Educate the Girls: Long run Effects of Secondary Schooling for Girls in Pakistan*

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Abstract

We estimate the long run effects of a conditional cash assistance program initiated in 2004 for girls attending secondary schools in Punjab, Pakistan on their marriage and fertility decisions, maternal healthcare utilization and the health outcomes of their children. We use data from four rounds of the MICS survey, using differences in women's exposure to the program to estimate the effects of the program. We find that each potential year of exposure to the program increased the probability of completing secondary school by 1.3 percent and decreases the probability of an early marriage by 3.5 percent compared to sample average. Exposure to the program also delays early childbirth and increases the likelihood of seeking prenatal and postnatal care later in life. We also find evidence of inter-generational effects - children of women exposed to the program are less likely to be underweight (-1.7 percent) or stunted (-1.15 percent). Evidence suggests that these effects may be driven by assortative matching in the marriage market, increased health awareness and empowerment of educated women. These findings imply that programs aimed at promoting girls' education lead to beneficial long run gains in multiple dimensions that must be factored in evaluating such policies.

1 Introduction

Improvements in health and education, especially for women and children, are universally accepted public policy goals in both developing and developed countries. Human development gaps in the developing countries are stark both in numbers affected as well as in the severity of the problem. Governments and international donor agencies have attempted to address these problems with a host of interventions ranging from free provision of health services to Conditional Cash Transfers (CCTs) for school attendance.

Programs focusing on educating girls are central to development policy, based on the premise that investment in the education of young girls and women will not only yield private but also social returns. Economic theory suggests educated women may lower fertility and enable better health care and education for their children (Becker and Lewis, 1973; Becker, 1992; Thomas et al., 1991; Strauss and Thomas, 1995). While the empirical correlation between female schooling, fertility and labor market decisions is well known, studies are seldom able to identify the causal impact of women's education on other long-run welfare outcomes, and only a small proportion of them have been conducted in the developing world (See Lochner (2011) and Mensch et al. (2019) for review).

In this study, we estimate the long run effects of a secondary public school program for girls in Punjab - the most populous province of Pakistan- where more than half of the 23 million out of school children in the country are based.[1] In 2004, the Government of Punjab implemented the Female Secondary School Stipend Program (FSSP) to encourage households to send their girls for secondary schooling.[2] Under the FSSP, households in eligible districts with girls enrolled in grades 6-10 receive a monthly cash stipend (USD 2.5) conditional on 80 percent attendance.[3] The program was implemented in districts with literacy rates of 40 percent or less according to the

[1] www.unicef.org/pakistan/education

[2] The FSSP was announced in 2003 but the stipend disbursements started in 2004 (Independent Evaluation Group, 2011).

[3] Primary schooling in Pakistan is grades 1-5. Secondary schooling is grades 6-10.

national census in 1998. 15 out of the 36 districts qualified on the basis of this rule.[4] As per official figures, cash transfers averaging USD 14 million annually have been disbursed since 2004 to more than 400,000 girls.[5] With the Government of Pakistan recently announcing secondary school stipends for girls from all low-income households in the country, these figures are only expected to increase in the future.[6]

We exploit the quasi-experimental variation in the introduction of the FSSP to investigate the long run effects of the program on women's education (secondary school completion and years of education), teenage marriage and childbirth, and maternal health care utilization (pre and post-natal care). Using four rounds of cross sectional data from Multiple Indicators Cluster Survey (MICS), collected in 2003, 2011, 2014 and 2018, we assign to each woman in our sample the number of years of exposure to the program based on district of residence (treatment vs. control) and her age at the initiation of the program. We find four main sets of results. First, the program met it's primary goal of increasing education for women - each year of exposure to the FSSP increases the probability of completing secondary schooling by 1.3 percent. Second, each year of exposure to the FSSP reduces the likelihood of marriage before the age of 16 by 3.5 percent and childbirth before the age of 17 by 3.8 percent.

Third, women exposed to the program are more likely to seek maternal health: an increase of 1 percent and 2 percent in take up of prenatal and postnatal checkups for each year of exposure, respectively. Fourth, we also find evidence of intergenerational effects. Specifically, children of women exposed to the program on average are less likely to be stunted (1.2 percent) or underweight (1.7 percent) and score higher on Weight-for-Age (WAZ) and Height-for-Age(HAZ) standardized scores.[7] Child mortality is lower for women exposed to the program in our sample. These results are robust to several specifications, including controlling for healthcare services over time, exclud-

[4] Districts are administrative units within a province.

[5] (Alam et al., 2011), http://www.pesrp.edu.pk/pages/Stipend-to-Girl

[6] https://www.dawn.com/news/1629948.

[7] The percentage change for all results is calculated based on the mean for the respective women who had no exposure to the program.

ing older cohorts of women with lower levels of exposure and restricting the sample to districts with similar literacy rates in 1998.

We also look into the potential mechanisms that may be driving these changes. We find that women exposed to the program are more likely to marry men who have completed secondary schooling (or higher) and are more likely to be aware about health related issues, which we proxy with their awareness of AIDS and HIV, providing insights into the the channels of estimated effects. We also find suggestive evidence for women's empowerment as a possible mechanism behind these effects. We do not however find any evidence of increased use of contraception playing a role in delayed child birth.

Our results are in line with a growing body of experimental and quasi-experimental studies that show women's education reduces early fertility (Osili and Long, 2008; Behrman, 2015; Keats, 2018; Duflo et al., 2015; Ozier, 2018), and is positively associated with their health and their children's health (see Grossman and Kaestner (1997); Grossman (2000, 2006) for review). Further, a related literature has shown that improvements in child health are likely to lead to improved education and labor market outcomes for these children later in their life (see Vogl (2012) for a review). However, many are correlational or descriptive studies that are unable to account for mother's education being potentially related to other unobserved characteristics that may affect child's well being as well. Conclusive evidence on a causal relationship between maternal education and child health in developing countries is relatively sparse.[8]

Our study contributes to the existing literature in several ways. First, we investigate the effects of a CCT program primarily designed to encourage secondary schooling, on primary and secondary schooling completion rates, fertility and maternal health outcomes. While secondary schooling

[8] Baird et al. (2011) find a CCT secondary education program in Malawai reduces early pregnancy but not as much as unconditional cash transfers. Quasi-experimental studies exploiting changes in compulsory schooling in Zimbabwe (Grépin and Bharadwaj, 2015), scholarships in Ghana (Duflo et al., 2019) and school construction program in Indonesia (Breierova and Duflo, 2004; Akresh et al., 2018; Mazumder et al., 2019) find women's schooling delays marriage and childbirth; reduces fertility and child mortality; and leads to improved labor market outcomes. Akresh et al. (2018) and Masuda and Yamauchi (2020) are the closest to this study in terms of outcomes measured, but investigate the effects of primary schooling.

programs are regularly evaluated for their impact on enrolment rates (see Baird et al. (2014) for a review) and evidence on the effects of secondary schooling on marriage, fertility and health is rare. The only other evidence, to the best of our knowledge, is from Zimbabwe (Grépin and Bharadwaj, 2015), Bangladesh (Hahn et al., 2018), Malawi (Baird et al., 2011) and Ghana (Duflo et al., 2019).

Second, with primary enrolment rates approaching 100 percent globally (UNESCO, 2016), growing concerns of minimal learning gains, and generally higher costs of secondary education relative to primary education, longer term impact and intergenerational impacts of secondary schooling become an important and policy-relevant outcomes to measure (Duflo et al., 2019; Warner et al., 2012). Duflo et al. (2019) provide evidence of increased public sector and formal employment among secondary schooling scholarship recipients in Ghana. We add to this literature and present novel evidence not only on women's own long outcomes, but also on the impact of a CCT on the health and well being of the children of the recipients.

The World Health Organization (WHO) views early childhood malnutrition as one of the biggest challenges the world currently faces, with nearly one out of every five (21%) children around the world under the age of five children being 'stunted'.[9] We join a small group of studies that examine the impact on the next generation's standardized weight and height measures for children under 5, incidence of underweight and stunting, and child mortality. These measures are commonly used indicators for current and long term health status of children and provide information on malnutrition and growth retardation, correlated with long term economic losses through lower cognition, educational performance, wages and productivity (Thomas et al., 1991; Gross et al., 1996; McGovern et al., 2017). Though the effects of FSSP that we measure on these indicators are small, literature suggests that they are likely meaningful, specially since the program is not primarily designed to counter child malnutrition . For instance, Hoddinott et al. (2013) show that for low-income households in Guatemala, a 1 SD improvement in HAZ lead to substantial (21 percent) increase in (later-life) household income, reducing the likelihood of poverty (by 10 percent). Similarly, Gertler et al. (2014) find that an intervention to reduce stunting in Jamaican

[9] The data is available online at the WHO Global Health Observatory.

toddlers led to an average increase of incomes by 25 percent, 20 years after the respondents first participated in the trial. Similar and significant improvements in later-life outcomes have been documented in other developing world contexts (McGovern et al., 2017).

Third, we present novel evidence for policy making in Pakistan; a country with one of the highest maternal, infant and child mortality rates in the world (see, for instance, Hogan et al. (2010) and Devine and Taylor (2018)). However, no prior evidence exists on the long run impact of secondary schooling on fertility, age of marriage and child-birth, and health care utilization for women in Pakistan.[10] The high rates of pregnancy related maternal mortality (251 per 100,000) and infant morality(1 in 20) in Pakistan, are attributed to low rates of maternal healthcare utilization (NIPS Pakistan and ICF, 2020). Our study makes an important contribution of documenting the increase in maternal healthcare utilization due to increased secondary schooling. Moreover, 18% of girls in Pakistan are married before the age of 18(NIPS Pakistan and ICF, 2020). Our results show that exposure to the FSSP results in significant reduction in probability of early marriage and childbirth.

We also fill in key gaps in the literature on inter-generational effects of educational programs in low-income settings with poor educational and health outcomes. Existing evidence on inter-generational gains from school construction programs and compulsory schooling laws focuses on child mortality (Grépin and Bharadwaj, 2015) and educational outcomes (Mazumder et al., 2019), albeit in relatively wealthier contexts, but does not speak on the quality of health and well-being of children. Andrabi et al. (2012) and Masuda and Yamauchi (2020) are the only studies that estimate the inter-generational transmission of human capital in similar settings as our study. Unlike their work that focuses on lower levels of education, our study looks at the impact of secondary education.

Lastly, our work contributes to the upcoming literature on long run impacts of CCT pro-

[10] A prior report found positive impacts of the program on school completion rates and fertility decision in the medium term (Independent Evaluation Group, 2011).

grams.[11] Evidence on the longer run benefits of CCT programs- studies in Columbia (Barrera-Osorio et al., 2017), Mexico (Parker and Vogl, 2018) and Nicaragua (Barham et al., 2013)- suggest positive impacts on long run educational achievement, labor force participation and mobility of early life beneficiaries. On the other hand, Araujo et al. (2017) find only modest improvements in intergenerational transmission of benefits in Ecuador. Unlike these other CCTs, however, the FSSP is unique in two respects: (i) it is a non-means tested program - i.e., not conditional on household resources - and (ii) the amount of the cash transfer is small (PKR 600 or USD 10 a quarter).[12] Unlike most CCTs, the cash transfer is not a significant income shock for the households. This coupled with the fact that the FSSP targeted girls, allows us to place our inter-generational findings within the context of direct or spillover impacts of maternal education.

The remainder of this paper is organized as follows: Section 2 provides program background and context. Section 3 discusses the data. Section 4 explains the estimation strategy. Section 5 presents the empirical results and Section 6 concludes.

2 Program background and context

Pakistan is one of the three countries in the world with more than 1 million adolescent girls out of school (UNESCO, 2015). The female gross enrollment rate for the primary level stands at 86 percent for Pakistan. This drops sharply to 35 percent for lower secondary (grades 6-8) and 20 percent for upper secondary (grades 9 and 10), despite no tuition fees in public schools (UNESCO, 2015). This is attributable to a host of subjective (e.g. cultural and psychological barriers) and objective barriers (e.g. costs of textbooks, transportation, street harassment, preference to the male child when resources are limited in the household, etc.). However, school enrolment at the

[11] Eighty countries currently have implemented CCT programs to improve socio-economic welfare. CCT programs targeting educational outcomes have been successful in their primary objective of increasing school enrolment and attendance. See, for instance, Baird et al. (2014); Behrman et al. (2005); Benedetti et al. (2016); Fiszbein et al. (2009); Ganimian and Murnane (2016); Schultz (2004); Todd and Wolpin (2006).

[12] Based on the exchange rate in 2004, when the program started disbursements.

secondary level is also majorly constrained by a scarcity of schools. For instance, about half the households in Punjab report a secondary school within a 15 minute walking radius, compared to more than four-fifths of the sample that reports a primary school within the same distance (Andrabi et al., 2011; Sathar et al., 2003). While there is an active private market for primary schools, the secondary level is dominated by the public sector (Independent Evaluation Group, 2011), with about 90% of the private schools in 2004 offering only primary classes (Andrabi et al., 2011). In 2011, less than third of the secondary school going children in Punjab were enrolled in private schools (Nguyen and Raju, 2015). Our evaluation of the FSSP is therefore relevant for a large proportion of the secondary school-going population in Punjab.

Historically, female enrollment in primary and secondary schools has been low, both in absolute terms and relative to boys (See Appendix Figure A.1; (Behrman and Schneider, 1993; Alderman et al., 2001; Holmes, 2003; Lloyd et al., 2005). Low female enrollment is compounded by low retention and completion rates for girls (Sawada and Lokshin, 2009). Further, child health in Pakistan is highly correlated with age and education level of mothers - neonatal mortality rates are 1.5 times higher for younger mothers (aged 20 years or less) and 2.4 times higher for less educated mothers (UNICEF, 2016). This may potentially explain the grim statistics on maternal and child health: 1 out of every 12 women give birth under the age of 18, and maternal and infant mortality rates are one of the highest in South Asia (Devine and Taylor, 2018; Hogan et al., 2010; UNICEF, 2016).

The Female School Stipend Program (FSSP) is an ongoing CCT program in the province of Punjab, designed to encourage female education using economic incentives. The Government of Punjab first disbursed quarterly stipends worth PKR 600 (USD 10) per female student attending a secondary government (public) school under the FSSP in 2004. Stipends were disbursed to eligible students in grades 6-8 in the first quarter of 2004 (Chhabra et al., 2019). In 2005, the program was extended to include grades 9 and 10. Stipends were disbursed quarterly and were conditional on girls maintaining 80 percent attendance (as reported by the school).[13] Based on the average out of pocket spending for attending secondary school, 80 percent of the stipend was designed to cover the costs of schooling related to transport, uniform and textbooks (factors commonly cited as barriers to girls' attendance), leaving 20 percent left over for the family to use for other needs (Alam et al., 2011; Chaudhury and Parajuli, 2010).

Stipend size has two important implications. First, it rules out large direct income shocks driving changes in outcomes. At 3.4 percent of median household expenditures of the recipient households in 2004, the monthly stipend is unlikely to have been a large income shock for households (Fiszbein et al., 2009; Chaudhury and Parajuli, 2010). Second, given the costs associated with migration (including giving up housing, livestock and livelihood), the cash stipend on its own is insufficient to incentivize migration from non-recipient to recipient districts.

The stipends were disbursed only in 15 districts out of 36 districts in the province that had literacy rates below 40 percent, as per the 1998 Population Census.[14] See Appendix Table A.1 for district literacy rates in 1998. Figure A.2 provides a map with district names. Figure 1 shows geography of the recipient and non-recipient districts. Recipient districts, which we refer to as the treatment districts in subsequent discussion, are located towards the south of the province and are spatially clustered close to each other. The low literacy rates in these districts correspond to these districts being economically poorer compared to non-recipient, or control, districts. By 2013, 411,000 girls in more than 6800 schools were enrolled in the program, at a cost of USD 14.2 million on average each year (Fiszbein et al., 2009). Based on data collected by the Programme Monitoring and Implementation Unit and the Punjab Education Sector Reform Programme (PMIU-PERSP), the number of enrolled and eligible students, i.e. students maintaining 80% attendance, increased

[13] The stipend was directly disbursed to the household via a postal order from the District Education Office. In 2017 the mode of delivery changed to using mobile money and the amount of the cash transfer increased four folds to PKR 1000 per month. These changes do not effect the cohorts we evaluate in our study.

[14] District is the third administrative tier in Pakistan, after provincial and national government. Added in response to saying how many districts in total: Punjab consisted of 34 districts in 2004. Two tehsils, Nankana Sahib in 2005 and Chinot in 2008 were separated from Sheikhupura and Jhang districts, respectively, and made into separate districts.

over time to 454,832 in 2016 and 470,837 in 2018.

Enrolment rates calculated from the Multiple Indicators Cluster Surveys (MICS) in 2003 and 2017, for girls in the relevant age group (11-16 years old), show that enrolment has increased overall since the FSSP was rolled out, but the recipient districts still lag behind the non-recipient districts (See Appendix Figure A.3). Existing evaluations have shown that the FSSP increased secondary school enrolment rates for girls in the short to medium term (Chhabra et al., 2019). Chaudhury and Parajuli (2010) show that the program was successful in increasing enrollment rates in schools for girls in recipient districts by 9 percentage points compared to the non-recipient districts and there is evidence that this effect lasted at least another 5 years (Alam et al., 2011). Moreover, beneficiary adolescent girls are more likely to complete middle school and work less (in terms of child labor). Existing evidence does not show spillover effects on male children in the households; however, long-run and inter-generational effects of the program are as yet unexplored.

