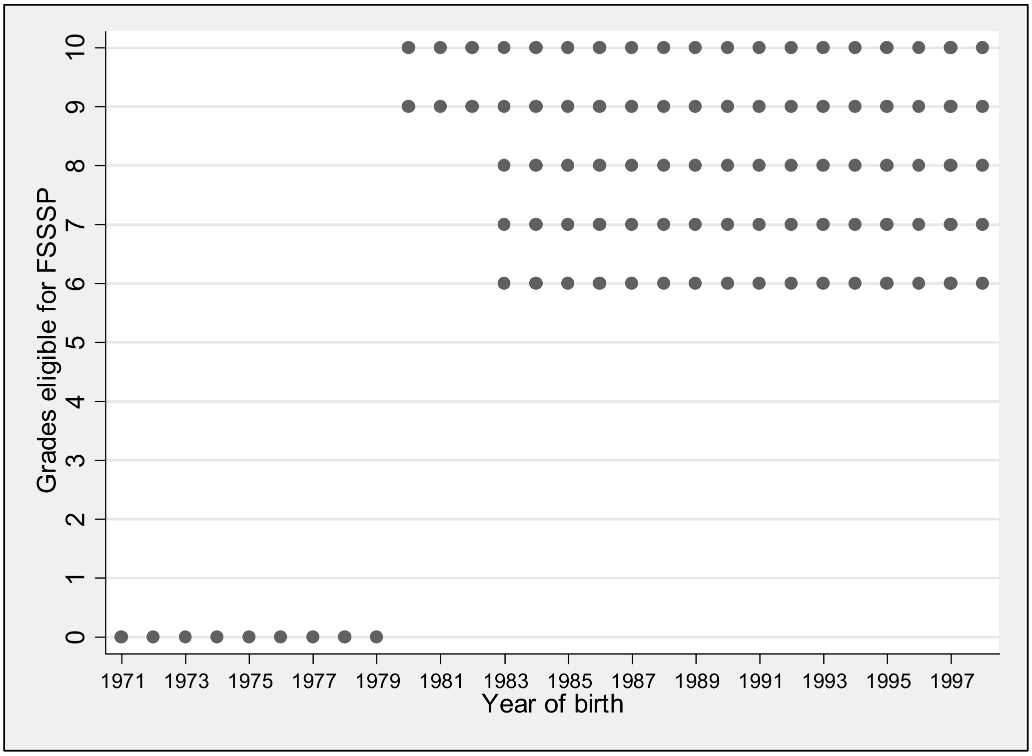

Fig. 1. Exposure to the FSSSP by birth cohort.
Notes: Mothers born after 1982 were eligible for 5 years from 1994 when they were in grade 6, while mothers born between 1980 and 1982 were eligible for 2 years. Mothers born before 1980 were not eligible as they were already in grade 10 in 1994. Mothers in grades 7 in 1994 and 8 in 1995 did not become eligible for a stipend, but they were eligible for it for 2 consecutive years in 1996 and 1997 (grades 9 and 10). Mothers in grade 8 in 1994 did not become eligible for a stipend but were eligible for it for 2 years in 1995 and 1996 (grades 9 and 10). Mothers in grade 9 in 1994 received a stipend for 2 years in 1994 and 1995.

while in 1995, stipends were US $20 in grade 7, US $45 in grade 10, and US$22 in grade 8 in 1996 (Hahn et al. 2018a). In comparison, the average annual income was US$1011.65 in rural areas and US$1920.71 in urban areas in 1993–1994.[6] The stipend was expected to cover up to 50 % of direct educational expenses (textbooks, uniforms, examination fees, etc.) and was paid directly to the girls in two equal annual installments in the presence of the school teachers and officers of Agrani Bank Limited, a state bank with branches in rural Bangladesh (Liang 1996; Mahmud 2003). The tuition subsidy was paid directly to the school. The nationwide rollout of FSSSP took place rapidly between 1994 and 1995. The number of FSSSP recipients increased from 1.9 million (9.3 % of girls enrolled in secondary school) in 1994 to 4.2 million (96.2 % of girls enrolled in secondary school) in 2002 (BANBEIS, 2006). Beginning in mid-2003, stipend awards were cut back and greater monitoring of disbursements was introduced leading to a drop in the number of recipients (Raynor et al. 2006).

The FSSSP has been widely studied and is generally considered to be successful in achieving its goal of increasing female secondary schooling (Khandker et al. 2003; Fuwa 2006; Begum et al. 2017; Hahn et al. 2018a, 2018b; Wu 2022). Using a difference-in-differences method, Hahn et al. (2018a) estimate that the FSSSP increased years of schooling by 1.2 years and secondary schooling completion by 5 percentage points among girls eligible for the stipend. There is evidence that the stipend program also increased the schooling of younger siblings (Begum et al. 2017). Xu et al. (2022) show that the FSSSP reduced the gender gap in school enrollment within households but not in total education expenditure conditional on enrollment nor in the share of education expenditure on items related to the quality of education (e.g., private tutoring).

3. Potential effects of the Female Secondary School Stipend Program

The FSSSP may affect child health through several potential pathways, which may interact with each other in complex ways. As noted above, the primary goal of the FSSSP was to increase secondary schooling among rural girls. This increase in education may increase household income due to higher labor earnings or positive assortative mating in the marriage market (see McCrary and Royer

[6] Authors' calculations based on income data from the 1995 Household Expenditure Survey and exchange rate information from the Bangladesh Bank 2008-2009; Bangladesh Bureau of Statistics, 1995.

2011 for a discussion of the conceptual framework). Consistent with the income channel, previous research has shown that education influences the labor supply of mothers and the characteristics of their partners (Breierova and Duflo 2004; Keats 2018; Hahn et al. 2018a, 2018b; Sara and Priyanka 2022). Higher income may influence child health inputs and outcomes by making health care more affordable. Higher labor earnings also increase opportunity costs of seeking care, however, which makes the theoretical effect of a mother's labor earnings on child health a-priori ambiguous. Since our data does not include information on income, we use labor supply, occupation, and husband's education to examine this channel (described below).

