

# The Impacts of Maternity Benefits on Early Education: Evidence from India

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## Abstract

Maternity benefits are targeted at improving both children's and mothers' outcomes, but many women in the informal sector are not eligible for traditional maternity leave programs. This paper investigates the impact of IGMSY, a unique maternity benefits program in India, on early childhood education. The program launched in 2011, was piloted in 52 out of India's 640 districts, and provided cash transfers to women for their first and second live births regardless of employment status. Using a difference-in-differences approach across districts and cohorts, I find that the program increased preschool enrollment by 9 percentage points but did not increase enrollment, reading, or math competency in primary school. The effects on enrollment are strongest for children from poorer households, likely due to both improvements in health-related outcomes and increases in income.

**Keywords:** Maternity benefits, cash transfer, education, preschool, mothers

**JEL Classification:** I25, I38, J13

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# 1 Introduction

Over 740 million women around the world are informally employed, leaving nearly 60% of the global female workforce without access to formal maternity leave ([International Labor Organization, 2018](#)). Informally-employed women who become pregnant face a sharp tradeoff: continue to work (and face the health risks associated with working while pregnant or shortly after childbirth) or stop working (and lose their source of income). Over 120 countries around the world legally guarantee paid maternity leave, though leave policies vary greatly in length and generosity of benefits ([International Labour Organization, 2025](#)). However, leave policies universally fail to provide for women employed in the informal sector, a particularly relevant demographic in developing economies.

In India, the majority of women are informally employed. Women often continue working up until the time of childbirth and begin working again shortly afterwards, especially in rural areas where households have limited savings. This can result in negative health effects for both the woman and her child ([Drèze et al., 2021](#)). These women face resource constraints that cannot be addressed through traditional maternal leave policies. As an alternative, different types of maternity benefits policies or programs may be used to improve outcomes for women in the informal workforce, as well as their children.

This paper investigates whether a maternity benefits cash transfer program for informally-employed and unemployed women in India impacts early childhood education for the children of eligible mothers. Existing evidence shows that the intervention directly improved health outcomes for women and their children ([von Haaren and Klonner, 2021](#)). These improvements in health could result in more indirect changes later on, such as improvements in education-related outcomes. These changes might occur years after the receipt of the last transfer payment. This paper answers the question of whether providing maternity benefits to women in the form of a conditional cash transfer also improves school enrollment and test scores for their children.

To address the fact that many women in India are informal workers and are not eligible for traditional maternity leave, the government of India took the novel approach of distributing conditional cash transfers as a form of maternity benefits, beginning in 2011. Through the Indira Gandhi Maternity Support Scheme (IGMSY), women could receive maternity benefits for their first or second child. Women were eligible for this program as long as they were not eligible for formal maternity leave, including women who were informally employed, unemployed, not in the labor force, or working in small firms that were exempt from the formal maternity leave policy. The stated goal of these transfers was to improve the health and nutrition of both women and their children ([Ministry of Women and Child Development, 2011](#)). Previous evaluations show that this program did make women and children healthier, reducing child mortality, increasing the number of vaccinations received, and marginally increasing weight-for-age ([Aizawa, 2022](#),

(Mathur and Sen, 2024, von Haaren and Klonner, 2021).<sup>1</sup> Since the IGMSY program is not conditional on employment status, it is also important to note that the program does not directly incentivize labor force participation.

There are a couple of ways in which this type of program could affect education. First, women who receive the transfer could invest more in their children's health, and children who are healthier might be more likely to be enrolled in school or to learn more quickly. Alternatively, households that receive the transfer may be able to invest more resources in their children's schooling.

To identify the causal impact of the IGMSY maternity benefits program on early childhood education, I use a difference-in-differences approach. I compare older (untreated) and younger (treated) cohorts in the districts that did and did not receive the program, investigating impacts both on the extensive margin (school enrollment) and intensive margin (reading and math competency) of education. Additionally, I investigate heterogeneity by gender, household wealth, caste, and parents' education. Data for India come from the 2010-2022 waves of the Annual Status of Education Report (ASER), the 2019-2021 Demographic and Health Survey (DHS), and the 2005 and 2011 waves of the India Human Development Survey (IHDS).

I find that the IGMSY program increased school enrollment of preschool-age children but did not increase enrollment of older children, likely because enrollment is already quite high for children ages 5 and up due to the compulsory nature of schooling in India. I also find that math and reading competency for older children remain unchanged, suggesting that improvements in preschool enrollment due to the program do not translate into increases in learning outcomes in primary school.

I provide evidence that the effects of the program on enrollment fade out over time, suggesting that, as the time since the mother's receipt of the cash transfer increases and as baseline enrollment for the child's age group increases, effects on children's enrollment decline. I also show that effects are larger for poorer households and children whose fathers have lower levels of education. This suggests that the impacts of the program on education (likely through receiving cash and improving health) are strongest for households that are the least well-off.

Additionally, I provide suggestive evidence that women's likelihood of working increases as a result of exposure to the program. This could also be due to the fact that their children are healthier or are more likely to be sent to a preschool center, providing suggestive evidence of complementarities between the IGMSY program and labor force participation. This is perhaps surprising given that receipt of the maternity benefits is not conditional on women working.