3 Data

We use the Multiple Indicators Cluster Survey (MICS) for Punjab for our analysis. MICS is a cross sectional household survey designed to monitor indicators related to well being of women and children worldwide. To date, over 300 rounds of surveys have been collected in more than 100 countries. This study uses data from MICS conducted in Punjab, Pakistan in 2003, 2011, 2014 and 2017. The survey contains detailed information regarding age, education and health of all members of the households. More importantly for our study, MICS is representative at the district level and has two questionnaires designed for women and children that collect information about maternal and child health. In particular, for women of childbearing age (15-49 years), MICS has information pertaining to age of marriage and first birth, number of births, and maternal health care utilization.[15] For children under the age of five, MICS collects information about current weight

[15] Information on maternal healthcare utilization is collected for births in the two years prior to the survey.

and height (anthropometric measures administered by the survey team).

Table 1 shows the summary statistics for the women in our sample, by treatment and control districts. Women in the treatment districts are exposed to the program for 3.4 years, on average and are 22 years of age. 87 percent report living in houses that their families own, with 7.8 other members living in the same household. While the differences in these variables between control and treatment districts are statistically significant, they are small in terms of economic magnitude and do not show any meaningful difference for practical purposes.[16]

As discussed in section 2, economic and educational outcomes are lower for individuals in the treated districts. Women in the treatment districts are less likely to have completed secondary school and, on average, complete only 4.6 years of schooling compared to 6.8 years of schooling for women in the control districts. Women in the treatment districts are also more likely to get married before the age of 16 and more likely to have their first child by the age of 17. We see a similar trend in prenatal and postnatal checkup rates.

Appendix Figure A.5 plots average education and maternal health outcomes for control and treatment districts over time. We find change over time in average rates of primary and secondary schooling completion remain qualitatively similar for treated and control districts.[17] On the other hand, the rate of early marriages and, subsequently, early births has declined over time. The proportion of women who report receiving prenatal care is consistently high, though postnatal care falls for more recent cohorts.

Table 1 also shows the summary statistics for the children in our sample (Panel (b)). This sample consists of all children under the age of 5 for the 2011, 2014 and 2018 rounds of the

[16] In section 5.2, we show that our main results are robust to the inclusion of these household characteristics as controls.

[17] Educational outcomes seem to have improved and slightly worsened over time, similar to what is observed for Pakistan overall. Planning Commission Pakistan (2013) reports rates of school enrolment and primary completion in Pakistan increased up to the mid-2000s and then slowed and fluctuated, with primary completion averaging at 50 percent in 2010, when the cohort born in 2000 would be expected to be 10 years old. More recent data shows stagnant rates between 2012 - 2018 (Department of School Education, 2020).

survey.[18] The average child in the treatment districts is 1.6 standard deviations below the average for Weight for age (WAZ) and 1.4 standard deviations below the average for Height for Age (HAZ), compared to 1.3 and 1.05 in the control districts, respectively.[19] These correspond to higher rates of stunting (32 percent) and being underweight (34.7 percent) in the treatment districts compared to the control districts (22.7 percent versus 25.5 percent, respectively).[20] Children across treatment and control districts, as expected, do not meaningfully differ on other characteristics such as age (1.8 years), proportion male (51 percent) and birth order (2.3). Table A.2 shows that conditional on being exposed to the program, mothers on average have 3.25 years of exposure to the program. This is lower than the exposure for the overall women's sample in Panel (a) of Table A.2 since only a sub-sample of women have children below the age of five. Appendix Figure A.6 plots district averages of child health outcomes over time. We see moderate improvements over time in the child health, with a decrease in the proportion of children who are reported as underweight and decreasing rates of child mortality in the control and treatment districts.

Our identifying variation, however, does not solely rely on treatment and control districts. We also use the birth cohort and age at the time of initiation of the FSSP in 2004 to assign *expected* years of exposure to the program (See Figure A.4 for details of exposure by cohort). In Table A.2, we show summary statistics for the sample disaggregated by no (expected) exposure to the program versus at least a year of exposure.[21] Table A.2 shows that conditional on any exposure,

[18] Children's data in MICS 2003 does not include identifiers for mothers and can not be linked to mother's information. We therefore are unable to include the child sample of 2003 in our analysis.

[19] The MICS survey follows World Health Organization's guidelines for constructing the WAZ and HAZ measures based on the children's anthropometric measures the survey teams collects. The WAZ is calculated by subtracting the the relevant group specific median weight from child's weight and dividing the difference by the corresponding standard deviation. A group is defined according to child's sex and birth month and year. The median weight and standard deviation for the group uses the reference distribution set by the WHO. HAZ is similarly calculated using the height measures.

[20] A child is considered underweight if the WAZ score falls more than two standard deviations below the WHO Child Growth Standard Median for weight. A child is considered stunted if the HAZ measure falls two standard deviations below the WHO Child Growth Standard Median for height.

[21] In addition, in Tables OA.1 and OA.2, we also show birth cohorts benefiting from the FSSP versus not and combination of birth cohort and being in a control or treatment district.

women in our sample on average have 5 years of exposure to the program.[22] Women not exposed to the program are, as expected, older (23.5 years vs. 20.1 years on average). On other characteristics (home ownership, members in the household, number of rooms in the household), they are similar to women who have at least one year of treatment. In our estimations, we include district and cohort fixed effects that allow us to control for the district and cohort level differences in education and healthcare measures in our sample.

4 Estimation strategy

We estimate the effects of the stipend program on women's education, their longer term outcomes and the inter-generational effects on children. We do not have individual-level data on whether the female's household actually received the stipend, or the number of years they received the stipend for. The identification in this setting comes from exposure to the program, which is based on two components. First, the woman needs to be a resident of the district receiving the program - that is, a treated district in this context. Women who reside in the non-recipient, or control districts, are part of the control sample.[23] Second, we exploit the exogenous timing of the introduction of the program in 2004 and women's age at that time. That is, the effects we investigate are the *intent-to-treat* effects of the program. Ex-ante, we expect the measurement error on actual exposure to the program to bias the outcomes towards zero. This mean that the results of subsequent estimations can be interpreted as a lower bound of the true effect of the program.

[22] We only include women aged 15 and older in our sample. A "lower" number of years to exposure (e.g. 2) is assigned to women who were already of secondary school age when the program started. In subsequent analysis, we consider both primary and secondary school years when assigning years of exposure on the premise that the incentive of receiving stipend at the secondary school level may encourage girls and their families to continue and complete primary school. This results in a maximum exposure of 10 years for women in our sample for whom the FSSP was in place through out their school going years. We discuss this in detail in Section 4.

[23] Approximately a fifth (19%) of women with zero years of exposure to FSSP (due to being in the older cohorts) are in treated districts. In Section 5, we provide average outcomes for women with zero exposure as well as for the full sample.

4.1 Effects of the FSPP on women's education and long run outcomes

We begin by estimating the intent to treat effect of the program on women's education in reduced form. District eligibility is determined on the basis of district literacy rates recorded in the 1998 Population Census. The program was initiated in all districts with literacy rates less than 40 percent. We can reasonably assume that individuals did not choose to locate in treatment or control districts in 2003 *in anticipation* of the policy being implemented in the near future. As explained earlier, the stipend amount even if known earlier, was not large enough to induce significant migration. In fact, overall migration of the potential recipient sample in subsequent years seems low - only 0.3 percent of families with girls report moving across districts for reasons related to education. (Pakistan Demographic and Health Survey 2012).[24]

The stipend is offered to girls in grades 6-10. Typically, girls aged 11 to 15 years are enrolled in these grades.[25] We retrospectively assign exposure to the program based on the treatment status of the district the woman belonged to and of number of years the program was in place during her school going years. Women in the control districts had no exposure to the program. Women aged 17 and older in 2004 in the treated districts were also too old to benefit from the program and hence had no exposure to the program. These two groups of women form our control sample. Years of exposure to the FSSP for girls aged 16 and below (in the treated districts) are calculated based on their age in 2004. For example, girls aged 14 in 2004 were exposed to FSSP for 2 years, while girls aged 6 in 2004 have 10 years of exposure, and so on. Appendix Figure A.4 summarizes the expected years of exposure based on birth cohort. Note that since the option of receiving stipend in the future at the secondary level can also be an important factor in households decision to enroll

[24] These are author's calculation from the Pakistan Demographic Health Survey of 2012 that documents detailed migration history of individuals and households. MICS does not provide any information on migration.

[25] In the initial two years the program targeted girls in grades 6-8 and later expanded to grades 9-10 as well. We adjust for this in assigning years of exposure to the women in our sample. In addition, owing to delayed enrollments and grade repetitions we can expect some over age enrollments. Our results remain robust to excluding cohorts that may potentially be exposed due to over-age enrolments and are available in Table OA.12 in the Online Appendix.

girls in school, we also include the years that the younger cohorts of girls spend in primary school as being 'exposed' to the program. Prior literature has also shown that gains in girls' schooling, as consequence of the reform may appear at each grade level including those where subsidy does not apply (Keats, 2018).[26] Our results support this assumption: as discussed in Section 5, the program increases primary school completion rates.

We first estimate the effect of the program on women's education as follows:

$$Y_{idk} = \alpha_0 + \alpha_1(\text{Years of exposure})_{idk} + \delta_d + \sigma_k + \epsilon_{idk} \tag{1}$$

Where Y_{idk} is an education outcome for individual i (years of education, indicator for completing primary school and indicator for completing secondary school), living in district d, from cohort k. The relevant sample for primary school completion are all women aged 10 years or older. For secondary schooling, we limit the sample to women who are at least 15 years old.

Years of exposure$_{idk}$ is the number of years the woman was exposed to the FSSP during her school going years. We restrict this estimation to women who were 16 years and older at the time of survey. This is to account for the fact that we can not ascertain the eventual age of marriage for women who were younger than 16 and were unmarried at the time of survey. For consistency, we therefore also exclude women who were below the age of 16 and married. These women are a small percentage (0.001) of our sample and re-estimating our regressions does not effect the results discussed in section 5.

All OLS regressions include district (δ_d) and cohort fixed (σ_k) effects to account for any differences across districts and cohorts other than the program that might be accounting for differences in educational attainment. This would, for instance, control for initial differences in educational and health indicators in the treated and control districts. Controlling for the interaction of district and cohort effects would absorb a large part of the variation that we are exploiting. There is a concern however that differential change in the provision of health services across district over time

[26] Note that unlike our assumption Keats (2018) estimates an effect on secondary school grades when the school fee was abolished for primary school in Uganda.

might be driving some of the effects. We address this concern in our robustness checks by adding indicators for availability of health services in districts over time. The coefficient, α_1, provides a measure of change in educational attainment due to an increase in exposure to the program by one year.[27]

Next, we estimate the impact of the FSSP on women's later life outcomes. We use exposure to the policy as the main variable of interest and estimate Equation 1 for long term outcomes. Specifically, we investigate impacts on marriage before the age of 16 and first birth before the age of 17 and maternal health care utilization (i.e. binary indicators for prenatal and postnatal check up).[28] As before, we include district and cohort fixed effects, δ'_d and σ'_k, respectively. β_1 captures the impact of one additional year of exposure on the outcome of interest. Ex-ante, we expect β_1 to be negative for probability of teenage marriage and pregnancy and positive for maternal health care utilization.

4.2 Inter-generational effects of the FSSP

Next, we estimate the inter-generational impact of the mother's exposure to FSSP on children using the following OLS regression:

$$C_{cidk} = \theta_0 + \theta_1(\text{Years of exposure})_{idk} + \theta(X_{cidk}) + \delta''_d + \sigma''_k + \gamma''_s + \upsilon_{cijk} \tag{2}$$

C_{cidk} is the outcome of interest for child c, born to woman i from cohort k, in district d. X_{cidk} is a set of child controls such as age of the child, gender of the child (and birth-order for robustness).

[27] Our model assumes a linear effect for each year of exposure. We also estimate non-linear models using binary indicators for total years of exposure as well as indicators for 0-5 years of exposure, 6-9 years of exposure and 10 years of exposure. These results largely support the linear model presented in section 5, with some effects becoming stronger as exposure increases. In the interest of brevity, we show results from binary ranges (0-5, 6-9 and 10 years) in Online Appendix Tables OA.3 - OA.5.

[28] Women in Pakistan typically do not have children out of wedlock due. In fact, cultural and religious norms would discourage reporting any such births to survey teams. In our main results, we therefore present results for marriage before the age of 16 and childbirth before the age of 17. Using ages 15, 16 and 17 as cutoffs, however, provide results consistent with those discussed in Section 5.

Outcomes of interest for children under the age of five include, current standardized weight and height scores and indicators for stunting and being underweight. In addition, we also estimate the impact of mother's exposure to the program on child mortality using an indicator for whether the mother ever had a child who later died. *Years of exposure*$_{idk}$ are the years the child's (c) mother (i) was exposed to the FSSP in her school going years. All other variables are as defined for Equation 1. Additionally for outcomes related to child weight and height, measured by the MICS survey team, we also control for survey year fixed effects (γ_s).

Note, some measures of education or health outcomes discussed in this section may proxy the same outcome. To deal with the possible multiple inference problems we report for each estimation both the the p-value for the estimated treatment effect, and a sharpened q-value, calculated within each listed outcome (see Benjamini et al. (2006)).

4.3 Threats to identification

One of the threats to identification is the concern regarding endogenous migration, i.e. migration of people to the treatment districts induced by the FSSP. There are several reasons why we expect this to not be an issue in this setting. First, the size of the transfer (USD 2.5 per month) is small and the amount leftover after accounting for costs associated with schooling (including transport, text books and uniforms), if any, is expected to be minimal. The transfer is not large enough to encourage households to move given the costs associated with migration itself.[29] Second, the non-stipend districts are on average economically better than the stipend districts, reducing the incentive to migrate from non-recipient to recipient districts even further. Third, while MICS does not provide any information on migration, we can make use of alternate data sources to estimate the role of treatment in driving migration in Punjab. We use data from the Pakistan Demographic Health Survey (PDHS), a cross sectional survey that provides health outcomes for a nationally representative sample of adults in the country. It also asks each individual about whether they

[29] It is estimated approximately USD 1 per quarter will be left over after schooling expenditures are covered (Alam et al., 2011).

now live in a different district than the one that they were born in. To test if exposure to FSSP changes the likelihood of migrating out of the district of their birth, we construct a variable for 'ever migrating' and estimate equation 1 on a sample from the 2012 and 2018 PDHS similar to the sample of women we use from the MICS survey. We find no evidence to indicate that the exposure to FSSP program impacts the likelihood of having ever migrated ($p - value = 0.692$).

Another important concern is whether pre-existing trends in outcomes for women in the treated and untreated districts may be explaining some of the effects we capture in our analysis. To address this concern, we test if trends were parallel for cohorts born between the years 1980 - 1986, who were already too old by the time the FSSP was implemented in 2004 to have been impacted from this program. We re-estimate equation 1, including indicators for birth year and test if the coefficients on birth year are different for women in treated and control districts. If outcome trends are parallel prior to the implementation of the FSSP, then the coefficients on these indicators should be statistically insignificant. This is indeed what we find - for women who were already too old to be exposed to the FSSP, there is no statistical difference in their educational, marital or child health outcomes by the treatment status of the districts they live in. Results are shown in Table A.3 in the Appendix.

Lastly, it is possible for differential changes in availability of health services across treatment and control districts over time to drive some of the longer-term effects on health-related outcomes. Figure A.7 plots the number of hospitals per capita and number of hospital beds per capita over time.[30] While control districts have both higher per capita hospitals and hospital beds, the trend over time is similar indicating that services were not changing differently over time across the two groups. To address this concern further, we add these per capita measures of healthcare facilities for each district, over time, in our main specifications. We show in Section 5.2 that our results remain robust to these controls, implying that our estimated coefficients are not capturing some other underlying trend in availability of health services and do in fact estimate the impact of the

[30] The data is obtained from Punjab District Development reports, available online at http://www.bos.gop.pk/developmentstat.

program.

5 Results

We begin by estimating the effect of exposure to the FSSP on educational outcomes of women. Table 2 shows the results from estimating Equation 1. Column 1 shows that each year of exposure to the FSSP, increases the likelihood of women completing primary schooling by 1.1 percentage points, which is an increase of 1.8 percent over the sample average. This finding implies that the incentive of receiving a cash transfer in secondary schools induces girls to complete primary schooling. Results in Column 2 show that women are 0.4 percentage points more likely to complete secondary schooling for each year they are exposed to the FSSP, an increase of 1.3 percent on the sample average secondary school completion rate of 31 percent. Column 3 shows that this increase in school completion rates corresponds to 0.05 more years of schooling; an increase of 0.8 percent over the average 6 years of schooling for the sample. These findings are inline with previous evaluations of the FSSP that estimate increase in school enrollment rates, at the school level, as a result of the program (e.g. Alam et al. (2011) and Independent Evaluation Group (2011)). In particular, evidence from India shows that improved access to secondary (Mukhopadhyay and Sahoo, 2016) and higher education (Jagnani and Khanna, 2020) increases enrolment at the primary level by reducing costs and increasing motivation for enrolling in school. However, these effects are smaller than the direct impact of primary school construction on primary schooling completion rates in Indonesia (Akresh et al., 2018).