The FSSSP may affect child health by changing fertility choices and introducing a quantity-quality tradeoff. Theoretically, a smaller number of children allows families to allocate more resources to each child (Becker and Lewis 1973; Becker and Tomes 1976; Doepke 2015) and empirical evidence on a negative relationship between the number of children and child outcomes supports the theory (see, for e.g., Millimet and Wang 2011; Kugler and Kumar 2017). Evidence that female education affects fertility patterns while simultaneously improving child health also suggests that a quality-quantity tradeoff may be an important mechanism by which female education affects intra-household allocations (Breierova and Duflo 2004; Keats 2018; Grepin and Bharadwaj 2015; Hahn et al. 2018a, 2018b). The FSSSP may also affect fertility by changing the age at marriage (Chari et al. 2017). As noted above, one of the conditions for receiving the stipend was that the girls had to remain unmarried. Previous studies have shown that secondary schooling programs with some features similar to the FSSSP such as conditional cash transfers (Baird et al. 2011) and scholarships (Duflo et al. 2021) led to improvements in educational outcomes and reductions in fertility.

The program may influence child health investments by increasing women's autonomy and decision-making power within the household. There is evidence that women are more likely to invest in child health and nutrition compared to men, and female caregivers invest more in girls' human capital while male caregivers invest more in boys' human capital (Thomas 1990, 1994; Duflo 2003; Qian 2008; Nyqvist and Jayachandran 2017; Armand et al. 2020; Dizon-Ross and Jayachandran 2022). A recent study on the intergenerational impacts of secondary education subsidies in Ghana finds a reduction in under-three mortality among children born to female scholarship recipients, but no corresponding impact for children of male recipients (Duflo et al. 2024). Furthermore, a secondary goal of the FSSSP was to reduce child marriage. Early marriage has been linked to worse female autonomy and child health (Garcia-Hombrados 2022; McGavock, 2021; Chari et al. 2017). There is evidence that later marriage decreases desired and actual fertility (Chari et al. 2017), which suggests that an increase in age at marriage may also influence child health via the quantity-quality tradeoff.

The allocative efficiency hypothesis suggests that better educated persons may possess greater health knowledge that enables them to pick a more efficient allocation of health inputs (Grossman 2006). Consistent with this channel, early work by Thomas et al. (1991) shows that most of the effect of maternal education on child height in Brazil can be explained by access to information via new media. The FSSSP may also increase interactions with health care providers, who are an important source of information on health care. Previous studies have shown that the FSSSP improved health inputs such as antenatal care, facility births, and postnatal care (Hahn et al. 2018b; Wu 2022). Women may learn about newborn care and recommended vaccinations during antenatal visits, facility births, and postnatal visits.

4. Data

We use data from the 1996–97 to 2017–18 waves of the Bangladesh Demographic and Health Survey (BDHS). The BDHS is a nationally representative survey of ever-married women of reproductive age in Bangladesh and is a segment of the worldwide Demographic and Health Surveys (DHS).[7] Our main analysis is limited to women born between 1971 and 1998 who are at least 16 years old at the time of the survey. We exclude 15-year-old women because they are still in grade 10 and therefore, eligible for the stipend. We use three different samples depending on the outcome variables being analyzed – mother sample, child sample, and birth sample. The mother sample includes all women, irrespective of their fertility, that meet the birth year and age restrictions described above.[8] The child sample includes all surviving children aged between 12 and 59 months born to mothers who meet the birth year and age restrictions mentioned above. We exclude children younger than 12 months as they are not old enough to receive the full set of recommended vaccine doses and we exclude children older than 59 months because the DHS does not collect data on immunizations for older children. The birth sample includes all births to women meeting the birth year and age restrictions, irrespective of the child's age or survival status. We use the mother sample to examine the impact of the FSSSP on maternal schooling and the birth sample to examine the impact of the FSSSP on neonatal, infant, and child mortality. We use the child sample to examine the impact of the FSSSP on child immunizations and other health inputs, child anthropometric measures, mother's fertility, labor supply, marriage decisions, autonomy, and exposure to media. For each regression, we exclude observations that have missing or inconsistent values for the key analysis variables and restrict the sample to represent a consistent population across all available waves of the data. Since the number of missing values varies across outcomes and some variables are not available in all waves, the sample size varies across regressions. Appendix Table A1 shows the number of observations at each step of the sample selection process.

[7] For the 1996-97 through 2004 waves, ever-married women aged 10 to 49 years were surveyed; for the 2007 and 2017-18 waves, ever-married women aged 15 to 49 years were surveyed; for the 2001 wave, ever-married women aged 12 to 49 years were surveyed. We restrict our sample to women aged 16 years or older across all waves. The data set can be obtained from DHS program website (https://dhsprogram.com/data/available-datasets.cfm) upon authorization.

[8] Women with children are older, have fewer years of schooling, and are more likely to reside in a rural area compared to women without children. There are no significant differences in religion between women with and without children (results are available on request).

Maternal education is measured using years of schooling and a binary indicator for attending secondary school or higher. Neonatal, infant, and child mortality are measured using binary indicators for age at death <1 month, age at death <12 months, and age at death <60 months, respectively. For vaccination status, we use binary indicators for being fully immunized and for receiving any immunization. The WHO recommends four vaccines (eight doses) for children in Bangladesh against tuberculosis (BCG), Diphtheria-Pertussis-Tetanus (DPT), Polio (OPV), and Measles (World Health Organization (WHO), 2017).[9] Vaccination status is classified as 'full immunization' if the child receives all eight recommended vaccine doses and as 'any immunization' if the child receives any one of the eight recommended vaccine doses.

Antenatal care is measured using the number of antenatal care visits, and binary indicators for receiving any antenatal care during pregnancy, for a doctor providing the antenatal care, and for a nurse, midwife, or paramedic providing the antenatal care. Information on antenatal care is only available for last-born children born in the 3 years before the survey. Postnatal care is measured using a binary indicator for a postnatal check within 2 months of delivery for the last-born child under age 5. This variable is only available in the 2011 to 2017–18 waves. Delivery care is measured using binary indicators for a facility delivery, a home delivery, and a doctor assisted delivery. The delivery variables are only available for children born in the 3 years before the survey. We use a binary indicator for whether the mother received an antenatal iron supplement, which is only available in the 2004 to 2017–18 waves, and a binary indicator for whether the child was given a Vitamin A supplement in the last 6 months, which is only available in the 1996–97 and 2004 to 2017–18 waves.

