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Educate the girls: Long run effects of secondary schooling for girls in Pakistan

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ABSTRACT

In 2004, the government of Punjab, Pakistan introduced a conditional cash assistance program for girls attending secondary schools. We exploit variations in exposure to the program across cohorts and regions to estimate the long run effects of the program on women's marriage and fertility decisions, maternal healthcare utilization and the health outcomes of their children. We find that each potential year of exposure to the program increases the probability of completing secondary school by 1.9 percent and decreases the probability of an early marriage by 3.5 percent. Exposure to the program also delays early childbirth and increases the likelihood of seeking prenatal 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 (−1.9 percent) than the comparison sample. Evidence suggests assortative matching in the marriage market, increased health awareness and empowerment of educated women may be among the potential drivers of these results. These findings imply that programs aimed at promoting girls' education can lead to beneficial long run gains in multiple dimensions that should 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 developing countries are stark both in the numbers affected as well as in the severity of the gaps. 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 yields not only private but also social returns. Economic theory suggests educated women may lower fertility and enable better health care and education for their children (Becker & Lewis, 1973; Becker, 1992; Thomas, Strauss, & Henriques, 1991; Strauss & Thomas, 1995). While the empirical correlation between female schooling, fertility and labor market decisions is well known, studies are often not 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, Chuang, Melnikas, & Psaki (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 reside.[1] In 2004, the Government of Punjab implemented the Female Secondary School Stipend Program (FSSP) to encourage households to send their girls for sec-

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

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ondary schooling. Under the FSSP, households in eligible districts with girls enrolled in grades 6–10 received a monthly cash stipend (USD 3.33) conditional on 80 percent attendance.[2] The program was implemented in districts with literacy rates of 40 percent or less according to the national census in 1998. 15 out of the 36 districts qualified on the basis of this rule.[3]

Our data allows us to identify eligible *districts* by literacy rates in 1998. We do not have sufficient density around localized bands of the district-level literacy rates to exploit a Regression Discontinuity Design without significant decrease in sample size and loss of statistical power to detect small effects. Nor do we have information on individuals' program participation or attendance rates. In order to estimate causal impacts of the FSSP on individual outcomes, we exploit 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 potential exposure to the program based on district of residence (treatment vs. comparison) 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 0.6 percentage points per year of exposure, or an increase of 1.8 percent relative to the average completion rate in the sample. The effect of full 10 years of exposure – 6 percent points – is comparable to those found for similar programs in other countries. For example, the secondary schooling stipend program in Bangladesh estimates an increase of 5 percentage points for the fully exposed cohort (Hahn et al., 2018).

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 relative to the unexposed sample. Third, women exposed to the program are more likely to seek maternal health: an increase of 0.7 percent in take up of prenatal checkups for each year of exposure, though we do not find any impact on postnatal care. Fourth, we also find evidence of intergenerational effects. Specifically, children of women exposed to the program on average are less likely to be stunted (1.9 percent) or underweight (1.7 percent) and score higher on Weight-for-Age (WAZ) and Height-for-Age (HAZ) standardized scores. Child mortality is also lower for women exposed to the program. These results are robust to several specifications, including controlling for healthcare services over time, excluding older cohorts of women with lower levels of exposure and restricting the sample to districts with similar literacy rates in 1998.

We look into the potential mechanisms that may be driving these changes. Though income and labor market effects may drive some of the long run changes we observe, our data does not allow us to investigate these channels. We do find evidence along other channels. 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. We also find suggestive evidence for women's empowerment as a possible mechanism behind these effects. We do not 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 & Long, 2008; Behrman, 2015; Keats, 2018; Duflo, Dupas, & Kremer, 2015; Ozier, 2018), and is positively associated with their health and their children's health (see Grossman & Kaestner (1997), Grossman (2000), Grossman (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.[4]

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 programs are regularly evaluated for their impact on enrolment rates (see Baird, Ferreira, Özler, & Woolcock (2014) for a review), evidence on the effects of *secondary* schooling on marriage, fertility and health is rare. Evidence in this domain is nascent and developing, with mixed evidence on the long-run impact from Zimbabwe (Grépin & Bharadwaj, 2015), Bangladesh (Hahn et al., 2018; Khandker, Samad, Fuwa, & Hayash, 2021), Malawi (Baird et al., 2011; Baird et al., 2019) and Ghana (Duflo et al., 2021).

Second, with primary enrolment rates approaching 100 percent globally (UNESCO, 2016) 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 irrespective of learning gains (Duflo et al., 2021; Warner, Malhotra, & McGonagle, 2012). 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, which has been recognized by WHO as one of the most important development goals the world currently faces.[5] 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 indicators have been used to provide information on malnutrition and are thought to be correlated with long term economic losses through lower cognition, educational performance, wages and pro-

[2] Based on the exchange rate in 2004, when the program started disbursements. We use this exchange rate throughout the paper.

[3] The FSSP was announced for eligible districts in 2003, with stipend disbursements starting in 2004 (Independent Evaluation Group, 2011). As per official figures, cash transfers to more than 400,000 girls per year have been disbursed since 2004 (See (Alam, Baez, & Del Carpio, 2011) andhttp://www.pesrp.edu.pk/pages/Stipend-to-Girl).

[4] Studies that look at causal effects can be divided into two large groups – those that look at supply side constraints, such as increased supply of schools (Breierova & Duflo, 2004; Akresh, Halim, & Kleemans, 2018; Mazumder, Rosales-Rueda, & Triyana, 2019) and the impact of compulsory schooling laws (Grépin & Bharadwaj, 2015); or similar to this study, those that look at demand side interventions designed to increase the willingness to send girls to school (Baird, McIntosh, & Özler, 2011, 2019; Duflo, Dupas, & Kremer, 2021; Hahn et al., 2018; Masuda & Yamauchi, 2020). Among demand-side intervention evaluations, Masuda and Yamauchi (2020) explore impacts of abolishing fee in primary schools in Uganda. Baird et al., 2011, 2019; Duflo et al., 2021 investigate the impact of scholarships and conditional cash transfers, but do not study intergenerational effects and the outcomes of the recipient's children. Both studies explore the impact of such programs in African countries. The context in South Asia are inherently different from the Africa and Latin America due to different social norms that dictate female mobility and educational choices. Hahn et al. (2018) are close to this study in terms of the context, program and the outcomes measured, but as we discuss later in this section, the program they investigate has stricter conditionalities on being eligible for stipends.

[5] The World Health Organization (WHO) views early childhood malnutrition as one of the biggest challenges today, with nearly one out of every five (21%) children around the world under the age of five children being 'stunted'. The data is available online at the WHO Global Health Observatory.

ductivity (Thomas et al., 1991; Gross, Landfried, & Herman, 1996; McGovern, Krishna, Aguayo, & Subramanian, 2017; Gertler et al., 2014). For instance, Hoddinott, Alderman, Behrman, Haddad, and Horton (2013) show that for low-income households in Guatemala, a 1 SD improvement in HAZ lead to a 21 percent increase in later-life household income, reducing the likelihood of poverty by 10 percent. Though the effects of FSSP that we measure on these indicators are small, literature suggests that they are likely meaningful, especially for a program that is not primarily designed to counter child malnutrition.

Unlike our study, the existing evidence on inter-generational gains from school construction programs (Breierova & Duflo, 2004; Akresh et al., 2018; Mazumder et al., 2019) and compulsory schooling laws investigate the impact of alleviating supply side constraints. They do not look at the willingness to invest in children's education from a demand perspective, focus on child mortality (Grépin & Bharadwaj, 2015) and educational outcomes (Mazumder et al., 2019) in relatively wealthier contexts, and do not speak about the quality of health of children. Andrabi, Das, and Khwaja (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.

Third, our work contributes to the upcoming literature on long run impacts of CCT programs.[6] Evidence on the longer run benefits of CCT programs in Columbia (Barrera-Osorio, Linden, & Saavedra, 2019), Mexico (Parker & Vogl, 2018) and Nicaragua (Barham, Macours, & Maluccio, 2013) suggest positive impacts on long run educational achievement, labor force participation and mobility of early life beneficiaries. On the other hand, Araujo, Bosch, and Schady (2017) find only modest improvements in intergenerational transmission of benefits in Ecuador. Unlike these other CCTs, however, the FSSP in Punjab 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 per quarter). 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.

Lastly, 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 & Taylor (2018)). Yet, 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.[7] 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 (Pakistan & ICF, 2020). Our study makes an important contribution of documenting the increase in maternal healthcare utilization due to increased secondary schooling. A fifth of the girls in Pakistan are married before the age of 18 (Pakistan & ICF, 2020). Our results show that exposure to the FSSP results in significant reduction in probability of early marriage and childbirth.

The program most similar to the CCT program in Pakistan is the long-running Female Secondary Schooling Stipend Programme in Bangladesh. There are important differences in the design and targeting of the two programs. The stipend in Bangladesh increases with years of education and is conditional on a certain level of proficiency in class level exams, providing increasing incentives to stay in school. In addition, the female recipient must remain unmarried to receive the stipend, a condition that is not required under the FSSP in Pakistan (Hahn et al., 2018). The effects of FSSP in Pakistan hence may be viewed as a lower bound of the short and long run impacts of providing cash incentives for girls to stay in school.

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 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 min walking radius, compared to more than four-fifths of the sample that reports a primary school within the same distance (Andrabi, Das, & Khwaja, 2011; Sathar, Lloyd, Mete, & ul Haque, 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., 2013). In 2011, less than third of the secondary school going children in Punjab were enrolled in private schools (Nguyen & 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 Fig. A.1 for trends in Pakistan; and Behrman & Schneider, 1993; Alderman, Orazem, & Paterno, 2001; Holmes, 2003; Lloyd, Mete, & Sathar, 2005 for similar trends in other developing countries). Low female enrollment is compounded by low retention and completion rates for girls (Sawada & 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 & 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, Najeeb, & Raju, 2019). In 2005, the program was extended to include

[6] 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, Sengupta, and Todd (2005), Benedetti et al. (2016), Fiszbein et al. (2009), Ganimian and Murnane (2016), Schultz (2004), Todd and Wolpin (2006).

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

grades 9 and 10. Stipends were disbursed quarterly and were conditional on girls maintaining 80 percent attendance (as reported by the school).[8] 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 & 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 & Parajuli, 2010). Second, given the costs associated with migration, including giving up housing, livestock and livelihood (McKenzie, 2022), 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 (See Appendix Table A.1 for district literacy rates in 1998).[9] Fig. 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 'comparison', 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, had increased 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 Fig. A.2). 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. Moreover, beneficiary adolescent girls are more likely to complete middle school and work less (in terms of child labor). Long-run and inter-generational effects of the program are as yet unexplored (Alam et al., 2011).