Next, we re-estimate Equation 1 to investigate how the program affects women's later life outcomes. Column 1 in Table 3 shows each year of exposure to FSSP reduces the likelihood of women being married before the age of 16 by 0.3 percentage points on average or by 3.5 percent.[31] We further estimate if this translates into delay in childbirth. As seen in Column 2, women are 0.2

[31] To ensure that the change in sample size due to age restrictions in our estimated equations is not driving the results, we estimate equation 1 when we include women who were at least 15 years old. The results are qualitatively similar and available in the Online Appendix in Table OA.7.

percentage points less likely to have their first child before the age of 17, a decrease of 3.8 percent on the sample average.[32]

We also find women exposed to the program are more likely to seek maternal healthcare. Column 3 in Table 3 shows women are 1 percentage point more likely to have a pre- and postnatal check ups. We do not unfortunately have a direct measure of maternal mortality in our data. A lack of maternal healthcare utilization, including prenatal and postnatal care, is one of the leading causes of maternal deaths in Pakistan (NIPS Pakistan and ICF, 2020). As such, our findings imply that the FSSP might be contributing to a reduction in maternal mortality in Pakistan by inducing women to seek maternal healthcare.

We estimate the inter-generational effects of the FSSP through Equation 2. Specifically we look at child health outcomes. Column 1 and 3 of Table 4 show that children of women exposed to the FSSP score higher by 0.014 standard deviations on standardized measures of Weight for Age (WAZ) and by 0.007 standard deviations on standardized measures of Height for Age (HAZ). Height-for-age, given gender, is a commonly used measure of child health and an indicator of long-term health status, whereas weight-for-age, given gender, is a measure of current child health status and provides information on the current malnutrition status (Thomas et al., 1991).

We also check for effects on the important margins for underweight (below 2 standard deviations of WAZ) and stunting (below 2 standard deviations of HAZ). Stunting reflects chronic malnutrition and linear growth retardation resulting from lack of adequate nutrition over a long period which may be exacerbated by recurrent and chronic illness (Gross et al., 1996). Columns 2 and 4 of Table 4 show children of women exposed to the program are 0.5 percentage points less likely to be underweight and 0.3 percentage points less likely to be stunted. These estimates imply

[32] We test for marriage and child birth before the ages of 15 and 17 as well and find similar results. The first study is one of the reviewer 2 suggested we add: These results are smaller than the 7 pp decrease in marriage before 18 for universal primary education beneficiaries in Uganda (Masuda and Yamauchi, 2020). In addition, in contrast to Duflo et al. (2019) who find a reduction of 6 percentage points in fertility of 22 year old women, we stop seeing a statistically significant effects on marriage and child birth at the 18 years cutoff. This potentially indicates that the impact may largely be driven from girls staying in school longer. Table OA.6 in the Online Appendix provides these results.

that children of women exposed to the program are 1.7 percent less likely to be underweight and 1.15 percent less likely to be stunted based on average underweight and stunting rates of 29 percent and 26 percent for the control group.[33]

Literature provides some evidence on the short to medium term effects of cash transfers on health outcomes of children (Evans et al., 2014; Akresh et al., 2018; Masuda and Yamauchi, 2020). In Pakistan, an unconditional social assistance program, Benazir Income Support Program (BISP), has been found to significantly reduce the likelihood of girls being underweight with no effect for boys of this age (Cheema et al., 2014). Our results support this latter view, with larger improvements in health outcomes for girls. Appendix Table A.5 shows the results for Equation 2 separately for boys (Panel (a)) and girls (Panel (b)). Columns 1 and 2 in both panels show that the magnitude of the effect of mother's exposure to the FSSP on child WAZ and HAZ is similar for boys and girls (p = 0.609 and p = 0.491, respectively), but the effect on boys HAZ is statistically insignificant, possibly due to a loss in statistical power from a smaller sample size than before. We also see reduction in incidence of stunting and being underweight for both boys and girls, though not statistically different (p = 0.647 and p = 0.348, respectively).

Finally, we look at child mortality. Column 5 in Table 4 shows that women exposed to the program are 0.3 percentage points less likely to experience the death of a child. With 17 percent of women in the sample reporting having have lost a child, this is an important reduction of 17.6 percent on sample average. While this estimated reduction does not differentiate between infant and child mortality, it lends support for using girls education program for long run meaningful gains in child mortality.We discuss some of the possible explanations for these results in Section 5.1.

Pakistan also has one of the highest rates of stunting (WaterAid, 2016) and child mortality rates in the world (Devine and Taylor, 2018). In recent years the issue of stunting in Pakistan has

[33] We do not include birth order in our main estimation because we consider the number of kids a women has to be impacted by the treatment and hence endogenous in the estimation of child health. Results however are largely robust to including birth order of the child. Appendix Table A.4 provides the results.

received both global and local attention. Our results show that programs that educated women may help in alleviating high rates of stunting and child mortality. Educated mothers may be better enablers of health by proactively seeking healthcare for their children and being more aware on nutrition, health and hygiene practices. It is worth noting that since young women's bodies are not ready for child-birth, delayed pregnancies can also improve child outcomes.

5.1 Potential mechanisms of change

While girls' school enrollment is central to international policies and programmes that intend to improve women's and children's health across the world, the understanding of how women's education impacts use of health services and health outcomes remain limited. In this section we bridge this gap in literature by exploring other downstream outcomes, which while being potential channels, may also be potentially endogenous to our main results on fertility, maternal healthcare and child health. In theory there may be several channels impacting the long term gains seen in Table 3 and Table 4. For example, women exposed to the FSSP, owing to their higher education, may participate and earn more in the labor market. This income effect may drive some of the improvements in health that we see. Unfortunately we do not have clean data on employment and income in MICS; it is a survey designed to track MDGs and focuses largely on those outcomes. We are therefore unable to analyze the impact on women's labor force participation and/or overall household income. However, prior research on Pakistan and alternate data sets do not indicate labor force participation to be a potential mediator for these effects. For instance, Andrabi et al. (2012) find no impact of women's education on their labor force participation and income. According to the Pakistan Social and Living Measurements Survey (PSLM 2004-05) more women in the treatment district (22%) than in control district (11%) report having worked for pay in the last month. The difference in labor force participation rates are driven by a substantially higher proportion of women working in agriculture in the treatment districts (15%) than in the control districts (7.5%) but not due to differing education levels. Women with at least 5 years of education (primary level) are not less likely to work, nor do they earn less than their counterparts with between 6 - 10 years of education (secondary but higher than primary). Overall, women with 6 - 10 years of education are earning only PKR 90 (~$1.5) more per month than women with 5 years of education. Though labor force participation rates increase over the next 10 years to approximately 32% in the treated districts and 13% in the control districts as per the 2014 PSLM data, average female labor force participation do not vary by primary and secondary education attainment in treatment and control districts.

Another potential mechanism of change may be assortative matching in the marriage market. More educated women may be marrying better educated men resulting in overall higher education in the household. This might increase income in the household leading to better health outcomes of both women and children. We are able to match a sub-sample of women to their husbands and their husband's education level.[34] Using the same estimation strategy as Equation 2, we test if women exposed to the FSSP are more likely to marry men who have at least completed secondary schooling, compared to those who were not exposed to the program. Column 1 in Table 5 shows that for each year of exposure to the program, women are 0.7 percentage points more likely to marry men who have completed secondary education or higher. At an average of 27 percent this translates into a 2.6 percent change, indicating that some of the changes in later life outcomes, including those of children, may be driven by marriage market effects. This may also be suggestive of higher income in the household as a result, though we can not estimate the change in household income due to data constraints.

Increased maternal health care utilization can also lead to improved health outcomes of children. Women exposed to the FSSP are more educated, and may be more aware and knowledgeable about health practices (Grossman, 2006) or more likely to be in a position of privilege that commands respect from health care providers (Gittelsohn et al., 1994). Literature documents education

[34] MICS identifies the relationship of each woman to the reported household head. This analysis is therefore limited to women whose husband is the reported household head. To rule out selection, we test whether exposure to the treatment increases likelihood of women residing in household's with their husband as the household head. We find no evidence of treatment impacting this (See Table OA.8).

may lead to higher use of health services in three main ways: through direct transfer of information through curriculum in school (Baker et al., 2011; Boerma et al., 1991; Bhuiya and Mostafa, 1993; Frost et al., 2005); through being able to comprehend health messages in news and info-media (Glewwe, 1999; LeVine et al., 2004; LeVine and Rowe, 2009); and by enhancing reasoning, decision making skills and trust in modern medicine that can affect health seeking behavior (Glewwe, 1999; Peters et al., 2010; Baker et al., 2011). We can not check for all of the possible channels of behind enhanced health knowledge. However following (Greenaway et al., 2012), we proxy for improved knowledge of health by using the question on HIV administered by the MICS survey. Each adult in the MICS survey is asked if they are aware of AIDS/HIV. We use this binary indicator on women's knowledge about AIDS/HIV as proxy to being more knowledgeable on health issues. Column 2 in Table 5 shows that each year of exposure to the FSSP makes women 0.8 percentage points more likely to be aware of HIV/AIDS, compared to those who are not exposed. While this question is not directly related to maternal or child health, it provides some suggestive evidence of exposure to the program improving awareness about health issues.

Similarly, the reduction in teenage child birth (as estimated in Table 3) may potentially be due to increased knowledge and use of contraception among women exposed to FSSP. In Column 3 of Table 5, we use a binary variable to indicate if the women has "ever used contraception" in her life. We find a positive but statistically insignificant effect of exposure to the FSSP on contraception use. In line with (Grépin and Bharadwaj, 2015) and in contrast to Keats (2018), we find no evidence of use of contraception driving the delay in fertility. This finding may point to either unmet contraception needs or use of contraception not changing with education. In case of the latter, our findings imply that educating girls may not be an effective tool to increase contraception and other policy measures maybe needed.

Finally, we borrow from the literature in sociology which posits that exposure to more education is an experience that changes women's attitudes and influences adoption of modern ideas, including attitudes towards traditional gender roles, mobility outside and agency in the household as women feel more empowered (Caldwell, 1979; Jejeebhoy, 1995).[35] To test this mechanism, we use data collected in the 2014 and 2018 rounds of MICS, where women are asked several questions about about situations where they think husbands are justified in beating their wives, for example, if the wife is neglecting children; argues with her husband; goes out of the house without informing the husband; burns food while cooking; and refuses to have sex.

Column 1 of Table A.6 in the Appendix shows the effect of the FSSP on women's response to the question on neglecting children. The variable takes a value of 1 if the woman thinks it is *not* justified for the husband to beat his wife if she neglects her children, and zero otherwise. In other words, a positive coefficient would be indicative of greater women empowerment. We see a positive and statistically significant effect; women exposed to the FSSP (for each year of exposure) are 1.78 percentage points more likely to say that beating in this scenario would not be justified. We find similar results for other outcomes as well. Columns 2 - 4, Table A.6 show results on all other scenarios women are questioned on as well. Women exposed to FSSP are more likely to believe that beating is unjustified if the wife argues with the husband, goes out of the house without his permission, refuses to have sex or burns food while cooking. We regard this as suggestive evidence that women's empowerment may be a possible mechanism behind the effects.

5.2 Robustness checks

We test the robustness of our estimates in three main ways. First, we address the concern of an underlying variation in provision of health services (or other programs) across districts over time that might be driving our results for improvements in maternal health care and child health measures. We re-estimate Equation 1 controlling for for hospitals per capita, hospital beds per

[35] Household decisions in Pakistan are often dominated by hierarchies based on gender and age. Constraints on women's physical mobility outside the home (such as contact with unrelated male) can restrict their ability to access healthcare. This may change due to greater exposure to modern institutions such as schools (Jejeebhoy, 1995; Jejeebhoy and Sathar, 2001; Basu, 1992; Das Gupta, 1990; Bloom et al., 2001).

capita and lady health workers per capita in the district.[36] Results are provided in Appendix Table A.7. Coefficients on all maternal and child health measures remain robust to these controls in terms of magnitude and direction of the impact, implying that differential expansion of services across districts over time is not driving the results we see.

Second, we check for robustness of our results to restricting the sample to districts that are similar in terms of initial levels of literacy. Specifically, we restrict the sample to districts that are closer to the policy cutoff of 40 percent district literacy rate (according to the 1998 census), Panels (a) and (b) in Table A.8 show the results for women's own outcomes when we restrict our sample to districts with literacy rates between 30 to 60 percent, thereby dropping two districts with very low and three districts with high rates in 1998. Results are consistent in terms of magnitude and direction of effect for all outcomes, though we lose statistical significance for secondary school completion, years of education, early births and for child mortality. Results for child health also remain robust, though once again the coefficients on HAZ and stunting have the expected signs but are no longer statistically significant.[37]

Third, we run a binary difference-in-difference estimation, regressing outcomes on measures of whether the woman lives in a treated district, has had (potential) exposure to the program in her school going age, and an interaction term of these two indicators. The interaction term represents a difference-in-difference estimation of the effect of FSSP. Results are provided in Appendix Table A.9. We find generally robust results, with both the signs and significance levels consistent with the regression results provided in Tables 2 - 4

[36] Lady health workers (LHW) are community members trained by the government to provide basic and essential health services (see WHO Case Study 2008) for details). Each LHW is attached to a local government facility, which provides them with training, basic medical supplies and a monthly allowance. Data for the number of LHWs per district is available from 2013 at the provicinal Programme Monitoring and Implementation Unit (PMIU). We also control for Basic Health Units (BHU) per capita, beds in BHUs per capita, Mother and Child Healthcare Centers (MCH) per capita and beds in MCHs per capita as controls. Results remain robust to inclusion of these health services measures and are available in the Online Appendix Table OA.9.

[37] In Online Appendix Table OA.10, we restrict the sample further to +/-10 percentage points of literacy rate around the 40% literacy threshold. This sample is reduced substantially, leading to statistically insignificant results on several outcomes. However, the direction of effects remain qualitatively similar.

In the Online Appendix, we show further tests. Specifically, we drop older cohorts of women who may not have had the sufficient exposure to the program. The sample for our main analysis in Table 2 (and Table 3) comprises of women born between 1980 and 2002. In Table OA.11 we drop women born between 1980 and 1985 and re-estimate the results in Tables 2 and 3. Panels (a) and (b) of Online Appendix Table OA.11 show that our results remain robust in terms of statistical significance and magnitude for women's educational, marital and maternal care outcomes. Likewise in Panel (c), WAZ and HAZ scores are higher, and children are less likely to be underweight or stunted.[38] Second, our main regressions assume that women aged 17 and older in 2004 are too old to benefit from the program and have had no exposure. However, over-age enrollments may be possible and including women who were 17 or older in 2004 in the control, rather than the treated, group may have overestimated average outcomes in the control group. We follow Grépin and Bharadwaj (2015) and test if our results are robust to the exclusion of these. Results are shown in Table OA.12 in the Online Appendix. We find that dropping women who were aged 17 or 18 in 2004 from our sample does not significantly change our results, indicating that our results are not being driven by this group of women. Similarly, in results not shown here, we also test if estimates are robust to the exclusion of the youngest 5 cohorts of our study sample. All results remain qualitatively similar, with the exception of the effect of the program on likelihood of first birth before the age of 17, which loses statistical significance. Finally, we add controls in the regression for women for their household characteristics on which we find initial differences in control and treated households. Results remain robust and are provided in Online Appendix Table OA.13.

6 Conclusion

In this paper we estimate the long run effects of a conditional cash transfer for girls attending public secondary school in Punjab, Pakistan. We find exposure to the program during school going

[38] The direction and magnitude of these coefficients are similar to that of an unrestricted sample shown in Table 3, though coefficients on the HAZ and stunting measures are not statistically significant, perhaps due to a loss of statistical power.

years increases the probability of completing secondary schooling, reduces the likelihood of early marriage and pregnancy, and increased take up of maternal healthcare. We find children of women exposed to the program score better on standardized measures of weight and height and are less likely to be underweight or stunted. These effects are potentially driven by assortative matching in the marriage market, increased awareness about health and women being more empowered.

Our work builds on the existing literature on impact of school construction, Universal Primary Education (UPE) and scholarship programs on women's later life outcomes (Andrabi et al., 2012; Barham et al., 2013; Keats, 2018; Osili and Long, 2008; Duflo et al., 2015, 2019; Masuda and Yamauchi, 2020). We add to this literature by evaluating the impact of *secondary* schooling through a unique non-means tested program. Moreover we look at important inter-generational health benefits, the evidence on which is sparse. Specifically the measures we look at are important policy targets. Early childhood malnutrition is one of the biggest healthcare challenges in developing world. According to the WHO, 141 million (21%) under-five children around the world are stunted[39]. In South Asia, under-five stunting is most prevalent in South Asia with 32.7% of under-five children stunted. This form of under nutrition is linked to worse economic outcomes as an adult. Prior research in other countries has shown that a 1 standard deviation increase in HAZ is associated with 14% increase in lifetime earnings, 21% increase in household per capita expenditure and a 10% point decrease in the probability of reporting living in poverty at ages 25 – 42 years (Hoddinott et al., 2013; Alderman et al., 2001). Further a review by (McGovern et al., 2017) suggests that countries facing high stunting prevalence such as those in South Asia should consider programs that improve childhood nutrition as cost-beneficial investments in the well-being of their populations and economies.