Anthropometric measures include height-for-age standard deviations from the median, weight-for-age standard deviations from the median, and weight-for-height standard deviations from the median. We also use binary indicators for being stunted, underweight, or wasted. Children are classified as stunted if their height-for-age is <2 standard deviations, as underweight if their weight-for-age is <2 standard deviations, and as wasted if their weight-for-height is <2 standard deviations. These variables are only available for children born 3 to 5 years before the survey.

To examine income effects, we use an indicator for whether the woman's husband attended secondary school or higher. We also use indicators for whether the woman is currently working, whether she works in the formal sector, and whether her husband works in the formal sector.[10] We measure fertility using age at first birth, the number of living children, and the self-reported ideal number of children. We measure marriage decisions, female autonomy, and decision making using age at first marriage or cohabitation, an indicator for age at marriage <18 years, and an indicator for whether the respondent can go to a health center or hospital alone or with children. We also use indicators for whether she has a final say, either alone or with her husband or someone else, on decisions related to own health care, large household purchases, and visits to family or relatives. We also construct an index which is the sum of the three decision making variables. We measure exposure to media using binary indicators for whether the respondent reads the newspaper at least once a week, watches TV at least once a week, and listens to the radio at least once a week. We also construct an index which is the sum of each of the individual media variables.

Table 1 presents summary statistics for a selected set of independent and dependent variables for each analysis sample. Panel A presents the statistics for the mother sample, Panel B for the child sample, and Panel C for the birth sample. We present statistics for the full sample and separately for each cohort group described above. On average, women have 5.3 years of education and about 47 % have attended secondary school or higher (Panel A, mother sample). Women who were fully exposed to FSSSP have 2.5 more years of schooling than women who were not exposed to FSSSP. Women who were partially exposed to FSSSP have 0.97 more years of schooling than unexposed women. About 81.2 % of all children are fully immunized and 96.7 % have received any immunization (Panel B, child sample). About 90.7 % of the child sample is Muslim and 70 % reside in a rural area. The BDHS measures residence at the time of the survey and does not include information on residence at the time of schooling. The neonatal mortality rate is 4.7 %, the infant mortality rate is 6.7 %, and the child mortality rate is 8 % in the full sample (Panel C, birth sample).

5. Methodology

Our approach is closely related to the difference-in-differences approach used by Hahn et al. (2018a, 2018b) and Wu (2022).[11] This approach exploits the plausibly exogenous variation in exposure to the FSSSP by birth cohort and rural residence to estimate the causal effect of maternal education on various outcomes. Our main regression equation is:

$$Y_{ijkt} = \beta_0 + \beta_1 \mathit{Fully\ Exposed}_k \times \mathit{Rural}_{ijkt} + \beta_2 \mathit{Partially\ Exposed}_k \times \mathit{Rural}_{ijkt} + \beta_3 \mathit{Rural}_{ijkt} + \beta_4 \mathrm{X}_{ijkt} + \gamma_k + \varepsilon_{ijkt} \tag{1}$$

Where Y_{ijt} represents the maternal and child outcomes described above, i indexes the child, j indexes the mother, k indexes the maternal birth year, and t indexes the survey interview year. Rural_{ijkt} is a binary indicator for residing in a rural area in year t. $\mathit{Fully\ Exposed}_k$ is one if the child's mother was born in 1983 or later, and zero otherwise. $\mathit{Partially\ Exposed}_k$ is one if the child's mother was born between 1980 and 1982 (inclusive), and zero otherwise. The reference category includes mothers born in 1979 or earlier (not

[9] The BCG vaccine is recommended at birth, DPT and OPV vaccines are recommended at the ages of 42 days (first dose), 70 days (second dose), and 98 days (third dose), and the measles vaccine is recommended at 273 days (Sarker et al. 2019).

[10] We follow Sara and Priyanka (2022) in defining the formal sector as work in professional occupations and business. The reference group includes work in agriculture, semi-skilled and unskilled labor, and no work.

[11] We do not use an instrumental variables approach since the FSSSP may affect child health via multiple pathways and therefore may not satisfy the exclusion restriction. For example, the FSSSP may affect child health via an increase in maternal education and a delay in maternal age at marriage.

Table 1
Summary statistics.