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 for births in the two years prior to the survey. For children under the age of five, MICS collects information about current weight and height (anthropometric measures administered by the survey team).

Table 1 shows the summary statistics for the women and children in our sample, by treatment and comparison districts. We describe outcomes as well as individual and household characteristics. Women the treatment districts are exposed to the program for an average of 3.4 years. 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 comparison districts. Women in the treatment districts are more likely to get married before the age of 16 and have their first child by the age of 17, and they are less likely to seek pre- and postnatal care. Appendix Fig. A.3 plots average education and maternal health outcomes for comparison and treatment districts over time and shows that comparison districts fare better than treatment districts on all outcome metrics over the sample period, though the trends are qualitatively similar.[10]

We similarly see that child health is relatively better in comparison districts than in the treatment districts. Table 1, Panel (b) shows the summary statistics for children in our sample. This sample consists of all children under the age of 5 for the 2011, 2014 and 2018 rounds of the survey.[11] Only a sub-sample of women have children below the age of five. Mothers in treated districts have an average of 1.34 years of exposure to the program. The average child in the treatment districts is 1.5 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 1.2 and 1.1 in the comparison districts, respectively.[12] These correspond to higher rates of stunting (32 percent) and being underweight (34.7 percent) in the treatment districts compared to the comparison districts (22.7 percent and 25.4 percent, respectively). Appendix Fig. A.4 plots district averages of child health outcomes over time. We see moderate improvements over time in child health, with a decrease in the proportion of children who are reported as underweight and decreasing rates of child mortality in the comparison and treatment districts.

[8] 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.

[9] District is the third administrative tier in Pakistan, after provincial and national government. 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.

[10] In line with the recommendations of Kahn-Lang and Lang (2020), we include a brief discussion on the historical context, and why the levels of educational and other long run outcomes of women differ between treatment and comparison districts in Online Appendix Section OA.4. We note that the original reason for differences in levels are unlikely to have led to differences in trends in the absence of the program. We also observe that trends in the educational outcomes for women in our sample, primary completion rates in particular, show an initial improvement and then stagnate, similar to what is observed for Pakistan overall (Planning Commission Pakistan, 2013). We plot the outcomes for men in the sample districts in Fig. OA.5 in the Online Appendix, and observe that they too are in line with national trends.

[11] 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.

[12] 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. 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.

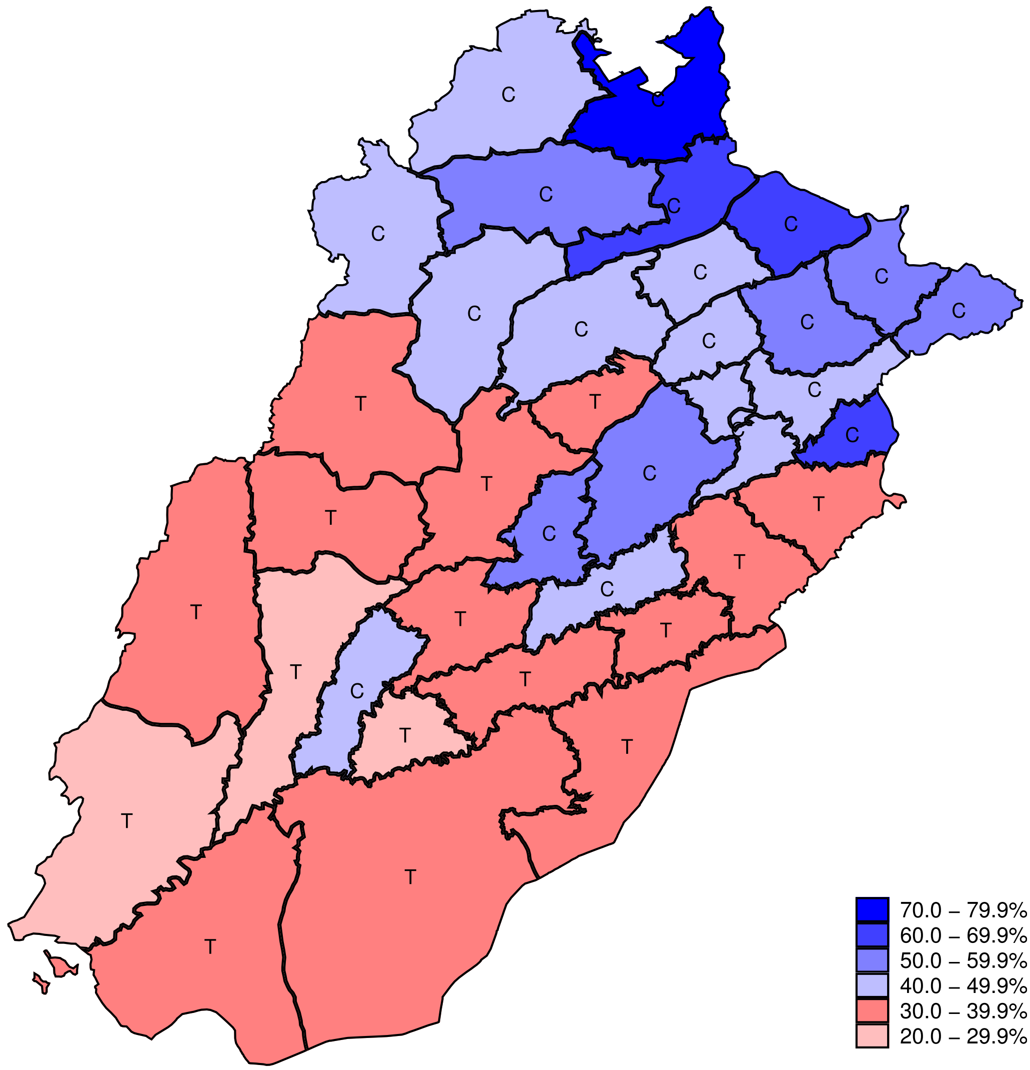


Fig. 1. FSSP treatment (recipient) and comparison (non-recipient) districts. Note: This figure plots district literacy rates, shown in Appendix Table A.1. Districts in pink (labelled 'T') are treatment or recipient districts, with literacy rates of 40 percent or below in the 1998 Population Census. Districts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

Women in the sample are 23 years old, on average. 87 percent report living in houses that their families own, with 7.8 other members living in the same household. Children across treatment and comparison districts, are 1.8 years old, 51 percent are male and the average child in our sample is 2nd or 3rd born in the family. While the differences in individual and household characteristics between comparison and treatment districts are statistically significant, they are small in terms of economic magnitude and do not show any meaningful difference for practical purposes.[13]

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 our 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 comparison districts, are part of the comparison sample.[14] 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. The results of our estimations can be interpreted as a lower bound of the true effect of the program.

4.1. Estimating the impact of FSSP on women's education

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 take woman's residence at the time of the MICS survey to determine if she was eligible for the FSSP on the basis of residence. MICS does not provide any infor-

[13] In Section 5.4, we show that our main results are robust to the inclusion of these characteristics as controls.

[14] 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 Appendix 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. Conditional on any exposure, women in our sample on average have 5 years of exposure to the program. Women not exposed to the program are, as expected, older (23.6 years vs. 20.1 years on average).

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

	Comparison Districts			Treatment Districts				
Panel (a): *Women*	(1) Mean	(2) SD	(3) N	(4) Mean	(5) SD	(6) N	(7) *Difference*	(8) $p-value$
Years of exposure	0.000	0.000	131266	3.408	3.611	87121	3.408	0.000***
Secondary completion	0.366	0.482	131266	0.219	0.414	87121	−0.147	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	121733	0.108	0.310	81134	0.048	0.000***
First birth before 17	0.036	0.186	108227	0.059	0.235	72946	0.023	0.000***
Prenatal checkup	0.911	0.284	23212	0.796	0.403	17943	−0.115	0.000***
Postnatal checkup	0.530	0.499	22932	0.511	0.500	17684	−0.019	0.000***
Child died	0.151	0.358	40820	0.197	0.398	30229	0.046	0.000***
Women (household) characteristics:								
Age	22.824	5.554	131266	22.579	5.480	87121	−0.245	0.000***
Members in the household	7.814	3.582	131266	7.863	3.668	87121	0.048	0.002***
Household hold head owns the home	0.869	0.337	131223	0.879	0.326	87097	0.010	0.000***
No. of rooms in the house	2.604	4.597	131266	2.398	4.732	87121	−0.206	0.000***
Panel (b): *Children*								
Mother's years of exposure	0.000	0.000	47681	1.341	2.277	36511	1.341	0.000***
Weight-for-age (z score)	−1.242	1.170	48994	−1.534	1.156	37790	−2.704	0.000***
Height-for-age (z score)	−1.056	1.344	48994	−1.364	1.375	37790	−2.708	0.000***
Stunted	0.227	0.419	48994	0.318	0.466	37790	−0.100	0.000***
Underweight	0.254	0.436	48865	0.347	0.476	37668	−0.088	0.000***
Child characteristics:								
Age of child	1.848	1.395	48994	1.869	1.412	37790	0.474	0.031**
Male	0.512	0.500	48994	0.515	0.500	37790	0.016	0.367
Birth order	2.294	1.302	48994	2.459	1.432	37790	1.157	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 comparison and treated districts. The data for Panel A comes from all four rounds of MICS, for 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.

mation on migration, nor on the district of residence of members of households in the past. We can reasonably assume that individuals did not choose to locate in treatment or comparison 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).[15] We discuss the implications of potential migration on internal validity of our estimates in Section 4.4.

The stipend is offered to girls in grades 6–10. Typically, girls aged 11 to 15 years are enrolled in these grades. We retrospectively assign exposure to the program based on (i) the treatment status of the district the woman belonged to, and (ii) number of years the program was in place during her school going years. Women in the comparison districts were not eligible for the program. Women aged 16 and older in 2004 in the treated districts were too old to benefit from the program. These two groups of women form our comparison sample, with zero years of exposure. Years of exposure to the FSSP for girls aged 15 and below at the start of the program in 2004 (in the treated districts) are calculated based on their age at that time. 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 Fig. A.5 summarizes the expected years of exposure based on birth cohort.[16]

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 (Duflo et al., 2021; Keats, 2018; Sandholtz, 2021). Since the option of receiving stipend in the future at the secondary level can also be an important factor in households' decision to enroll girls in school, we also include the years that the younger cohorts of girls spend in primary school as being 'exposed' to the program.[17]

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

$$Y_{idk} = \alpha_0 + \alpha_1(\text{Yearsofexposure})_{idk} + \delta_d + \sigma_k + \gamma_s + \epsilon_{idk} \quad (1)$$

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. We limit the sample to women who are at least 15 years old (i.e. are old enough to have potentially completed secondary school). $Yearsofexposure_{idk}$ is the number of years the woman was exposed to the FSSP during her school going years.