This paper contributes to several strands of the literature. The first is the literature on

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<sup>1</sup>This is also consistent with findings that other, similar cash transfer programs to mothers in India produce improvements in nutrition and children's functional development (Weaver et al., 2025).

conditional cash transfers and, more specifically, the impacts of conditional cash transfers (CCTs) on education. Existing literature shows that CCTs in various settings improve education and health outcomes, especially in the short run and when education-related requirements are among the conditions for receiving the transfer (Akresh et al., 2016, Attanasio et al., 2012, Baird et al., 2014, 2016, Barrera-Osorio et al., 2019, Behrman et al., 2011, Garcia and Saavedra, 2022, Macours et al., 2012, Paxson and Schady, 2010, Schultz, 2004).<sup>2</sup> Most CCTs in this literature are targeted at poorer households, aimed at a much more specific group of women than the broad range targeted by the IGMSY program (which did not have eligibility requirements related to household income). Additionally, the setting of the IGMSY program is unique in that the transfer was specifically targeted at health outcomes and conditional on meeting a set of health-related requirements. However, receipt of the cash transfers was not contingent on any education-related conditions. I contribute to this literature by estimating the education impacts of a universal program targeted at health. Many CCT programs have education-specific goals included in their conditions, but I show how a program that is focused on health outcomes leads to a 9pp increase in preschool enrollment.

I also add to the literature evaluating the impact of the IGMSY program. Previous papers have examined the first-stage effects of the IGMSY transfers on health, finding health improvements for mothers and children, impacts on fertility, and reductions in maternal and child mortality (Aizawa, 2022, Jain, 2018, Mathur and Sen, 2024, von Haaren and Klonner, 2021).<sup>3</sup> To the best of my knowledge, this is the first paper to look at the impact of this program on later-life educational outcomes for the children whose mothers were eligible for the maternity benefits, as well as the first to study the impacts of IGMSY on women's employment. This adds to our understanding of the comprehensive effects of the IGMSY maternity benefits program on women and their children.

This paper also contributes to the literature that studies the second-order effects of other health and nutrition-focused interventions. For example, Baird et al. (2016) find positive long-run effects on adult education and labor supply as a result of a deworming program in Kenya. Maluccio et al. (2009) find that in Guatemala, nutrition interventions for young children improved adult educational attainment, reading comprehension, and cognitive ability. In Sweden, Lundborg et al. (2022) find that free school lunches increase educational attainment and lifetime earnings. Many of these papers focus on long-run effects of health interventions on adult outcomes. In contrast, this paper examines the more immediate effects of a health-focused program on early childhood education, pro-

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<sup>2</sup>There is also an extensive literature on the effects of cash transfers given to mothers on health, nutrition, and anthropometrics. See, for example, Ahmed et al. (2025), Field and Maffioli (2025), Weaver et al. (2025).

<sup>3</sup>There are relatively few papers examining the impact of the IGMSY program. Besides the ones cited above, Mittal (2018) summarizes findings from all cash transfers programs in India and finds only one report on the IGMSY program. Vij (2017) looks at the resource gap in the funding of the IGMSY program.

viding a fuller understanding of how health interventions can impact education over the life cycle.

Finally, this paper contributes to the literature on maternity leave and benefits. Much work has been done on maternity leave in developed countries, and there is an emerging literature on maternity leave in developing country contexts ([Baker and Milligan, 2008, 2015](#), [Butikofer et al., 2021](#), [Dahl et al., 2016](#), [Schönberg and Ludsteck, 2014](#)). However, most of the existing literature deals with leave policies that are available in formal employment settings, which are not necessarily relevant or generalizable to the setting of India. This is because maternity leave policies in India apply only to a very small fraction of women who work in the formal sector, leaving women who work in informal jobs or at small firms unprotected. The impacts of maternity leave policies and other forms of maternity benefits are likely quite different. This paper adds to our understanding of the effects of an innovative maternity benefits policy for informally-employed or unemployed women.

The rest of this paper examines the impacts of the IGMSY program on children’s educational outcomes. Section 2 provides background information on maternity benefits in India, Section 3 introduces the data and sample, Section 4 describes the methodology, Section 5 presents the results, Section 6 summarizes the heterogeneity analysis, Section 7 provides a set of robustness checks, and Section 8 concludes.

## 2 Background

Maternity leave in India for women in the formal sector was instituted in 1961, but very few women are eligible for maternity leave. About 90% of Indian women who are employed work in the informal sector ([International Labor Organization, 2018](#)). Of informally-employed individuals, 49% are self-employed, 30% earn daily wages, and 21% earn standard wages ([Gurtoo and Williams, 2009](#)). Informal employment for women includes working as street vendors, as petty traders, in other retail activities, or as agricultural laborers ([Abraham, 2019](#), [Turnbull, 2019](#)). Even women with formal jobs may not be eligible for paid maternity leave if they work at small firms with fewer than 10 employees. A government census in 2008 found that less than 2% of firms in India employed 10 or more workers ([The Economic Times, 2008](#)). As a result, less than 10% of employed women in India are eligible for paid maternity leave.

Many pregnant and nursing women in India also do not have adequate access to nutrition, experience little or no weight gain during pregnancy, and must work up until and soon after the delivery of their children ([Drèze et al., 2021](#)). This leads to underweight babies. In-utero and early life health, including birth weight, are associated with later-life health outcomes. Thus, in cases where mothers do not have enough food and rest, children could experience negative impacts later in life. To make matters worse, provision

of antenatal care for women and their children is extremely limited ([Singh et al., 2022](#)).

