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ABSTRACT

The Impact of Maternal Education on Child Immunization: Evidence from Bangladesh

Vaccine-preventable diseases remain a significant public health concern in Bangladesh. We examine the role of maternal education in improving immunization rates among Bangladeshi children. We exploit the 1994 Female Secondary School Stipend Program (FSSSP), which significantly increased education among rural girls, to identify causal effects. 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 5.5 percent 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. Results from event study specifications and placebo tests support a causal interpretation of the impact of maternal education on child immunization.

JEL Classification: H52, I12, J13, O12

Keywords: maternal education, school stipend program, child immunization, Bangladesh

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Introduction

A large literature has documented that children born to better-educated mothers tend to be healthier than children born to mothers with lower levels of education in low-income countries. While much of the early work has focused on correlations, recent studies have attempted to identify the causal effect of maternal education on child health by relying on quasi-experimental approaches. Since the early 1990s, several low- and middle-income countries have introduced policies such as compulsory schooling laws, tuition subsidies, or school construction programs, which generate plausibly exogenous variation in access to education. Studies relying on such policies to estimate the causal impact of maternal education on child health find mixed results. [2] While several studies find improvements in infant or child mortality (Breierova and Duflo 2004, Grepin and Bharadwaj 2015, Wu 2022, Makate and Makate 2016, Chou et al. 2010) and in anthropometric measures such as birth weight, height-for-age, and weight-for-age (Hahn et al. 2018a, 2018b, Gunes 2015), some find no changes in child health (Keats 2018, Ali and Elsayed 2018, Zhang 2012). Further, it is unclear whether the difference in findings across these studies is due to variation in the margin of education that was targeted (e.g., primary vs secondary schooling) or due to contextual factors that are specific to the policy or country being studied. Appendix Table A1 describes the variation in the margin of education, type of education policy, country, health measures, and findings across key articles in this literature.

In addition to the mixed findings on child health, there is limited empirical evidence on the *pathways* by which maternal education influences child health. There are several channels through which maternal education may affect child health status. First, maternal education may increase

[2] 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.

household income due to higher labor earnings or positive assortative mating in the marriage market (see McCrary and Royer 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, Gertler 2004, Hahn et al. 2018a, 2018b). Second, evidence that female education affects fertility patterns while simultaneously improving child health suggests that a quality-quantity trade-off may be at play (Breierova and Duflo 2004, Keats 2018, Grepin and Bharadwaj 2015, Hahn et al. 2018a, 2018b). Third, 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. Prior literature has also examined the impact of maternal education on various health inputs, with mixed findings. Recent studies have found that maternal education increases prenatal care initiation (Gunes 2015), the likelihood of a doctor or midwife assisted delivery (Keats 2018), and child immunizations (Özer et al. 2018, Keats 2018). In contrast, Grepin and Bharadwaj (2015) find no evidence of changes in health inputs such as smoking, antenatal care, or child immunizations. Thus, there is a need for further research to understand the role of health inputs.

In this study, we examine the relationship between maternal education and childhood immunizations in Bangladesh. To identify causal effects, we rely on the Female Secondary School Stipend Program (FSSSP) which was introduced in 1994 and provides a cash stipend to girls in rural areas enrolled in secondary school (grades 6-11). Several studies have shown that the FSSSP significantly increased years of schooling among girls eligible for the stipend (Hahn et al. 2018a, 2018b, Fuwa 2006, Hong and Sarr 2012, Khandker et al. 2021, Khandker et al. 2003, Wu 2022, Raynor 2006, Sara and Priyanka 2022, Schurmann 2009).[3] In terms of child health, there is evidence of a reduction in infant and child mortality (Wu 2022) and improvements in height-for-age, weight-for-age and hemoglobin levels but not in birthweight or anemia (Hahn et al. 2018a,b). Evidence of increases in institutional delivery, antenatal care use, prenatal care use, and knowledge of AIDS suggests that these might be important pathways by which maternal education affects child health in Bangladesh (Hahn et al. 2018a, b, Wu 2022).

However, to our knowledge, no study has examined the impact of this stipend program on child immunizations. Using a difference-in-differences approach based on differential exposure to the FSSSP by cohort of birth and rural residence, we find that the rate of full immunization increases by 4 percentage points and the rate of any immunization increases by 1.6 percentage points among children born to mothers exposed to FSSSP for 5 years, relative to children of mothers who were not exposed. In contrast, immunization rates did not change among children born to mothers exposed to FSSSP for only 2 years.

We do not find significant differences in the effect of FSSSP on immunizations by the gender of the child or by household wealth. However, among wealthier households, the increase in full immunization rates is higher by 7.5 percentage points for female children compared to male children. The greater investment in female children among wealthier households suggests that investment in maternal education may lead to narrower gender disparities in the next generation, but only for a relatively wealthy subpopulation.

[3] 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).

Our study contributes to the literature in several ways. First, we add another piece of evidence to the limited and conflicting literature on this topic. Specifically, previous research has shown that policies targeting primary education increase child immunizations (Keats 2018, Özer 2018) but policies targeting secondary education have no impact on child immunizations (Grepin and Bharadwaj 2015). In contrast, we find that the FSSSP, which targeted secondary education in Bangladesh, significantly increased child immunizations. Second, unlike previous studies which examined policies that increased education among both boys and girls, the FSSSP only targeted girls. While we cannot explicitly disentangle the effect of maternal education from that of paternal education due to assortative mating (Hahn et al. 2018b), our results suggest that policies that only target girls' education can have significant effects on child health.

Third, there is limited evidence on the causal effect of maternal education on child immunization in Bangladesh and other South Asian countries.[4] Bangladesh is an interesting setting because it has seen tremendous improvements in girls' education (Figure 1) and child immunization rates (Figure 2) in recent years. Figure 1 shows that girls' secondary school enrollment rate was about 13 percentage points lower than boys' in 1990. Enrollment among both genders increased significantly following the introduction of the FSSSP in 1994 but by the year 2000, girls' enrollment had surpassed that of boys. Panel A of Figure 2 shows that full immunization rates among children increased by 31.4 percentage points among rural residents and by 19.2 percentage points among urban residents between 1993 and 2018 in Bangladesh. Moreover, immunization rates among female children increased more than among male children

[4] There is, however, an extensive literature documenting a positive correlation between maternal education and child immunizations in Bangladesh. For example, see Rahman and Obaida-Nasrin (2010), Sheikh et al. (2018), Sarker et al. (2019), Banerjee et al. (2021) and Acharya et al. (2022)

during this period (Panel B, Figure 2). Our study suggests that the FSSSP may have played a role in explaining these aggregate trends.

Fourth, we examine whether the effect on child immunization differs by gender and wealth. A large literature has documented the prevalence of son preference in Bangladesh, defined as parents' desire to have at least one son, which often manifests as worse investments in daughters compared to sons (Chen et al. 1981; Houssain and Glass 1988; Chowdhury and Bairagi 1990; Rahman and DaVanzo 1993; Jayachandran and Pande 2017).[5] Earlier research has shown patterns of intrahousehold allocation and fertility stopping behavior that are consistent with son preference, including evidence that female children have lower nutritional intake and are more likely to be malnourished compared to male children; parents are less likely to buy medications and pursue medical care when daughters are ill compared to sons; and the number of sons is negatively related to subsequent fertility among families who have at least one daughter and can access contraceptives (Chen et al. 1981; Houssain and Glass 1988; Chowdhury and Bairagi 1990; Rahman and DaVanzo 1993). More recent studies, however, provide mixed evidence about gender inequities within the household, fertility preferences, and fertility decisions. D'Souza and Tandon (2019) show that the relationship between income and calories is similar for girls and boys in rural Bangladesh while Brown et al. (2021) find that female children and male children are allocated similar shares of the total household budget. Our study contributes to this literature by examining whether increases in maternal education leads to greater investment in the health of female children. In addition, we examine differences in the impact of FSSSP by household wealth and the intersection of gender and wealth. Halder and Kabir (2008) show that, despite the availability of free immunizations, Bangladeshi children born to households in the richest wealth quintile are

[5] The literature on son-preference in South Asian countries is extensive. We refer to Jayachandran and Pande (2017) for a review.

five times more likely to be fully vaccinated compared to children born to households in the poorest wealth quintile. Our findings suggest that increases in maternal education reduce gender disparities in child immunizations, but only for wealthier households.

Fifth, we provide some evidence of a "dose-response" effect of education programs. Specifically, mothers who were eligible for a 5-year stipend under the FSSSP received more education and were more likely to immunize their children compared to ineligible mothers. In contrast, mothers who were only eligible for a 2-year stipend saw modest increases in education and no significant increases in child immunizations compared to ineligible mothers.

The 1994 Female Secondary School Stipend program (FSSSP) in Bangladesh

Bangladesh introduced the Female Secondary School Stipend Program (FSSSP) in 1994 with the goal of reducing gender inequality in secondary education (grades 6-10, ages 11-15).[6] 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.

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)

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

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). Figure 3 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, 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.[7] The stipend was expected to cover up to 50% of direct educational expenses (textbooks, uniforms, examination fees, etc.) and was paid directly to a bank account in the girl's name in two equal annual installments. 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).

[7] Authors' calculations based on income data from the 1995 Household Expenditure Survey and exchange rate information from the Bangladesh Bank 2008-2009.

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).