Our identification strategy assumes that outcomes of interest in the treated and comparison groups would have continued to trend in a parallel fashion in the absence of the treatment. We show in Section 4.4 that outcomes of interest do indeed trend similarly in the control and treatment districts prior to the program and differentially post the treatment. All OLS regressions include district (δ_d), survey year (γ_s) and cohort fixed (σ_k) effects to account for any differences across districts, cohorts and survey measurement other

[15] Based on author's calculation from the Pakistan Demographic Health Survey of 2012 which documents detailed migration history of individuals and households.

[16] 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.

[17] In the Online Appendix Tables OA.1 and OA.2, we show birth cohorts benefiting from the FSSP versus not and combination of birth cohort and being in a comparison or treatment district.

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 comparison districts. Conditional on these fixed effects, our identification assumes that α_1 is the effect of the program. All errors are clustered at the district level.

Controlling for district-by-cohort fixed effects would absorb the variation that we are exploiting. We, however, control for time varying district characteristics using relevant time varying district characteristics as part of our robustness checks. For example, there may be a concern that differential change in the provision of health services across district over time might be driving some of the effects. We address this concern in our robustness checks by controlling 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. An alternate specification, similar to Hahn et al. (2018), would involve estimating a non-linear model 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. For simplicity of exposition our main regressions estimate a constant, linear effect for each year of exposure. Results for binary ranges of exposure (0–5, 6–9 and 10 years) are available in the Online Appendix Tables OA.6 – OA.8. The results largely support the linear model presented in Section 5, with effects becoming stronger as exposure increases.

There is a recent and growing literature that also draws attention to the limitations of the canonical two-way fixed effects for estimating average effects when there are multiple periods and when the treatment may be staggered over time (See (Chaisemartin & D'Haultfoeuille, 2022) for a comprehensive summary of this literature). For instance, the traditional TWFE can provide diluted average estimates when treatment effects vary over time (Goodman-Bacon, 2021). Note that our study set-up does not involve variation in treatment status over time: in our cross-sectional data, individuals that are 'non-treated' do not transition to the 'treated' group over time (or vice versa) and treatment is provided to all treated groups at the same time (i.e. in 2004). In Online Appendix Section OA.2, we conduct a robustness check and find the estimates from the Callaway and Sant'Anna (2021) estimation to be generally consistent with differential effects by cohort and treatment status investigated in Section 4.4 and Section 5.1.

4.2. Long run effects of the FSSP on women's outcomes

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 Eq. 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).[18] 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 proportion of our sample (0.001) and re-estimating our regressions does not effect the results discussed in Section 5.[19]

As before, we include district, survey year and cohort fixed effects, and cluster errors at the district level. Ex-ante, we expect the treatment effects to be negative for probability of teenage marriage and pregnancy and positive for maternal health care utilization.

4.3. Estimating inter-generational effects of the FSSP

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

$$C_{cidk} = \theta_0 + \theta_1(\text{Yearsofexposure})_{idk} + \theta(\text{X}_{cidk}) + \delta\prime\prime_d + \sigma\prime\prime_k + \gamma\prime\prime_s + \upsilon_{cijk} \quad (2)$$

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).[20] 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. $Yearsofexposure_{idk}$ 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 Eq. (1). Errors are clustered at the mother's level.

Note, MICS collects height and weight data only for children aged 5 or less at the time of the survey. We are unable to comment on the health outcomes of older children, who may be healthier if they had to compete for household resources with fewer members, or worse off if born to young mothers. Though we do not have the data to parse out how older children may have been impacted, we control for mother's age in all regressions. We also show robustness of child health outcomes by controlling for child birth order (Table A.3). 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.

Some measures of education or health outcomes discussed in Sections 4.1,2,3,4.3 may proxy the same outcome. To deal with the possible multiple inference problems we report for each estimation both the p-value for the estimated treatment effect, and a sharpened q-value, calculated within each listed outcome (see Benjamini, Krieger, & Yekutieli (2006)).

[18] Women in Pakistan typically do not have children out of wedlock. 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. Alternate measures of these outcomes, such as age at marriage or first birth, would exclude women who would not have been married or had their first child by the time of survey. If this delay is partly due to the program, excluding these women would bias the treatment effect.

[19] Age at marriage can be misreported due to recall issues or tendency to 'heap' age of marriage to the socially or legally acceptable age. We expect this to be less of a concern in our case since the MICS survey does not directly ask for age at first marriage, but instead records the year the woman got married for the first time. We calculate age at marriage as the difference between the woman's reported year of birth and the year of her first marriage. Systematic misreporting related to treatment can bias the estimates when the outcome is the exact age of marriage. In our case where the outcome is the probability of marriage by age 16, misreporting is unlikely to bias estimates unless there is systematic fudging around the age of 16 by treatment status. For instance, if treated women were, say, less likely to suffer from recall issues or social pressures to report a younger age of marriage (i.e. 16 years) than women in comparison districts. However, we do not see any evidence of heaping at younger ages in the treatment and comparison districts in our data to indicate a biased estimate (distributions available on request).

[20] To allow comparison of our results with recent studies (Grépin & Bharadwaj, 2015; Dursun, Cesur, & Kelly, 2022), we similarly control for child age in years. However, it is also possible that height and weight values may be naturally more frequent at some age in months, not necessarily linearly. In Online Appendix Table OA.12 we show results are robust when we include dummies for child age in months (Panel a) and in bands of 3 months (Panel b).

4.4. Threats to identification

One of the threats to identification is whether underlying trends in outcomes for women in the treated and comparison groups districts may be driving the effects we attribute to the FSSP in the analysis. That is, whether we can assume that the difference in trends in educational outcomes in treated and comparison samples is constant over time in the absence of the FSSP. To test this assumption, we run a modified version of Eq. (1) for women's educational outcomes on indicators for birth year, which define the potential years of exposure to the program. If outcome trends are parallel before the FSSP was implemented, we should see no differential trend by treatment status in cohorts that do not qualify for the FSSP, i.e. born before 1991. This is indeed what we find. Results are presented in Fig. 2. For women who were too old to be exposed to the FSSP, i.e. those born before 1991, we generally see no statistically significant difference in the rates of primary (Fig. 2a) and secondary education (Fig. 2b) completion rates, or the years of education (Fig. 2c). For all three outcomes, we begin to see a persistent uptick for cohorts born after 1991, i.e. after the first year of exposure for primary completion rates and years of education, and for cohorts born after 1993 for secondary completion rates.[21]

Another important concern is potential misclassification of women as treatment or comparison on the basis of their residence at the time of the survey. We could expect three types of migration due to the FSSP in the interim between the start of FSSP and the MICS survey which could bias estimates of the impact of FSSP. One, families could have migrated during a woman's school-going years to take advantage of the FSSP stipends. For example, a household with a young girl born in the year 2000 in a control district could migrate to treated districts during her secondary going years. These households, being in the treated district at the time of the survey, would be included in the treated sample with full exposure, when they may have had only partial exposure to the FSSP. This would bias the expected positive impact of FSSP towards zero. Second, even if the size of the stipend may be insufficient to motivate the entire household to migrate, families could send girls of eligible age to reside with relatives in treated districts to receive the stipend. This would mean that women belonging to households residing in the control districts at the time of the survey may have lived in treated districts during their school-going age. If the treatment is expected to have positive effects, the inclusion of incorrect inclusion of treated women in the comparison sample would, once again, lead to the benefits of FSSP being underestimated. Third, households may have migrate after they cease being eligible for the program as a result of the FSSP. Treated women who benefited from the program may migrate to a neighboring, more developed district in the comparison sample to take advantage of the greater economic opportunities in those districts. Though treated, they would be living in a comparison district at the time of the survey, and would be incorrectly classified as part of the 'control' sample. As in the second case, we would expect the estimated positive impacts of the FSSP to be biased towards zero in this case, representing a lower bound of the actual impact.

There are three main reasons why we believe migration did not significantly impacted our estimates. First, the size of the transfer

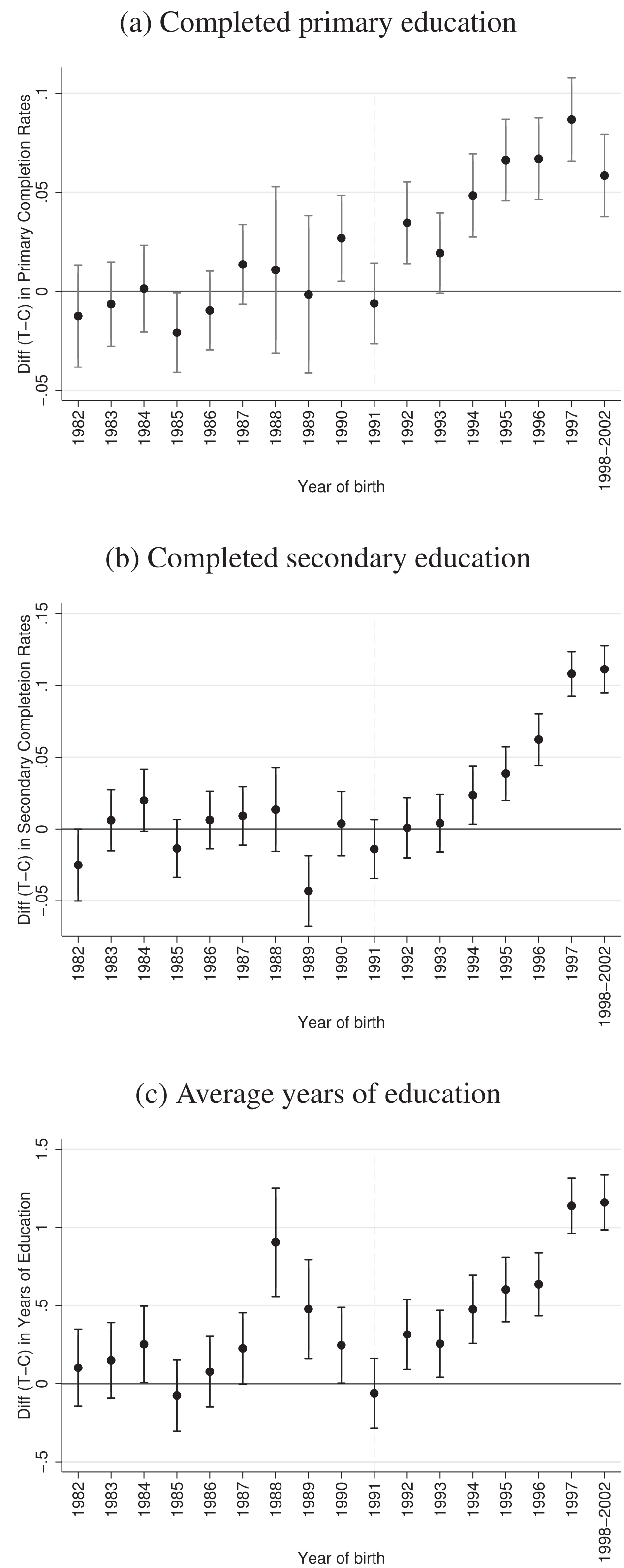


Fig. 2. Event Study Plot: Difference-in-difference estimates for educational outcomes. Note: The figure plots the difference-in-difference in educational outcomes of the women in the treatment and comparison sample for two decades (1982–2002), by year of birth cohorts (y axis) against year of birth (x axis). Each dot represents an estimate of the interaction term of residing in the treatment district (T, vs. comparison (C) district) and cohort dummy, from a regression similar to Eq. 1, but with 1981–2002 cohort dummies and interaction of residing in treatment and the cohort dummies, instead of a variable for years of exposure. Bars represent the 95% confidence interval. Cohorts 1998–2002 have identical exposure under study design. The dashed line is to indicate the first cohort expected to have exposure to the FSSP.