	(1) Full Sample Mean (Std. Dev.)	(2) Fully Exposed	(3) Partially Exposed	(4) Unexposed	(5) (2)-(4) Mean (*t* statistic)	(6) (3)-(4)
Panel A: Mother Sample						
Years of Schooling	5.256	6.394	4.890	3.918	2.476***	0.972***
	(4.139)	(3.683)	(4.197)	(4.249)	(76.35)	(18.96)
Secondary or Higher Education	0.470	0.607	0.421	0.310	0.297***	0.110***
	(0.499)	(0.488)	(0.494)	(0.463)	(75.56)	(19.36)
(Maternal) Age	27.639	23.741	28.131	32.484	−8.743***	−4.354***
	(7.295)	(4.633)	(6.293)	(7.468)	(−175.95)	(−50.07)
Rural	0.659	0.650	0.656	0.671	−0.0201***	−0.0148**
	(0.474)	(0.477)	(0.475)	(0.470)	(−5.16)	(−2.59)
Muslim	0.899	0.907	0.898	0.890	0.0164***	0.00733
	(0.301)	(0.291)	(0.303)	(0.312)	(6.62)	(1.95)
Observations	69,235	33,784	9216	26,235	0.0164***	0.00733
Panel B: Child Sample						
Full Immunization	0.812	0.866	0.805	0.736	0.130***	0.0689***
	(0.390)	(0.341)	(0.396)	(0.441)	(26.33)	(8.62)
Any Immunization	0.967	0.979	0.967	0.948	0.0312***	0.0188***
	(0.180)	(0.143)	(0.179)	(0.222)	(13.60)	(4.78)
(Maternal) Years of Schooling	5.024	6.076	4.619	3.619	2.457***	0.999***
	(4.060)	(3.620)	(4.214)	(4.161)	(49.89)	(12.83)
(Maternal) Secondary or Higher Education	0.448	0.575	0.394	0.282	0.292***	0.112***
	(0.497)	(0.494)	(0.489)	(0.450)	(47.87)	(13.00)
(Maternal) Age	24.838	23.086	25.436	27.206	−4.120***	−1.770***
	(4.989)	(3.790)	(5.206)	(5.416)	(−71.28)	(−17.71)
Rural	0.700	0.684	0.690	0.728	−0.0435***	−0.0380***
	(0.458)	(0.465)	(0.463)	(0.445)	(−7.44)	(−4.53)
Muslim	0.907	0.912	0.906	0.900	0.0114**	0.00546
	(0.290)	(0.284)	(0.292)	(0.299)	(3.08)	(0.98)
Observations	29,383	15,166	4005	10,212	−0.0435***	−0.0380***
					(−7.44)	(−4.53)
Panel C: Birth Sample						
Neonatal Mortality	0.047	0.041	0.044	0.052	−0.0114***	−0.00855***
	(0.211)	(0.198)	(0.204)	(0.222)	(−9.46)	(−5.03)
Infant Mortality	0.067	0.054	0.062	0.078	−0.0237***	−0.0159***
	(0.250)	(0.226)	(0.241)	(0.268)	(−16.63)	(−7.83)
Child Mortality	0.080	0.061	0.072	0.096	−0.0348***	−0.0236***
	(0.272)	(0.240)	(0.259)	(0.295)	(−22.50)	(−10.55)
(Maternal) Years of Schooling	4.196	5.602	4.233	3.210	2.392***	1.023***
	(3.916)	(3.602)	(3.931)	(3.821)	(113.23)	(34.24)
(Maternal) Secondary or Higher Education	0.355	0.520	0.351	0.241	0.279***	0.110***
	(0.478)	(0.500)	(0.477)	(0.428)	(107.45)	(32.29)
(Maternal) Age	30.658	25.684	30.147	34.253	−8.569***	−4.106***
	(7.088)	(4.443)	(5.330)	(6.876)	(−252.30)	(−80.38)
Rural	0.689	0.679	0.681	0.698	−0.0193***	−0.0171***
	(0.463)	(0.467)	(0.466)	(0.459)	(−7.39)	(−4.77)
Muslim	0.910	0.915	0.907	0.908	0.00733***	−0.000433
	(0.286)	(0.279)	(0.290)	(0.289)	(4.54)	(−0.19)
Observations	150,175	52,874	21,137	76,164		

Notes: Fully exposed cohorts are born between 1983 and 1998, partially exposed cohorts are born between 1980 and 1982, and unexposed cohorts are born between 1971 and 1979. Full immunization indicates that the child has received all eight doses of WHO recommended vaccines. Any immunization indicates that the child has received at least one of the eight recommended vaccine doses. Neonatal mortality is defined as age at death < 1 month, infant mortality as age at death < 12 months, and child mortality as age at death < 60 months. ***
** and * represent statistical significance at 0.01, 0.05, and 0.1 levels, respectively.
Source: Bangladesh Demographic and Health Surveys, 1993–94 through 2017–18 (Panel A), Bangladesh Demographic and Health Surveys, 1996–97 through 2017–18 (Panel B).

exposed to FSSSP). X_{ijkt} denotes a vector of covariates including survey wave fixed effects, a binary indicator for being Muslim (the reference group includes all other religions), and division fixed effects. γ_k is a vector of maternal birth year fixed effects. The main parameters of interest in Eq. (1) are the coefficients on the interaction terms, β_1 and β_2. β_1 estimates the rural-urban difference in outcomes of children born to fully exposed mothers relative to children born to unexposed mothers. β_2 estimates the rural-urban difference in outcomes of children born to partially exposed mothers relative to children born to unexposed mothers. Standard errors are clustered at the birth year × rural level, following the recommendation to cluster at the treatment assignment level (Abadie et al., 2023). Since there are many dependent variables in these analyses, we account for multiple hypothesis testing by aggregating all outcomes within a domain (education, individual vaccine doses, health inputs, mortality, anthropometric measures, income, fertility, autonomy, and media) using the simple average of z-scores as a summary index (Kling et al., 2007).[12] Appendix Table A4 presents the estimates for the summary indexes.

Identification is based on the assumption that, had the FSSSP not been introduced, outcomes would have evolved similarly across maternal birth cohorts and rural/urban areas. As this assumption cannot be tested directly, we examine rural-urban differences in outcomes across maternal birth cohorts using an event study regression. The event study regression replaces the cohort dummies in Eq. (1) with a full set of maternal birth year fixed effects (1979 is the reference group). If changes in outcomes over maternal cohorts evolved similarly in urban and rural areas for children of unexposed mothers, this would provide indirect support for the parallel-trends assumption. In other words, we test for significant coefficients on the interactions between the rural dummy and the fixed effects for birth years 1971–1978 (i.e., the unexposed cohorts). A lack of significance provides support for the parallel-trends assumption. We also conduct various robustness checks to rule out bias and assess the sensitivity of our estimates to alternative specifications. One set of robustness checks involves adding child birth year fixed effects and their interactions with the rural indicator to address concerns about differential trends caused by unobserved variables. The second set of robustness checks uses alternative samples to rule out bias due to measurement error, migration, and selection into motherhood. These results are discussed in further detail in the Appendix (Tables A2 and A3).

6. Results

We first present difference-in-differences estimates for the maternal education and child immunization outcomes in Table 2. Panel A presents the effects on education using the mother's sample, Panel B presents the effects on education using the child sample, and Panel C presents the effect on immunizations using the child sample. We find that the difference in years of schooling between rural and urban women is higher by 1.4 years for fully exposed cohorts relative to unexposed cohorts and by 0.5 years for partially exposed cohorts relative to unexposed cohorts (Panel A, Column 2). We also find that the rural-urban difference in the probability of attending secondary or higher schooling for fully and partially exposed cohorts relative to unexposed cohorts is higher by 14.9 percentage points and by 5.7 percentage points, respectively (Panel A, Column 4). We find similar estimates when using the child sample (Panel B).[13] These results are consistent with the findings of Hahn et al. (2018a, 2018b) and Wu (2022). We find significant increases in the likelihood of full immunization and any immunization for the children born to women who were eligible for a 5-year stipend but not for children born to women eligible for a 2-year stipend relative to children of ineligible women. Specifically, the rural-urban difference in the likelihood of full immunization is higher by 4.2 percentage points for children born to fully exposed women relative to those born to unexposed women (Panel C, Column 2). The corresponding estimate for any immunization is 1.4 percentage points (Panel C, Column 4). In contrast, the coefficient on the interaction between the rural indicator and the partially exposed indicator is not statistically significant for either outcome.[14]