In 2011, the government of India responded to these concerns by introducing the pilot program for a maternity benefits scheme, called the Indira Gandhi Maternity Support Scheme (IGMSY), which provided cash transfers to women during pregnancy and shortly after giving birth. This program operated in 52 of India's 640 districts and provided cash transfers totaling 4000 INR to women who were age 19 or older for their first and second live birth. In 2004, the median household income in India was 27,856 INR, so this program would represent approximately a 14 percent increase in income for the median household ([Desai et al., 2010](#)).<sup>4</sup> The IGMSY program sought to address issues related to the health and nutrition of children and their mothers by encouraging mothers to follow health recommendations for their children and by providing additional resources to supply adequate nutrition.<sup>5</sup> The IGMSY transfers were provided in three installments, and the total value of the transfers was equal to approximately 40 days of work at the minimum wage.

The transfers were conditional on the women meeting several health-related requirements, both for themselves and their children.<sup>6</sup> The first installment of the transfer was given at the end of the second trimester and was conditional on registration of the pregnancy at a health center within four months of the start of the pregnancy, one antenatal care appointment, and one counseling session, which would be verified by the provision of a registration card from the local health center. The second transfer was given three months after delivery and was conditional on the birth of the child being registered, the child receiving six doses of various vaccines, and the parent attending two growth monitoring and counselling sessions. This would be verified through the growth monitoring chart and immunization register. The third and final installment was given six months after delivery and was conditional on exclusive breastfeeding for six months (self-reported), the child receiving two additional vaccine doses, and the mother attending two additional growth monitoring and counselling sessions. The mother received 1500 INR in each of the first two transfers and 1000 INR in the third transfer ([Ministry of Women and Child Development, 2011](#)). Since all of these transfers were provided before or during the child's first year of life, the mothers were no longer receiving transfers when the child became old enough to attend preschool or primary school.<sup>7</sup>

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<sup>4</sup>There is also a lot of variation across households in rural and urban areas. In rural areas, the median household income is 22,400 INR, while in urban areas the median household income is 51,200 INR.

<sup>5</sup>Cash transfer programs with similar characteristics have been run in nearby countries. For example, Indonesia's Family Hope Program (PKH) provides transfers for pregnant women and for children up through age 15 in order to increase both health/nutrition outcomes and education outcomes ([Huda et al., 2020](#)). However, a unique feature of the cash transfers in India is that they are not directly related to education, not conditional on educational outcomes, and not provided for children of school age.

<sup>6</sup>For policy details regarding the specific timing and requirements for the policy, see Figure B1.

<sup>7</sup>In 2017, this pilot program was extended to the entire country of India. A few minor updates were made between 2011 and the final nationwide rollout of the program in 2017: the transfer amount was changed, and instead of three transfers, the cash was provided in two installments ([Drèze et al., 2021](#)).

The IGMSY program operated with an open roster, meaning that women who became pregnant with their first or second child any time after the implementation of the program were eligible for the transfer. Because of this, the program could have impacts on the number of children that women chose to have or on how soon they chose to have them. Essentially, IGMSY could impact birth spacing and fertility, which is important to consider when interpreting any findings. To deal with this, I focus mainly on the effects for first-born children.

This program was a hybrid between a traditional maternity leave program and a conditional cash transfer program. The actual policy was implemented in the form of a conditional cash transfer. However, the target group was mothers, and the program had the same goal as that of a traditional maternity leave policy — improving health outcomes for women and their children. In settings with high levels of informality where traditional maternity leave policies do not work well, this type of program offers a potential solution to close the gap in coverage.

It is important to note that not all eligible women were actually able to receive the transfers. A JABS survey suggests that many women could not receive the transfers for various reasons, including issues with identification documents or bank account information ([Drèze et al., 2021](#)). To the extent that compliance or receipt of transfers is not 100%, this will make my estimates a lower bound for the effects of the program with full compliance. Information on the percentage of women who received the cash transfers could be used to scale up the effects as a counterfactual for the case where the program is implemented with full compliance.

While the program was not conditional on any education-related outcomes, there is potential for IGMSY to impact children’s schooling. Existing literature shows that there are links between health and schooling. Children who are healthier are likely to learn better, and parents might be more willing to invest in healthier children ([Basch, 2011](#), [Datar et al., 2010](#)). Depending on how health, cash transfers, and schooling are linked, any effects on education could either persist over time or fade out over time. Additionally, increases in income could be linked to increases in education if parents change their education-related investments when their income rises.<sup>8</sup>

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Additionally, the program was adjusted so that the mother was only eligible for the program for her first live birth. Take-up of both the pilot program and the full rollout was very slow, as the budget was not expanded enough to be able to give transfers to all women who were eligible ([von Haaren and Klonner, 2021](#)). Implementation of the projects was done by the states ([Baruah, 2011](#)), and this lack of central coordination could be one of the reasons why the rollout was slower than originally planned. This paper focuses on the initial pilot program.

<sup>8</sup>The goal of the maternity benefits program was to promote healthy behaviors that would improve the health and nutrition of the women and children who were receiving the transfers. However, none of the aspects of the program were conditional on any education-related outcomes, and the transfers ended long before the child would be enrolled in school. While there were health-related conditions for receiving the transfers, there were no conditions for how the money might be spent. In many settings with transfers that do not have restrictions on how they can be spent, women may still believe that there are requirements for how to spend the money, and these beliefs can be impacted by societal beliefs or

School enrollment in India is relatively high, and preschool enrollment is also high compared to other contexts. Globally, pre-primary enrollment in 2010 was 46% ([World Bank, 2022](#)). In 2016, before the full nationwide rollout of the IGMSY program, the government of India reported that 70% of children were enrolled in preschool centers ([Rao et al., 2021](#)). This is similar to Colombia, Chile, and Mexico, who in 2019 reported 68-71% enrollment of three- and four-year-olds in pre-primary education programs. In the US during the same time period, the enrollment rate was only about 54% ([National Center for Education Statistics, 2023](#)).