[21] Note, among older cohorts who were too old to be exposed to the program, we see no sustained trends in the rates of primary or secondary completion rates. We see positive difference for years of education for the 1988 and 1989 cohorts but this difference is not sustained and does not follow a systematic trend. In addition, in Online Appendix OA.3, we discuss an additional placebo test to determine the validity of our identification strategy. We use a sample of comparison districts and assigning the districts that neighbor the real treated districts as 'pseudo' treatment districts for this exercise. We see no differential impact of the exposure to the program in this sample, which indicates that the impacts observed in the main analysis are a product of the exposure to the program, not geographic developments.

(USD 3.33 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.[22] The program itself is unlikely to be a sufficiently large motivation for households to decide to migrate or send their girls to live elsewhere given the costs associated with migration, and social-cultural norms that discourage sending girls to another location for education.

Second, data indicates that the overall migration rates between treatment and comparison districts are low. The MICS does not provide any information on migration but the Pakistan Demographic Health Survey (PDHS 2012, 2018) estimates an average rate of 6% migration from comparison to treatment districts, and a migration rate of 9% from treatment to comparison districts. We use data from these two rounds of PDHS and find that the probability of ever-migrating is not correlated to the exposure to FSSP program ($p-value = 0.692$). This is in line with the age and age-grade distributions seen in the data used in our analysis. The distribution of ages in grade 6 – the first grade girls become eligible for the stipend shows a slight leftward shift over time, but does not differ by treatment status. The age grade distribution for grade 10 is remarkably similar over time, in both treatment and comparison districts (Fig. OA.3 in the Online Appendix). We see a similar trend in the age distribution (Fig. OA.4) – on average, women's age increase over time, but the change does not differ by treatment status. These trends indicate that the treatment did not induce demographic changes, e.g., via girls in treatment districts to step back and remain enrolled in secondary schooling to receive the stipend, nor did it lead to younger girls migrating to treatment districts to receive the stipends. Third, as an additional test of robustness of estimates to inclusion errors in treated and comparison samples, we conduct a simulation exercise using the MICS sample data by re-assigning 10% of the eligible treated sample to comparison. Similarly, we simulate the opposite – e.g., 10% of the control sample had migrated from treatment district and should have been classified as treated. We repeat this exercise 1000 times, each time with a new, random sub-sample of the sample that can be assumed to have migrated at some point before the date of the survey and hence erroneously assumed to have been part of the treated (or comparison) cohorts. Estimates of the impact on education from this exercise are reassuringly similar to results from the main analysis, and statistically significant. For instance, the average effect for primary completion rates differs from the main estimates only by 0.0006 (or 0.06 percentage points), a change that is within 0.3 standard deviation of the effect discussed in Section 5.1. Corresponding changes for secondary completion rates and years of education, are within 0.3 and 0.6 standard deviations of the main estimates, respectively. Average estimates remain significant at the 5% level.

Lastly, it is possible for differential changes in availability of secondary schools for girls due to other concurrent educational programs across treatment and comparison districts over time to drive some of the effects on girl's education. To address this concern, we control for per capita measures of schooling facilities available in each district, over time, in our main specifications. The data for this exercise comes from two additional sources – the District Census Reports of Pakistan for data on district population, and the National Education Census (NEC) for Pakistan for the number of public primary and secondary schools for girls. We show in Table A.4 in the Appendix that our results remain robust to controlling for secondary schools per capita in the district, implying that our estimated coefficients are not capturing some other underlying trend or program related to girls education and do in fact estimate the impact of the program. In Table OA.11 in the Online Appendix, we show results remain robust when we include primary schools per capita in the district as controls.

5. Results

5.1. Effects of FSSP on women's outcomes

We begin by estimating the effect of exposure to the FSSP on educational outcomes of women. Table 2 shows the results from estimating Eq. 1. Column 1 shows that each year of exposure to the FSSP, increases the likelihood of women completing primary schooling by 0.8 percentage point, which is an increase of 1.3 percent over the sample mean. This finding implies that the incentive of receiving a cash transfer in secondary schools induces girls to complete primary schooling and is in line with recent literature from similar settings. In particular, evidence from India shows that improved access to secondary (Mukhopadhyay & Sahoo, 2016) and higher education (Jagnani & Khanna, 2020) increases enrolment at the primary level by reducing costs and increasing motivation for enrolling in school. Sandholtz (2021) finds similar spillover effects of free secondary schooling on the probability of students transitioning from primary to secondary schools. These effects are smaller than the impact of a supply-side intervention in Indonesia, targeting primary school-going children directly, which increased primary completion rates by 4.1 percentage points (Akresh et al., 2018).

Results in Table 2, Column 2 show that women are 0.6 percentage points more likely to complete secondary schooling for each year they are exposed to the FSSP, an increase of 1.9 percent on the sample average secondary school completion rate of 31 percent. Column 3 shows that this increase in school completion rates corresponds to 0.09 more years of schooling; an increase of 1.4 percent over the mean 6 years of schooling for the sample. These findings are inline with previous, short and medium-term evaluations of the FSSP that find a cummulative 9% increase in secondary school enrollment rates as a result of the program (e.g. Alam et al. (2011) and Independent Evaluation Group (2011)). The effects are also comparable to a secondary schooling stipend program in Bangladesh, which Hahn et al. (2018) found to have led to a 2.5 and 5 percentage increase in completion rates for partially and fully exposed cohorts, respectively. In comparison, secondary completion rates increase by 6 percentage points (i.e. 0.06 x 10) for cohorts with the full, 10 year exposure in our sample. Similarly, the overall increase in years of education are comparable, albeit slightly lower, at 0.9 years for cohorts for full (10 year) exposure in our sample compared to the increase of 1.6 years estimated by Hahn et al. (2018).[23]

Next, we re-estimate Eq. 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 over the sample mean.[24] Women are 0.2 percentage points less likely to have their first child before the age of 17, a decrease of 3.8 percent on the sample average (Column 2). These results are smaller than the 7 pp. decrease in marriage before 18 for universal primary education beneficiaries in Uganda

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

[23] In Online Appendix Table OA.5, we test and show the impact the program may have had on educational outcomes for men in the sample. Coefficients are positive but much smaller than those for women, and significant only in the case of secondary education. That is, the program may have lead to a positive spillover impact on the likelihood of boys completing secondary schooling, possibly due to a change in household preferences towards secondary education.

[24] To ensure that the change in sample size due to age restrictions in our estimated equations is not driving the results, we re-estimate Eq. 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.9.

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

	(1) Completed primary	(2) Completed secondary	(3) Years of education
Years of exposure	0.008^AAA	0.006^AAA	0.086^AAA
	(0.002)***	(0.002)***	(0.024)***
Mean(comparison)	0.648	0.336	6.323
Mean(full sample)	0.616	0.308	5.969
N	218387	218387	218387
R^2	0.094	0.075	0.102

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 15 years or older. *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 district, survey year and cohort fixed effects. Mean (comparison) is the average values for the sample with no exposure to the FSSP. 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, ^AASignificance at 5% level, ^ASignificance at 10% level.

(Masuda & Yamauchi, 2020), and similar to the 2.3 pp. decrease in probability of birth at 16 from a secondary school expansion in Zimbabwe (Grépin & Bharadwaj, 2015). In addition, in contrast to Duflo et al., 2021 who find a reduction of 6 pp. in fertility of 22 year old women, we stop seeing statistically significant effects on marriage and child birth at the 18 years cutoff. This potentially indicates that these effects may largely be driven from girls staying in school marginally longer. [25]

Studies investigating the causal impact of schooling on maternal health care in developing countries are rare.[26] Unfortunately, we do not have a direct measure of maternal mortality in our data. Data suggests, however, that lack of maternal healthcare utilization, including prenatal care, is one of the leading causes of maternal deaths in Pakistan (Pakistan & ICF, 2020). We therefore estimate if exposure to FSSP leads to better maternal health seeking behaviour. We find women exposed to the program are more likely to seek maternal healthcare along some dimensions but not others. Column 3 in Table 3 shows women are 0.6 percentage point more likely to have a prenatal check up. In Column 4, however, we find no evidence of impact on take up of postnatal check ups. 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.

5.2. Intergenerational effects of the FSSP

We estimate the inter-generational effects of the FSSP on child health outcomes using Eq. 2. Column 1 and 3 of Table 4 show that children of women exposed to the FSSP score higher by 0.015 standard deviations on standardized measures of Weight for Age (WAZ) and by 0.018 standard deviations on standardized measures of Height for Age (HAZ).

We also check for effects on the important margins of underweight (below 2 standard deviations of WAZ) and stunting (below 2 standard deviations of HAZ) (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 0.5 percentage points less likely to be stunted, a reduction of 1.7 percent and 1.9 percent on sample mean values of being underweight or stunted.[27] At an estimated 0.15 SD and 0.18 SD improvement in HAZ and WAZ from full 10 years of exposure, respectively, our results are similar to the 0.21 increase in HAZ and 0.11 increase in WAZ from full program exposure in Bangladesh (Hahn et al., 2018).[28]

In Pakistan, an unconditional cash transfer program for the ultra-poor, 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 general trend, with larger improvements in health outcomes of girls. Appendix Table A.5 shows the results for Eq. 2 separately for boys (Panel a) and girls (Panel b). Columns 1 and 3 in both panels show that the magnitude of the effect of mother's exposure to the FSSP on child WAZ, HAZ and stunting is larger for girls than for boys, though the difference is not statistically different (p-values on difference are 0.700, 0.202 and 0.360 for WAZ, HAZ and stunting, respectively).