Our estimates suggest that an additional year of schooling due to the FSSSP increases the probability of full immunization by 3.4 percentage points (=0.0418/1.231). As a comparison, Grepin and Bharadwaj (2015) do not find statistically significant effects of an additional year of maternal schooling induced by a secondary schooling reform in Zimbabwe on the probability of full immunization. Özer et al. (2018) find that an additional year of education induced by a compulsory primary schooling reform in Turkey increases the probability of completing the third dose of DPT vaccines by 13 percentage points, while Keats (2018) finds that completing one more grade due to an Ugandan primary schooling reform increases child immunizations by 10.6 percentage points for tuberculosis, 11.6 percentage points for measles, 7.7 percentage points for polio, and 8 percentage points for diptheria.

Fig. 2 presents results from the event study regressions for maternal education using the child sample. The graph plots the coefficient and 95 % confidence intervals corresponding to each rural-birth year interaction. The graph for years of schooling exhibits a relatively flat trend for birth cohorts born in 1979 or earlier and a steep increase for birth cohorts born in 1980 or later. We find similar trends for attending secondary or higher schooling. Although the pre-1979 interaction coefficients are statistically significant, the graphs show an overall flat trend for the unexposed cohorts and a sharp break in the trend beginning from birth year 1980 (exposed cohorts), which we attribute to the FSSSP. Event study graphs for full and any immunizations are presented in Appendix Figure A1. The graph for full immunization exhibits a flat trend for cohorts that were not eligible for the stipend program, and we do not see a clear trend for cohorts that were only eligible for a 2-year stipend. However, there is a sharp increase in child immunizations for cohorts that were eligible for a 5-year stipend. The graph for any immunization is noisier but the overall trend is similar to that of full immunizations. Both immunization outcomes exhibit a sharp decline in 1974 likely due to the famine that affected Bangladesh in that year (Sen 1981; Hernández-Julián et al. 2014). In Appendix Tables A2 and A3 we present several additional robustness checks in support of the identification strategy. We show that our results are robust to alternative specifications that account for unobserved factors that may be correlated with exposure to the FSSSP and may also affect child health (Table A2). We also show that our results are not driven by selective migration or selection into motherhood (Table A3). We present event study graphs for all other outcomes in

[12] We switch the sign of each outcome if needed so that a positive sign is associated with an improved outcome. We then demean each outcome and divide it by the standard deviation of the control group to create a z-score and construct the index by taking the simple average of z-scores. We follow Hoynes et al. (2016) in using the mean and standard deviation of the control group (unexposed cohorts) to calculate the z-scores.

[13] We also find significant effects when we examine the impact on the likelihood of completing secondary schooling or higher. There is a 5.9 percentage point increase in completion among fully exposed cohorts, and a 3.9 percentage point increase among partially exposed cohorts.

[14] We also find significant increases in each individual vaccine dose for the fully exposed cohorts but not for the partially exposed cohorts (results available on request).

Table 2
Effect of FSSSP on maternal education and child immunization.

	(1)	(2)	(3)	(4)
Panel A: Mother Sample				
	Years of Schooling		Secondary or Higher Education	
Fully Exposed X Rural	1.418***	1.372***	0.154***	0.149***
	(0.0780)	(0.0790)	(0.00805)	(0.00828)
Partially Exposed X Rural	0.549***	0.516***	0.0617***	0.0570***
	(0.0595)	(0.0553)	(0.00369)	(0.00337)
Covariates		X		X
Observations	69,235	69,235	69,235	69,235
Dep. Var. Mean	5.256	5.256	0.470	0.470
Panel B: Child Sample				
	Years of Schooling		Secondary or Higher Education	
Fully Exposed X Rural	1.386***	1.231***	0.147***	0.132***
	(0.110)	(0.111)	(0.0108)	(0.0108)
Partially Exposed X Rural	0.676***	0.555***	0.0730***	0.0604***
	(0.0966)	(0.0864)	(0.00861)	(0.00702)
Covariates		X		X
Observations	32,751	32,751	32,751	32,751
Dep. Var. Mean	5.167	5.167	0.463	0.463
Panel C: Child Sample				
	Full Immunization		Any Immunization	
Fully Exposed X Rural	0.0783***	0.0418***	0.0218***	0.0135***
	(0.00885)	(0.00874)	(0.00434)	(0.00441)
Partially Exposed X Rural	0.0136	−0.0101	0.0102	0.00423
	(0.0109)	(0.0103)	(0.00695)	(0.00592)
Covariates		X		X
Observations	29,383	29,383	29,383	29,383
Dep. Var. Mean	0.812	0.812	0.967	0.967

Notes: The mother's sample includes women born between 1971 and 1998 and the child sample includes children under 5 born to women born between 1971 and 1998. Full immunization indicates that the child received all eight doses of WHO recommended vaccines, and any immunization indicates that the child received at least one of the eight recommended vaccine doses. Fully exposed cohorts are born between 1983 and 1998, partially exposed cohorts are born between 1980 and 1982, and unexposed cohorts (reference group) are born between 1971 and 1979. Covariates include a binary indicator for rural residence, a binary indicator for Muslim, maternal birth year fixed effects, survey wave fixed effects, and division fixed effects. Standard errors in parentheses are clustered at the maternal year of birth × rural levels. ***
** and * represent statistical significance at 0.01, 0.05, and 0.1 levels, respectively.
Source: Bangladesh Demographic and Health Surveys, 1996–97 through 2017–18.

Appendix Figures A2-A9.

Table 3 presents evidence on the impact of the FSSSP on other health inputs. For the fully exposed cohorts, we find significant increases in the probability of receiving any antenatal care (Column 1), the probability that antenatal care is provided by a doctor (Column 2), the number of antenatal care visits (Column 4), the probability that delivery occurred in a facility (Column 6), the probability that a doctor assisted with delivery (Column 8), and the probability of receiving any postnatal care (Column 10). We also find that the FSSSP increased the probability that a woman received iron supplements during pregnancy (Column 5) but did not change the probability that a child was given a Vitamin A capsule in the last 6 months (Column 9). We also find significant but smaller effects for the partially exposed cohorts in most cases. An exception is home delivery, for which we find larger magnitudes for the partially exposed cohorts. The difference in estimates between fully exposed and partially exposed women may be because partially exposed women received the stipend for fewer years, or they may reflect heterogeneous treatment effects. Given that we cannot conclusively rule out any one explanation and the estimates for partially exposed cohorts are noisier in some cases, these differences should be interpreted with caution.