Data

We use publicly available data from the 1993-94 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). [8] The BDHS collects the childhood immunization history of all surviving children aged 59 months or younger using face-to-face interviews and records on vaccine cards. The survey includes the following questions: *i) Do you have a card or other paper/document where (NAME)'s vaccinations are written? ii) Did you ever have a vaccination card for (NAME)? iii) May I see the card or other document where (NAME) 's vaccinations are written down.* If the mother responds *"No,"* the child's vaccination status is 'unvaccinated.' If the response is *"Vaccination date on card"/"Reported by mother"/"Vaccination*

[8] For the 1993-94 through 2004 waves women aged 10 to 49 years were surveyed and for the 2007 through 2017-18 waves women aged 15 to 49 years were surveyed. We restrict our sample to women aged 15 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.

marked on the card," then the child is considered to be 'vaccinated.' We exclude observations for which the response is *"Don't know."*

The WHO recommends four vaccines (eight doses) for children in Bangladesh – a vaccine against tuberculosis (BCG), a vaccine against Diphtheria-Pertussis-Tetanus (DPT), a vaccine against Polio (OPV), and a vaccine against Measles. Table 1 (based on Table 1 of Sarker et al. 2019) presents the recommended vaccination schedule for all four vaccines. We use a binary indicator for being fully immunized and a binary indicator for receiving any immunization as dependent variables. Vaccination status is classified as 'fully immunized' if the child receives all eight recommended vaccine doses: one dose of BCG, three doses of DPT, three doses of OPV, and one dose of the Measles vaccine. Vaccination status is classified as 'any immunization' if the child receives any one of the eight recommended vaccine doses. In Appendix Table A2, we present results separately for each of the eight vaccine doses. We also use the mother's years of education and a binary indicator for completing secondary school or higher as dependent variables.

Our main analysis is limited to women born between 1975 and 1988 who are at least 15 years old at the time of the survey. We exclude birth cohorts born before 1975 since the period between 1971 and 1974 was characterized by substantial change and uncertainty in Bangladesh – the country gained independence in 1971 and experienced a famine in 1974 (Sen 1981, Hernández-Julián et al. 2014). However, our main results are robust to including all women born between 1971 and 1988 (see Table 7). We also restrict our sample to children aged between 12 and 59 months since children younger than 12 months are not old enough to receive the full set of recommended vaccine doses. For the analysis of maternal education, we do not restrict the sample to women with children aged 12 to 59 months. Since education may affect fertility, this allows us to avoid concerns about sample selection bias. However, our main estimates for education are robust to using the child immunization sample (results available on request). Our final sample consists of 18,345 child-mother-year observations for the child immunization analysis and 42,685 women-year observations for the maternal education analysis.[9]

Table 2 presents summary statistics for the full sample and separately for the three cohort groups described above. Panel A presents the statistics for the maternal education sample and Panel B for the child immunization sample. On average, women have 5 years of education and about 43.7% have completed secondary or higher education. Women who were fully exposed to FSSSP have 1.7 more years of schooling than women who were not exposed to FSSSP. Women who were partially exposed to FSSSP have 0.7 more years of schooling than unexposed women. About 80% of all children in the child sample are fully immunized. Children born to women fully exposed to FSSSP are 10.8 percentage points more likely to be fully immunized than children born to unexposed women. Children born to women partially exposed to FSSSP are 5.6 percentage points more likely to be fully immunized than children born to unexposed women. Approximately 90% of the child sample is Muslim and 69.7% reside in a rural area.

Methodology

We follow the difference-in-differences approach used by Hahn et al. (2018a, 2018b) and Wu (2021) to estimate the causal effect of the FSSSP on child immunization. This approach exploits the plausibly exogenous variation in exposure to the FSSSP by birth cohort and rural residence. Our main regression equation is:

[9] We use the children's recode files of the DHS to construct the sample for the immunization analysis and the individual recode files of the DHS to construct the sample for the education analysis.

$$Y_{ijt} = \beta_0 + \beta_1 Fully\ Exposed_{ij} \times Rural_{ijt} + \beta_2 Partially\ Exposed_{ij} \times Rural_{ijt} +$$
$$\beta_3 Rural_{ijt} + \beta_4 Fully\ Exposed_{ij} + \beta_5 Partially\ Exposed_{ij} + \beta_6 X_{ijt} + \varepsilon_{ijt} \quad (1)$$

Where Y_{ijt} is an indicator for the full (or any) immunization status of child i born to mother j who was interviewed in survey year t. In the analysis of maternal education, the dependent variable is the years of schooling or a binary indicator that equals one if mother j has completed secondary school. $Rural_{ijt}$ is a binary indicator for residing in a rural area in year t.[10] $Fully\ Exposed_{ij}$ is one if child i's mother was born in 1983 or later, and zero otherwise. $Partially\ Exposed_{ij}$ is one if child i's mother was born between 1980 and 1982 (inclusive), and zero otherwise. The reference category includes mothers born in 1979 or earlier (not exposed to FSSSP). X_{ijt} denotes a vector of covariates including maternal age fixed effects, a binary indicator for being Muslim (the reference group includes all other religions), and division fixed effects.[11] Standard errors are clustered at the mother and maternal year of birth levels using a two-way wild cluster bootstrap method (Cameron and Miller 2015).

The main parameters of interest in equation (1) are the coefficients on the interaction terms, β_1 and β_2. β_1 estimates the rural-urban difference in the immunization rates of children born to

[10] Ideally, we would use information on the mother's residence in a rural or urban area during her secondary schooling years (ages 11-15) since eligibility for the FSSSP is based on rural residence at the time of schooling. Unfortunately, this information is not available in the BDHS. However, the internal migration rate in Bangladesh, is quite low. According to Bangladesh Population and Housing Census 2011 the migration rate from rural to urban areas was 4.29% whereas the urban to rural areas migration rate was 0.36% (Bangladesh Bureau of Statistics 2012a). This suggests that using current rural residence is unlikely to substantially bias our estimates. Further, to the extent that migrants to urban areas have greater access to healthcare services or are more likely to vaccinate their children, our estimates would be a lower bound of the effect of FSSSP on child immunization.

[11] The number of administrative divisions in Bangladesh changed during our study period. At the time of the 1993-94 wave, Bangladesh was divided into five administrative divisions (Barisal, Chittagong, Dhaka, Khulna, and Rajshahi). Sylhet separated from Chittagong in 1995, Rangpur separated from Rajshahi in 2010, and Mymensingh separated from Dhaka in 2015. To have consistent regions across waves, we use the original five divisions for the maternal education analysis. For the child immunization analysis, we use data from 1996-97 onwards since respondents in the 1993-94 wave do not meet our sample selection criteria. Therefore, for the immunization analysis, we use six divisions – the original divisions and Sylhet.

fully exposed mothers relative to children born to unexposed mothers. β_2 estimates the rural-urban difference in the immunization rates of children born to partially exposed mothers relative to children born to unexposed mothers.

Identification is based on the assumption that, had the FSSSP not been introduced, child immunization rates over maternal cohorts would have evolved similarly in rural and urban areas. As this assumption is inherently untestable, we examine rural-urban differences in child immunizations by maternal birth cohorts using an event study regression. The event study regression replaces the cohort dummies in equation (1) with a full set of maternal birth year fixed effects (1979 is the reference group). If changes in child immunizations 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 addition, we use a placebo test to assess our identifying assumption. We restrict the placebo analysis to cohorts born between 1956 and 1979, who were not eligible for the stipend program. We define the fully exposed placebo cohort as women born between 1973 and 1979 and the partially exposed placebo cohort as women born between 1970 and 1972 (women born between 1956 and 1969 form the reference group). We then re-estimate equation (1) using these placebo cohorts in place of our original treatment and control groups. Since none of the mothers belonging to the placebo cohorts were eligible for FSSSP, we should not find any significant differences in education or immunization rates between urban and rural residents and by birth cohorts. If instead, we find significant coefficients on the interaction terms, this would suggest that there are unobserved factors that are correlated with rural residence and birth cohort and our outcomes.

Results

We first present difference-in-differences estimates for the education outcomes in Table 3. We find that the difference in years of schooling between rural and urban women is higher by 1.1 years for fully exposed cohorts relative to unexposed cohorts and by 0.6 years for partially exposed cohorts relative to unexposed cohorts (Column 2, Table 3). We also find that the rural-urban difference in the probability of completing secondary or higher schooling for fully and partially exposed cohorts relative to unexposed cohorts is higher by 12.4 percentage points and by 6.1 percentage points, respectively (Column 4, Table 3). These results are consistent with the findings of Hahn et al. (2018a, b) and Wu (2022), although our point estimates are slightly different likely because we use a narrower set of birth cohorts.[12]

Table 4 presents the difference-in-differences estimates for child immunizations. 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 percentage points for children born to fully exposed women relative to those born to unexposed women (Column 2, Table 4). This is equivalent to an increase of 5.5 percent relative to the sample mean for the treatment group before treatment (unexposed cohorts in rural areas). The corresponding estimate for any immunization is 1.6 percentage points (Column 4, Table 4). In contrast, the coefficient on the interaction between the rural indicator and the partially exposed indicator is not statistically significant for either

[12] We find qualitatively similar results when using the child immunization sample to estimate the impact on maternal education. The difference-in-differences estimate for years of schooling is 0.91 (p-value<0.001) for fully exposed cohorts and 0.61 (p-value=0.002) for partially exposed cohorts. The difference-in-differences estimate for the indicator for secondary or higher schooling is 10.4 (p-value<0.001) for fully exposed cohorts and 6.5 (p-value=0.02) for partially exposed cohorts.

outcome. In Appendix Table A2, we present results for each individual vaccine dose and for the vaccine index described above. We find significant increases in all vaccine doses for the children of women eligible for a 5-year stipend but not for the children of women eligible for a 2-year stipend.