Our study shows that programs designed to increase higher schooling for girls may have substantial effects not just in terms of increased schooling for girls, but also for important issues of teenage marriage and maternal health. These outcomes are especially important for low-income

[39] The data is available online at https://www.who.int/data/gho/data/indicators/indicator-details/GHO/gho-jme-stunting-prevalence

countries with poor health and educational outcomes like Pakistan that contribute significantly to the global burden of maternal deaths. Such programs may also lead to gains in improving child mortality and child health and alleviate high rates of stunting observed in many developing countries. These benefits must be considered when evaluating such policies aimed at increasing schooling for girls. For the case of FSSP, the cash transfer amounts to USD 200 per girl, for the entire five years of secondary school. While we do not have detailed cost information on overheads, literature suggests benefits may considerably outweigh the costs (see, for instance, McGovern et al. (2017) for a review).

Data limitations highlight several avenues that future research may improve on. First, while we do not find any change in contraceptive use in the age range in our sample, we are only able to conduct our analysis on young women, few years post secondary school age. Effects on lifetime reduction in fertility remain to be seen. Similarly, the effect on women's labor force participation and income may shed more light on the mechanisms behind the observed changed. Second, while our research documents important changes in child health as a result of the FSSP, further research maybe helpful in looking at the impact on older children and important outcomes like child labor. Third, we find the stipend for secondary schooling increases primary enrolment. Similar results from recent studies in India indicate that better access to higher education may reduce costs and/or improve motivation for primary education (Mukhopadhyay and Sahoo, 2016; Jagnani and Khanna, 2020). While data limitation do not allow us to explore the mechanism behind an increase in primary education in our context, data on changes in household expenditure or infrastructural access to secondary education items may help shed light on potential drivers of this effect. Lastly, the amount of the FSSP increased four folds in 2017 to PKR 1000 per month. Future research can exploit the variation in stipend over time to explore the effect of size of stipend on outcomes of interest.

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Tables

Table 1: Average characteristics of women and children in control and treated districts

	Control Districts			Treatment Districts				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel (a): *Women*	Mean	SD	N	Mean	SD	N	*Difference*	$p-value$
Years of Exposure	0.000	0.000	131500	3.406	3.612	87215	-3.406	0.000***
Secondary Completion	0.376	0.484	131308	0.223	0.417	87132	0.152	0.000***
Highest Grade	6.847	4.811	131266	4.644	4.877	87121	2.203	0.000***
Married before 16	0.060	0.238	121912	0.108	0.310	81205	-0.048	0.000***
First Birth before 17	0.036	0.186	108424	0.059	0.235	73018	-0.023	0.000***
Prenatal Checkup	0.911	0.284	23243	0.796	0.403	17964	0.115	0.000***
Postnatal Checkup	0.529	0.499	22961	0.510	0.500	17705	0.019	0.001***
Child Died	0.150	0.358	40870	0.197	0.197	30253	-0.047	0.000***
Age	22.819	5.553	131500	22.577	5.480	87215	0.242	0.000***
Members in the household	7.816	3.582	131500	7.864	3.668	87215	-0.048	0.003***
Household Head Owns Home	0.869	0.337	131449	0.879	0.326	87190	-0.009	0.000***
No. of Rooms in the House	2.615	4.705	131500	2.400	4.752	87215	0.215	0.000***
Panel (b): *Children*								
Weight-for-age (z-score)	-1.309	1.155	48994	-1.586	1.117	37790	0.278	0.000***
Height-for-age (z-score)	-1.056	1.344	48994	-1.364	1.375	37790	0.308	0.000***
Stunted	0.227	0.419	48994	0.318	0.466	37790	-0.092	0.000***
Underweight	0.255	0.436	48865	0.347	0.476	37668	-0.093	0.000***
Age of Child	1.848	1.395	48994	1.870	1.412	37790	-0.021	0.040**
Male	0.512	0.500	48994	0.515	0.500	37790	-0.003	0.300
Birth Order	2.294	1.302	48994	2.459	1.432	37790	-0.165	0.000***
Mother's Years of Treatment	0.000	0.000	47681	1.341	2.277	36511	-1.341	0.000***

Note: We report mean, standard deviation and number of observations for variable listed in rows for women in panel (a) and for children in panel (b) for control and treated districts. The data for Panel A comes from all four rounds of MICS. Sample in Panel A is women born between 1980 and 2002 who were aged 15 years of older at the time of survey. Years of Exposure is the years of exposure the woman had to FSSP during her school going years. Information on *Prenatal checkup* and Postnatal checkup are binary indicators for whether the woman had a prenatal and postnatal checkup and is only available for women who gave birth in the two years prior to the survey. *Child Died* is reported as a proportion of women who report ever giving birth. The data for Panel B comes from three rounds of MICS (2003 is excluded because data does not allow matching mothers and children). Column (7) report difference in means reported in columns (1) and (4). Finally, column (8) reports $p-values$ from t-tests of the difference in means in columns (1) and (4). *** p<0.01, ** p<0.05, * p<0.1.

Table 2: Effect of FSSP on Women's Educational Outcomes

	(1) Completed primary	(2) Completed secondary	(3) Years of education
Years of exposure	0.011***	0.004**	0.050**
	(0.002)^{AAA}	(0.002)^{AA}	(0.024)^{AA}
Mean (control)	0.63	0.34	6.32
Mean (full sample)	0.60	0.31	5.96
Observations	225,195	218,387	218,387
R^2	0.079	0.067	0.093

Notes: This table shows the estimation results from Equation 1. The data comes from pooling four rounds of MICS. The sample consists of all women born between 1980 and 2002 who were at least (1) 10 years or older for primary school completion (Column 1) and (2) 15 years or older for secondary school completion and completed years of education (Column 2 and Column 3). *Completed primary* is binary indicator for 5 years of education or more. *Completed secondary* is an indicator for 10 years of education or more. *Years of education* are completed years of education. *Years of exposure* is the number of years the woman was exposed to the FSSP during her school going years, which is 0 for women in the control districts. All regressions control for an indicator for treatment district, and district and cohort fixed effects. Standard errors (in parentheses) are clustered by district. *** p<0.01, ** p<0.05, * p<0.1. Adjusting critical values following the approach by Benjamini and Hochberg, 1995: ^{AAA}Significance at 1% level, ^{AA}Significance at 5% level, ^{A}Significance at 10% level.

Table 3: Effect of FSSP on Women's Later Life Outcomes

	(1) Married Before 16	(2) First Birth Before 17	(3) Prenatal	(4) Postnatal
Years of exposure	-0.003***	-0.001*	0.011***	0.010**
	(0.001)^{AAA}	(0.001)^{AA}	(0.003)^{AAA}	(0.004)^{AA}
Mean (control)	0.081	0.052	0.87	0.53
Mean (full sample)	0.085	0.053	0.86	0.52
Observations	188,461	151,714	41,177	40,637
R^2	0.028	0.018	0.049	0.031

Notes: The data comes from pooling four rounds of MICS. The sample consists of all women born between 1980 and 2002 who were at least (1) 16 years or older for *married before 16* years of age indicator (Column 1), (2) 17 years or older for *first birth before 17* years of age indicator (Column 2), (3) Had given birth in the two years prior to the survey for *prenatal* and *postnatal* care (binary indicators for any pre- or postnatal checkup during pregnancy for Columns 3 and 4, respectively). *Years of exposure* is the number of years the women was exposed to the FSSP during her school going years MICS administers the questions related to age of marriage and first birth to all women in the sample. The question pertaining to maternal health care utilization are only administered to women who had given birth within the two years prior to the survey. This is why we see a drop in observations in Columns 3 compared to the first two Columns. All regressions control for an indicator for treatment district, and district and cohort fixed effects. Standard errors (in parentheses) are clustered by district. *** p<0.01, ** p<0.05, * p<0.1. Adjusting critical values following the approach by Benjamini and Hochberg, 1995: ^{AAA}Significance at 1% level, ^{AA}Significance at 5% level, ^{A}Significance at 10% level.

Table 4: Intergenerational Effects of the FSSP on Child Health and Mortality

	(1) WAZ	(2) Underweight	(3) HAZ	(4) Stunted	(5) Child Mortality
(Mother's) Years	0.014***	-0.005***	0.007*	-0.003**	-0.003**
of exposure	(0.004)^{AAA}	(0.002)^{AA}	(0.004)^{AAA}	(0.002)^{AA}	(0.001)^{AA}
Mean (control)	-1.35	0.29	-1.16	0.26	0.17
Mean (full sample)	-1.38	0.3	-1.19	0.27	0.17
Observations	85,608	85,362	84,683	84,683	71,123
R^2	0.047	0.028	0.044	0.034	0.015

Notes: The data for Columns 1-4 comes from pooling three rounds of MICS. The 2003 MICS does not provide mother identifiers to link mothers to children. The sample consists of children under the age of five in the household, whose mothers were born between 1980 and 2002. The outcomes are as follows: (1) *Weight for Age Standardized score (WAZ)*, (2) Binary indicator for child being *underweight* (i.e. two standard deviations below the WHO standard for WAZ, (3) *Height for Age Standardized score (HAZ)*, (4) Binary indicator for being *stunted* (two standard deviations below the WHO standard for HAZ) and (5) *Child Mortality*, an Indicator for whether the mother reports having a child who later died. For Columns 1 and 2 we restrict the sample to children whose WAZ is between -5 and +5. For Columns 3 and 4 we restrict the sample to children whose HAZ is between -5 and +5. MICS administers the question on child death to all women who have ever given birth. Sample for Column 4 therefore comes from all four rounds of MICS. All regressions control for an indicator for treatment district, child's gender, child's age and district and mother's cohort fixed effects. We also control for survey year fixed effects since these outcomes include measurements of height and weight taken by the survey team each year. Standard errors (in parentheses) are clustered on mother's id. Years of exposure is the number of years the mother was exposed to the FSSP during her school going years. *** p<0.01, ** p<0.05, * p<0.1. Adjusting critical values following the approach by Benjamini and Hochberg, 1995: ^{AAA}Significance at 1% level, ^{AA}Significance at 5% level, ^{A}Significance at 10% level.

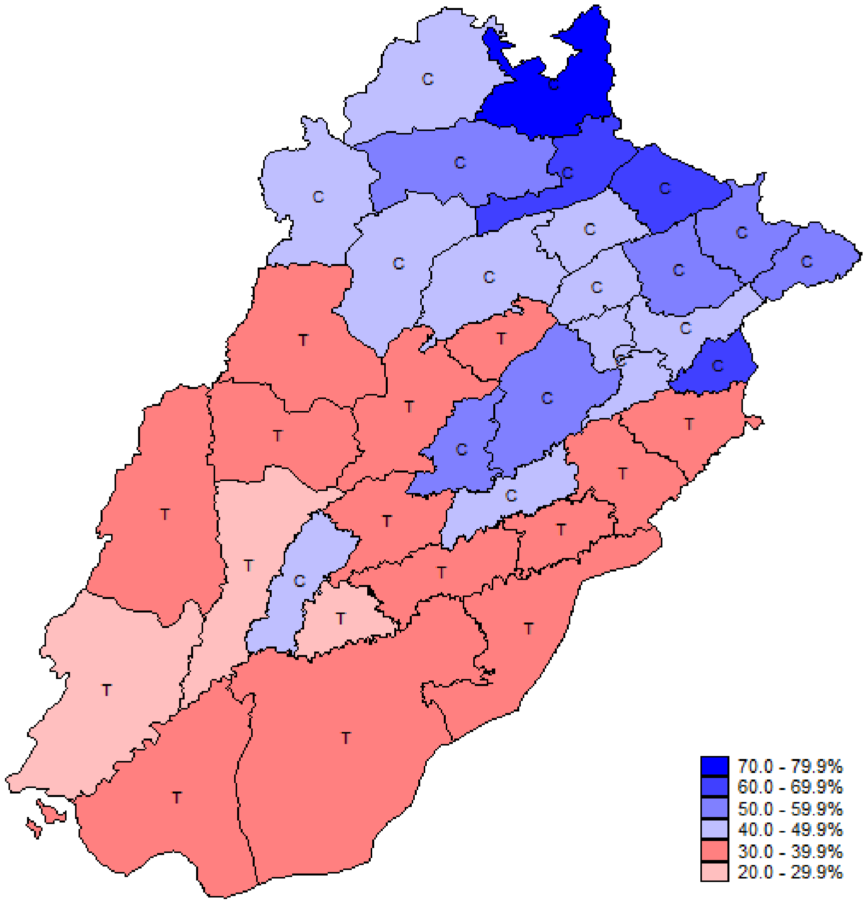
Table 5: Potential Mechanisms of Effects of FSSP

	(1) Husband's Education	(2) Knowledge of HIV/AIDS	(3) Ever Used Contraception
Years of exposure	0.007***	0.008***	0.003
	(0.003)^{AAA}	(0.003)^{AAA}	(0.003)^{A}
Mean (control)	0.36	0.36	0.38
Mean (full sample)	0.34	0.32	0.34
Observations	41,823	38,494	38,267
R-squared	0.026	0.063	0.044

The data comes from pooling three rounds of MICS. The 2003 MICS does not administer any of these questions. The sample women born between 1980 and 2002. For 2011,2014 and 2017 rounds we have identifiers for household heads and the relationship of the women with the household head. Column 1 therefore is a subsample of women whose husbands are also household heads (note: Treatment variable does not predict husband being identified as the household head). Outcomes in Column 2 and 3 are only administered to women who had given birth two years prior to the survey (this explains the smaller sample size). Outcomes of interest are as follows: (1) *Husband's Education* is a binary indicator for husband having completed at least secondary school, (2) *Knowledge of HIV/AIDS* is a binary indicator taking a value of one if the woman knows what HIV/AIDS is and zero otherwise and (3) *Ever Used Contraception* is a binary indicator taking a value of one if the woman has ever used contraception in her lifetime and zero otherwise. All regressions control for an indicator for treatment district, and district and cohort fixed effects. Standard errors (in parentheses) are clustered at the district level. Years of exposure is the number of years the woman was exposed to the FSSP during her school going years. *** p<0.01, ** p<0.05, * p<0.1. Adjusting critical values following the approach by Benjamini and Hochberg, 1995: ^{AAA}Significance at 1% level, ^{AA}Significance at 5% level, ^{A}Significance at 10% level.

Figures

Figure 1: FSSP treatment (recipient) and control (non-recipient) districts



Note: This figure plots district literacy rates, shown in Appendix Table A.1. Districts in pink (labelled 'T') are treatment or recipient districts, with literacy rates of 40 percent or below in the 1998 Population Census. Districts in blue (labelled 'C') are control or non-recipient districts, with literacy rates in excess of 40 percent.

Appendix

Tables

Table A.1: District Literacy Rates for Punjab based on 1998 Population Census by Government of Pakistan

Treatment (recipient) districts		Control (non-recipient) districts	
District	Literacy Rate	District	Literacy Rate
Rajanpur	20.7	Khushab	40.5
Muzaffargarh	28.5	Hafizabad	40.7
Lodhran	29.9	Mianwali	42.8
D.G.Khan	30.6	Multan	43.4
Rahmiyar Khan	33.1	Shiekhupura	43.8
Bhakkar	34.2	Sahiwal	43.9
Pakpattan	34.7	Sargodha	46.3
Bahwalpur	35.0	MandiBahuddin	47.4
Bahawalnagar	35.1	Attock	49.3
Kasur	36.2	T.T.Sing	50.5
Vehari	36.8	Faisalabad	51.9
Jhang	37.1	Narowal	52.7
Okara	37.8	Gujranwala	56.6
Layyah	38.7	Chakwal	56.7
Khanewal	39.9	Sialkot	58.9
		Gujrat	62.2
		Jhelum	63.9
		Lahore	64.7
		Rawalpindi	70.5

Note: The table reports district literacy rates from the 1998 Population Census conducted by the Government of Pakistan. Two current districts, Nankana Sahib and Chiniot, were part of the Sheikhupura and Jhang districts, respectively, in 1998.

Table A.2: Average characteristics of women and children by years of exposure.