### 3 Data & Sample

The data come from three sources. First, I use the Annual Status of Education Report (ASER) 2010, 2011, 2012, 2013, 2014, 2016, 2018, and 2022 waves ([Pratham, 2022](#)). The ASER was completed in rural districts in India and contains individual-level data. The ASER does not include individuals from urban areas, meaning that some of the wealthiest areas of the country are not represented in this dataset. Second, I use the Demographic and Health Surveys (DHS) 2019-2021 wave for India ([IIIPS, 2022](#)). The DHS contains individual-level data on a limited set of education outcomes for children across India, and the sample is designed to be representative of women ages 15-49.<sup>9</sup> These are the main sources of education data for India over the relevant time period ([Johnson and Parrado, 2021](#)). For the additional outcomes and program take-up, I also use data from the India Human Development Survey (IHDS) 2004-05 and 2011 waves ([Desai et al., 2009, 2015](#)).

The ASER data comes from a survey conducted in households and records enrollment status, test scores, tuition payments, and demographic information for the school-aged children in these households, as well as household asset ownership, geographic location, and parents' education and demographic information. The ASER data provides information on school and preschool enrollment for children in rural households between ages 3-16.<sup>10</sup> Categorical math and reading scores are provided for children ages 5-16, where the score provides the child's level of competency. These variables can be used to measure both the extensive margin of schooling (enrollment) and the intensive margin of schooling (math and reading competency).

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by the organization that is implementing the transfers ([Gram et al., 2019](#)). In this case, it is difficult to know whether these types of beliefs influenced the way that the transfer was spent, but it is important to bear this possibility in mind when interpreting the results.

<sup>9</sup>As such, the children of these women are included in the dataset. However, it is important to note that this data may not be representative at the child level.

<sup>10</sup>There are two enrollment variables: “ever enrolled in school” and “currently enrolled in school.” Unless otherwise specified, the analysis will focus on the “ever enrolled in school” variable to avoid dealing with the issue of children who have dropped out of school, though the two measures are very similar in practice. For waves before the ASER 2018, children below age 5 were only included in the survey conditional on being enrolled in school.

I restrict the sample of children from the ASER data to individuals between three and 12 years of age to avoid including older children who are more likely to drop out of school (for some outcomes, including the math and reading competency, the sample will be only children between five and 12 years of age due to data availability). I calculate a child's year of birth by subtracting the child's age from the year of the survey. For example, children aged 3-12 in the 2018 ASER were born between 2006 and 2015, and children born between 2011-2015 in the treated districts were the ones that were actually affected by the IGMSY program. In this context, mothers of children aged seven or younger at the time of the survey were eligible to receive maternity benefits, while mothers of children aged eight or older were not eligible due to the child's birth occurring before the rollout of the policy.

The ASER data does not provide information on whether a woman received maternity benefits for her children, so I use the eligibility requirements of the maternity benefits program to classify women as eligible or ineligible for the program and calculate the intent-to-treat estimates. First, I restrict the sample to children whose mother was at least age 19 at the time of the child's birth, since only women ages 19 or older were eligible for the program. Mother's age at the time of the child's birth is calculated by subtracting the child's age from the woman's age.

Since only the first and second births were eligible for the policy, it would be ideal to restrict the sample to only the first and second-born children for each woman. The demographic information in the ASER provides some data on the household structure. However, one disadvantage of the ASER is that it does not provide a full household roster. Instead, information is provided on the father, the mother, and all children between ages 3-16. Since only the first two children born to each woman were eligible for the transfer, when constructing the sample I assume that the oldest two children listed on the household roster are the woman's first two children (essentially assuming that she does not have any children older than age 16). For my main analysis, I restrict to the first child listed in the household roster as a proxy for the mother's first live birth.<sup>11</sup> However, there is the possibility that there may be older children who are not included in the dataset. Introducing measurement error through this channel will attenuate my estimates towards zero, since I may be mistakenly classifying children as treated who were not eligible for the transfer.

The DHS data also comes from a household survey and provides information on current school and preschool enrollment for individuals ages 2 to 14. For younger children, this represents preschool enrollment, while for older children this represents enrollment in primary and secondary schools. Additionally, the DHS provides demographic data, a net

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<sup>11</sup>For the main analysis, I use only the first child in case this program has impacts on birth spacing or fertility decisions, which could endogenously affect the second child's outcomes directly. Results are robust to including both the first and second child.

asset index for the household, and geographical location. While the DHS provides more detailed demographic information than the ASER, it does not have the same coverage for education-related outcomes. The DHS only measures the extensive margin of enrollment and does not measure the intensive margin of learning.

Like the ASER, the DHS does not ask a mother whether she received maternity benefits. Once again, this means that I cannot compare women who actually received the transfer with those who did not. Instead, I calculate intent-to-treat (ITT) estimates and compare women who met the eligibility requirements for the transfers in treated districts with the equivalent population of women who would have met the eligibility requirements in the untreated districts.