Identification

The results from the event study regressions for education and child immunization are presented in Figures 4 and 5, respectively. The graphs plot the coefficient 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 the binary indicator for completing secondary or higher schooling although the change between fully and partially exposed cohorts is not as sharp as for years of schooling. 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. The full set of interaction coefficients and confidence intervals are also presented in Appendix Tables A3 and A4. For all four regressions, the Wild cluster bootstrap confidence intervals are quite wide and none of the interaction coefficients are statistically significant. We do not present confidence intervals in Figures 3 and 4 since the wide confidence intervals mask the changes in education and immunizations for fully and partially exposed cohorts. Event study graphs including the confidence intervals are available on request.

Table 5 presents results from the placebo analysis as a further check of our identifying assumption. Panel A presents the results for maternal education and Panel B for child immunizations. The difference-in-differences estimates are not statistically significant or are the wrong sign in these placebo regressions, suggesting that our identifying assumption is likely to be satisfied. In other words, there are no unobserved factors that differentially affect birth cohorts and rural residents and the difference-in-differences estimates can be interpreted as the causal effect of the stipend program.

Robustness Checks and Alternative Approaches to Inference

Table 6 presents the results from various robustness checks. In Panel A, we narrow the sample to include only children of mothers born between 1978 and 1988. This ensures that our main estimates are not being driven by comparisons between distant generations who might differ in unobserved ways. The estimates are less precise, as might be expected given the smaller sample, but they are similar in magnitude to our main estimates. As before, we find significant effects for fully exposed cohorts but not for partially exposed cohorts. In Panel B, we broaden the sample to include children of mothers born between 1971 and 1988. These are the cohorts used by Hahn et al. (2018a, b) and Wu (2022). The difference-in-differences estimates are more precise and slightly larger in magnitude when we use the broader sample.

In Panel C, we restrict the sample to the oldest child under 5 of each woman born between 1975 and 1988 and aged 15 years or older at the time of the survey. Hahn et al. (2018a) find that the FSSSP decreased the number of children born to eligible women, which implies that the FSSSP affects the composition of our main sample. Restricting the sample to the oldest child allows us to examine the extent to which our main estimates are explained by such compositional changes. We find statistically significant and larger estimates for full and any immunizations - 6.5 percentage points and 4 percentage points, respectively.

In Panel D, we address selection into motherhood. Hahn et al (2018a) show that the FSSSP led eligible women to delay the birth of their first child. In our sample, 95% of women have their first child by age 24, and Table A5 shows that the FSSSP did not change the probability of having at least one birth among women who are older than 24. Therefore, we restrict the sample to women who are older than 24 at the time of the survey so that our estimates of the impact of the FSSSP on child immunization are not confounded by differences in mother's age at first birth. We find a statistically significant increase in the full immunization rate (3 percentage points) among children of fully exposed women but not among children of partially exposed women. When we restrict the sample to the oldest child under 5 of women older than 24, the coefficient estimates for full immunization become significantly larger in magnitude (Panel E). Although these results should be interpreted with caution due to the small sample size, they suggest that, in the absence of changes in fertility behavior, the impact of the FSSSP on child immunization rates may be larger. Overall, our conclusion that the FSSSP increased full immunization rates among the children of women eligible for a 5-year stipend remains robust to these alternative specifications.

Finally, following the recommendations in MacKinnon et al. (2022), we consider alternative approaches for inference (Table 7). In the main results, we cluster the standard errors two-ways at the mother and mother's year of birth levels using wild cluster bootstrap method (Panel A, Table 7). Given the structure of the data and the research design used in this paper, we consider three other clustering dimensions. First, as treatment varies according to the mother's birth year, we consider one-way clustering at the mother's birth year using a wild cluster bootstrap method (Panel B, Table 7). Second, we consider clustering one-way at the division-mother's year of birth using traditional clustering to account for the fact that the implementation of the reform may have differed across divisions (Panel C, Table 7). Finally, as we have multiple observations per mother, we consider two-way clustering at the mother and division-mother's year of birth (Panel D, Table 7). The difference-in-differences estimate for full immunizations among children of fully exposed women is statistically significant at the 5% level using all three alternative approaches while the corresponding estimate for children of partially exposed women is insignificant across all three alternative approaches. In summary, our conclusions remain unchanged.

Heterogeneity by gender and wealth

One of the goals of the FSSSP was to improve the social status of women in the community and reduce gender gaps (Fuwa 2006). In this section, we explore whether such effects persist to the next generation through health investments in daughters. Panel A of Table 8 presents results from a triple difference regression, in which the third difference is based on the gender of the child. We find that the increase in full immunizations is 3.7 percentage points higher for female children compared to male children, but this difference is not statistically significant. In Panel B of Table 8, we explore heterogeneity by wealth levels since there is evidence that child immunization rates differ by household wealth (Halder and Kabir 2008). The sample for this analysis is smaller because information on household wealth is not available in the DHS surveys collected before 2004.[13] We use a binary indicator, labeled "poor", which equals one if households belong to the lowest two quintiles of the wealth distribution and zero if they belong to the top three wealth quintiles.[14] We find that wealthier households are more likely to vaccinate their children in

[13] Our main estimates for full immunization are robust to restricting the sample to surveys with information on household wealth. The estimates for any immunization for this restricted sample are qualitatively similar to our main estimates but are less precisely estimated. Results are available upon request.

[14] The DHS calculates a continuous scale of household wealth using principal component analysis over a set of variables that includes ownership of assets, house characteristics, and type of drinking water, among others; the index is standardized to have mean zero and standard deviation one, and quintiles are calculated based on the distribution of the standardized measure (https://dhsprogram.com/topics/wealth-index/).

response to the FSSSP, but these differences are not statistically significant.[15] Next, we estimate the triple difference regression based on the child's gender separately for "poor" vs "wealthy" households. These results are presented in Panel C of Table 8. We find that the triple difference estimate is statistically significant for the "wealthy" subsample but not for the "poor" subsample. Specifically, the FSSSP induced increase in full immunization rates is higher by 7.5 percentage points for female children compared to male children in wealthier households but not in poorer households. In other words, increases in maternal education help reduce gender disparities in health investments in the next generation but only for wealthier households.

Conclusion

We find that the 1994 Female Secondary School Stipend Program significantly increased education among women eligible for the stipend and increased immunization rates among their children. Specifically, the probability of full immunization increased by 4 percentage points (or 5.5 percent) among children of eligible women relative to the children of ineligible women. Such an increase represents a meaningful step toward achieving the Sustainable Development Goal to fully immunize 95 percent of children by 2030 (UNICEF 2021).

Our findings suggest that, consistent with the allocative efficiency hypothesis, one of the pathways through which maternal education affects child health is a more efficient allocation of health inputs, specifically, immunizations. Previous literature has found that programs targeting primary education increase child immunizations (Özer 2018, Keats 2018) but not programs targeting secondary education (Grepin and Bharadwaj 2015). Our results suggest that these

[15] Although an increase in household wealth is a potential mechanism for the estimated effect of the FSSSP on childhood immunizations, Appendix Table A5 shows that the FSSSP did not have a statistically significant impact on the probability that women live in a household in the lowest two quintiles of the wealth distribution.

differences may be related to other contextual factors specific to the country or policy being studied rather than the margin of education targeted by the programs.

Our findings also suggest that policies targeting girls' education have important spillover effects on public health and future generations. Vaccinations are some of the most effective means of protecting children from life-threatening diseases, especially in low-income countries where access to affordable health care is limited. Although Bangladesh has made tremendous progress in child immunizations over the past four decades, gaps remain (Jamil 1999, Halder and Kabir 2008, Sarkar et al. 2015). Moreover, pockets of low coverage may lead to dangerous outbreaks of vaccine-preventable diseases: Bangladesh experienced 82 outbreaks of measles in 2019 alone (UNICEF 2021). Along with investment in public health resources, educating girls may be an effective approach to addressing these concerns.

However, it is important to note that such policies are not effective under all circumstances. We find no impact on immunizations for children born to partially exposed cohorts, suggesting that small changes in maternal education do not have beneficial effects on the next generation. We also find evidence that maternal education reduces gender disparities among non-poor households only, suggesting that resource constraints continue to be an important barrier to vaccinations in Bangladesh.