Finally, we look at child mortality. Column 5 in Table 4 shows that women exposed to the program are 0.6 percentage points less likely to experience the death of a child. With 17 percent of women in the sample reporting having lost a child, this is an important reduction of 3.5 percent on the sample average. These results can be compared from a 4 year long intervention to improve water quality in Kenya, which resulted in a decrease of 1.4 pp in child mortality (Haushofer, Kremer, Maertens, & Tan, 2021). In contrast to the intervention in Kenya which was directly targeting child health, the indirect impacts of the FSSP are meaningful. While the reduction estimated in this study does not differentiate between infant and child mortality, it lends support for using girls education program for long run meaningful reductions in child mortality.

Pakistan has one of the highest rates of stunting (WaterAid, 2016) and child mortality rates in the world (Devine & Taylor, 2018), receiving both global and local attention. Our results show that programs that educate women may help alleviate high rates of stunting and child mortality. Educated mothers may become enablers of health by proactively seeking healthcare for their children and being more aware on nutrition, health and hygiene practices than the less educated. It is worth noting that since young women's bodies are not ready for child-birth, delayed pregnancies can also improve child outcomes. We discuss some of the possible explanations for inter-generational results next.

5.3. 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 channels that may impact 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

[25] We test for marriage and child birth before the ages of 15 and 17 as well and find similar results. Table OA.10 in the Online Appendix provides these results.

[26] Grépin and Bharadwaj, 2015 explore the effects of a secondary school expansion program in Zimbabwe and find no effects on antenatal care

[27] Results however are largely robust to including birth order of the child. Appendix Table A.3 provides the results.

[28] For evidence on the short to medium term effects of cash transfers on health outcomes of children, see Evans, Hauslade, Kosec, and Reese (2014), Masuda and Yamauchi (2020).

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^AAA	−0.002^AA	0.006^AAA	−0.001
	(0.001)***	(0.001)*	(0.001)***	(0.002)
Mean(comparison)	0.081	0.052	0.870	0.527
Mean(full sample)	0.085	0.053	0.861	0.521
N	188461	151714	41177	40637
R^2	0.036	0.029	0.072	0.231

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 & 4 compared to the first two Columns. All regressions control for district, survey year and cohort fixed effects. Mean (comparison) is the average values for the sample with no exposure to the FSSP. 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	0.015^AAA	−0.005^AAA	0.018^AAA	−0.005^AAA	−0.006^AAA
of exposure	(0.004)***	(0.002)***	(0.005)***	(0.002)***	(0.002)***
Mean(comparison)	−1.353	0.291	−1.148	0.256	0.173
Mean(full sample)	−1.382	0.300	−1.193	0.268	0.171
N	85608	85362	84683	84683	71123
R^2	0.052	0.032	0.049	0.036	0.021

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 child's gender, child's age and district, mother's cohort and survey year fixed effects. Mean (comparison) is the average values for the sample with no exposure to the FSSP. 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.

data sets do not indicate labor force participation to be a significant 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 comparison 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 comparison 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. Indeed, studies report that returns to education for women do not significantly increase until they graduate from college (Field & Vyborny, 2016). Though labor force participation rates increase over the next 10 years to approximately 32% in the treated districts and 13% in the comparison districts as per the 2014 PSLM data, average female labor force participation do not vary by primary and secondary education attainment in treatment and comparison districts.

However, we can investigate channels other than income, such as assortative matching in the marriage market. 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.[29] Using the same estimation strategy as Eq. 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 0.6 percentage points more likely to marry men who have completed secondary education or higher. With a sample average of 27 percent this translates into a 2.2 percent increase, indicating that some of the changes in later life outcomes, including those of children, may be driven by marriage market effects. The coefficient on husband's education is twice is large than the coefficient on boy's secondary education completion (Table OA.5), indicating that the marriage market channel we see

[29] 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. Table OA.3 summarizes outcomes and characteristics of married women in the sample, and by whether they are married to the head or some other member of the household. On average, women married to household heads are older and live in smaller households. They also have lower levels of exposure to the program, which suggests that the estimates discussed in this section may be an underestimate of the impact of full exposure to the program.

Table 5
Potential Mechanisms of Effects of FSSP.

	(1) Husband's Education	(2) Knowledge of HIV/AIDS	(3) Ever Used Contraception
Years of exposure	0.006^{AA}	0.009^{AAA}	0.000
	$(0.002)^{**}$	$(0.002)^{***}$	(0.003)
Mean(comparison)	0.282	0.307	0.450
Mean(full sample)	0.266	0.283	0.422
N	41823	38494	38267
R^2	0.081	0.095	0.079

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 district, survey year and cohort fixed effects. Mean (comparison) is the average values for the sample with no exposure to the FSSP. 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.

here is not entirely because of positive spillovers of the program for men, but also that educated women are marrying more educated men. The assortative mating channel 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. While our data does not allow us to comment on monetary returns in the marriage market, literature suggests another channel through which education may have impacted the marriage market is through bride price. Specifically, the bride price received by parents from the family of the groom increases with the bride's education level and may incentivize parents keep girls in school for longer (Ashraf, Bau, Nunn, & Voena, 2020; Khan, 2021; Makino, 2019).

Increased maternal health care utilization can also lead to improved health outcomes of children. Women exposed to the FSSP are more educated, may be more aware and knowledgeable about health practices (Grossman, 2006), and more likely to be in a position of privilege that commands respect from health care providers (Gittelsohn et al., 1994). Literature documents education may lead to higher use of health services in three main ways: direct transfer of information through curriculum in school (Baker et al., 2011; Boerma, Sommerfelt, & Rutstein, 1991; Bhuiya & Mostafa, 1993; Frost, Forste, & Haas, 2005), being able to comprehend health messages in news and info-media (Glewwe, 1999; LeVine, LeVine, Rowe, & Schnell-Anzola, 2004; LeVine & Rowe, 2009), and by enhancing reasoning, decision making skills and trust in modern medicine that can affect health seeking behavior (Glewwe, 1999; Peters, Baker, Dieckmann, Leon, & Collins, 2010; Baker et al., 2011). We can not check for all possible ways in which health knowledge may have improved. However, following (Greenaway, Leon, & Baker, 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 0.9 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. In line with (Grépin & Bharadwaj, 2015) and in contrast with 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 use.

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).[30] To test this mechanism, we use data collected in the 2014 and 2018 rounds of MICS, where women are asked several questions 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 0.3 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). 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.[31]

5.4. 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. Fig. A.6 plots the number of hospitals per capita and number of hospital beds per capita over time.[32] While comparison districts have both higher per capita

[30] 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 & Sathar, 2001; Basu, 1992; Das Gupta, 1990; Bloom, Wypij, & Gupta, 2001).

[31] We cannot test, and do not claim, that the underlying mechanisms of change for all outcomes are the same. For instance, as discussed by Glewwe (1999), maternal literacy and numeracy alone may be sufficient to change child health outcomes; but may not necessarily lead to a delay in marriage or fertility decisions.

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

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 re-estimate Eq. 1 controlling for hospitals per capita, hospital beds per capita and lady health workers per capita in the district.[33] Results are provided in Appendix Table A.7. Coefficients on all maternal and child health measures remain robust to these controls, 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 four districts with high rates in 1998. We lose statistical power due to a significant decrease in the sample size in these regressions but results are consistent in terms of magnitude and direction of effect for all outcomes. Impacts on secondary school completion, years of education, early births and child mortality are less significant than before. Results for child health also remain robust, though the coefficient on being underweight is no longer statistically significant.[34]

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 then mirrors the canonical difference-in-difference estimation approach. 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.

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.15 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.15 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.[35] 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 comparison, rather than the treated, group may have overestimated average outcomes in the comparison group. We follow Grépin and Bharadwaj (2015) and (Osili & Long, 2008) and test if our results are robust to the exclusion of overage students. Results are shown in Table OA.16 in the Online Appendix. 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 comparison and treated households. Results are robust and are provided in Online Appendix Table OA.17.

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 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. Possible channels of the later-life and inter-generational impacts include 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, Das, & Khwaja, 2012; Duflo et al., 2015, 2021; Keats, 2018; Masuda & Yamauchi, 2020; Osili & Long, 2008; Barham et al., 2013). We add to this literature by evaluating the impact of *secondary* schooling through a unique non-means tested program. We look at important inter-generational health benefits, the evidence on which is sparse. 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 important policy targets, especially for low-income countries with poor health and educational outcomes and results indicate that longer term benefits must be considered when evaluating policies aimed at increasing schooling for girls. In the case of FSSP, the cash transfer amount analysed in this study, set at the start of the program in 2004 to PKR 600 per quarter, amounts to USD PPP 185 per girl for one year of secondary school.[36] While we do not have detailed cost information on overheads, literature suggests benefits may considerably outweigh the costs (McGovern et al., 2017).

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 a sample of relatively younger women who are not yet at the end of their fertile years. 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 changes. 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 and Tanzania indicate that better access to higher education may reduce costs and/or improve motivation for primary education

[33] 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.13.

[34] In Online Appendix Table OA.14, we restrict the sample further to +/-10 percentage points of literacy rate around the 40% literacy threshold. The direction of effects remain qualitatively similar.

[35] 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.

[36] Source:https://data.worldbank.org/indicator/PA.NUS.PPP?locations=PK.

(Mukhopadhyay & Sahoo, 2016; Jagnani & Khanna, 2020; Sandholtz, 2021). 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.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. Appendix

Figs. A.1,A.2,A.3,A.4,A.5,A.6, A.1,A.2,A.3,A.4,A.5,A.6,A.7,A.8,A.9.