To create the DHS sample, I restrict attention to children whose mothers were at least 19 years old when their child was born. Since the DHS also provides information on birth order and timing, I include only the first child (or the first and second child, for robustness). While this will include some women and children who did not receive the transfers, this provides the full group of individuals who were eligible to receive the transfer. Children born in or after 2011 in the treated districts were eligible for the program, so I assign treatment status based on year of birth and district of current residence. For the DHS dataset, because the survey was conducted over three years, not all of the children in any given birth cohort are the same age at the time of the survey. For example, of the children born in 2016, those surveyed in 2019 are three years old, those surveyed in 2020 are four years old, and those surveyed in 2021 are five years old in the dataset.

Table A1 provides descriptive statistics for the main sample of first-born children, with Panel A describing the sample from ASER and Panel B describing the sample from the DHS. The ages of the children from the two data sources are similar, but those in the DHS are less likely to be enrolled in school.<sup>12</sup>

For additional outcomes, I use data from the IHDS. The IHDS is a nationally-representative panel survey with two waves, one in 2005 and one in 2011. The IHDS data contains information on household income, year of birth for young children in the household, whether anyone in the household was receiving maternity benefits, and whether anyone in the household received income from NREGA. Additionally, for some women in the household (all women in the 2005 wave and a selected sample of women in the 2011 wave), the dataset provides information on whether the women were working at the time of the survey.

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<sup>12</sup>Note that the wealth indices are not directly comparable across the two datasets, since the indicators for wealthy households are calculated using two different metrics.

## 4 Methodology

To estimate the effects of the conditional maternity benefits cash transfer on children's education outcomes, I use several sources of variation. First, there is variation based on the district in which the child was born, because only 52 districts received the pilot program. Second, there is variation based on the timing of the child's birth, since children who were born before the policy were not eligible for the program. Variation in birth timing allows for comparisons both within and across survey waves, since within a survey wave, mothers of younger children would have been eligible for the program, but mothers of older children would not.<sup>13</sup> Finally, there is variation based on the birth order of the child, since only the first and second live births were eligible to receive the transfer.<sup>14</sup> My main empirical strategy will exploit the first two sources of variation, and the third will be used to investigate spillovers to siblings.

To make use of these sources of variation, I use an event-study difference-in-differences specification. My main specification is run at the individual level, and I also conduct the analysis at the district level for robustness. Note that because I do not observe receipt of the transfers, all estimates are intent-to-treat estimates, providing a treatment effect based on the eligibility of the child and mother to receive the transfer.

In my main empirical specification, I use the variation in a child's district of residence and year of birth to capture effects at the individual level. I use a single wave of data and compare cohorts within that wave. The empirical specification is as follows:

$$Y_{idt} = \alpha + \sum_{t \neq 2010} \beta_t Treated_d \times Cohort_t + \delta_d + \mu_t + \epsilon_{idt} \quad (1)$$

where  $Y_{idt}$  is an indicator for whether child  $i$  in district  $d$  with birth year  $t$  was ever enrolled in school (or other education-related outcomes),  $Treated_d$  is an indicator for districts that received the pilot IGMSY program,  $Cohort_t$  is an indicator for the child's birth cohort (with children born in 2011 or later being treated by the policy), and  $\delta_d$  and  $\mu_t$  are district and birth year fixed effects, respectively.  $t$  indexes time relative to the implementation of the policy. As described above, I restrict the sample to mothers who were eligible to receive the policy. The specification in equation (1) above compares individuals of different ages who were born after the policy in the treated districts with individuals who were born in untreated districts and individuals who were born before

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<sup>13</sup>However, comparisons across survey waves in the ASER are not used in the main analysis. This is because information is not always consistently collected across all age groups (information for some 3-4-year-olds is missing in many of the early survey waves), and test scores are measured differently in different survey years. Because of this, variation across survey waves is only used for the district-level analysis.

<sup>14</sup>There is also variation based on whether the mother was old enough at the time of the child's birth to be eligible for the program, since mothers were required to be at least 19 years of age in order to receive the transfer. However, due to concerns over enforcement of this condition and the small sample of mothers under age 19, I do not use this variation for my main analysis.

the program. This specification assumes that the trends among cohorts are parallel across treated and untreated districts. The  $\beta_t$  terms will provide the effects of the policy by cohort.

For outcomes, I focus on school (or preschool) enrollment, math competency, reading competency, and whether tuition is paid. I use the 2018 wave of the ASER, which includes information on enrollment for children ages 3-16. I also estimate impacts on any reading or math competency, which is available for children ages 5-16, and on paying any tuition, which is available for children ages 5-16 and could act as a proxy for attending a private school. I use DHS data as an additional measure of school enrollment, as well as showing that the results are robust to probit or logit specifications.

Figure 1 shows the individual-level trends in any school enrollment, current enrollment (at the time of the survey), reading competency, and math competency by age for the ASER data, with children ages 8 or older being ineligible for the treatment. For the older cohorts, the enrollment patterns between the two districts appear to be quite similar. Note that for math competency and reading competency, the youngest age shown is the cohort of five-year-olds, because there is no data available for younger children. Figure B2 shows the trends for enrollment in the DHS data. In all cases, the pre-trends appear to be parallel, indicating that the difference-in-differences and event study approaches outlined above are likely valid.<sup>15</sup> These figures show that school enrollment is quite high for older children, since schooling in India becomes compulsory starting at age six. This means that the scope for the IGMSY program to impact children ages six or older is mechanically limited. To deal with the differences between baseline enrollment across age cohorts, I also use an alternative specification at the district level.