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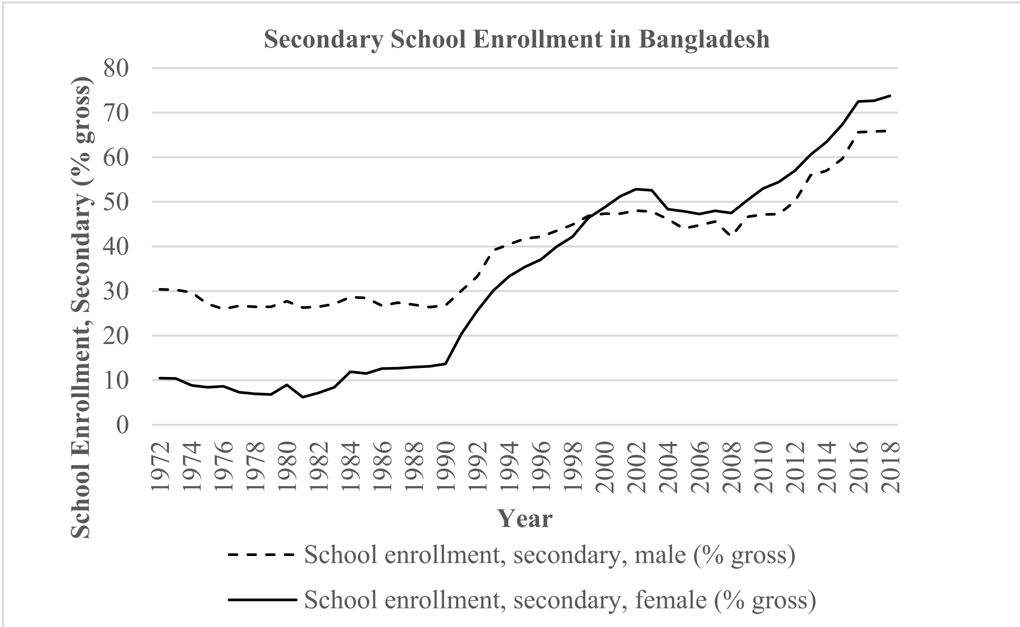
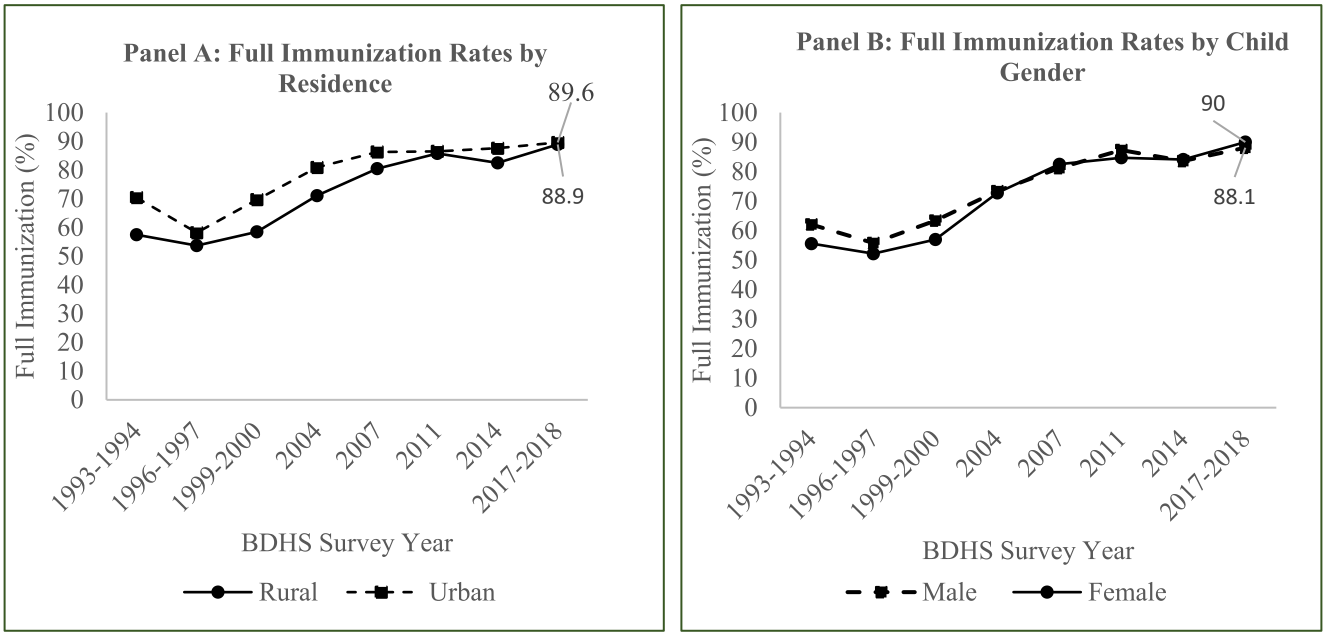
Figure 1: Gender Differences in School Enrollment in Bangladesh



Notes: Secondary school enrollment by gender, 1972-2018. The solid line is for females; the dashed line is for males. The gross enrollment rate is computed by dividing the number of students in grades 6-10 by the relevant population in that age group (ages 11-15).

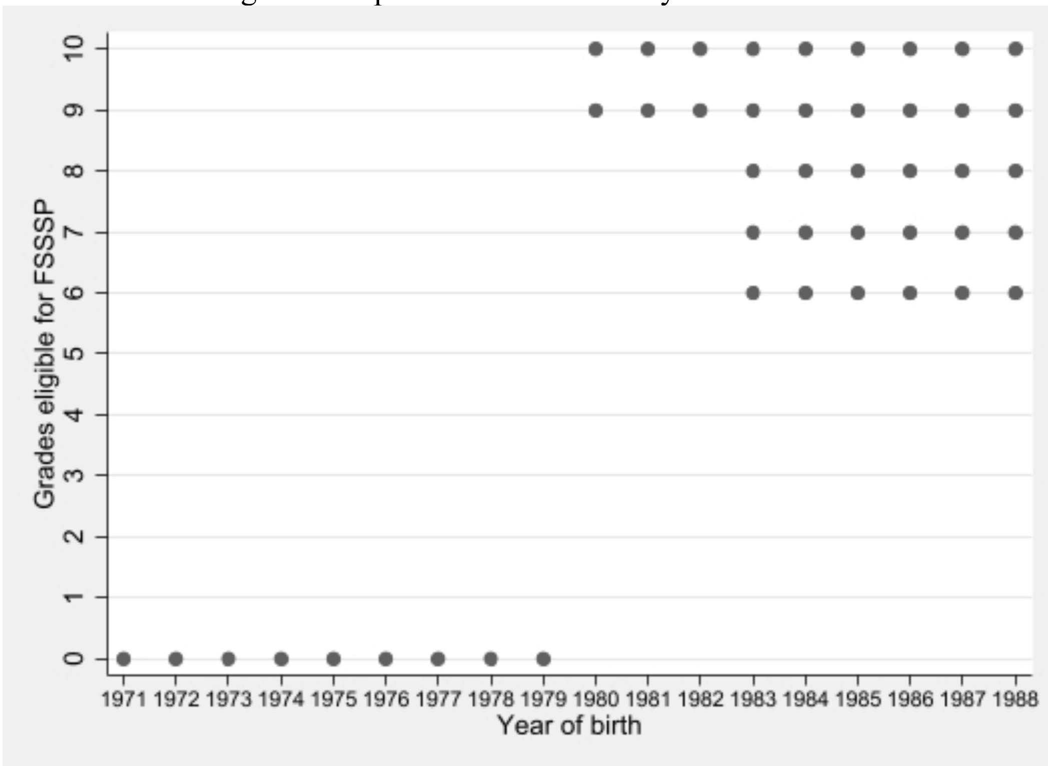
Source: Bangladesh Bureau of Educational Information and Statistics (BANBEIS), 2018.

Figure 2: Trends in Child Immunization Rates by Residence and Child Gender



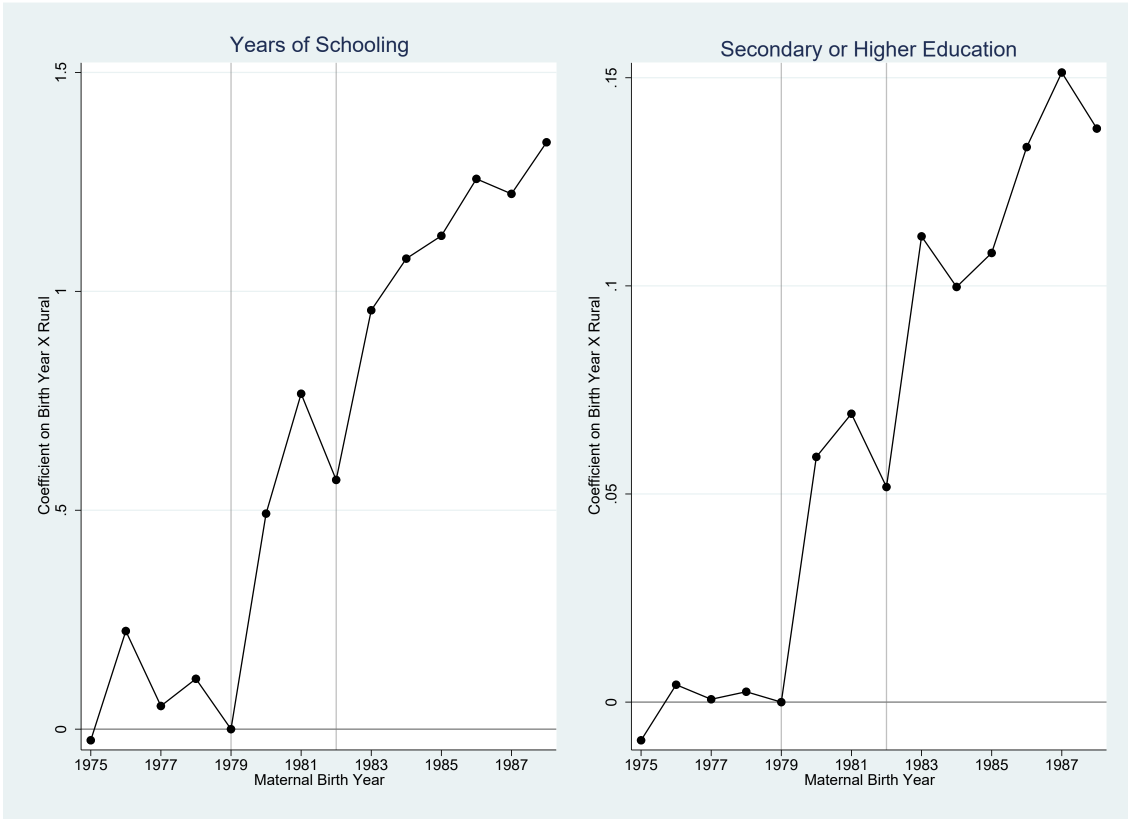
Notes: Graphs present full immunization rates (%) among children aged 12–59 months in Bangladesh. Full immunization indicates that the child has received all eight doses of WHO recommended vaccines.
Source: Bangladesh Demographic and Health Surveys, 1993-94 through 2017-18.