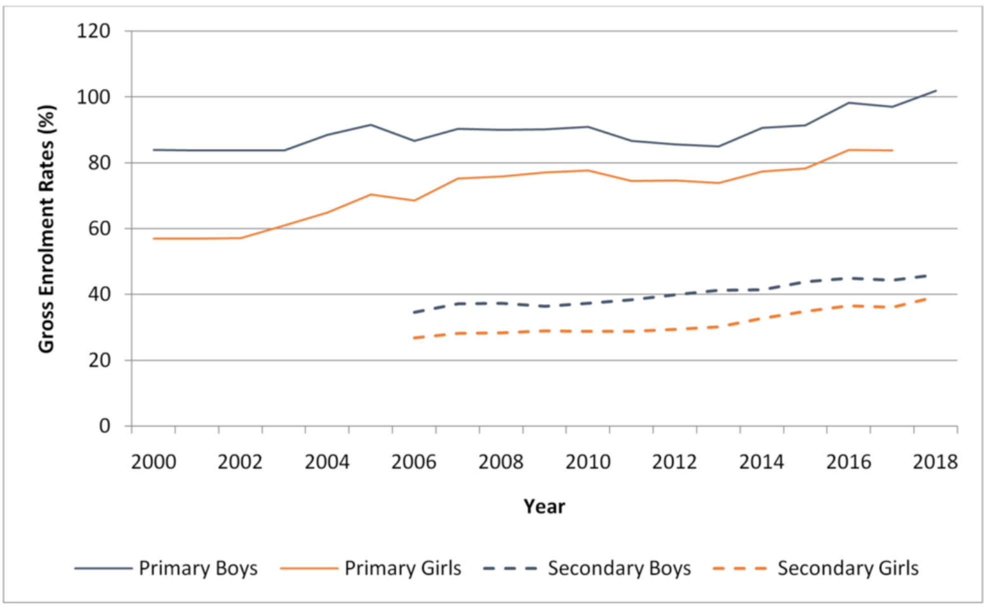
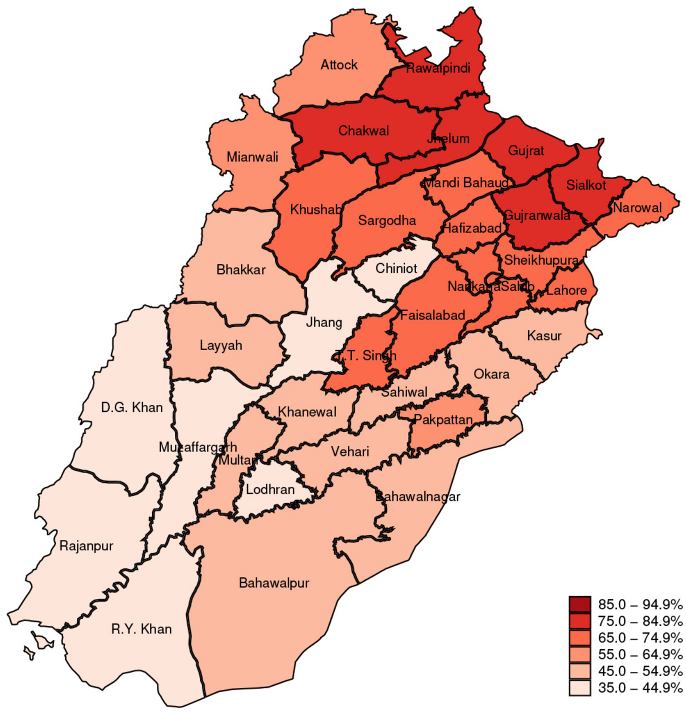
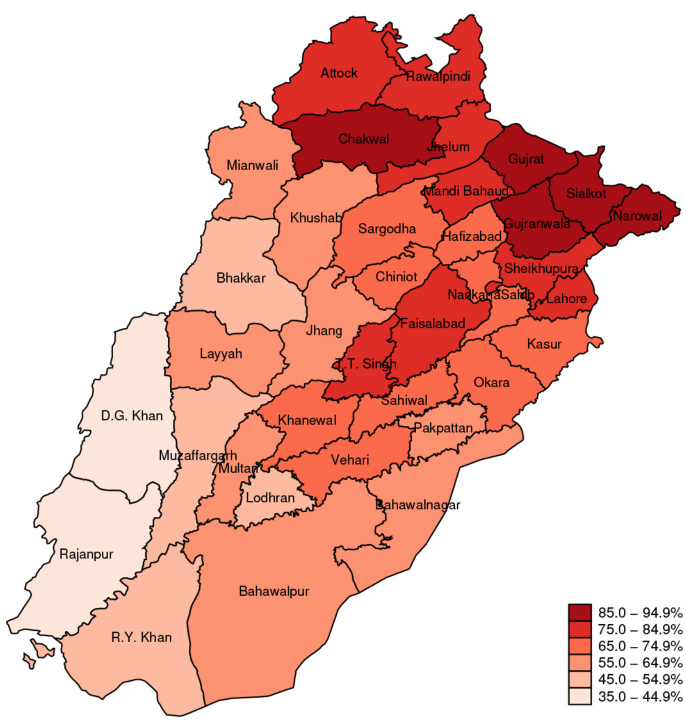


Fig. A.1. Primary and Secondary School Enrollment Rates in Pakistan. Note: This figure plots Gross Enrollment Rates (GER) rates by gender in primary and secondary schools in Pakistan. GER are calculated as the ratio of number of students enrolled in a given level of education, regardless of age, to the 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.



(a) Enrolment rates 2003



(b) Enrolment rates 2018

Fig. A.2. Enrolment rates for girls aged 11–15 years. Note: Based on authors' calculation from MICS 2003 and MICS 2018.

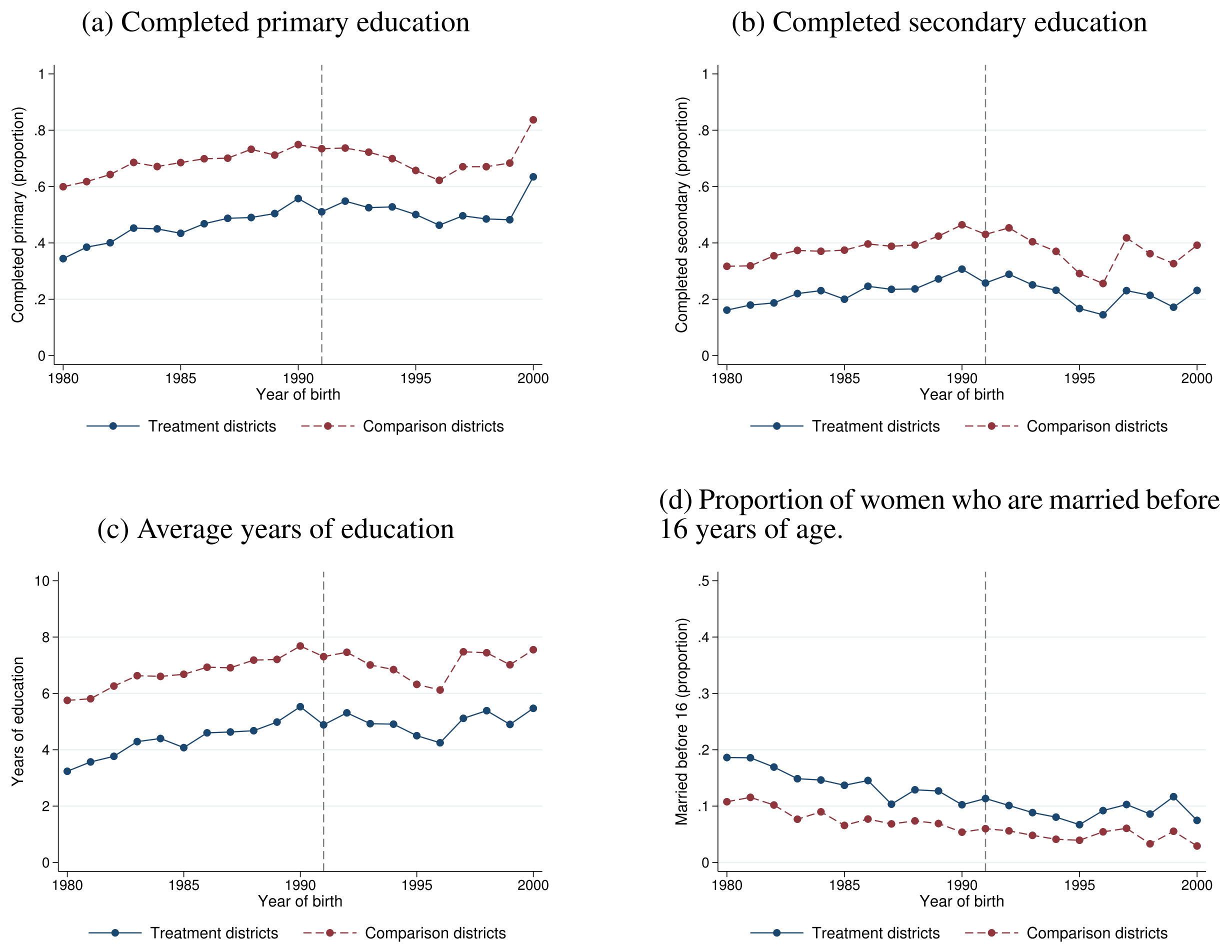
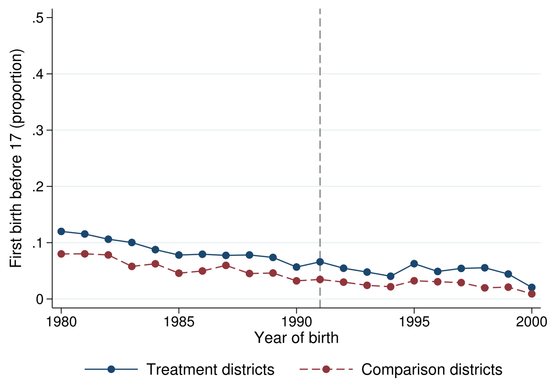
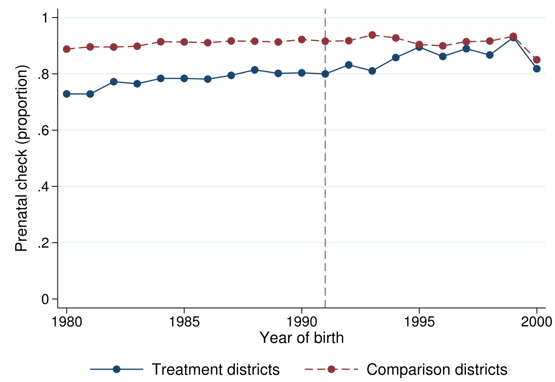


Fig. A.3. Outcomes over time for women in treated and comparison districts. Note: Each panel plots average value of the specified outcome, for each cohort displayed on the x-axis. We use data from all 4 rounds of MICS. Outcomes are as defined in Section 4.1. The dashed line is to indicate the first cohort (born in 1991) expected to have exposure to the FSSP.