Panel A of Table 4 presents evidence on child mortality outcomes using the birth sample. Consistent with the findings of Wu (2022), we find significant decreases in the probability of infant mortality (defined as age of death <12 months) and in the probability of mortality among children aged younger than 5 years born to fully exposed mothers (Columns 4 and 6). For partially exposed mothers, only the estimate for mortality among children aged younger than 5 years is statistically significant at the 10 % level (Column 6). Panel B of Table 4 presents results for anthropometric measures using the child sample. Consistent with previous literature, we find improvements in height-for-age and weight-for-age among children born to both fully exposed and partially exposed women. The estimate for weight-for-age for partially exposed women is only significant at the 10 % level. We also find a significant reduction in the probability of being stunted for children born to fully exposed women (3.7 percentage points) and partially exposed women (3.5 percentage points). The estimate for being underweight is only significant at the 10 % level for fully exposed cohorts and is insignificant for partially exposed cohorts.

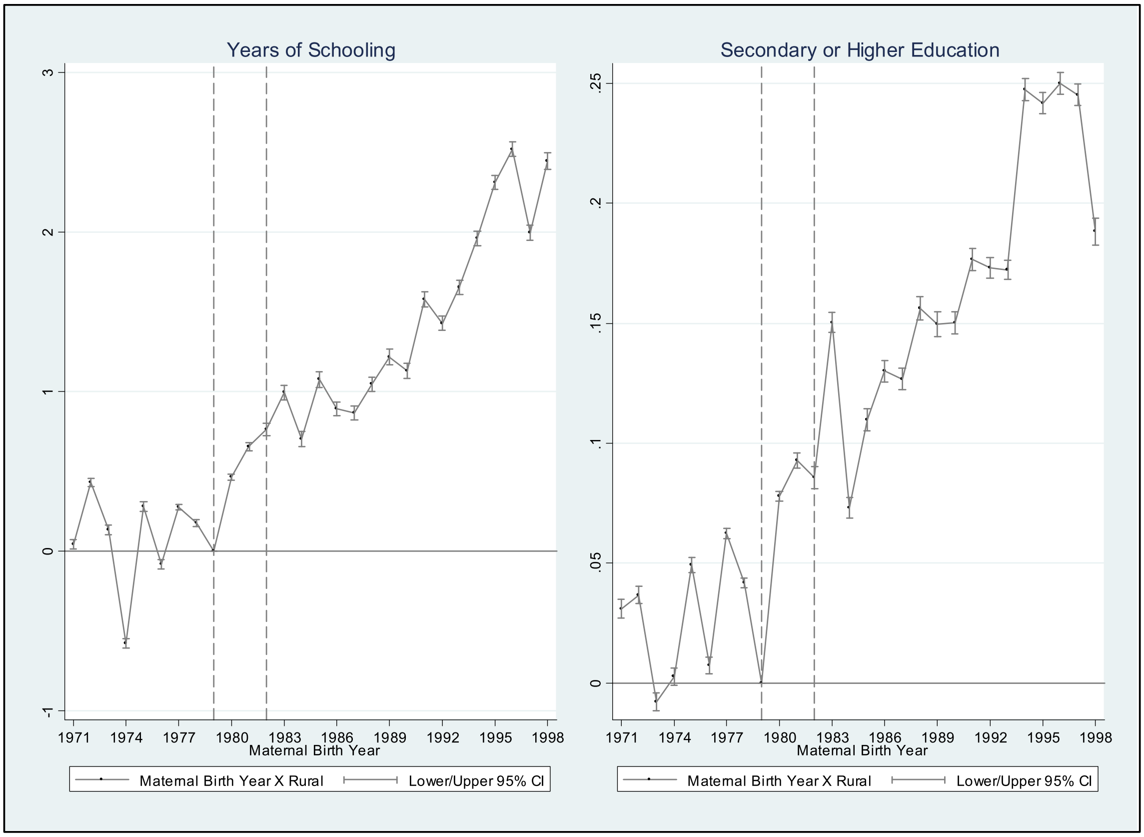

Fig. 2. Event study graphs for maternal education.
Notes: Graphs present interaction coefficients and 95 % confidence intervals from an event study regression of maternal education on interactions between a rural dummy and maternal birth year fixed effects, a binary indicator for rural, maternal birth year fixed effects, a binary indicator for Muslim, survey wave fixed effects, and division fixed effects. The sample includes cohorts born between 1971 and 1998 (reference birth year 1979). Source: Bangladesh Demographic and Health Surveys, 1996–97 through 2017–18.

Next, we examine the impact of the FSSSP on husband's education and labor supply (Table 5). For fully exposed women, we find that the program significantly increases the likelihood that the respondent's husband has attended secondary or higher schooling by 9.1 percentage points (Column 1), that she is currently working by 1.9 percentage points (Column 2), that she works in the formal sector by 1.2 percentage points (Column 3), and that her husband works in the formal sector by 5.6 percentage points (Column 4).[15] For partially exposed women, we find a 5.5 percentage point increase in the likelihood that the respondent's husband has attended secondary or higher schooling and 3.8 percentage point decrease in the likelihood of currently working. As noted above, the differences between fully exposed and partially exposed women may be because partially exposed women received the stipend for fewer years or may reflect heterogenous treatment effects.