This second specification relies on variation across districts and across survey waves, allowing for comparisons within a single age group over time. I look at changes in district-level outcomes, focusing on enrollment.<sup>16</sup> For this approach, I aggregate the total number of enrolled children by age group in each district, and I estimate the effects of the program on district-level enrollment separately for each age group. This allows me to compare outcomes for the youngest children, those who are pre-school age (ages three or four), across survey waves. This is only possible at the district level, since three- and four-year-olds who were not enrolled in school were not included in the earlier waves of the ASER. I estimate the following:

$$Y_{dt} = \alpha + \sum_{t \neq -1} \beta_t Treated_d \times Cohort_t + \delta_d + \mu_t + \epsilon_{dt} \quad (2)$$

In this equation, the interpretation of the variables is similar to equation (1) above.

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<sup>15</sup>These figures plot averages for the two sets of districts separately, but I also plot the differences between the averages, allowing a direct test for differences in pre-trends. This is done in Figure B3 and Figure B4.

<sup>16</sup>I do not examine changes in math and reading competency over time for a single age group, since the measurements of those outcomes changed across survey waves.

Everything is aggregated to the district level, and  $Y_{dt}$  is no longer an indicator, but instead a continuous variable for the total number of children enrolled in school at the district level.  $t$  indexes the timing of the survey relative to the start of the program, omitting the survey wave that contains the children born right before the start of the program (which survey wave this is will vary based on the age group). For this analysis, I again restrict my sample to children of mothers who were eligible for the program. Thus, this district-level analysis estimates the impact on the total enrollment of children in the district who are eligible (or would have been eligible) for the program. I include all available waves of the ASER data in the analysis.

## 5 Results

This section discusses the impact of the IGMSY program on children's early education. First, I discuss the impacts on school and preschool enrollment, showing that only the youngest cohort of children was impacted by the program. Then, I show that there is no impact on math and reading competency for older children. I present evidence that this is likely due to a fadeout of the program's effects over time.

### 5.1 Impacts on Enrollment

I first estimate the impact of the IGMSY program on enrollment, the extensive margin of education. To capture the full picture of how the program impacted enrollment across cohorts, age groups, and years, I first use the ASER and DHS data and variation in cohorts and program rollout to estimate the individual-level effects on enrollment. Next, I use district-level variation to compare children of the same age group over time in the ASER data.

First, I compare school and preschool enrollment of children born between 2006-2010 (before the program) and 2011-2015 (after the program) in districts that did and did not receive the pilot, using the 2018 wave of the ASER data. Figure 2 shows the impacts on ever being enrolled in school, following the event study specification in equation (1). Year of birth is on the x-axis, and I compare cohorts born before and after the policy was implemented.<sup>17</sup> The dashed line represents the beginning of the policy, and the points to the left of the dashed line show the coefficient estimates for earlier cohorts. Reassuringly, these coefficients are all extremely small and statistically insignificant, providing credibility to the assumption of parallel trends between the treated and untreated districts. To the right, there is no large or statistically significant impact on cohorts born in 2011-2014, but there is a large increase in the probability of ever being enrolled in school for children

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<sup>17</sup>Note that I observe all children at the same time, meaning that cohorts born earlier are older at the time of the survey. For example, individuals born in 2008 will be 10 years old at the time of observation, and individuals born in 2014 will be four years old at the time of observation.

born in 2015. These are the youngest children included in the ASER data, three years of age at the time of the survey, so they would be enrolled in preschool. This pattern suggests that older, primary-school-age children do not increase their enrollment in response to the program, but younger, preschool-age children do increase their enrollment by around 9 percentage points.<sup>18</sup>

Figure 2 also shows the impacts on current school enrollment, defined as being enrolled in school at the time of the survey. The results are similar as any enrollment, except that in this case the pre-trends reveal some statistically significant differences between the treated and untreated groups in the pre-period relative to the omitted group. Again, the results in this figure suggest that maternity benefits increase school enrollment for children whose mothers received the benefits, but only for preschool enrollment for the youngest cohort. These children experience a large positive effect on school enrollment relative to their counterparts in the untreated districts. Table 1 presents these estimates.<sup>19</sup>

To check the robustness of these results, I use data from the DHS survey, which was collected in the years after the ASER, to show that the effects are similar. The third and fourth panels of Figure 2 and Table 1 show the estimates for the impacts on enrollment using the DHS data. The third panel of Figure 2 shows the estimates without age fixed effects, and the fourth panel shows the estimates with age fixed effects. The DHS results show a very similar pattern and magnitude as the estimates using data from the ASER, though the effect on the youngest cohort is only statistically significant when age fixed effects are included in the regression.

Because the 2018 ASER and the DHS survey were conducted in different years, this can shed some light into the mechanisms behind why only the youngest children benefit from this program. Interestingly, the cohort of children born in 2015 (the three-year-olds in the ASER data and four-year-olds in the DHS data) do not appear to experience effects on enrollment beyond age three. In the ASER data, there was a positive and statistically significant effect on enrollment for children age three, who were born in 2015. However, when examining the impact on this same cohort (children born in 2015) a year later in the DHS data, there is no effect. This suggests that preschool enrollment was higher in 2018 for the cohort born in 2015. However, their enrollment was not longer increased relative to the control in 2019 at age four. Thus, it appears that the policy is only effective in increasing enrollment for very young children.