	Treated years = 0			Treated years > 0		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel (a): *Women*	Mean	SD	N	Mean	SD	N
Years of Exposure	0.000	0.000	164053	5.435	3.128	54662
Secondary Completion	0.344	0.475	163797	0.228	0.420	54643
Highest Grade	6.323	4.975	163752	4.905	4.744	54635
Married before 16	0.076	0.265	151590	0.088	0.283	51527
First Birth before 17	0.045	0.208	134558	0.044	0.204	46884
Prenatal Checkup	0.870	0.336	33094	0.825	0.380	8113
Postnatal Checkup	0.527	0.499	32684	0.498	0.500	7982
Child Died	0.519	0.500	164053	0.582	0.493	54662
Age	23.598	5.736	164053	20.095	3.770	54662
Members in the HH	7.794	3.623	164053	7.958	3.594	54662
Household head owns home	0.871	0.335	163994	0.879	0.326	54645
No. of rooms in house	2.594	4.906	164053	2.336	4.129	54662
Panel (b): *Children*						
Weight-for-age (z-score)	-1.411	1.151	69125	-1.522	1.125	15067
Height-for-age (z-score)	-1.145	1.371	69125	-1.360	1.327	15067
Stunted	0.255	0.436	69125	0.308	0.462	15067
Underweight	0.286	0.452	68934	0.332	0.471	15016
Age of Child	1.902	1.402	69125	1.631	1.382	15067
Female (?)	0.513	0.500	69125	0.516	0.500	15067
Birth Order	2.453	1.385	69125	1.911	1.092	15067
Mother's Years of Treatment	0.000	0.000	69125	3.250	2.522	15067

Note: We report mean, standard deviation and number of observations for variable listed in rows for women in panel (a) and for children in panel (b) by years of expected exposure of the women. The data for Panel A comes from all four rounds of MICS. Sample in Panel A is women born between 1980 and 2002 who were aged 15 years of older at the time of survey. Years of Exposure is the years of exposure the woman had to FSSP during her school going years. Information on *Prenatal checkup* and Postnatal checkup are binary indicators for whether the woman had a prenatal and postnatal checkup and is only available for women who gave birth in the two years prior to the survey. *Child Died* is reported as a proportion of women who report ever giving birth. The data for Panel B comes from three rounds of MICS (2003 is excluded because data does not allow matching mothers and children). Sample in Panel B is all children in the household under the age of 5. *Child Died* is reported as a proportion of women who report ever giving birth.

Table A.3: Effect of FSSP among older cohort unexposed to the FSSP

	(1) Completed Primary	(2) Completed Secondary	(3) Married Before 16	(4) Prenatal	(5) WAZ	(6) HAZ
T*BirthYear=1981	0.003	0.014	0.001	-0.032	-0.035	-0.040
	(0.015)	(0.012)	(0.012)	(0.020)	(0.043)	(0.050)
T*BirthYear=1982	-0.011	-0.015	-0.002	0.006	-0.042	-0.0983*
	(0.013)	(0.015)	(0.010)	(0.015)	(0.044)	(0.051)
T*BirthYear=1983	0.000	-0.001	0.002	0.002	-0.026	-0.048
	(0.011)	(0.012)	(0.009)	(0.022)	(0.043)	(0.050)
T*BirthYear=1984	0.006	0.009	-0.011	-0.002	0.040	0.039
	(0.013)	(0.011)	(0.009)	(0.014)	(0.044)	(0.051)
T*BirthYear=1985	-0.0189	-0.0238*	-0.0006	0.00599	-0.004	-0.051
	(0.012)	(0.012)	(0.008)	(0.015)	(0.042)	(0.049)
Observations	76883	76690	69069	22582	51472	50970
R-squared	0.102	0.059	0.027	0.058	0.042	0.041

Notes: The data comes from pooling four rounds of MICS. The sample consists of all women born between 1980 - 1986, who were too old to be exposed to the FSSP when it was first implemented in 2004. *T* is a binary variable for women belonging to a treated district. *BirthYear* is a binary variable denoting the year in which the woman was born. All regressions control for for district and cohort fixed effects. Standard errors (in parentheses) are clustered by district. *** p<0.01, ** p<0.05, * p<0.1.
In the interest of brevity, we summarize results on selected outcomes in this table. We find similar, insignificant results on other outcomes. Results available upon request.

Table A.4: Inter-generational Effects of the FSSP on Child Health and Mortality controlling for birth order

	(1) WAZ	(2) Underweight	(3) HAZ	(4) Stunted
(Mother's) Years of exposure	0.0100**	-0.00428***	0.00304	-0.00176
	(0.00402)	(0.00150)[AA]	(0.00444)	(0.00146)
Observations	85,608	85,362	84,683	84,683
R^2	0.059	0.036	0.057	0.045

Notes: This table re-estimates the regressions in Table 4 controlling for birth order of the child. The data for Columns 1-4 comes from pooling three rounds of MICS. The 2003 MICS does not provide mother identifiers to link mothers to children. The sample consists of children under the age of five in the household, whose mothers were born between 1980 and 2002. Years of exposure is the number of years the mother was exposed to the FSSP during her school going years. The outcomes are as follows: (1) *Weight for Age Standardized score (WAZ)*, (2) Binary indicator for child being *underweight* (i.e. two standard deviations below the WHO standard for WAZ, (3) *Height for Age Standardized score (HAZ)* and (4) Binary indicator for being *stunted* (two standard deviations below the WHO standard for HAZ). For Columns 1 and 2 we restrict the sample to children whose WAZ is between -5 and +5. For Columns 3 and 4 we restrict the sample to children whose HAZ is between -5 and +5. MICS administers the question on child death to all women who have ever given birth. Sample for Column 4 therefore comes from all four rounds of MICS. We do not include regressions for child mortality in this table. The MICS survey asks women if they experienced death of a newly born child but it does not ask for the child birth order or gender. All regressions control for an indicator for treatment district, child's gender, child's age and district, mother's cohort fixed effects and survey year fixed effects. Standard errors (in parentheses) are clustered on mother's id. *** p<0.01, ** p<0.05, * p<0.1. Adjusting critical values following the approach by Benjamini and Hochberg, 1995: [AAA]Significance at 1% level, [AA]Significance at 5% level, [A]Significance at 10% level.

Table A.5: Inter-generational Effects of the FSSP on Child Health and Mortality by child gender

Panel (a): Male children	WAZ	HAZ	Underweight	Stunted
Years of exposure	0.0132**	0.00500	-0.00720***	-0.00253
	(0.0126)AA	(0.402)	(0.000365)AAA	(0.203)
Observations	43,977	43,539	43,847	43,539
R^2	0.046	0.041	0.027	0.030
Panel (b): Female children	**WAZ**	**HAZ**	**Underweight**	**Stunted**
Years of exposure	0.0149***	0.0105*	-0.00399*	-0.00384*
	(0.00730)AA	(0.0836)A	(0.0543)A	(0.0572)A
Observations	41,631	41,144	41,515	41,144
R^2	0.052	0.050	0.032	0.039
$p-value(Male = Female)$	0.609	0.491	0.348	0.647

Notes : Outcomes are as defined in Table 4. Panel (a) provides regression results for male children, Panel (b) displays the results for female children. Standard errors (in parentheses) are clustered at mothers id. We do not include regressions for child mortality in this table. The MICS survey asks women if they experienced death of a newly born child but it does not ask for the child birth order or gender. All regressions control for an indicator for treatment district, child's gender, child's age and district and mother's cohort fixed effects. We also control for survey year fixed effects since these outcomes include measurements of height and weight taken by the survey team. Years of exposure is the number of years the child's (mother) was exposed to the FSSP during her school going years. $p-value(Male = Female)$ are from Wald test of equality of respective coefficients from male and female sub-sample regressions. *** p<0.01, ** p<0.05, * p<0.1. Adjusting critical values following the approach by Benjamini and Hochberg, 1995: AAASignificance at 1% level, AASignificance at 5% level, ASignificance at 10% level.

Table A.6: Potential Mechanisms of Effects of FSSP

	(1) Neglecting Child	(2) Going Out	(3) Arguing	(4) Refuse Sex	(5) Burn Food
Years of exposure	0.003***	0.002***	0.003***	0.005***	0.003***
	(0.001)^{AAA}	(0.001)^{AA}	(0.001)^{AA}	(0.002)^{AA}	(0.000)^{AA}
Observations	90,225	90,460	89,823	84,287	90,200
R-squared	0.044	0.037	0.038	0.046	0.037

The data comes from pooling three rounds of MICS. The 2003 MICS does not administer any of these questions. Outcomes of interest measure if the woman says its is not justified for a husband to beat his wife if she neglects children; goes out without informing him; if she argues with him; if she refuses sex or if she burns food while cooking. All regressions control for an indicator for treatment district, and district and cohort fixed effects. Standard errors (in parentheses) are clustered at the district level. Years of exposure is the number of years the woman was exposed to the FSSP during her school going years. *** p<0.01, ** p<0.05, * p<0.1. Adjusting critical values following the approach by Benjamini and Hochberg, 1995: ^{AAA}Significance at 1% level, ^{AA}Significance at 5% level, ^{A}Significance at 10% level.

Table A.7: Robustness test: Controlling for health facilities

	(1)	(2)	(3)	(4)	(5)	(6)
Panel (a)	Prenatal	Prenatal	Prenatal	Postnatal	Postnatal	Postnatal
Years of exposure	0.011***	0.011***	0.008***	0.012***	0.012***	0.004
	(0.000)	(0.000)	(0.001)	(0.005)	(0.005)	(0.004)
Observations	41177	41177	22415	40637	40637	21990
R^2	0.049	0.049	0.058	0.033	0.033	0.294
Panel (b)	Child mortality	Child mortality	Child mortality	WAZ	WAZ	WAZ
Mother's years	-0.003**	-0.003**	-0.005***	0.013***	0.013***	0.012***
of exposure	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.005)
Observations	71123	71123	42527	85608	85608	49475
R^2	0.016	0.015	0.015	0.047	0.047	0.052
Panel (c)	Underweight	Underweight	Underweight	HAZ	HAZ	HAZ
Mother's years	-0.005***	-0.006***	-0.004**	0.007	0.005	0.012**
of exposure	(0.000)	(0.000)	(0.002)	(0.004)	(0.004)	(0.005)
Observations	85362	85362	49338	84683	84683	49005
R^2	0.028	0.028	0.031	0.045	0.045	0.045
Panel (d)	Stunted	Stunted	Stunted			
Mother's years	-0.003**	-0.003*	-0.004**			
of exposure	(0.002)	(0.002)	(0.002)			
Observations	84683	84683	49005			
R^2	0.034	0.034	0.035			
Hospitals/capita	x			x		
Hosp. bed/capita		x			x	
LHW/capita			x			x

Notes: Outcomes are as defined in Tables 3 and 4. The data for Panel A comes from pooling four rounds of MICS. The sample consists of all women born between 1980 and 2002 who were had given birth in the two years prior to the survey. *Years of exposure* is the number of years the women (mother) was exposed to the FSSP during her school going years. For Panel (b) and (c) sample is kids under the age of 5 from 3 rounds of MICS (excludes the 2003 round). The same sample restrictions apply as Table 3. Hospitals per capita, hospital beds per capita and LHW per capita are controls for hospitals, hospital beds and lady health workers per million of the district population. These variables changes over time for each year of survey. The data on LHW is available only f... Regressions in Panel A and columns 1 and 2 of Panel b control for an indicator for treatment district, and district and cohort fixed effects. Standard errors (in parentheses) are clustered at the district level for Panel (a) and mothers id for Panel (b) and (c). *** p<0.01, ** p<0.05, * p<0.1. Significance at 1% level.

Table A.8: Robustness test: Restricting to districts with literacy rates between 30 and 60 percent.

Panel (a)	(1) Completed primary	(2) Completed secondary	(3) Years of education	(4) Married Before 16
Years of exposure	0.009***	0.003	0.035	-0.002**
	(0.002)^{AAA}	(0.002)	(0.026)	(0.001)^{AA}
Observations	183,256	177,646	177,646	153,457
R^2	0.065	0.052	0.072	0.022

Panel (b)	(1) First Birth Before 17	(2) Prenatal	(3) Postnatal	(4) Child Mortality
(Mother's) years of exposure	-0.001	0.009***	0.007*	-0.002
	(0.001)	(0.003)^{AA}	(0.004)^{A}	(0.002)
Observations	123,778	33,578	33,148	44,626
R^2	0.016	0.042	0.03	0.012

Panel (c)	(1) WAZ	(2) HAZ	(3) Underweight	(4) Stunted
Mother's years of exposure	0.009**	0.002	-0.004**	-0.003
	(0.004)	(0.005)	(0.002)	(0.002)
Observations	69,708	68,996	69,499	68,996
R^2	0.041	0.039	0.025	0.029

Notes : Outcomes are as defined in Tables 2, 3 and 4. The only difference in sample is on districts included: we exclude a total of six districts that had literacy rates above 60 and below 30 percent in 1998. All regressions in Panel A and B control for an indicator for treatment district, and district and cohort fixed effects. Standard errors (in parentheses) are clustered at the district level for Panel (a) and (b), and at mothers id for Panel (c). All regressions in Panel (c) control for an indicator for treatment district, child's gender, child's age and district and mother's cohort fixed effects. We also control for survey year fixed effects in Panel C since these outcomes include measurements of height and weight taken by the survey team. (Mother's) Years of exposure is the number of years the (mother) woman was exposed to the FSSP during her school going years. *** p<0.01, ** p<0.05, * p<0.1. Adjusting critical values following the approach by Benjamini and Hochberg, 1995: ^{AAA}Significance at 1% level, ^{AA}Significance at 5% level, ^{A}Significance at 10% level.

Table A.9: Robustness test: Using a binary indicator for being exposed to FSSP in the treatment district.

	(1)	(2)	(3)	(4)
Panel (a)	Completed primary	Completed secondary	Years of education	Married Before 16
Treatment*Exposed	0.073***	0.288**	0.011	-0.017**
	(0.010)^{AAA}	(0.139)^{AA}	(0.010)	(0.006)^{AAA}
Observations	225,195	218,387	218,387	188,461
R^2	0.079	0.092	0.067	0.028

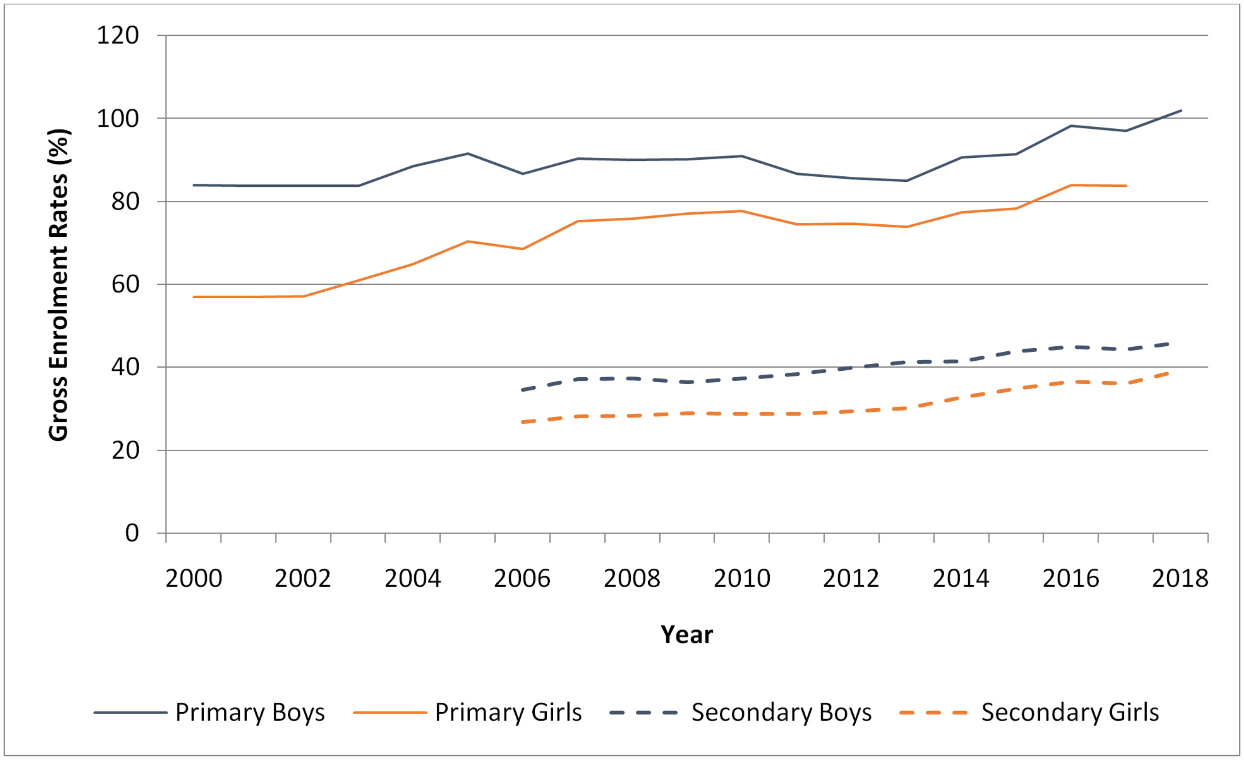
	(1)	(2)	(3)	(4)
Panel (b)	First Birth Before 17	Prenatal	Postnatal	Child Mortality
Treatment*Exposed	-0.00624	0.0371***	0.0517***	-0.0210***
	(0.005)^{AA}	(0.011)^{AAA}	(0.016)^{AAA}	(0.006)^{AAA}
Observations	151,714	41,177	40,637	71,123
R^2	0.018	0.048	0.031	0.015

	(1)	(2)	(3)	(4)
Panel (c)	WAZ	HAZ	Underweight	Stunted
Treatment*Exposed	0.0467**	0.0198	-0.0255***	-0.0100
	(0.019)^{AA}	(0.021)	(0.007)^{AAA}	(0.007)^{A}
Observations	87,914	86,952	87,660	86,952
R^2	0.047	0.045	0.028	0.034

Notes : Outcomes are as defined in Tables 2, 3 and 4. The only difference in sample is on districts included: we exclude a total of six districts that had literacy rates above 60 and below 30 percent in 1998. All regressions in Panel A and B control for an indicator for treatment district, and district and cohort fixed effects. Standard errors (in parentheses) are clustered at the district level for Panel (a) and (b), and at mothers id for Panel (c). All regressions in Panel (c) control for an indicator for treatment district, child's gender, child's age and district and mother's cohort fixed effects. We also control for survey year fixed effects in Panel C since these outcomes include measurements of height and weight taken by the survey team. 'Treatment*Exposed' is an indicator for if the woman (child's mother) was exposed to the FSSP program during her school going years while living in the treated districts in Panels (a) and (b) (Panel c). *** p<0.01, ** p<0.05, * p<0.1. Adjusting critical values following the approach by Benjamini and Hochberg, 1995: ^{AAA}Significance at 1% level, ^{AA}Significance at 5% level, ^{A}Significance at 10% level.