In Table 6, we present evidence on changes in fertility patterns due to the FSSSP. For fully exposed cohorts, we find that the FSSSP increases maternal age at first birth by 0.7 years, and significantly decreases the number of (living) children, the ideal number of children reported by the woman, and the likelihood of having a daughter. Table 7 presents evidence on the FSSSP's impact on early marriage, decision-making, and autonomy. We find that the program significantly increases the age at first marriage or cohabitation by 0.8 years and decreases the likelihood of child marriage (before age 18) by 8.5 percentage points for fully exposed cohorts. We find slightly smaller impacts for partially exposed cohorts. It is plausible that increased schooling as well as the eligibility requirement to remain unmarried contribute to this effect. We also find significant improvements in whether the respondent has a say in decisions regarding their own health care, large household purchases, and visits to family or relatives, and in the decision-making index. Surprisingly, there is a significant decrease in whether the respondent is allowed to go to a health center or hospital alone or with children, suggesting that autonomy does not improve across all dimensions. When we use the z-score summary index which combines all the variables in Table 7, we find an overall improvement in autonomy (Appendix Table A4).

In Table 8 we examine the respondent's exposure to media as a proxy for knowledge. We find that the FSSSP significantly increases

[15] We find a 6.1 percentage point increase in the probability that the respondent's husband has completed secondary schooling or higher for fully exposed women (not shown).

Table 3
Effect of FSSSP on other health inputs.

	(1) Any antenatal care	(2) Doctor gave antenatal care	(3) Nurse gave antenatal care	(4) Number of antenatal visits	(5) Antenatal Iron Supplements	(6) Facility Delivery	(7) Home Delivery	(8) Doctor Assisted Delivery	(9) Vitamin A in last 6 months	(10) Any Postnatal Care
Fully Exposed X Rural	0.143***	0.115***	0.00318	0.610***	0.0752***	0.0493***	−0.0341*	0.0654***	0.00684	0.201***
	(0.0199)	(0.0193)	(0.0101)	(0.0792)	(0.0164)	(0.0126)	(0.0172)	(0.00870)	(0.00827)	(0.0328)
Partially Exposed X Rural	0.0518**	0.0662***	−0.00924	0.288***	−0.0172	0.0370***	−0.0652***	0.0629***	0.0193	0.189***
	(0.0222)	(0.0236)	(0.00875)	(0.0759)	(0.0137)	(0.00983)	(0.0174)	(0.00942)	(0.0195)	(0.0385)
N	14,604	14,601	14,601	14,597	9247	16,569	16,569	16,570	29,191	8525
Dep. Var. Mean	0.676	0.479	0.089	2.523	0.618	0.274	0.606	0.340	0.762	0.611

Notes: Fully exposed cohorts are born between 1983 and 1998; partially exposed cohorts are born between 1980 and 1982, while unexposed cohorts are born between 1971 and 1979. All regressions include a binary indicator for rural residence, a binary indicator for Muslim, maternal birth year fixed effects, survey wave fixed effects, and division fixed effects. Standard errors in parentheses are clustered at the maternal year of birth $\times$ rural levels. ***
** and * represent statistical significance at 0.01, 0.05, and 0.1 levels, respectively.
Source: Bangladesh Demographic and Health Surveys, 1996–97 through 2017–18.

Table 4
Effect of FSSSP on child mortality and anthropometric measures.

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Child Mortality (Birth Sample)						
	Age at Death < 1 month		Age at Death < 12 months		Age at Death < 60 months	
Fully Exposed X Rural	−0.00413**	−0.00323	−0.00911***	−0.00761***	−0.0124***	−0.0109***
	(0.00198)	(0.00200)	(0.00253)	(0.00255)	(0.00290)	(0.00288)
Partially Exposed X Rural	−0.00114	−0.000170	−0.00697	−0.00552	−0.0107**	−0.00937*
	(0.00260)	(0.00261)	(0.00497)	(0.00499)	(0.00507)	(0.00508)
Covariates		X		X		X
Observations	150,175	150,175	150,175	150,175	150,175	150,175
Dep. Var. Mean	0.047	0.047	0.067	0.067	0.080	0.080
Panel B: Anthropometric Measures (Child Sample)						
	Height for age	Weight for age	Weight for height	Stunted	Underweight	Wasted
Fully Exposed X Rural	0.152***	0.0774**	−0.00233	−0.0374***	−0.0243*	−0.00270
	(0.0406)	(0.0292)	(0.0193)	(0.0115)	(0.0124)	(0.00768)
Partially Exposed X Rural	0.135***	0.0587*	−0.0101	−0.0351**	−0.0242	0.00579
	(0.0394)	(0.0345)	(0.0230)	(0.0160)	(0.0165)	(0.00763)
N	29,974	29,974	29,976	29,974	29,974	29,976
Dep. Var. Mean	−1.683	−1.844	−1.076	0.397	0.467	0.141

Notes: Fully exposed cohorts are born between 1983 and 1998; partially exposed cohorts are born between 1980 and 1982, while unexposed cohorts are born between 1971 and 1979. Covariates include a binary indicator for rural residence, a binary indicator for Muslim, maternal birth year fixed effects, survey wave fixed effects, and division fixed effects. All regressions in Panel B include covariates. Standard errors in parentheses are clustered at the maternal year of birth × rural levels. ***.
** and * represent statistical significance at 0.01, 0.05, and 0.1 levels, respectively.
Source: Bangladesh Demographic and Health Surveys, 1996–97 through 2017–18.

Table 5
Effect of FSSSP on husband's education and labor supply.

	(1) Husband attended secondary or higher schooling	(2) Currently working	(3) Works in formal sector	(4) Husband works in formal sector
Fully Exposed X Rural	0.0913***	0.0188**	0.0118***	0.0555***
	(0.0133)	(0.00914)	(0.00388)	(0.0124)
Partially Exposed X Rural	0.0551***	−0.0381***	0.00644	0.0145
	(0.00955)	(0.0119)	(0.00533)	(0.00961)
Observations	32,534	32,745	32,706	32,504
Dep. Var. Mean	0.407	0.245	0.026	0.282

Notes: Fully exposed cohorts are born between 1983 and 1998; partially exposed cohorts are born between 1980 and 1982, while unexposed cohorts are born between 1971 and 1979. All regressions include a binary indicator for rural residence, a binary indicator for Muslim, maternal birth year fixed effects, survey wave fixed effects, and division fixed effects. Standard errors in parentheses are clustered at the maternal year of birth × rural levels. ***
** and * represent statistical significance at 0.01, 0.05, and 0.1 levels, respectively.
Source: Bangladesh Demographic and Health Surveys, 1996–97 through 2017–18.

the likelihood that a (fully exposed) woman reads a newspaper at least once a week by 9 percentage points, watches TV at least once a week by 10.9 percentage points, and listens to the radio at least once a week by 4.1 percentage points. We also find significant effects for partially exposed cohorts. To the extent that media exposes individuals to public health announcements and other health information, these findings suggest that improved knowledge may be an important pathway for explaining the health effects among children born to women exposed to the stipend program. This is consistent with prior work showing that the FSSSP improved women's knowledge of AIDS (Wu 2022).