Figure 3: Exposure to the FSSSP by Birth Cohort



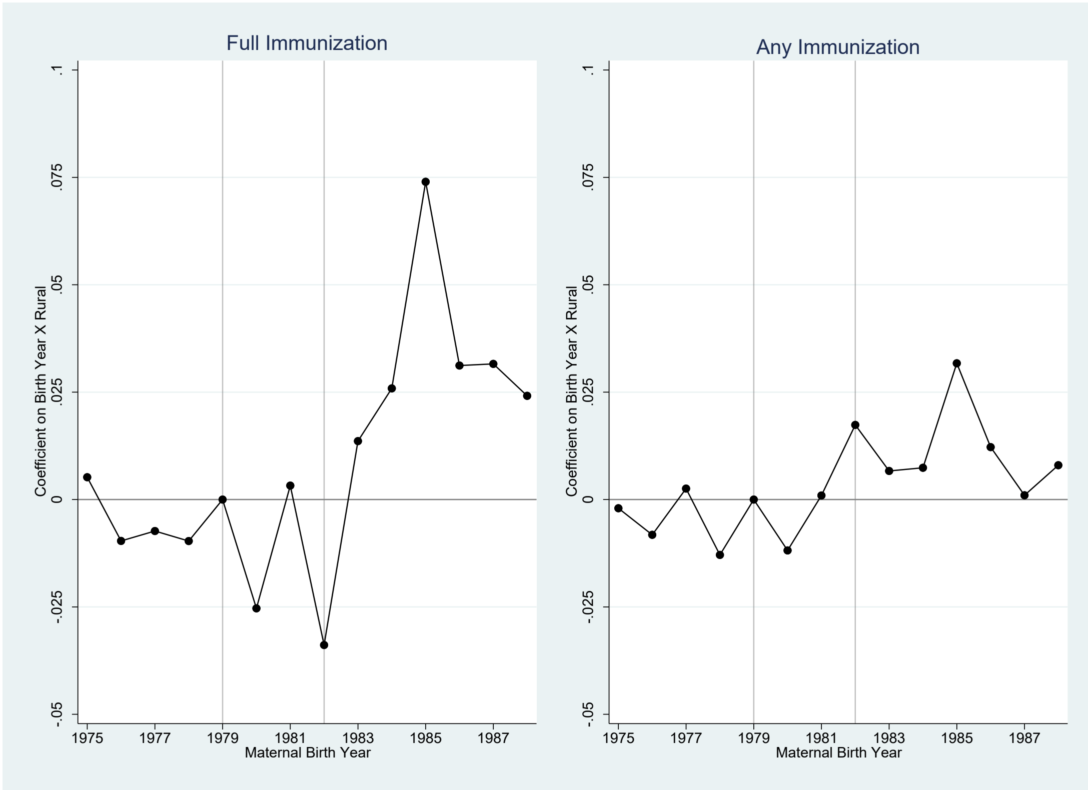
Notes: Mothers born after 1983 were eligible for 5 years from 1994 when they were in grade 6, while mothers born between 1980 and 1982 were eligible for 2 years. Mothers born before 1980 were not eligible as they were already in grade 10 in 1994. Mothers in grades 7 in 1994 and 8 in 1995 did not become 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.

Figure 4: Event Study Graphs for Maternal Education



Notes: Graphs present interaction coefficients 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, maternal age fixed effects, and division fixed effects. The sample includes cohorts born between 1975 and 1988 (reference birth year 1979).
Source: Bangladesh Demographic and Health Surveys, 1993-94 through 2017-18.

Figure 5: Event Study Graphs for Child Immunizations



Notes: Graphs present interaction coefficients from an event study regression of child immunization on interactions between rural dummy and maternal birth year fixed effects, a binary indicator for rural, maternal birth year fixed effects, a binary indicator for Muslim, maternal age fixed effects, and division fixed effects. The sample includes cohorts born between 1975 and 1988 (reference birth year 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.
Source: Bangladesh Demographic and Health Surveys, 1996-97 through 2017-18.

Table 1: The Expanded Program on Immunizations (EPI) Schedule in Bangladesh

Diseases	Vaccine	Recommended Age
Childhood tuberculosis (TB)	BCG	At birth/0 day
Diphtheria/ Pertussis/Tetanus	DPT 1	42 days
	DPT 2	70 days
	DPT 3	98 days
Poliomyelitis	OPV 1	42 days
	OPV 2	70 days
	OPV 3	98 days
Measles	Measles	273 days

Notes: BCG=Bacillus Calmette-Guérin; DPT= Diphtheria-Pertussis-Tetanus; OPV= Oral Polio Vaccine

Source: WHO South-East Asia: Expanded Programme on Immunization (EPI) REGIONAL FACT SHEET 2017.

Table 2: Study Summary Statistics

	(1)	(2)	(3)	(4)	(5)	(6)
	Full Sample	Fully Exposed Cohorts	Partially Exposed Cohorts	Unexposed Cohorts	Difference between Fully and Unexposed Cohorts	Difference between Partially and Unexposed Cohorts
Variables	Mean (Std. dev.)	Mean (Std. dev.)	Mean (Std. dev.)	Mean (Std. dev.)	Mean (*t* statistic)	Mean (*t* statistic)
Panel A: Maternal Education Sample						
Years of Schooling	5.010	5.886	4.856	4.140	1.746***	0.716***
	(4.159)	(3.863)	(4.187)	(4.260)	(39.24)	(12.99)
Secondary or Higher Education	0.437	0.542	0.417	0.332	0.210***	0.0848***
	(0.496)	(0.498)	(0.493)	(0.471)	(39.38)	(13.58)
(Maternal) Age	27.682	25.653	27.905	29.775	-4.122***	-1.870***
	(6.672)	(5.021)	(6.467)	(7.640)	(-58.67)	(-19.87)
Rural	0.667	0.652	0.660	0.687	-0.0342***	-0.0266***
	(0.471)	(0.476)	(0.474)	(0.464)	(-6.62)	(-4.37)
Muslim	0.899	0.902	0.898	0.896	0.00598	0.00194
	(0.301)	(0.297)	(0.302)	(0.305)	(1.81)	(0.49)
Observations	42,685	17,422	9,377	15,886		
Panel B: Child Immunization Sample						
Full Immunization	0.808	0.856	0.804	0.748	0.108***	0.0561***
	(0.394)	(0.351)	(0.397)	(0.434)	(16.52)	(6.61)
Any Immunization	0.967	0.977	0.967	0.954	0.0232***	0.0124**
	(0.178)	(0.148)	(0.179)	(0.209)	(7.76)	(3.11)
(Maternal) Age	25.281	24.496	25.392	26.212	-1.716***	-0.820***
	(4.911)	(4.090)	(5.239)	(5.456)	(-21.51)	(-7.56)
Rural	0.697	0.680	0.690	0.723	-0.0429***	-0.0325***
	(0.460)	(0.466)	(0.462)	(0.448)	(-5.56)	(-3.55)
Muslim	0.907	0.909	0.906	0.905	0.00390	0.000710
	(0.290)	(0.287)	(0.292)	(0.293)	(0.80)	(0.12)
Observations	18,345	8,029	4,022	6,294		

Notes: Fully exposed cohorts are born between 1983 and 1988, partially exposed cohorts are born between 1980 and 1982, and unexposed cohorts are born between 1975 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. ***, ** 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).

Table 3: Difference-in-Differences Estimates of the Impact of FSSSP on Maternal Education

Outcome variables	Years of Schooling		Secondary or Higher Education	
	(1)	(2)	(3)	(4)
Fully Exposed × Rural	1.140***	1.098***	0.126***	0.124***
	[0.974, 1.307]	[0.919, 1.266]	[0.105, 0.148]	[0.103, 0.145]
Partially Exposed × Rural	0.543***	0.546***	0.059***	0.061***
	[0.357, 0.736]	[0.346, 0.779]	[0.0508, 0.0684]	[0.0495, 0.0747]
Fully Exposed	0.919***	0.954***	0.120***	0.118***
	[0.472, 1.383]	[0.547, 1.450]	[0.0727, 0.169]	[0.0772, 0.166]
Partially Exposed	0.293	0.272	0.040*	0.035*
	[-0.169, 0.838]	[-0.202, 0.745]	[-0.00994, 0.0999]	[-0.00735, 0.0860]
Rural	-2.448**	-2.378**	-0.228***	-0.225**
	[-2.633, -2.598]	[-2.493, -2.204]	[-0.233, -0.223]	[-0.322, -0.214]
Covariates		X		X
Observations	42,685	42,685	42,685	42,685
Dep. Var. Mean	5.010	5.010	0.437	0.437

Notes: The sample includes women born between 1975 and 1988. Fully exposed cohorts are born between 1983 and 1988, partially exposed cohorts are born between 1980 and 1982, and unexposed cohorts (reference group) are born between 1975 and 1979. Covariates include a binary indicator for Muslim, maternal age fixed effects, and division fixed effects. Standard errors are clustered at the mother and maternal year of birth levels using a two-way Wild cluster bootstrap approach. ***, ** and * represent statistical significance at 0.01, 0.05, and 0.1 levels, respectively and 95% confidence intervals are in square brackets.

Source: Bangladesh Demographic and Health Surveys, 1993-94 through 2017-18.