As another test for whether impacts on enrollment are specific to three-year-olds, I

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<sup>18</sup>Randomization inference for this point estimate is found in Figure B5.

<sup>19</sup>Results also hold when I use a logistic regression model (see Table B1), or a probit regression model (see Table B2). For robustness, I also estimate the basic two-by-two difference-in-differences in Table B3. The point estimates are much smaller because the coefficient takes the average across all cohorts. Additionally, I estimate the event study with the entire pre-period grouped into a single reference period (see Table B4). These effects are quite similar to the main estimates. The control group means in this table (and all tables in the paper) include children in the untreated districts across all age cohorts. The means are generally lower for younger children and higher for older children, as seen in Figure 1.

estimate a district-level specification, which allows for comparison of the same age group across waves in the ASER data. I present estimates of equation (2) for the impacts on school enrollment at the district level. These estimates use survey-wave timing and district variation in program rollout to estimate district-level impacts on each age group separately. Figure 3 shows the impacts on enrollment at the district level for three-year-olds, four-year-olds, and five-year-olds. There is an increase the number of three-year-olds enrolled, while there is no increase in enrollment at the district level for older children. Table 2 present the point estimates, and in enrollment for three-year-olds in 2018 increased by roughly 12% of the control group mean.<sup>20</sup> There are no significant effects on four- or five-year-olds.

These results show that exposure to the IGMSY program increases school enrollment for three-year-olds.<sup>21</sup> However, by the time these children turn four or five, their enrollment looks very similar to that of their peers who did not receive the program. There are a few potential explanations for this. One is that the wealth and health impacts of the transfer only persist for the first few years of the child’s life. In other words, when children are young, the family has not yet spent the entirety of the transfer, so they are able to invest more in children’s education. However, as the child gets older, the money has already been spent, and the family’s wealth returns to its former equilibrium. This could also be the case if young children are initially healthier as a result of the transfer or its conditions, which would increase the likelihood of the child attending school around the time when the family received the transfer. The positive health effects might fade over time, leading the education effects to also fade.

A second possibility is that there is little margin to improve enrollment for older cohorts. Older children in both the treated and control districts have high levels of school enrollment, close to 100%. As a result, there is little margin to increase schooling among older children. However, for younger children who are preschool age, school enrollment is not quite as high, meaning that there is more margin for the program to have an effect. Thus, treatment effects would mechanically fade out for older children as the potential to impact the outcome diminished. In the following section, I examine the effects of the IGMSY program on the intensive margin of schooling, where there is more room to shift outcomes for older children.<sup>22</sup>

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<sup>20</sup>The magnitude of the point estimates is small because this is a survey and does not include all enrolled children in the district, just those sampled. The percentage change is similar to the increase in the probability of enrollment at the individual level.

<sup>21</sup>It is worth noting that these effects did were not achieved for the cohort born right after the program started. This is likely because take-up was low near the beginning of the pilot program [von Haaren and Klonner \(2021\)](#).

<sup>22</sup>An additional factor is the possibility that program only began to have an impact several years after implementation. This would be the case, for example, if take-up was very low for the first couple of years of the program (the 6- and 7-year-olds in my data) but increased over time (for the younger cohorts, the 3- and 4-year-olds). This is unlikely to be the driving force behind the pattern of results, because the impacts on enrollment are visible for the 2015 birth cohort when they are three years old but are no

## 5.2 Impacts on Math & Reading Competency

From the results presented above, it appears that the impacts of the IGMSY program on enrollment, the extensive margin of schooling, are strongest for young children. However, the program could also impact the intensive margin of schooling. To determine whether the effects of the transfer completely disappear for older children, I also estimate the impacts on reading and math competency. If there are differences in reading and math competency across the children who were treated by the program and those who were not, we can deduce that not all of the impacts of the transfer have disappeared. This would provide suggestive evidence that enrollment for older children does not respond to the program because there is not enough margin for a response. If, instead, there are no effects on math or reading competency, this aligns more clearly with the story that the impacts of the program fade out as the child ages.<sup>23</sup>

I use ASER data to determine whether students' learning increased as a result of the transfer. Using the categorical outcomes that the ASER provides for math and reading (which are available for children ages 5+), I create an indicator for whether a child has any reading or math competency. I then estimate equation (2) with reading and math competency as outcomes. Figure 4 shows the estimates for the effects on reading competency and math competency. These results suggest that the treatment does not improve reading and math competency. Note, however, that due to the large size of the standard errors, it is not possible to rule out relatively large effects, either positive or negative. Table 1 presents the coefficient estimates of equation (2) for reading and math competency.

The lack of evidence of a positive effect on learning outcomes for older children aligns with a scenario where the effects of the program fade out over time. Enrollment only increases for very young children (three years old), while reading and math scores are only reported for older children (starting at five years old). If the effects of the program have faded out over the first five years of the child's life, learning outcomes may not change.<sup>24</sup> This suggests that there are no persistent effects of the maternity benefits cash transfer on education, likely because the cash has run out by the time children reach five years of age or because any health benefits which would increase educational outcomes have faded relative to the control. However, I cannot rule out that there could still be other positive persistent benefits of the program that are not directly tied to enrollment or competency scores.<sup>25</sup>

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longer visible when that same cohort is four years old. This indicates that the dynamics exist across the lifespan of the child, not just across cohorts.

<sup>23</sup>This provides suggestive evidence only. With this approach, it is not possible to conclusively rule out either of the explanations for the lack of effects on enrollment for older children.