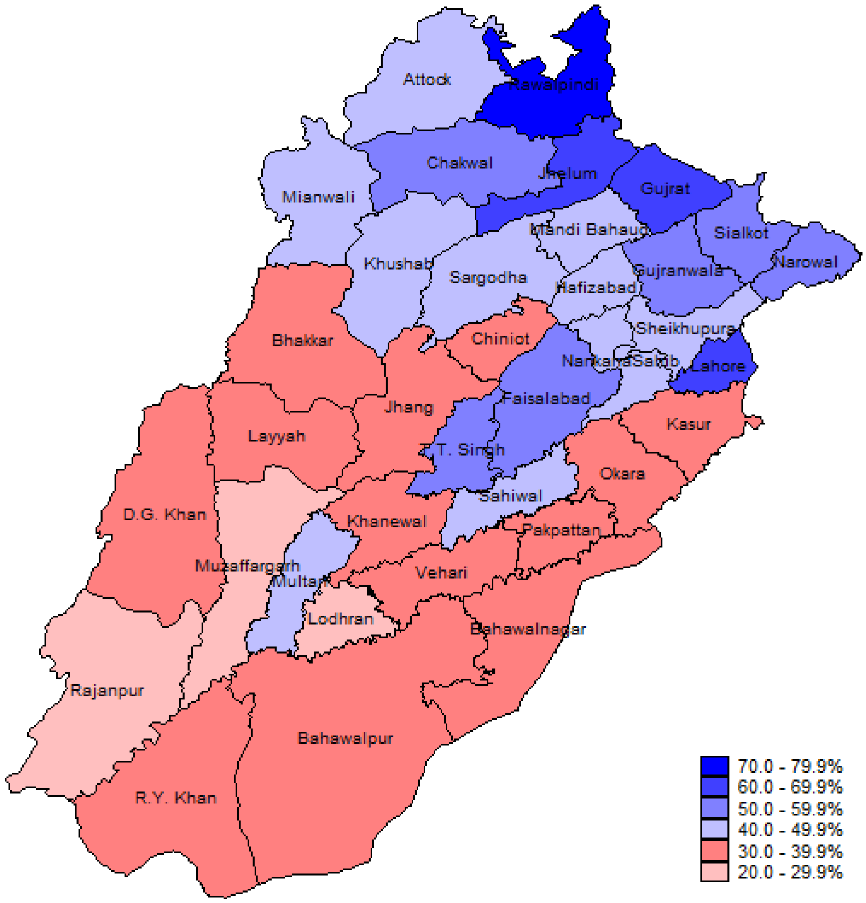
Figures

Figure A.1: Primary and Secondary School Enrollment Rates in Pakistan



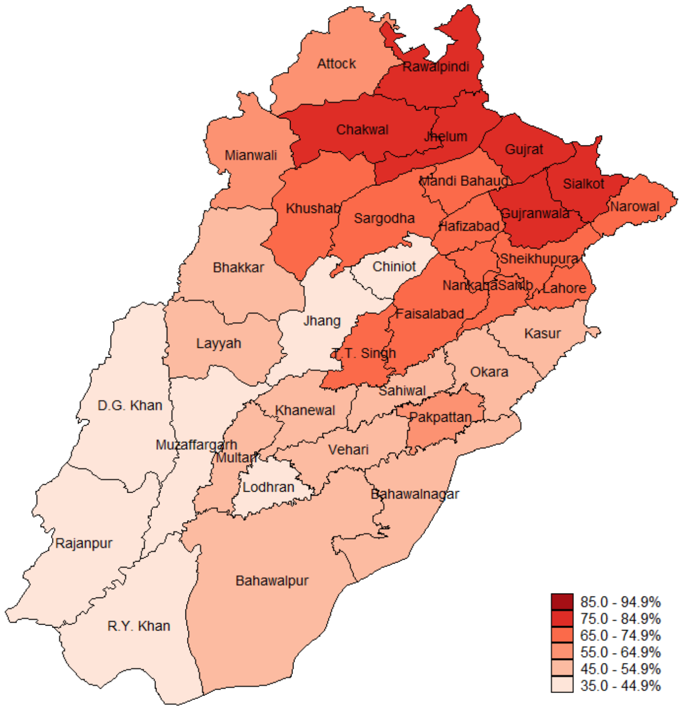
Note: This figure plots Gross Enrollment Rates (GER) rates by gender in primary and secondary schools in Pakistan. GER are calculated as the ratio of number of students enrolled in a given level of education, regardless of age, to the population of the age group which officially corresponds to the given level of education. Data is retrieved from https://data.worldbank.org/ and is available for secondary enrollment from 2005.

Figure A.2: FSSP treatment (recipient) and control (non-recipient) districts

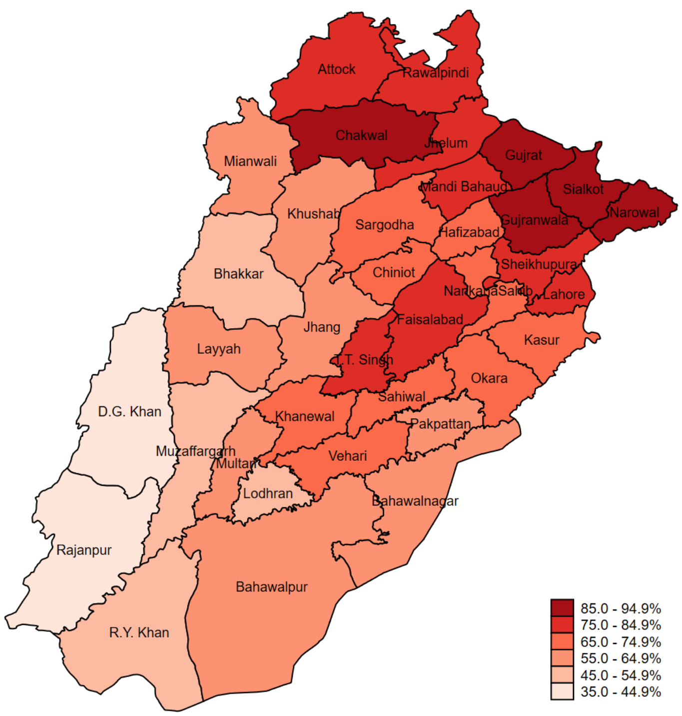


Note: This figure plots district literacy rates, shown in Appendix Table A.1. Districts in pink are treatment or recipient districts, with literacy rates of 40 percent or below in the 1998 Population Census. Districts in blue are control or non-recipient districts, with literacy rates in excess of 40 percent. Chiniot became the 36th district of Punjab, and the 16th FSSP recipient district in 2009.

Figure A.3: Enrolment rates for girls aged 11-15 years.



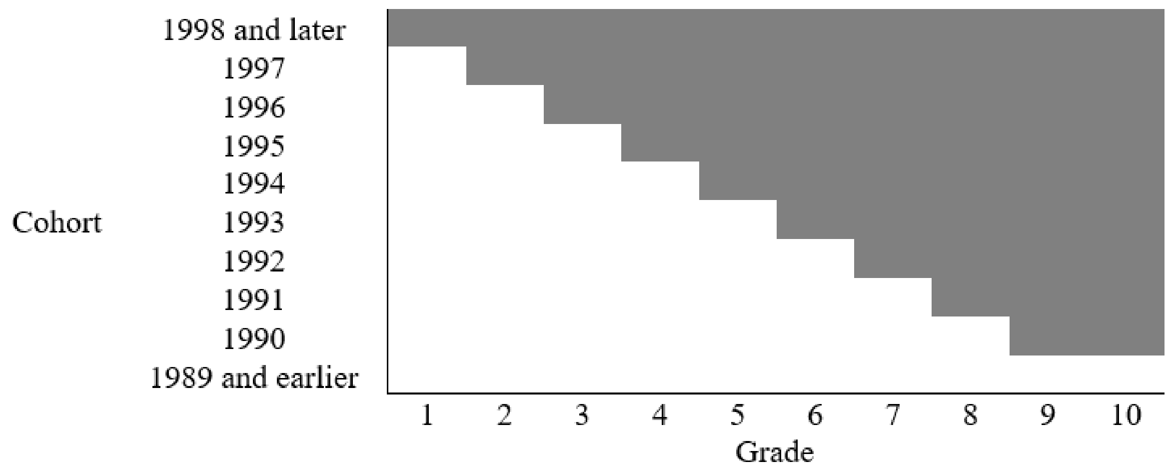
(a) Enrolment rates 2003



(b) Enrolment rates 2018

Note: Based on authors' calculation from MICS 2003 and MICS 2018.

Figure A.4: Cohort Wise Exposure to the FSSP

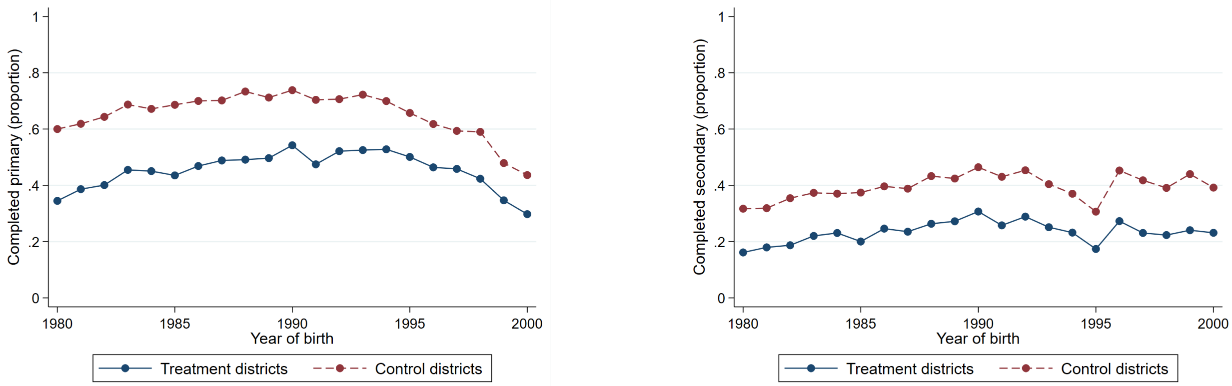


Note: The vertical axis plots women's year of birth. The horizontal axis plots the grades. The yellow highlighted blocks represents the grades (or school going years) for which the FSSP was in place for each cohort. These years define the '*years of exposure*' assigned to women in our analysis.

Figure A.5: Outcomes over time for women in treated and control districts

(a) Completed primary education

(b) Completed secondary education



(c) Average years of education

(d) Proportion of women who are married before 16 years of age.

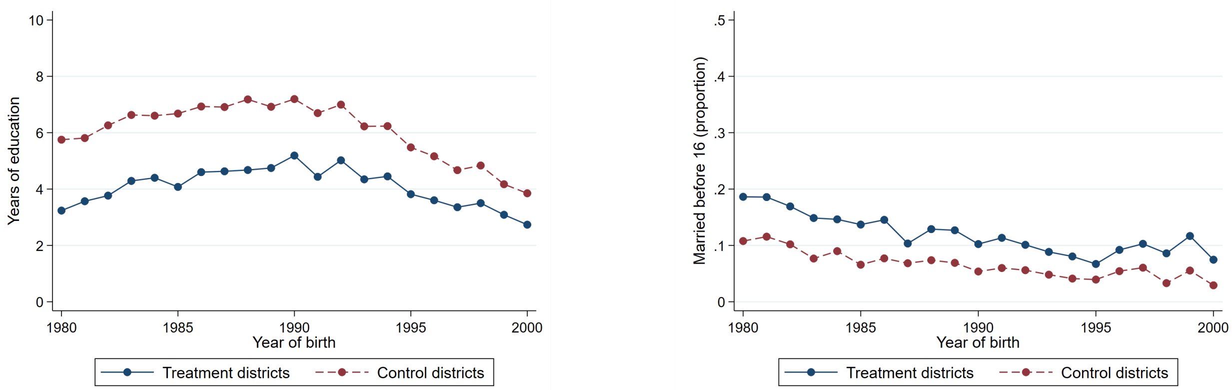
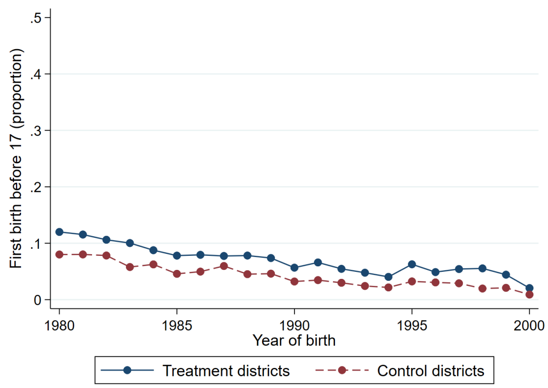
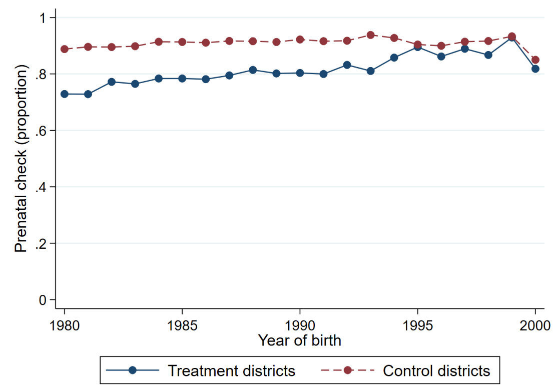


Figure A.5: Outcomes over time for women in treated and control districts (cont.)

(e) Proportion of women who give birth before 17 years of age



(f) Proportion of women who receive prenatal care



(g) Proportion who receive postnatal care

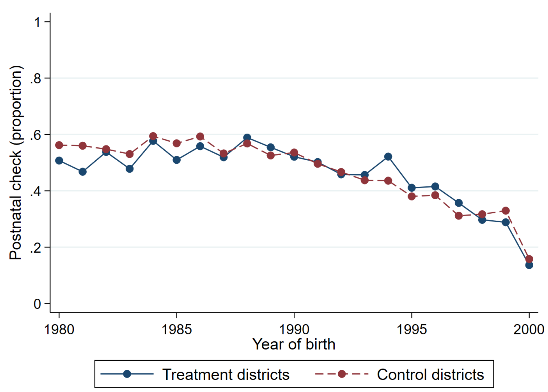
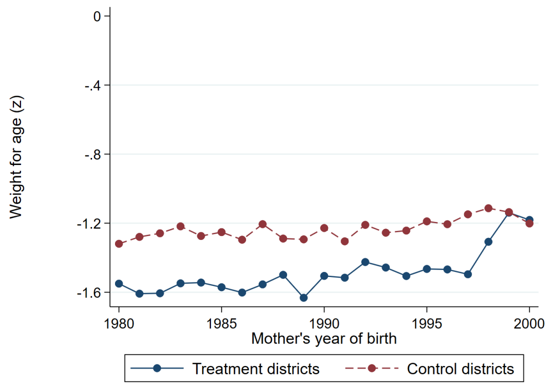
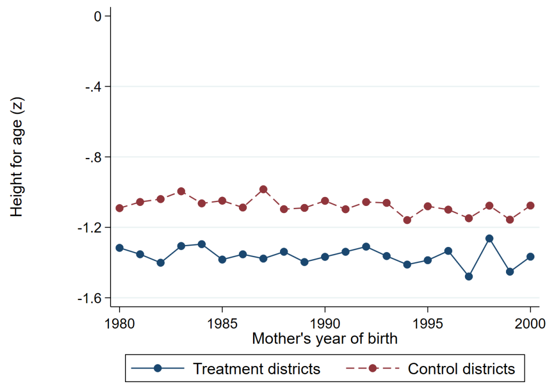


Figure A.6: Outcomes over time for children under 5 years old in treated and control districts

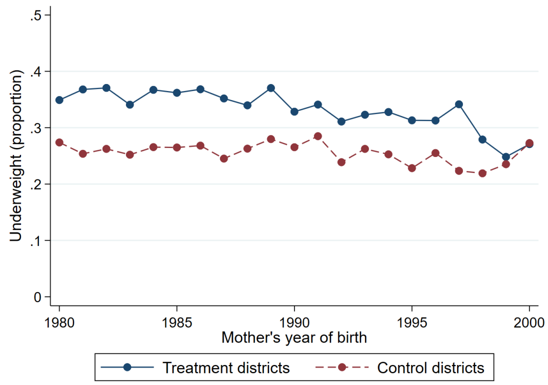
(a) Average weight-for-age (z-score)



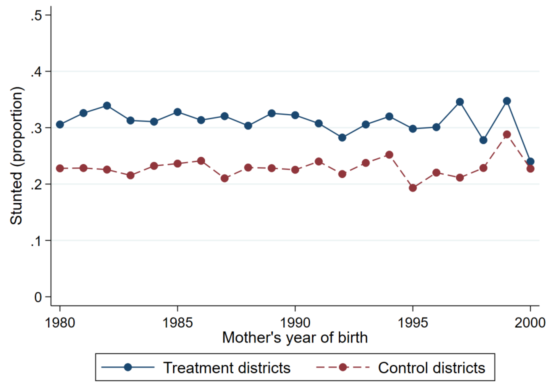
(b) Average height-for-age (z-score)



(c) Proportion of children who are underweight



(d) Proportion of children who are stunted.