Table 4: Difference-in-Differences Estimates of the Impact of FSSSP on Child Immunizations

Outcome variables	Full Immunization		Any Immunization	
	(1)	(2)	(3)	(4)
Fully Exposed × Rural	0.0598***	0.0401***	0.0175***	0.0159***
	[0.0384, 0.0846]	[0.0193, 0.0638]	[0.00651, 0.0299]	[0.00522, 0.0286]
Partially Exposed × Rural	-0.000917	-0.0119	0.00685	0.00680
	[-0.0277, 0.0325]	[-0.0300, 0.0176]	[-0.0148, 0.0280]	[-0.0123, 0.0275]
Fully Exposed	0.0643***	0.0990***	0.0102*	0.0138**
	[0.0315, 0.103]	[0.0577, 0.151]	[-0.000389, 0.0222]	[0.00324, 0.0272]
Partially Exposed	0.0542**	0.0718**	0.00687	0.00868*
	[0.0159, 0.0964]	[0.0188, 0.134]	[-0.00424, 0.0182]	[-0.00144, 0.0208]
Rural	-0.0781***	-0.0513**	-0.0260**	-0.0226***
	[-0.0851, -0.0591]	[-0.302, -0.0414]	[-0.0342, -0.0179]	[-0.0895, -0.0139]
Covariates		X		X
Observations	18,345	18,345	18,345	18,345
Dep. Var. Mean				
Full sample	0.808	0.808	0.967	0.967
Rural, Unexposed Cohorts	0.726	0.726	0.947	0.947

Notes: The sample includes children under 5 born to women born between 1975 and 1988. 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 1988, partially exposed cohorts are born between 1980 and 1982, and unexposed cohorts (reference group) are born between 1975 and 1979. Covariates include a binary indicator for Muslim, maternal age fixed effects, and division fixed effects. Standard errors are clustered at the mother and maternal year of birth levels using a two-way Wild cluster bootstrap approach. ***, ** and * represent statistical significance at 0.01, 0.05, and 0.1 levels, respectively and 95% confidence intervals are in square brackets.
Source: Bangladesh Demographic and Health Surveys, 1996-97 through 2017-18.

Table 5: Placebo Estimates of the Impact of FSSSP on Education and Child Immunizations

	(1)	(2)
Panel A: Maternal Education		
Outcome variables	Years of Schooling	Secondary or Higher Education
Placebo Fully Exposed X Rural	-0.0222	0.00895
	[-0.159, 0.119]	[-0.00658, 0.0252]
Placebo Partially Exposed X Rural	-0.131	-0.00592
	[-0.415, 0.110]	[-0.0330, 0.0254]
Observations	56,858	56,858
Dep. Var. Mean	3.281	0.252
Panel B: Child Immunization		
Outcome variables	Full Immunization	Any Immunization
Placebo Fully Exposed X Rural	-0.0223	0.0152
	[-0.125, 0.0485]	[-0.0275, 0.0740]
Placebo Partially Exposed X Rural	-0.0522	-0.00313
	[-0.134, 0.0369]	[-0.0533, 0.0635]
Observations	15,149	15,149
Dep. Var. Mean	0.702	0.932

Notes: The sample includes children under 5 born to women born between 1956 and 1979. 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. Placebo fully exposed cohorts are born between 1969 and 1979, partially exposed cohorts are born between 1966 and 1968, and unexposed cohorts (reference group) are born between 1956 and 1965. Covariates include a binary indicator for fully exposed cohort, a binary indicator for partially exposed cohort, a binary indicator for rural, a binary indicator for Muslim, maternal age fixed effects, and division fixed effects. Standard errors are clustered at the mother and maternal year of birth levels using a two-way Wild cluster bootstrap approach. ***, ** and * represent statistical significance at 0.01, 0.05, and 0.1 levels, respectively and 95% confidence intervals are in square brackets.
Source: Bangladesh Demographic and Health Surveys, 1993-94 through 2017-18.

Table 6: Robustness checks

Outcome variables	(1) Full Immunization	(2) Any Immunization
Panel A: Birth Cohorts 1978-1988		
Fully Exposed × Rural	0.0427*	0.0193*
	[-0.00550, 0.116]	[-0.00971, 0.0504]
Partially Exposed × Rural	-0.0102	0.00989
	[-0.0407, 0.0535]	[-0.0724, 1.214]
Observations	14,719	14,719
Dep. Var. Mean	0.825	0.971
Panel B: Birth Cohorts 1971-1988		
Fully Exposed × Rural	0.0575***	0.0195***
	[0.0294, 0.0891]	[0.00547, 0.0343]
Partially Exposed × Rural	0.00465	0.00977
	[-0.0330, 0.0432]	[-0.0122, 0.0310]
Observations	22,263	22,263
Dep. Var. Mean	0.792	0.962
Panel C: Oldest Child Under 5		
Fully Exposed × Rural	0.0652***	0.0400***
	[0.0282, 0.106]	[0.0257, 0.0585]
Partially Exposed × Rural	-0.00146	0.0205
	[-0.0647, 0.0710]	[-0.000589, 0.0452]
Observations	5,916	5,916
Dep. Var. Mean	0.792	0.972
Panel D: Maternal Age > 24 (95^{th} percentile of age at 1^{st} birth)		
Fully Exposed × Rural	0.0304**	0.00428
	[0.00135, 0.0573]	[-0.0132, 0.0227]
Partially Exposed × Rural	0.00155	-0.00353
	[-0.0484, 0.0337]	[-0.0262, 0.0185]
Observations	9,670	9,670
Dep. Var. Mean	0.862	0.973
Panel E: Oldest Child Under 5 & Maternal Age > 24		
Fully Exposed × Rural	0.132*	0.0104
	[-0.00216, 0.227]	[-0.0208, 0.0432]
Partially Exposed × Rural	0.114	0.0113
	[0.00592, 0.216]	[-0.0314, 0.0432]
Observations	1,138	1, 138
Dep. Var. Mean	0.915	0.989

Notes: 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 those born between 1983 and 1988; partially exposed cohorts are those born between 1980 and 1982, while unexposed cohorts are those born between 1978 and 1979 (Panel A), 1971 and 1979 (Panel B), 1975 and 1978 (Panel C, Panel D & Panel E). Covariates include a binary indicator for Muslim, maternal age fixed effects, and division fixed effects. Standard errors are clustered at the mother and maternal year of birth levels using a two-way Wild cluster bootstrap approach. ***, ** and * represent statistical significance at 0.01, 0.05, and 0.1 levels, respectively and 95% confidence intervals are in square brackets.
Source: Bangladesh Demographic and Health Surveys, 1996-97 through 2017-18.

Table 7: Alternative Clustering Approaches

	(1) Full Immunization	(2) Any Immunization
Panel A: Two-way Wild Cluster Bootstrap at Mother and Maternal Birth Year Levels		
Fully Exposed × Rural	0.0401***	0.0159***
	[0.0193, 0.0638]	[0.00522, 0.0286]
Partially Exposed × Rural	-0.0119	0.00680
	[-0.0300, 0.0176]	[-0.0123, 0.0275]
Panel B: One-way Wild Cluster Bootstrap at Birth Year Level (14 groups)		
Fully Exposed × Rural	[0.0192, 0.0639] ***	[0.0052, 0.0286] ***
Partially Exposed × Rural	[-0.0300, 0.0176]	[-0.0123, 0.0275]
C: Clustering at Division-Birth Year Level		
Fully Exposed × Rural	[0.0170, 0.0633] ***	[0.0038, 0.0280] **
Partially Exposed × Rural	[-0.0386, 0.0147]	[-0.0093, 0.0229]
Panel D: Two-way Wild Cluster Bootstrap at Mother and Division-Birth Year Levels		
Fully Exposed × Rural	[0.0173, 0.0629] ***	[0.0040, 0.0279] ***
Partially Exposed × Rural	[-0.0382, 0.0143]	[-0.0090, 0.0226]

Notes: The sample includes children under 5 born to women born between 1978 and 1988. 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 1988; partially exposed cohorts are born between 1980 and 1982, while unexposed cohorts are born between 1978 and 1979. All regressions include a binary indicator for fully exposed cohorts, a binary indicator for partially exposed cohorts, a binary indicator for rural, a binary indicator for Muslim, maternal age fixed effects, and division fixed effects. ***, ** and * represent statistical significance at 0.01, 0.05, and 0.1 levels, respectively and 95% confidence intervals are in square brackets.

Source: Bangladesh Demographic and Health Surveys, 1996-97 through 2017-18.

Table 8: Heterogeneous Effects

Panel A: Heterogeneity by Gender		
Outcome variables	Full Immunization	Any Immunization
	(1)	(2)
Fully Exposed × Rural × Female Child	0.0367	0.00130
	[-0.00885, 0.0852]	[-0.0302, 0.0345]
Partially Exposed × Rural × Female Child	0.0282	0.00123
	[-0.0383, 0.0781]	[-0.0682, 0.0494]
Observations	18,345	18,345
Dep. Var. Mean	0.808	0.967

Panel B: Heterogeneity by Wealth		
Outcome variables	Full Immunization	Any Immunization
	(1)	(2)
Fully Exposed × Rural × Poor	0.0340	0.0389**
	[-0.0465, 0.129]	[0.00692, 0.0663]
Partially Exposed × Rural × Poor	0.0312	0.0350*
	[-0.0825, 0.192]	[-0.00867, 0.0805]
Observations	14,961	14,961
Dep. Var. Mean	0.851	0.976

Panel C: Heterogeneity by Gender and Wealth				
Sub-sample	Poor	Wealthy	Poor	Wealthy
Outcome variables	Full Immunization	Full Immunization	Any Immunization	Any Immunization
	(1)	(2)	(3)	(4)
Fully Exposed × Rural × Female Child	-0.0703	0.0750**	-0.0507	0.000569
	[-0.309, 0.132]	[0.0113, 0.137]	[-0.148, 0.0173]	[-0.0340, 0.0374]
Partially Exposed × Rural × Female Child	-0.0275	0.0789***	-0.0751	0.0198
	[-0.369, 0.234]	[0.0493, 0.113]	[-0.235, 0.0733]	[-0.0266, 0.0669]
Observations	6,019	8,942	6,019	8,942
Dep. Var. Mean	0.805	0.881	0.962	0.985

Notes: The sample includes children under 5 born to women born between 1975 and 1988. 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 1988, partially exposed cohorts are born between 1980 and 1982, and unexposed cohorts (reference group) are born between 1975 and 1979. All regressions include a binary indicator for fully exposed cohorts, a binary indicator for partially exposed cohorts, a binary indicator for rural, a binary indicator for Muslim, maternal age fixed effects, and division fixed effects. Regressions in Panels A and C also include a binary

indicator for a female child, and two-way interactions between the cohort, rural and female child dummies. Regressions in Panel B also include a binary indicator for being poor (defined as belonging to the bottom two wealth quintiles), and two-way interactions between the cohort, rural and poor dummies, Standard errors are clustered at the mother and maternal year of birth levels using a two-way Wild cluster bootstrap approach. ***, ** and * represent statistical significance at 0.01, 0.05, and 0.1 levels, respectively and 95% confidence intervals are in square brackets.
Source: Bangladesh Demographic and Health Surveys, 1996-97 through 2017-18 (Panel A), Bangladesh Demographic and Health Surveys, 2004 through 2017-18 (Panels B and C).