(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

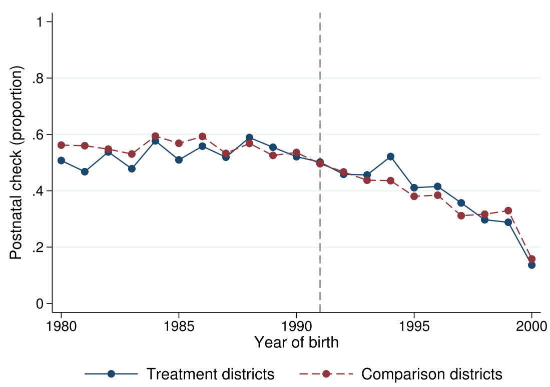


Fig. A.3. (*continued*)

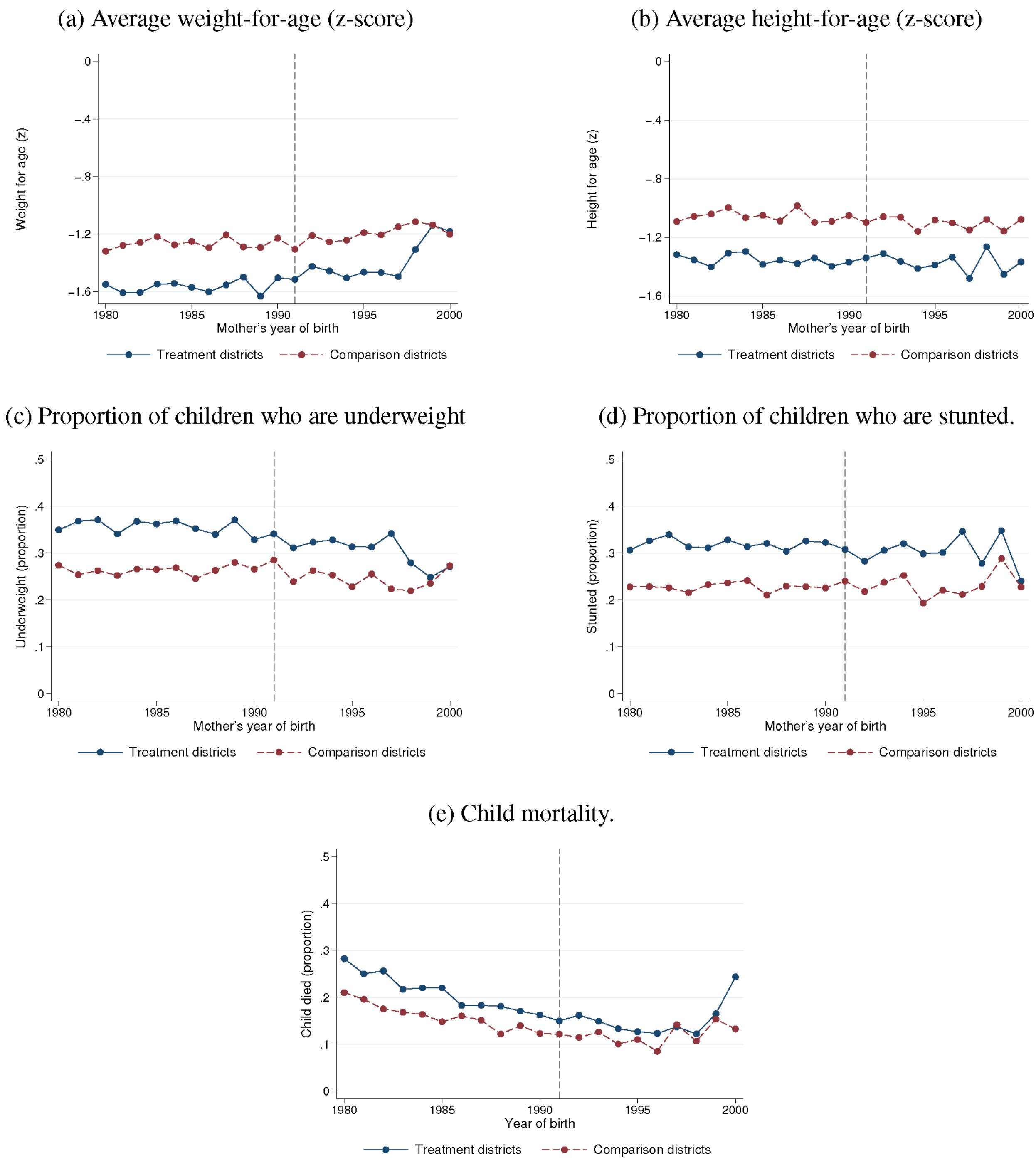


Fig. A.4. Outcomes over time for children under 5 years old in treated and comparison districts. Note: Each panel plots average value of the specified outcome, for children of each cohort displayed on the x-axis. We use data from 2011, 2014 and 2017 rounds of MICS where we have indicators to link mothers to children. Outcomes are as defined in Section 4.3. The dashed line is to indicate the first cohort (born in 1991) expected to have exposure to the FSSP.

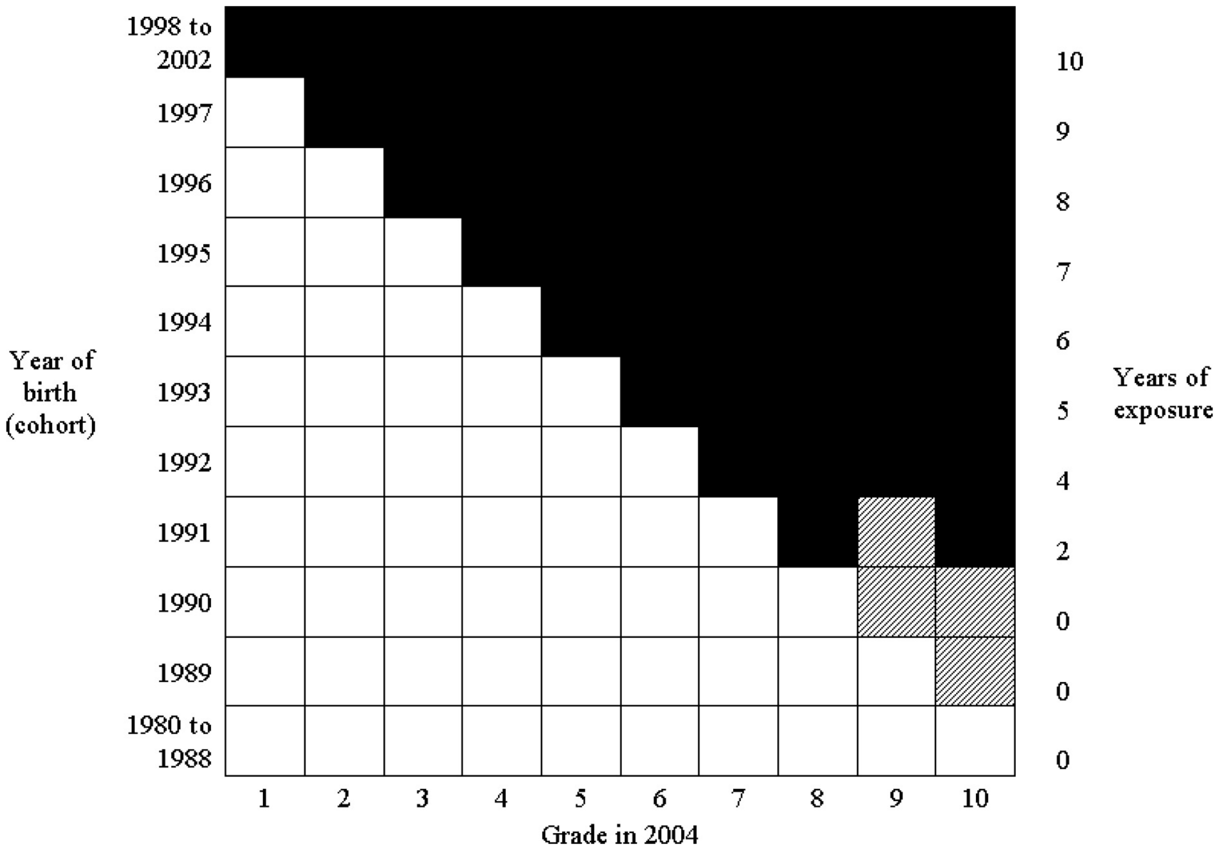
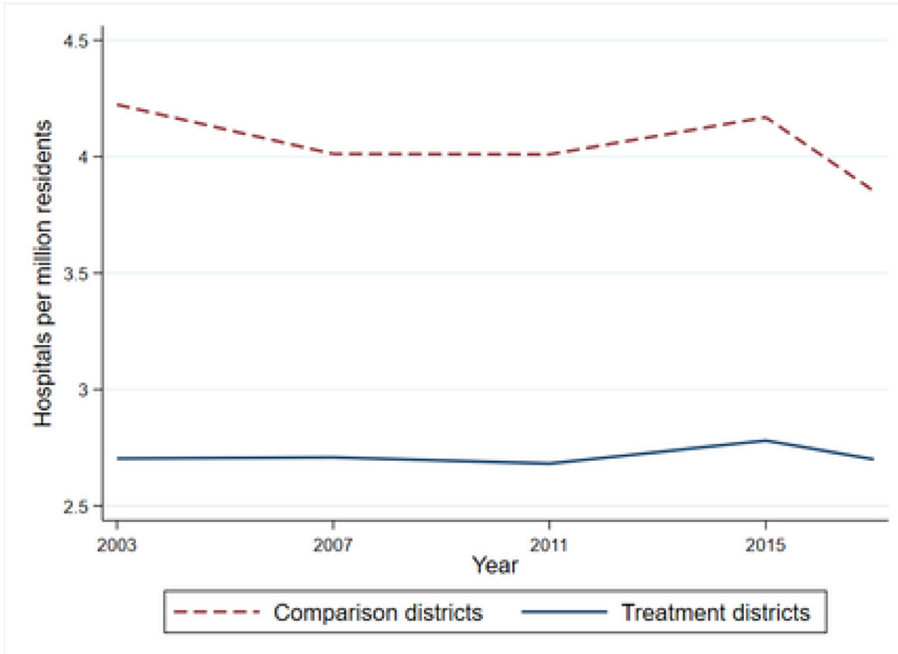
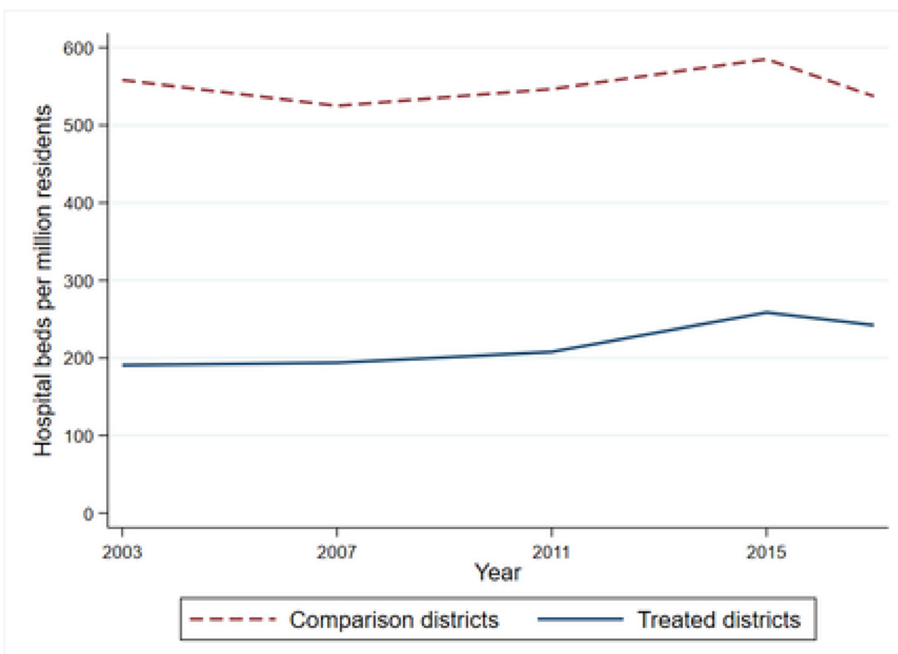