(e) Child mortality.

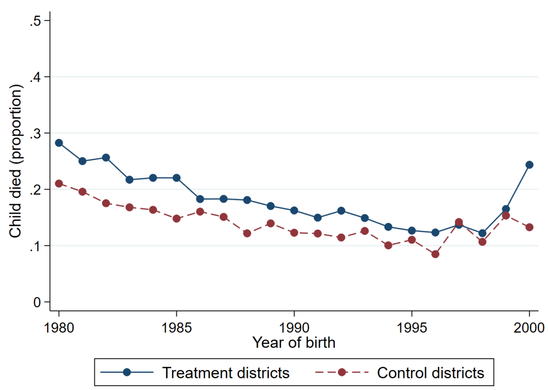
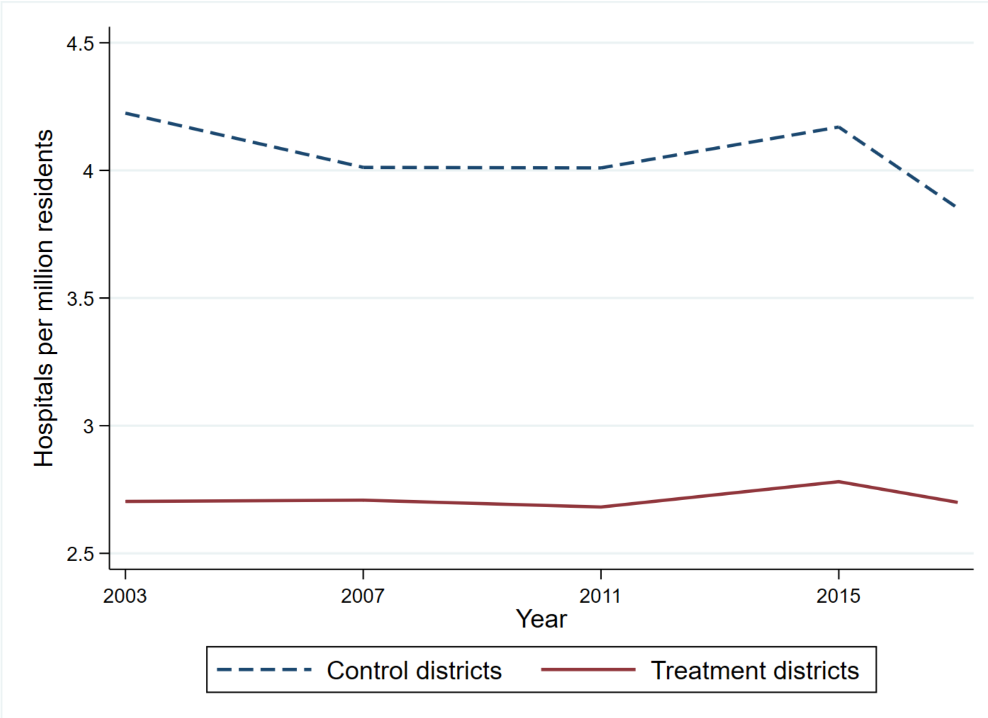
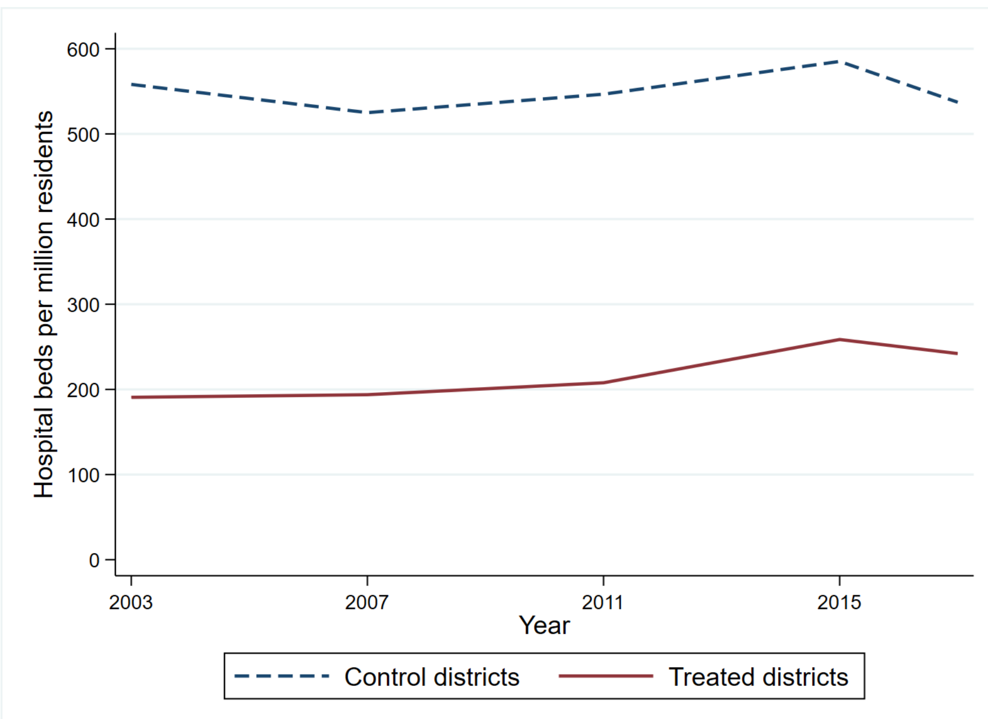


Figure A.7: District health facilities 2003 - 2018.



(a) District hospitals per capita (million)



(b) District hospital beds per capita (million)

Note: Based on Punjab Development Reports 2003, 2011, 2014, 2017.

Appendix: For Online Publication

Descriptive statistics

In this section, we provide sample characteristics when we divide the sample by (i) whether a woman's (mother's) cohort was (potentially) exposed to the FSSP (Table OA.1) and (ii) whether a woman's (mother's) cohort was (potentially) exposed to the FSSP and she resided in a control or treatment district (Table OA.2).

Table OA.1: Average characteristics of women and children by cohorts.

	Cohort not exposed			Cohort exposed		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel (a): *Women*	`Mean`	`SD`	`N`	`Mean`	`SD`	`N`
Years of Exposure	0.000	0.000	83165	2.192	3.325	135550
Secondary Completion	0.309	0.462	82932	0.319	0.466	135508
Highest Grade	5.638	5.201	82918	6.171	4.789	135469
Married before 16	0.105	0.307	75296	0.064	0.244	127821
First Birth before 17	0.068	0.251	65351	0.032	0.176	116091
Prenatal Checkup	0.849	0.358	23018	0.876	0.329	18189
Postnatal Checkup	0.544	0.498	22724	0.492	0.500	17942
Child Died	0.486	0.500	83165	0.565	0.496	135550
Age	26.668	5.579	83165	20.302	3.842	135550
Members in the HH	7.770	3.754	83165	7.874	3.529	135550
Household head owns home	0.873	0.333	83142	0.873	0.333	135497
No. of rooms in house	2.701	5.843	83165	2.424	3.879	135550
Panel (b): *Children*						
Weight-for-age (z-score)	-1.462	1.143	50116	-1.385	1.152	34076
Height-for-age (z-score)	-1.171	1.388	50116	-1.202	1.334	34076
Stunted	0.266	0.442	50116	0.262	0.440	34076
Underweight	0.298	0.457	49978	0.289	0.453	33972
Age of Child	2.012	1.401	50116	1.619	1.371	34076
Female (?)	0.514	0.500	50116	0.514	0.500	34076
Birth Order	2.688	1.432	50116	1.868	1.053	34076
Mother's Years of Treatment	0.000	0.000	50116	1.437	2.328	34076

Note: We report mean, standard deviation and number of observations for variable listed in rows for women in panel (a) and for children in panel (b) by whether a woman (mother's) birth cohort was exposed to the FSSP. The data for Panel A comes from all four rounds of MICS. Sample in Panel A is women born between 1980 and 2002 who were aged 15 years of older at the time of survey. Years of Exposure is the years of exposure the woman had to FSSP during her school going years. Information on *Prenatal checkup* and Postnatal checkup are binary indicators for whether the woman had a prenatal and postnatal checkup and is only available for women who gave birth in the two years prior to the survey. *Child Died* is reported as a proportion of women who report ever giving birth. The data for Panel B comes from three rounds of MICS (2003 is excluded because data does not allow matching mothers and children). Sample in Panel B is all children in the household under the age of 5. *Child Died* is reported as a proportion of women who report ever giving birth.

Table OA.2: Mean characteristics of women and children by years of exposure and district treatment status

	Control		Treatment	
Panel (a): Women	Cohort Not Exposed	Cohort exposed	Cohort not exposed	Cohort exposed
Years of Exposure	0.000	0.000	0.000	5.435
	(0.000)	(0.000)	(0.000)	(3.128)
Secondary Completion	0.370	0.379	0.215	0.228
	(0.483)	(0.485)	(0.411)	(0.420)
Highest Grade	6.559	7.027	4.207	4.905
	(5.079)	(4.627)	(5.062)	(4.744)
Married before 16	0.081	0.048	0.142	0.088
	(0.273)	(0.213)	(0.349)	(0.283)
First Birth before 17	0.056	0.024	0.085	0.044
	(0.230)	(0.154)	(0.279)	(0.204)
Prenatal Checkup	0.907	0.918	0.772	0.825
	(0.291)	(0.275)	(0.419)	(0.380)
Postnatal Checkup	0.562	0.487	0.520	0.498
	(0.496)	(0.500)	(0.500)	(0.500)
Child Died	0.491	0.554	0.476	0.582
	(0.500)	(0.497)	(0.499)	(0.493)
Age	26.619	20.442	26.745	20.095
	(5.706)	(3.884)	(5.373)	(3.770)
Members in the HH	7.813	7.817	7.704	7.958
	(3.734)	(3.483)	(3.785)	(3.594)
Household head owns home	0.869	0.870	0.879	0.879
	(0.338)	(0.337)	(0.326)	(0.326)
No. of rooms in house	2.825	2.484	2.509	2.336
	(5.964)	(3.699)	(5.644)	(4.129)
Panel (b): Children				
Weight-for-age (z-score)	-1.332	-1.277	-1.635	-1.522
	(1.149)	(1.161)	(1.110)	(1.125)
Height-for-age (z-score)	-1.035	-1.076	-1.353	-1.360
	(1.355)	(1.325)	(1.409)	(1.327)
Stunted	0.225	0.226	0.321	0.308
	(0.418)	(0.418)	(0.467)	(0.462)
Underweight	0.253	0.255	0.357	0.332
	(0.435)	(0.436)	(0.479)	(0.471)
Age of Child	1.998	1.609	2.032	1.631
	(1.394)	(1.362)	(1.408)	(1.382)
Female	0.513	0.511	0.515	0.516
	(0.500)	(0.500)	(0.500)	(0.500)
Birth Order	2.586	1.834	2.823	1.911
	(1.373)	(1.019)	(1.497)	(1.092)

Mother's Years of Treatment	0.000	0.000	0.000	3.250
	(0.000)	(0.000)	(0.000)	(2.522)

Note: We report mean for variable listed in rows for women in panel (a) and for children in panel (b) by whether a woman (mother's) birth cohort was exposed to the FSSP and by treatment status of the district they live in. Standard deviation are reported in parentheses. The data for Panel A comes from all four rounds of MICS. Sample in Panel A is women born between 1980 and 2002 who were aged 15 years of older at the time of survey. Years of Exposure is the years of exposure the woman had to FSSP during her school going years. Information on *Prenatal checkup* and Postnatal checkup are binary indicators for whether the woman had a prenatal and postnatal checkup and is only available for women who gave birth in the two years prior to the survey. *Child Died* is reported as a proportion of women who report ever giving birth. The data for Panel B comes from three rounds of MICS (2003 is excluded because data does not allow matching mothers and children). Sample in Panel B is all children in the household under the age of 5. *Child Died* is reported as a proportion of women who report ever giving birth.

Non-linear estimates

Table OA.3: Effect of FSSP on Women's Educational Outcomes

	(1) Completed primary	(2) Completed secondary	(3) Years of education
1 - 5 years of exposure	0.050***	0.123	0.003
	(0.009)	(0.093)	(0.009)
6 - 9 years of exposure	0.088***	0.420**	0.029*
	(0.0161)	(0.200)	(0.0165)
More than 9 years of exposure	0.081***	0.478**	0.027
	(0.019)	(0.214)	(0.017)
Observations	225,195	218,387	218,715
R^2	0.079	0.093	0.065

Notes: This table shows the estimation results when we assume a non-linear effect of exposure. We include indicator variable for whether exposure is between 1-5 years, 6-9 years or more than 9 (or maximum) exposure. The base category is no years of exposure. The data comes from pooling four rounds of MICS. The sample consists of all women born between 1980 and 2002 who were at least (1) 10 years or older for primary school completion (Column 1) and (2) 15 years or older for secondary school completion and completed years of education (Column 2 and Column 3). *Completed primary* is binary indicator for 5 years of education or more. *Completed secondary* is an indicator for 10 years of education or more. *Years of education* are completed years of education. *Years of exposure* is the number of years the woman was exposed to the FSSP during her school going years, which is 0 for women in the control districts. All regressions control for an indicator for treatment district, and district and cohort fixed effects. Standard errors (in parentheses) are clustered by district. *** p<0.01, ** p<0.05, * p<0.1.

Table OA.4: Effect of FSSP on Women's Later Life Outcomes

	(1) Married Before 16	(2) First Birth Before 17	(3) Prenatal	(4) Postnatal
1 - 5 years of exposure	-0.012	-0.003	0.026**	0.052***
	(0.007)	(0.005)	(0.010)	(0.015)
6 - 9 years of exposure	-0.027***	-0.009	0.093***	0.101***
	(0.007)	(0.006)	(0.019)	(0.032)
More than 9 years of exposure	-0.018*	-0.011**	0.098 ***	0.016
	(0.010)	(0.005)	(0.024)	(0.045)
Observations	188,461	151,714	41,177	40,637
R^2	0.028	0.018	0.049	0.031

Notes: This table shows the estimation results when we assume a non-linear effect of exposure. We include indicator variable for whether exposure is between 1-5 years, 6-9 years or more than 9 (or maximum) exposure. The base category is no years of exposure. The data comes from pooling four rounds of MICS. The sample consists of all women born between 1980 and 2002 who were at least (1) 16 years or older for *married before 16* years of age indicator (Column 1), (2) 17 years or older for *first birth before 17* years of age indicator (Column 2), (3) Had given birth in the two years prior to the survey for *prenatal* and *postnatal* care (binary indicators for any pre- or postnatal checkup during pregnancy for Columns 3 and 4, respectively). *Years of exposure* is the number of years the women was exposed to the FSSP during her school going years MICS administers the questions related to age of marriage and first birth to all women in the sample. The question pertaining to maternal health care utilization are only administered to women who had given birth within the two years prior to the survey. This is why we see a drop in observations in Columns 3 compared to the first two Columns. All regressions control for an indicator for treatment district, and district and cohort fixed effects. Standard errors (in parentheses) are clustered by district. *** p<0.01, ** p<0.05, * p<0.1.