Appendix

Table A1: Literature

Citation	Level of education	Country	Policy used	Child health outcomes	Results – increase/decrease/no change
Ali, F.R.M. and Elsayed, M.A., 2018. The effect of parental education on child health: Quasi-experimental evidence from a reduction in the length of primary schooling in Egypt. *Health economics*, *27*(4), pp.649-662.	Primary and preparatory school	Egypt	1988 reform *reduced* the years of primary schooling	under-five, infant and neonatal mortality, stunting, wasting, and overweight, prenatal visits	*decrease*: some evidence that paternal education reduces stunting *no change*: no effect of maternal education on child mortality and nutritional status, utilization of antenatal care; no effect of paternal education on child mortality and other measures of nutritional status
Aslam, M. and Kingdon, G.G., 2012. Parental education and child health—understanding the pathways of impact in Pakistan. *World Development*, *40*(10), pp.2014-2032.		Pakistan	no educational policy used	immunization status (any immunization), height-for-age z-score (HAZ), weight-for-age z-score (WAZ)	*increase*: father's education matters for immunization status and mother's education and her empowerment matter for long-term health outcomes.
Breierova, L., and Duflo, E. 2004. The impact of education on fertility and child mortality: Do fathers really matter less than mothers? *NBER Working Paper*, (w10513). National Bureau of Economic Research, Inc.	primary	Indonesia	INPRES school construction program	child mortality	*decrease*: child mortality

Chen, Y. and Li, H., 2009. Mother's education and child health: Is there a nurturing effect?. *Journal of health economics*, *28*(2), pp.413-426.		China	no educational policy used. Large sample of adoptees.	HAZ, WAZ, vaccine (adoptees vs own birth children)	*increase*: HAZ, WAZ, vaccine
Currie, J. and Moretti, E., 2003. Mother's education and the intergenerational transmission of human capital: Evidence from college openings. *The Quarterly journal of economics*, *118*(4), pp.1495-1532.	College education: 2-year college and 4-year college	USA	opening of public colleges	birth weight, gestational age, parity (birth order), prenatal care	*increase*: use of prenatal care *decrease*: low birth weight
Dinçer, M.A., Kaushal, N. and Grossman, M., 2014. Women's education: Harbinger of another spring? Evidence from a natural experiment in Turkey. *World Development*, *64*, pp.243-258.	Primary	Turkey	1997 compulsory schooling law (extended from 5 to 8 years)	child mortality, antenatal visit	*decrease*: child mortality (suggestive).
Dursun, B., Cesur, R. and Kelly, I.R., 2022. Mandatory Schooling of Girls Improved Their Children's Health: Evidence from Turkey's 1997 Education Reform. *Journal of Policy Analysis and Management*.	Primary	Turkey	1997 compulsory schooling law (extended from 5 to 8 years)	child health at birth (birth weight: very low birth weight, low birth weight, and high birth weight) premature birth, gestational age, child mortality, normal birth	*increase:* log birth weight, log gestational age, normal birth. Natural birth delivery *decrease:* very low birth weight, low birth weight, and high birth weight, preterm<37 weeks, child mortality
Grépin, K.A. and Bharadwaj, P., 2015. Maternal education and child mortality in	Secondary	Zimbabwe	blacks gained access after independence	child mortality, institutional delivery,	*decrease*: child mortality *no change*: other health outcomes

Zimbabwe. *Journal of health economics*, *44*, pp.97-117.				antenatal care, immunization	
Güneş, P.M., 2015. The role of maternal education in child health: Evidence from a compulsory schooling law. *Economics of Education Review*, *47*, pp.1-16.	Primary	Turkey	1997 compulsory schooling law (extended from 5 to 8 years)	VLBW, HAZ, WAZ, prenatal care initiation, length of facility stays after delivery	*decrease*: in VLBW and LOS, *increase*: in HAZ, WAZ and *prenatal* care initiation
Hahn, Y., Islam, A., Nuzhat, K., Smyth, R., and Yang, H. S. 2018a. Education, marriage, and fertility: Long-term evidence from a female stipend program in Bangladesh. *Economic Development and Cultural Change, 66(2),* 383-415.	Secondary	Bangladesh	female stipend	child: height for age, weight for age, hemoglobin, anemia	*increase:* child height for age, child weight for age *no change:* hemoglobin, anemia
Hahn, Y., Nuzhat, K., and Yang, H. S. 2018b. The effect of female education on marital matches and child health in Bangladesh. *Journal of Population Economics, 31(3),* 915-936.	Secondary	Bangladesh	female stipend	child: height for age, weight for age, hemoglobin, anemia, low birthweight, antenatal care, postnatal care, institutional delivery	*increase:* child height for age, child weight for age, hemoglobin, antenatal care, postnatal care, institutional delivery *no change:* anemia, low birthweight
Keats, A., 2018. Women's schooling, fertility, and child health outcomes: Evidence from Uganda's free primary education program. *Journal of Development Economics*, *135*, pp.142-159.	Primary	Uganda	1997 universal primary education program (eliminated primary school fee)	height for age-stunting, weight for age-wasting, anemia, infant mortality	*increase:* height for age-stunting, *decrease:* anemia *no change:* weight for age-wasting

Makate, M. and Makate, C., 2016. The causal effect of increased primary schooling on child mortality in Malawi: Universal primary education as a natural experiment. *Social Science & Medicine*, *168*, pp.72-83.	Primary	Malawi	1994 universal primary education policy	infant and under-five mortality, prenatal care visits, delivery by c-section, breastfeeding child vaccination	*increase:* prenatal care visits, *decrease:* infant and under-five mortality, child immunizations *no change:* neonatal, delivery by c-section, breastfeeding
McCrary, J. and Royer, H., 2011. The effect of female education on fertility and infant health: evidence from school entry policies using exact date of birth. *American economic review*, *101*(1), pp.158-95.	Primary	Texas and California, USA	age at school entry policies	birth weight, gestational length (prematurity), and infant mortality	*no change:* low birth weight, gestational length (prematurity) *no change:* infant mortality (very small effect)
Özer, M., Fidrmuc, J. and Eryurt, M.A., 2018. Maternal education and childhood immunization in Turkey. *Health economics*, *27*(8), pp.1218-1229.	Primary	Turkey	1997 compulsory schooling law (extended from 5 to 8 years)	immunization – DPT and Hep B	*increase:* probability of completing full course of DTP and Hep B
Thomas, D., Strauss, J. and Henriques, M.H., 1991. How does mother's education affect child height?. *Journal of human resources*, pp.183-211.		Brazil	no educational policy used.	Child height-standardized by age and sex	*increase:* child height
Wu, H. (2022). The Effect of Maternal Education on Child Mortality in Bangladesh. *Population and Development Review*.	Secondary	Bangladesh [IV]	female stipend	child mortality, infant mortality	*decrease:* under-five mortality, infant mortality

Zhang, S., 2012. *Mother's education and infant health: Evidence from closure of high schools in China*. Cornell University working paper. 2012 2011 Oct.	high school education	China	high school closures in rural areas after cultural revolution 1976	gestational age, prematurity, neonatal and infant mortality, birthweight, low birthweight, any prenatal check, hospital delivered	*no change:* prematurity, low birthweight, neonatal mortality and infant mortality