Fig. A.5. Cohort-wise Exposure to the FSSP. Note: The vertical axis on the left plots women's year of birth, which represent cohorts included our sample. The horizontal axis plots the grades cohorts will be enrolled in at the time of program start in 2004. Each block in black represents a year of exposure to the FSSP. The vertical axis on the right provides the years of potential exposure for each cohort, which is a sum of the black blocks in that row. The FSSP was provided to students in the lower secondary level in 2004 and 2005, i.e. grades 6–8, and was extended to grades 9 and 10 from 2006. The shaded blocks in gray were enrolled in higher secondary schooling in 2004 and 2005 and were not eligible for the FSSP in those years. Specifically, cohorts born in 1989 and 1990, aged 15 and 14 in 2004, respectively, were in grades 9 and 10 in 2004 and 2005 and were not eligible for the FSSP. The cohort born in 1991 was in grade 9 in 2005 and was not eligible for the FSSP. The 1991 cohort received 2 years of exposure: in grade 8 in 2004 and in grade 10 in 2006. Cohorts born in 1992 and later were exposed to the program for as long as they were enrolled in primary or secondary school for which the FSSP was in place (i.e. between the grades 1–10). This amounts to 4 years of exposure for women born in 1992; 5 years of exposure for women born in 1993, and so on till women born in 1998 or later, who have a full 10 years of exposure to the FSSP, from grade 1–10.



(a) District hospitals per capita (million)



(b) District hospital beds per capita (million)

Fig. A.6. District health facilities 2003–2018. Note: Based on number of hospitals and district size reported in Punjab Development Reports 2003, 2011, 2014, 2017.

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

Treatment (recipient) districts		Comparison (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. Fig. OA.2 in the Online Appendix provides a map with district names.

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	163752	5.434	3.128	54635
Secondary completion	0.336	0.472	163752	0.223	0.416	54635
Highest grade	6.323	4.975	163752	4.905	4.744	54635
Married before 16	0.076	0.265	151358	0.088	0.283	51509
First birth before 17	0.045	0.208	134304	0.044	0.204	46869
Prenatal checkup	0.870	0.336	33046	0.825	0.380	8109
Postnatal checkup	0.527	0.499	32638	0.498	0.500	7978
Child died	0.173	0.378	59245	0.160	0.367	11804
Age	23.605	5.736	163752	20.095	3.769	54635
Members in the household	7.792	3.623	163752	7.958	3.594	54635
Household hold head owns the home	0.871	0.335	163701	0.879	0.326	54619
No. of rooms in the house	2.584	4.813	163752	2.336	4.130	54635
Panel (b): *Children*						
Mother's years of exposure	0.000	0.000	69125	3.250	2.522	15067
Weight-for-age (z score)	−1.341	1.177	69125	−1.494	1.152	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
Male	0.513	0.500	69125	0.516	0.500	15067
Birth order	2.453	1.385	69125	1.911	1.092	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.

Table A.3
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	0.009 (0.004)**	−0.003 (0.002)*	0.012 (0.005)**	−0.003 (0.002)**
N	85608	85362	84683	84683
R^2	0.063	0.040	0.062	0.047

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 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.

Table A.4
Effect of FSSP on Women's Educational Outcomes, controlling for secondary school per capita in each district.

	(1) Completed primary	(2) Completed secondary	(3) Years of education
Years of exposure	0.008 (0.002)***	0.007 (0.002)***	0.086 (0.024)***
N	218387	218387	218387
R^2	0.094	0.075	0.102

Notes: This table shows the estimation results from Equation 1 with controls added for school per capita. The data for the number of schools in each district comes from the National Education Census (NEC) for Pakistan, on population from District Census Reports of Pakistan (1981 and 1998) to calculate schools per capita in each district. The remaining data comes from pooling four rounds of MICS. The sample consists of all women born between 1980 and 2002 who were at least 15 years or older. *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 district, survey year and cohort fixed effects. Standard errors (in parentheses) are clustered by district. *** p < 0.01, ** p < 0.05, * p < 0.1.

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

Panel (a): Male children	(1) WAZ	(2) Underweight	(3) HAZ	(4) Stunted
(Mother's) Years of exposure	0.014 (0.006)**	−0.006 (0.002)***	0.013 (0.007)*	−0.004 (0.002)*
N	43977	43847	43539	43539
R^2	0.051	0.032	0.046	0.033
Panel (b): Female children	WAZ	Underweight	HAZ	Stunted
(Mother's) Years of exposure	0.017 (0.006)***	−0.004 (0.002)	0.024 (0.007)***	−0.007 (0.002)***
N	41631	41515	41144	41144
R^2	0.057	0.037	0.056	0.043
$p - value(Male = Female)$	0.700	0.469	0.202	0.360

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 child's gender, child's age and district, mother's cohort and survey year fixed effects. 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.

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	0.004	0.003
	(0.001)***	(0.001)**	(0.001)**	(0.001)***	(0.001)***
N	90225	90460	89823	84287	90200
R^2	0.067	0.064	0.061	0.065	0.045

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 district, survey year 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.

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	0.006	0.006	0.006	−0.001	−0.001	−0.002
	(0.001)***	(0.001)***	(0.001)***	(0.002)	(0.002)	(0.002)
N	41177	41177	22415	40637	40637	21990
R^2	0.072	0.072	0.067	0.231	0.231	0.355
Panel (b)	Child mortality	Child mortality	Child mortality	WAZ	WAZ	WAZ
(Mother's) years of exposure	−0.006	−0.006	−0.005	0.015	0.015	0.014
	(0.002)***	(0.002)***	(0.002)***	(0.004)***	(0.004)***	(0.005)***
N	71123	71123	42527	85608	85608	49475
R^2	0.021	0.021	0.021	0.052	0.052	0.055
Panel (c)	Underweight	Underweight	Underweight	HAZ	HAZ	HAZ
(Mother's) years of exposure	−0.005	−0.005	−0.004	0.018	0.018	0.019
	(0.002)***	(0.002)***	(0.002)**	(0.005)***	(0.005)***	(0.005)***
N	85362	85362	49338	84683	84683	49005
R^2	0.032	0.032	0.033	0.049	0.049	0.050
Panel (d)	Stunted	Stunted	Stunted			
(Mother's) years of exposure	−0.005	−0.005	−0.005			
	(0.002)***	(0.002)***	(0.002)***			
N	84683	84683	49005			
R^2	0.036	0.036	0.037			
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. LHW data is only available corresponding to the last two rounds of MICS, leading to a reduced sample size for regressions that control for LHW per capita. All regressions control for district, mother's cohort and survey year 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	0.006	0.004	0.056	−0.002
	(0.002)***	(0.002)**	(0.024)**	(0.001)**
N	177646	177646	177646	153457
R^2	0.078	0.058	0.080	0.030
Panel (b)	**(1) First Birth Before 17**	**(2) Prenatal**	**(3) Postnatal**	**(4) Child Mortality**
Years of exposure	−0.001	0.005	−0.002	−0.004
	(0.001)	(0.001)***	(0.002)	(0.001)**
N	123778	33578	33148	57601
R^2	0.027	0.065	0.231	0.018
Panel (c)	**(1) WAZ**	**(2) Underweight**	**(3) HAZ**	**(4) Stunted**
Years of exposure	0.010	−0.003	0.011	−0.005
	(0.005)**	(0.002)	(0.005)**	(0.002)***
N	69708	69499	68996	68996
R^2	0.045	0.028	0.043	0.032

Notes: Outcomes are as defined in Tables 2–4. 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 control for district, mother's cohort and survey year 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 child's gender and child's age. (Mother's) Years of exposure is the number of years the (mother) woman was exposed to the FSSP during her school going years. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

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

Panel (a)	(1) Completed primary	(2) Completed secondary	(3) Years of education	(4) Married Before 16
Treatment*Exposed	0.047	0.016	0.374	−0.016
	(0.013)***	(0.010)	(0.145)**	(0.007)**
N	218387	218387	218387	188461
R^2	0.093	0.074	0.102	0.036
Panel (b)	**(1) First Birth Before 17**	**(2) Prenatal**	**(3) Postnatal**	**(4) Child Mortality**
Treatment*Exposed	−0.003	0.017	0.001	−0.030
	(0.005)	(0.008)**	(0.009)	(0.006)***
N	151714	41177	40637	71123
R^2	0.029	0.072	0.231	0.021
Panel (c)	**(1) WAZ**	**(2) Underweight**	**(3) HAZ**	**(4) Stunted**
Treatment*Exposed	0.050	−0.024	0.056	−0.018
	(0.020)**	(0.007)***	(0.022)**	(0.007)**
N	87914	87660	86952	86952
R^2	0.051	0.031	0.050	0.037

Notes: Outcomes are as defined in Tables 2–4. All regressions in Panel A and B control for an indicator for treatment district, and district, cohort and survey year 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 child's gender, child's age and district, mother's cohort and survey year fixed effects. '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). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix B. Supplementary data

Supplementary data associated with this article can be found, in the online version, at https://doi.org/10.1016/j.worlddev.2022.106115.

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