Table OA.5: Intergenerational Effects of the FSSP on Child Health and Mortality

	(1) WAZ	(2) Underweight	(3) HAZ	(4) Stunted	(5) Child Mortality
Mother's 1-5 years	0.072***	0.041*	-0.029***	-0.017**	-0.013**
of exposure	(0.020)	(0.023)	(0.008)	(0.007)	(0.006)
(Mother's) 6-9 years	0.038	0.041	-0.023	-0.008	-0.030**
of exposure	(0.038)	(0.041)	(0.014)	(0.014)	(0.011)
(Mother's) more than 9 years	0.192**	0.096	-0.056*	-0.046	-0.009
of exposure	(0.084)	(0.090)	(0.029)	(0.030)	(0.026)
Observations	85,608	84,683	84,362	84,683	71,123
R^2	0.047	0.044	0.028	0.034	0.015

Notes: This table shows the estimation results when we assume a non-linear effect of exposure. We include indicator variable for whether exposure is between 1-5 years, 6-9 years or more than 9 (or maximum) exposure. The base category is no years of exposure. The data for Columns 1-4 comes from pooling three rounds of MICS. The 2003 MICS does not provide mother identifiers to link mothers to children. The sample consists of children under the age of five in the household, whose mothers were born between 1980 and 2002. The outcomes are as follows: (1) *Weight for Age Standardized score (WAZ)*, (2) Binary indicator for child being *underweight* (i.e. two standard deviations below the WHO standard for WAZ, (3) *Height for Age Standardized score (HAZ)*, (4) Binary indicator for being *stunted* (two standard deviations below the WHO standard for HAZ) and (5) *Child Mortality*, an Indicator for whether the mother reports having a child who later died. For Columns 1 and 2 we restrict the sample to children whose WAZ is between -5 and +5. For Columns 3 and 4 we restrict the sample to children whose HAZ is between -5 and +5. MICS administers the question on child death to all women who have ever given birth. Sample for Column 4 therefore comes from all four rounds of MICS. All regressions control for an indicator for treatment district, child's gender, child's age and district and mother's cohort fixed effects. We also control for survey year fixed effects since these outcomes include measurements of height and weight taken by the survey team each year. Standard errors (in parentheses) are clustered on mother's id. Years of exposure is the number of years the mother was exposed to the FSSP during her school going years. *** p<0.01, ** p<0.05, * p<0.1.

Effect of FSSP on women's marital and fertility outcomes

Table OA.6: Effect of FSSP on Women's Later Life Outcomes (by age)

Panel (a)	(1) Married before 15	(2) Married before 16	(3) Married before 17	(4) Married before 18
Years of exposure	-0.001***	-0.003***	-0.002*	0.000
	(0.000)	(0.001)	(0.001)	(0.001)
Observations	218,715	188,461	173,415	160,990
R^2	0.012	0.028	0.032	0.034
Panel (b)	**First birth before 15**	**First birth before 16**	**First birth before 17**	**First birth before 18**
Years of exposure	-0.0003**	-0.001**	-0.001*	-0.002*
	(0.0001)	(0.0004)	(0.0005)	(0.0008)
Observations	218,715	165,914	151,714	153,174
R^2	0.003	0.013	0.018	0.026

Notes: The data comes from pooling four rounds of MICS. The sample consists of all women born between 1980 and 2002 who were at least as old as the age being tested for marriage and first birth. *Years of exposure* is the number of years the women was exposed to the FSSP during her school going years MICS administers the questions related to age of marriage and first birth to all women in the sample. All regressions control for an indicator for treatment district, and district and cohort fixed effects. Standard errors (in parentheses) are clustered by district. *** p<0.01, ** p<0.05, * p<0.1.

Table OA.7: Effect of FSSP on Women's Later Life Outcomes - including younger women

	(1) Married Before 16	(2) First Birth Before 17
Years of exposure	-0.003***	-0.002***
	(0.001)	(0.001)
Observations	205,246	205,246
R^2	0.030	0.015

Notes: This table re-estimates regressions in Table 3, for women who are 15 years or older. The data comes from pooling four rounds of MICS. *Years of exposure* is the number of years the woman was exposed to the FSSP during her school going years, which is 0 for women in the control districts. All regressions control for an indicator for treatment district, and district and cohort fixed effects. Standard errors (in parentheses) are clustered by district. *** p<0.01, ** p<0.05, * p<0.1.

Effect of FSSP exposure on the households women lives in

Table OA.8: Effect of FSSP whether the women live in households where husband is household head or households based in urban areas

	(1) Husband is household head	(2) Urban residence
Years of exposure	-0.000	-0.001
	(0.002)	(0.002)
Observations	59,522	218,715
R^2	0.071	0.070

Notes: This table tests if the years of exposure determine the binary outcomes if the woman marries the household head (column 1) or if she lives in a household in urban area (column 2). The data comes from pooling four rounds of MICS. *Years of exposure* is the number of years the woman was exposed to the FSSP during her school going years, which is 0 for women in the control districts. All regressions control for an indicator for treatment district, and district and cohort fixed effects. Standard errors (in parentheses) are clustered by district. *** p<0.01, ** p<0.05, * p<0.1.

Additional robustness checks

In this section, we provide additional robustness checks as follows:

- In Tables OA.9, we control health facilities, specifically for district BHU per capita and BHU beds per capita; MCH per capita and MCH beds per capita; and all (hospitals, BHU, MCH) facilities and beds in all facilities per capita.
- In Table OA.10, we re-estimate results for districts that have literacy rates +/- 10 percentage points of the 40% district literacy rate threshold required for schools in a district to be eligible to receive the FSSP stipends.
- In Table OA.11, we re-estimate our main results, dropping older cohorts born between 1980-1985.
- In Table OA.12, we exclude from our sample women who may have been too old to have been exposed to the FSSP i.e. women aged 17 and 18 in 2004.
- In Table OA.13, we include controls for household characteristics on which we find initial imbalance between control and treatment samples in Table 1

Results shown here Are generally consistent with the main results show in text, in Tables 2 - 4: Coefficient signs remain the same, though some of the results are no longer statistically significant, perhaps owing to a decrease in statistical power when we restrict samples as required by a specific robustness check.

Table OA.9: Robustness test: Controlling for health facilities

Panel (a): Prenatal	(1)	(2)	(3)	(4)	(5)	(6)
Years of exposure	0.010***	0.011***	0.011***	0.010***	0.011***	0.010***
	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)
Observations	41,177	41,177	41,177	41,177	41,177	41,177
R^2	0.05	0.05	0.049	0.05	0.05	0.051
Panel (b): Postnatal	(1)	(2)	(3)	(4)	(5)	(6)
Years of exposure	0.011**	0.009*	0.009**	0.009**	0.011***	0.010**
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
Observations	40,637	40,637	40,637	40,637	-2.833	-2.134
R^2	0.039	0.039	0.032	0.031	0.00396	0.00459
Panel (c): Child mortality	(1)	(2)	(3)	(4)	(5)	(6)
Mother's years	-0.004**	-0.003**	-0.003**	-0.004**	-0.003**	-0.003**
of exposure	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
Observations	71,123	71,123	71,123	71,123	71,123	71,123
R^2	0.015	0.016	0.016	0.016	0.016	0.016
Panel (d): WAZ	(1)	(2)	(3)	(4)	(5)	(6)
Mother's years	0.013***	0.014***	0.016***	0.014***	0.015***	0.014***
of exposure	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	85,608	85,608	85,608	85,608	85,608	85,608
R^2	0.048	0.048	0.048	0.047	0.048	0.048
BHUs per capita	x					
BHU beds per capita		x				
MCHs per capita			x			
MCH beds per capita				x		
All per capita					x	
Beds (all) per capita						x

Panel (e): HAZ	(1)	(2)	(3)	(4)	(5)	(6)

Mother's years	0.007	0.008*	0.009**	0.008*	0.009*	0.007
of exposure	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Observations	84,683	84,683	84,683	84,683	84,683	84,683
R^2	0.044	0.045	0.045	0.044	0.045	0.045

Panel (f): Underweight	(1)	(2)	(3)	(4)	(5)	(6)
Mother's years	-0.005***	-0.006***	-0.004**	-0.003**	-0.006***	-0.006***
of exposure	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Observations	85,362	85,362	84,683	84,683	85,362	85,362
R^2	0.028	0.028	0.034	0.034	0.028	0.028

Panel (g): Stunting	(1)	(2)	(3)	(4)	(5)	(6)
Mother's years	-0.003**	-0.003**	-0.006***	-0.005***	-0.004**	-0.003**
of exposure	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Observations	84,683	84,683	85,362	85,362	84,683	84,683
R^2	0.034	0.034	0.028	0.028	0.034	0.034
BHUs per capita	x					
BHU beds per capita		x				
MCHs per capita			x			
MCH beds per capita				x		
All per capita					x	
Beds (all) per capita						x

Notes: Outcomes are as defined in Tables 3 and 4. The data for Panel A comes from pooling four rounds of MICS. The sample consists of all women born between 1980 and 2002 who were had given birth in the two years prior to the survey. For Panel (a), outcome of interest in Column 1 and 2 is binary indicator for prenatal care and in Column 3 and 4 is binary indicator for postnatal care. *Years of exposure* is the number of years the women (mother) was exposed to the FSSP during her school going years. For Panel (b) and (c) sample is kids under the age of 5 from 3 rounds of MICS (excludes the 2003 round). The same sample restrictions apply as Table 3. BHUs per capita and BHU beds per capita are controls for Basic Health Units (BHU) and Basic Health Units (BHU) beds per million of the district population. MCHs per capita and MCH beds per capita are controls for Mother and Child Healthcare Centres (MCHs) and Mother and Child Healthcare Centres (MCH) beds per million of the district population. 'All' facilities include hospitals, BHUs and MCHs in district; 'Beds (all)' include hospital, BHU and MCH beds in the district, both measured per million of the district population. These variables changes over time for each year of survey. Regressions in Panel A control for an indicator for treatment district, and district and cohort fixed effects. Standard errors (in parentheses) are clustered at the district level for Panels (a) - (c) and mothers id for Panels (d) - (g). *** p<0.01, ** p<0.05, * p<0.1.

Table OA.10: Robustness test: Restricting to districts with literacy rates between 30 and 50 percent.

Panel (a)	(1) Completed primary	(2) Completed secondary	(3) Years of education	(4) Married Before 16
Years of exposure	0.005**	0.0003	0.006	-0.002*
	(0.002)^{A}	(0.002)	(0.026)	(0.001)^{A}
Observations	136,171	132,285	132,285	114,051
R^2	0.037	0.032	0.041	0.018

Panel (b)	(1) First Birth Before 17	(2) Prenatal	(3) Postnatal	(4) Child Mortality
Mother's years of exposure	-0.001	0.007**	0.003	-0.002
	(0.001)	(0.003)^{AA}	(0.004)	(0.002)
Observations	100,255	25,614	25,256	44,082
R^2	0.010	0.036	0.033	0.011

Panel (c)	(1) WAZ	(2) HAZ	(3) Underweight	(4) Stunted
Mother's years of exposure	0.007	0.006	-0.003*	-0.003*
	(0.005)	(0.005)	(0.002)	(0.002)
Observations	52,410	51,876	52,241	51,876
R^2	0.039	0.038	0.023	0.030

Notes : Outcomes are as defined in Tables 2, 3 and 4. The only difference in sample is on districts included: we exclude a total of six districts that had literacy rates above 50 and below 30 percent in 1998. All regressions in Panel A and B control for an indicator for treatment district, and district and cohort fixed effects. Standard errors (in parentheses) are clustered at the district level for Panel (a) and (b), and at mothers id for Panel (c). All regressions in Panel (c) control for an indicator for treatment district, child's gender, child's age and district and mother's cohort fixed effects. We also control for survey year fixed effects in Panel C since these outcomes include measurements of height and weight taken by the survey team. Years of exposure is the number of years the woman (mother) was exposed to the FSSP during her school going years in Panels (a) and (b) (Panel c). *** p<0.01, ** p<0.05, * p<0.1. Adjusting critical values following the approach by Benjamini and Hochberg, 1995: ^{AAA}Significance at 1% level, ^{AA}Significance at 5% level, ^{A}Significance at 10% level.

Table OA.11: Robustness test: Excluding Women born between 1980 and 1985

Panel (a)	(1) Completed primary	(2) Completed secondary	(3) Years of education	(4) Married Before 16
Years of exposure	0.011***	0.005**	0.055**	-0.002***
	(0.002)^{AAA}	(0.002)^{AA}	(0.025)^{AA}	(0.001)^{AAA}
Observations	182,918	176,215	176,215	150,793
R^2	0.074	0.07	0.089	0.024

Panel (b)	(1) First Birth Before 17	(2) Prenatal	(3) Postnatal	(4) Child Mortality
(Mother's) years of exposure	-0.001*	0.009***	0.008**	-0.002
	(0.001)^{AA}	(0.002)^{AAA}	(0.004)^{AA}	(0.002)
Observations	118,997	28,481	28,107	44,626
R^2	0.014	0.043	0.034	0.012

Panel (c)	(1) WAZ	(2) HAZ	(3) Underweight	(4) Stunted
(Mother's) years of exposure	0.010**	0.006	-0.004**	-0.002
	(0.004)^{A}	(0.005)	(0.002)^{A}	(0.002)
Observations	57,305	56,652	57,134	56,652
R^2	0.051	0.046	0.03	0.036

Notes : Outcomes are as defined in Tables 2, 3 and 4. The only difference in sample is by birth cohort: women born between 1980 and 1985 are not included in this sample. All regressions in Panel (a) and (b) control for an indicator for treatment district, and district and cohort fixed effects. Standard errors (in parentheses) are clustered at the district level for Panel (a) and (b), and at mothers id for Panel (c). All regressions in Panel (c) control for an indicator for treatment district, child's gender, child's age and district and mother's cohort fixed effects. We also control for survey year fixed effects in Panel (c) since these outcomes include measurements of height and weight taken by the survey team. (Mother's) Years of exposure is the number of years the (mother) woman was exposed to the FSSP during her school going years. *** p<0.01, ** p<0.05, * p<0.1. Adjusting critical values following the approach by Benjamini and Hochberg, 1995: ^{AAA}Significance at 1% level, ^{AA}Significance at 5% level, ^{A}Significance at 10% level.

Table OA.12: Robustness test: Excluding potential 'over-age' enrollments (women aged 17 and 18 in 2004)

Panel (a)	(1) Completed primary	(2) Completed secondary	(3) Years of education	(4) Married Before 16
Years of exposure	0.011***	0.004**	0.052**	-0.003***
	(0.002)AAA	(0.002)AA	(0.024)AA	(0.000)AAA
Observations	199,313	192,579	192,579	164,706
R^2	0.077	0.069	0.094	0.029

Panel (b)	(1) First Birth Before 17	(2) Prenatal	(3) Postnatal	(4) Child Mortality
(Mother's) years of exposure	-0.001**	0.011***	0.010**	-0.005***
	(0.000)AA	(0.000)AAA	(0.013)AAA	(0.002)AAA
Observations	134,227	34,230	33,766	59,505
R^2	0.019	0.049	0.031	0.017

Panel (c)	(1) WAZ	(2) HAZ	(3) Underweight	(4) Stunted
(Mother's) years of exposure	0.013***	0.006	-0.005***	-0.003**
	(0.004)AAA	(0.005)A	(0.002)AAA	(0.002)AA
Observations	70,730	69,993	70,528	69,993
R^2	0.046	0.043	0.027	0.033

Notes : Outcomes are as defined in Tables 2, 3 and 4. The only difference in sample is by age at the time of exposure: we exclude 17 and 18 year old women here who may have been too old to have been exposed to the FSSP when stipends were first disbursed in 2004. All regressions in Panel (a) and (b) control for an indicator for treatment district, and district and cohort fixed effects. Standard errors (in parentheses) are clustered at the district level for Panel (a) and (b), and at mothers id for Panel (c). All regressions in Panel (c) control for an indicator for treatment district, child's gender, child's age and district and mother's cohort fixed effects. We also control for survey year fixed effects in Panel (c) since these outcomes include measurements of height and weight taken by the survey team. (Mother's) Years of exposure is the number of years the (mother) woman was exposed to the FSSP during her school going years. *** p<0.01, ** p<0.05, * p<0.1. Adjusting critical values following the approach by Benjamini and Hochberg, 1995: AAASignificance at 1% level, AASignificance at 5% level, ASignificance at 10% level.

Table OA.13: Robustness test: Controlling for household characteristics.

Panel (a)	(1) Completed primary	(2) Completed secondary	(3) Years of education	(4) Married Before 16
Years of exposure	0.00969*** (0.00194)^{AAA}	0.00739*** (0.00209)^{AAA}	0.0513* (0.0258)^{AA}	-0.00266*** (0.000868)^{AAA}
Observations	225,123	224,718	218,320	188,421
R^2	0.125	0.110	0.173	0.034

Panel (b)	(1) First Birth Before 17	(2) Prenatal	(3) Postnatal	(4) Child Mortality
Years of exposure	-0.00113** (0.000548)^{AA}	0.0111*** (0.00250)^{AAA}	0.0105*** (0.00381)^{AA}	-0.00341** (0.00145)^{AA}
Observations	151,686	41,170	40,630	71,123
R^2	0.023	0.060	0.042	0.015

Notes : Outcomes are as defined in Tables 2, 3 and 4. All regressions control for household characteristics that measure household size, number of rooms in the house, an indicator for whether the house is owned by the household head (vs. no owned) and an indicator for urban (vs. rural) residence. Standard errors (in parentheses) are clustered at the district level. Years of exposure is the number of years the woman (or mother in the case of child mortality) was exposed to the FSSP during her school going years. *** p<0.01, ** p<0.05, * p<0.1. Adjusting critical values following the approach by Benjamini and Hochberg, 1995: ^{AAA}Significance at 1% level, ^{AA}Significance at 5% level, ^{A}Significance at 10% level.