Table A2: Difference-in-Differences Estimates for Individual Vaccine Doses

Outcome variables	BCG Vaccine	DPT 1 Vaccine	DPT 2 Vaccine	DPT 3 Vaccine	OPV 1 Vaccine	OPV 2 Vaccine	OPV 3 Vaccine	Measles Vaccine
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fully Exposed × Rural	0.0190***	0.0283***	0.0365***	0.0354***	0.0199***	0.0399***	0.0542***	0.0285***
	[0.00713, 0.0309]	[0.0159, 0.0410]	[0.0210, 0.0519]	[0.0144, 0.0539]	[0.0103, 0.0318]	[0.0261, 0.0545]	[0.0398, 0.0703]	[0.00767, 0.0551]
Partially Exposed × Rural	-0.00246	0.00537	-0.00406	-0.0189	0.00733	0.00275	0.00757	-0.0152
	[-0.0161, 0.0126]	[-0.0109, 0.0178]	[-0.0152, 0.00565]	[-0.0462, 0.0114]	[-0.0104, 0.0327]	[-0.0211, 0.0231]	[-0.0130, 0.0369]	[-0.0395, 0.0214]
Fully Exposed	0.0237***	0.0230***	0.0361***	0.0710***	0.0154**	0.0315***	0.0808***	0.0438***
	[0.00882, 0.0434]	[0.00710, 0.0429]	[0.0178, 0.0596]	[0.0374, 0.114]	[0.00341, 0.0318]	[0.0133, 0.0536]	[0.0471, 0.123]	[0.0200, 0.0671]
Partially Exposed	0.0218**	0.0182**	0.0243**	0.0493**	0.0100	0.0215*	0.0558**	0.0369**
	[0.00879, 0.0369]	[0.00574, 0.0345]	[0.00539, 0.0478]	[0.00487, 0.102]	[-0.00403, 0.0260]	[-0.000111, 0.0444]	[0.00978, 0.113]	[0.0111, 0.0623]
Rural	-0.0226***	-0.0320***	-0.0413***	-0.0440***	-0.0253**	-0.0424***	-0.0554**	-0.0391**
	[-0.0345, -0.0149]	[-0.0430, -0.0240]	[-0.0503, -0.0274]	[-0.0738, -0.0234]	[-0.0547, -0.0197]	[-0.0559, -0.0242]	[-0.405, -0.0166]	[-0.0686, -0.0292]
Covariates	X	X	X	X	X	X	X	X
Observations	18,345	18,345	18,345	18,345	18,345	18,345	18,345	18,345
Dep. Var. Mean	0.957	0.951	0.923	0.878	0.962	0.928	0.876	0.852

Notes: The sample includes children under 5 born to women born between 1975 and 1988. The dependent variable in each column indicates whether the child received the BCG/ DPT1/ DPT2/ DPT3/OPV 1/ OPV 2/, OPV 3/ Measles vaccine. BCG=Bacillus Calmette-

Guérin; DPT= Diphtheria-Pertussis-Tetanus; OPV= Oral Polio Vaccine. Fully exposed cohorts are born between 1983 and 1988, partially exposed cohorts are born between 1980 and 1982, and unexposed cohorts (reference group) are born between 1975 and 1979. Covariates include a binary indicator for Muslim, maternal age fixed effects, and division fixed effects. Standard errors are clustered at the mother and maternal year of birth levels using a two-way Wild cluster bootstrap approach. ***, ** and * represent statistical significance at 0.01, 0.05, and 0.1 levels, respectively and 95% confidence intervals are in square brackets.
Source: Bangladesh Demographic and Health Surveys between 1996-1997 and 2017-2018.

Table A3: Event Study Estimates of the Impact of FSSSP on Education

Outcome variables	Years of Schooling	Secondary or Higher Education
	(1)	(2)
Birth Year 1975 × Rural	-0.025	-0.009
	[-18.04, 18.58]	[-2.224, 1.918]
Birth Year 1976 × Rural	0.224	0.004
	[-18.86, 14.57]	[-1.922, 1.403]
Birth Year 1977 × Rural	0.053	0.001
	[-8.947, 13.83]	[-1.172, 1.590]
Birth Year 1978 × Rural	0.115	0.002
	[-10.85, 11.12]	[-0.963, 0.980]
Birth Year 1980 X Rural	0.492	0.059*
	[-12.02, 10.80]	[-1.057, 0.664]
Birth Year 1981 × Rural	0.766	0.069
	[-10.50, 10.98]	[-1.210, 1.197]
Birth Year 1982 × Rural	0.569	0.052
	[-19.06, 22.87]	[-2.070, 2.637]
Birth Year 1983 × Rural	0.957	0.112
	[-13.09, 14.89]	[-1.136, 1.438]
Birth Year 1984 × Rural	1.075	0.100
	[-9.990, 9.189]	[-1.132, 0.794]
Birth Year 1985 × Rural	1.127	0.108
	[-12.51, 13.26]	[-1.750, 1.761]
Birth Year 1986 × Rural	1.257	0.133
	[-16.02, 18.75]	[-2.154, 1.858]
Birth Year 1987 × Rural	1.223	0.151
	[-15.68, 13.09]	[-1.282, 1.188]
Birth Year 1988 × Rural	1.341	0.138
	[-12.87, 11.68]	[-1.783, 1.445]
Observations	42,685	42,685
Dep. Var. Mean	5.010	0.437

Notes: The sample includes women born between 1975 and 1988 (reference birth year is 1979). All regressions include a binary indicator for rural, maternal birth year fixed effects, a binary

indicator for Muslim, maternal age fixed effects, and division fixed effects. Standard errors are clustered at the mother and maternal year of birth levels using a two-way Wild cluster bootstrap approach. ***, ** and * represent statistical significance at 0.01, 0.05, and 0.1 levels, respectively and 95% confidence intervals are in square brackets.
Source: Bangladesh Demographic and Health Surveys, 1993-94 through 2017-18.

Table A4: Event Study Estimates of the Impact of FSSSP on Child Immunizations

Outcome variables	Full Immunization	Any Immunization
	(1)	(2)
Birth Year 1975 × Rural	0.00520	-0.00203
	[-2.242, 2.095]	[-0.503, 0.547]
Birth Year 1976 × Rural	-0.00964	-0.00821
	[-1.899, 1.774]	[-0.822, 0.602]
Birth Year 1977 × Rural	-0.00733	0.00256
	[-1.735, 2.111]	[-0.669, 0.739]
Birth Year 1978 × Rural	-0.00966	-0.0129
	[-1.765, 1.451]	[-0.702, 0.604]
Birth Year 1980 X Rural	-0.0253	-0.0119
	[-2.568, 2.895]	[-0.758, 0.923]
Birth Year 1981 × Rural	0.00325	0.000945
	[-2.322, 2.631]	[-0.815, 1.068]
Birth Year 1982 × Rural	-0.0339	0.0174
	[-2.928, 3.740]	[-0.896, 1.111]
Birth Year 1983 × Rural	0.0136	0.00665
	[-2.388, 2.908]	[-0.715, 1.065]
Birth Year 1984 × Rural	0.0259	0.00740
	[-2.330, 2.594]	[-0.711, 0.850]
Birth Year 1985 × Rural	0.0740	0.0317
	[-1.886, 3.030]	[-0.865, 1.255]
Birth Year 1986 × Rural	0.0312	0.0122
	[-2.793, 2.475]	[-1.199, 1.130]
Birth Year 1987 × Rural	0.0316	0.000990
	[-2.241, 2.838]	[-1.080, 0.960]
Birth Year 1988 × Rural	0.0241	0.00799
	[-1.469, 2.182]	[-0.637, 0.978]
Observations	18,345	18,345
Dep. Var. Mean	0.808	0.967

Notes: The sample includes children under 5 born to women born between 1975 and 1988 (reference birth year is 1979). All regressions include a binary indicator for rural, maternal birth

year fixed effects, a binary indicator for Muslim, maternal age fixed effects, and division fixed effects. Standard errors are clustered at the mother and maternal year of birth levels using a two-way Wild cluster bootstrap approach. ***, ** and * represent statistical significance at 0.01, 0.05, and 0.1 levels, respectively and 95% confidence intervals are in square brackets.
Source: Bangladesh Demographic and Health Surveys, 1996-97 through 2017-18.

Table A5: Difference-in-Differences Estimates of the Impact of FSSSP on Household Wealth and Motherhood

	(1)	(2)	(3)	(4)
	Household Wealth Bottom 40%		Ever Became a Mother (Age>24)	
Fully Exposed X Rural	-0.019	-0.018	0.002	0.003
	[-0.0407, 0.00566]	[-0.0381, 0.00620]	[-0.0100, 0.0131]	[-0.00978, 0.0148]
Partially Exposed X Rural	0.003	0.004	-0.005	-0.005
	[-0.0290, 0.0331]	[-0.0273, 0.0326]	[-0.0196, 0.00619]	[-0.0197, 0.00748]
Fully Exposed	-0.001	0.006	-0.012**	-0.001
	[-0.0373, 0.0259]	[-0.0284, 0.0360]	[-0.0227, -0.00196]	[-0.00983, 0.00682]
Partially Exposed	-0.012	-0.009	0.003	0.006
	[-0.0459, 0.0276]	[-0.0435, 0.0307]	[-0.00644, 0.0127]	[-0.00312, 0.0166]
Rural	0.341***	0.334**	0.016**	0.015**
	[0.316, 0.361]	[0.284, 0.351]	[0.00507, 0.0267]	[0.00312, 0.0268]
Covariates		X		X
Observations	36,210	36,210	28,161	28,161
Dep. Var. Mean	0.368	0.368	0.968	0.968

Notes: The sample includes women born between 1975 and 1988. Fully exposed cohorts are born between 1983 and 1988, partially exposed cohorts are born between 1980 and 1982, and unexposed cohorts (reference group) are born between 1975 and 1979. In columns 3-4 the sample is restricted to women who are older than 24 at the time of the survey. The dependent variable is an indicator that the household is in the two lowest quintiles of household wealth in Columns 1-2 and an indicator that the woman had at least one birth in Columns 3-4. Covariates include a binary indicator for Muslim, maternal age fixed effects, and division fixed effects. Standard errors are clustered at the mother and maternal year of birth levels using a two-way Wild cluster bootstrap approach. ***, ** and * represent statistical significance at 0.01, 0.05, and 0.1 levels, respectively and 95% confidence intervals are in square brackets.
Source: Bangladesh Demographic and Health Surveys, 2004 through 2017-18 (Columns 1-2); Bangladesh Demographic and Health Surveys, 1993-94 through 2017-18 (Columns 3-4).