7. Conclusion

We find that the 1994 Female Secondary School Stipend Program significantly increased education among women eligible for the stipend and improved the health of their children. Full immunization rates increased by 4.2 percentage points and any immunization rates by 1.4 percentage points among children of fully exposed women relative to the children of unexposed women. We also find

Table 6
Effect of FSSSP on fertility.

	(1) Age at first birth	(2) Number of living children	(3) Ideal number of children	(4) Has a daughter	(5) Ideal number of girls is zero
Fully Exposed X Rural	0.672***	−0.282***	−0.127***	−0.0230**	−0.0111
	(0.0940)	(0.0283)	(0.0158)	(0.0107)	(0.00876)
Partially Exposed X Rural	0.315***	−0.169***	−0.0527**	0.0133	0.0173*
	(0.105)	(0.0323)	(0.0205)	(0.0199)	(0.00887)
Observations	32,751	32,751	32,251	32,751	32,748
Dep. Var. Mean	17.823	2.258	2.303	0.721	0.191

Notes: Fully exposed cohorts are born between 1983 and 1998; partially exposed cohorts are born between 1980 and 1982, while unexposed cohorts are born between 1971 and 1979. All regressions include a binary indicator for rural residence, a binary indicator for Muslim, maternal birth year fixed effects, survey wave fixed effects, and division fixed effects. Standard errors in parentheses are clustered at the maternal year of birth × rural levels. ***
** and * represent statistical significance at 0.01, 0.05, and 0.1 levels, respectively.
Source: Bangladesh Demographic and Health Surveys, 1996–97 through 2017–18.

Table 7
Effect of FSSSP on early marriage, female autonomy, and decision making.

	(1) Age at first marriage or cohabitation	(2) Age at marriage < 18 years	(3) Decides on respondent's health care	(4) Decides on large household purchases	(5) Decides on visits to family or relatives	(6) Decision making index (sum)	(7) Can go to health center/hospital alone or with children
Fully Exposed X Rural	0.832***	−0.0845***	0.0261***	0.0356***	0.0447***	0.107***	−0.0133***
	(0.0815)	(0.0116)	(0.00960)	(0.00969)	(0.0125)	(0.0247)	(0.00388)
Partially Exposed X Rural	0.476***	−0.0333***	−0.0132	0.000406	−0.0171	−0.0301	−0.00273
	(0.0594)	(0.00955)	(0.0124)	(0.0147)	(0.0135)	(0.0299)	(0.00681)
Observations	32,751	32,751	26,763	26,765	26,758	26,756	24,132
Dep. Var. Mean	15.696	0.777	0.626	0.608	0.627	1.861	0.065

Notes: Fully exposed cohorts are born between 1983 and 1998; partially exposed cohorts are born between 1980 and 1982, while unexposed cohorts are born between 1971 and 1979. All regressions include a binary indicator for rural residence, a binary indicator for Muslim, maternal birth year fixed effects, survey wave fixed effects, and division fixed effects. Standard errors in parentheses are clustered at the maternal year of birth × rural levels. ***
** and * represent statistical significance at 0.01, 0.05, and 0.1 levels, respectively.
Source: Bangladesh Demographic and Health Surveys, 1996–97 through 2017–18.

significant improvements in antenatal, postnatal, and delivery care among exposed cohorts. Child health outcomes such as infant mortality rates, mortality rates among children younger than 5 years, and stunting also exhibit improvements. The stipend program is associated with reduced fertility, more formal sector work, more decision-making power, and greater media exposure. This suggests that income effects, quantity-quality tradeoffs, female autonomy, and improved knowledge may be potential pathways by which the program affects child health investments and health outcomes. Our findings imply that policies targeting girls' education have important spillover effects on public health and future generations.

Data availability

This study uses data from the Bangladesh Demographic and Health Surveys, which are available upon registration at: https://dhsprogram.com/.

CRediT authorship contribution statement

Md Shahjahan: Conceptualization, Methodology, Software, Formal analysis, Investigation, Data curation, Writing – original draft, Writing – review & editing, Visualization. **Giulia La Mattina:** Conceptualization, Methodology, Software, Formal analysis, Investigation, Data curation, Writing – original draft, Writing – review & editing, Visualization. **Padmaja Ayyagari:** Conceptualization, Methodology, Software, Formal analysis, Investigation, Data curation, Writing – original draft, Writing – review & editing,

Table 8
Effect of FSSSP on exposure to media.

	(1) Reads newspaper at least once a week	(2) Watches TV at least once a week	(3) Listen to radio at least once a week	(4) Sources of media exposure (Sum)
Fully Exposed X Rural	0.0903***	0.109***	0.0413***	0.240***
	(0.0110)	(0.00952)	(0.00997)	(0.0227)
Partially Exposed X Rural	0.0403***	0.0219*	0.0425***	0.106***
	(0.0111)	(0.0123)	(0.0103)	(0.0276)
Observations	32,727	32,740	32,744	32,709
Dep. Var. Mean	0.064	0.466	0.142	0.671

Notes: Fully exposed cohorts are born between 1983 and 1998; partially exposed cohorts are born between 1980 and 1982, while unexposed cohorts are born between 1971 and 1979. All regressions include a binary indicator for rural residence, a binary indicator for Muslim, maternal birth year fixed effects, survey wave fixed effects, and division fixed effects. Standard errors in parentheses are clustered at the maternal year of birth × rural levels. ***
** and * represent statistical significance at 0.01, 0.05, and 0.1 levels, respectively.
Source: Bangladesh Demographic and Health Surveys, 1996–97 through 2017–18.

Visualization, Supervision, Project administration.

Declaration of competing interest

Padmaja Ayyagari reports receipt of a fellowship for a senior female visiting scholar from the Tilburg University School of Economics and Management. All other authors have no conflicts of interest to report.

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jhealeco.2025.103024.

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