<sup>24</sup>Similarly, there are no effects on the probability of paying tuition, which could proxy for parents' investments in children's schooling. Trends and differences in means are shown in Figure B6 and Figure B7, respectively. Figure B8 presents the event study.

<sup>25</sup>For example, children's perceptions of, or satisfaction with, their own learning could be impacted by

Overall, across specifications and datasets, it appears that the maternity benefits had an impact on the extensive-margin outcome of enrollment for the youngest cohorts of children and did not have an effect on intensive-margin outcomes, such as math and reading competency. Once children reach age five, I cannot identify any effects of this maternity benefits cash transfer program on education. This suggests that the effects of maternity benefits cash transfers on education are not persistent over time and may only exist for the youngest cohort.

This is not very surprising, given that many papers studying preschool enrollment find convergence in test scores during the primary school years. Evidence on whether preschool positively affects children's outcomes is mixed, increasing school enrollment and test scores in some cases, and having no effect in others. Additionally, positive effects of preschool attendance often fade before or during adolescence, sometimes resurfacing later in adult life ([Cascio and Schanzenbach, 2013](#), [Duncan and Magnuson, 2013](#), [Goodman and Sianesi, 2005](#)). [Ansari \(2018\)](#) and [Yoshikawa et al. \(2016\)](#) find that children who attended preschool in the US tend to have higher test scores than their peers, but that these children's outcomes converge over time. This is similar to what I find, though in my setting the outcomes for these children converge quite quickly, within just a couple of years.

### 5.3 Sibling Spillovers

Since only first and second births were eligible to receive the IGMSY program, spillover effects to siblings can be estimated by looking at the impacts on third and fourth children. There are two caveats to the sibling analysis. First, if receipt of the program influences whether households have a third or fourth child, then the effects on schooling cannot be causally interpreted. I run an event study regression with the birth parity of the child as the outcome and find no differences between districts that received the program and those that did not, suggesting that the transfer amount was not large enough to impact fertility decisions on the extensive margin for third and fourth born children (see Figure B9 and Table 3). Second, if implementation of the program was imperfect and some third- and fourth-born children did receive the transfers, then the estimates of the effects on third- and fourth-born children would be a combination of direct effects and spillover effects.

Figure 5 and Table 3 present the impacts on education for third and fourth children. The size of the point estimates is similar to the direct effects on firstborn children, suggesting that these estimates may be capturing a combination of direct effects (due to imperfect implementation) and spillover effects from siblings. At a minimum, these results show that the program did not have a negative impact on the younger siblings of children eligible for the program.

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the program. I do not have a way to test this directly.

## 5.4 Take-up & Employment

To understand more clearly the impacts of the IGMSY program on preschool enrollment, it helps to have a clear picture of the program's take-up and how the program impacted women's employment. Anecdotally, take-up was much lower in 2011 than in the following years ([von Haaren and Klonner, 2021](#)). However, the ASER and DHS do not record whether women were receiving maternity benefits at the time of their child's birth. To get an estimate of take-up rates, I use data from the IHDS, which has two waves, collected in 2005 and 2011. This survey provides information on whether there are any young children in the household (born in the year of the survey or the year prior) and whether a woman in the household was receiving maternity benefits at the time of the survey. I restrict the sample to households with a child born in the year of or year prior to a survey, and I estimate the impacts of being in an early roll-out district on the probability that a woman in the household reports receiving maternity benefits.<sup>26</sup>

When comparing households in districts that did and did not receive the pilot program in the 2011 data, Table 4 column 1 shows that being in a treated district is associated with an 8.3 percentage point increase in the probability of receiving maternity benefits. Column 3 shows the difference-in-differences comparison across 2011 and 2005; the point estimate remains similar, but the coefficient is no longer statistically significant. This small increase in take-up of maternity benefits in 2011 is consistent with the anecdotal evidence that the program did not take off widely during its first year of implementation.

Maternity benefits may also affect women's labor force participation, either by acting as substitutes or complements for women returning to the labor force. For example, women could use the money provided by the maternity benefits to pay for childcare, allowing them to return to work sooner. Conversely, the cash transfer might alleviate the pressure to work immediately to maintain household income. I use the IHDS to look at whether the program impacted the probability that a woman is currently working. In columns 2 and 4 of Table 4, I show the correlation between living in a district that received the pilot program and the probability of working (this could include any type of employment, such as self employment). Column 2 shows the correlation using 2011 data only, while column 4 uses the difference-in-differences estimate between treated and untreated districts in 2005 and 2011. This table shows suggestive evidence of an increase in the probability that a woman is working associated with the IGMSY program.<sup>27</sup>

This suggests that the maternity benefits provided by the IGMSY program may be

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<sup>26</sup>Note that the variable for maternity benefits could include any type of maternity benefits program, not just the IGMSY program.

<sup>27</sup>The relevant sample of women is defined differently in the 2005 and 2011 IHDS. For 2011, only one woman per household (the mother of the child) was surveyed. For 2005, all women in the household were surveyed. So, the control group mean in column 4 is higher than in column 2, since the one in column 4 is just an indicator for any woman in the household working. Note that these numbers also align well with the reported overall female labor force participation in India in 2005 and 2011, which was 35% and 28%, respectively ([World Bank, 2025](#)).

complementary with women working outside the household. If this is the case, this program is likely not the most effective way to encourage women not to work immediately before and after giving birth.

## 6 Heterogeneity

Since the maternity benefits cash transfer was conditional on health-related outcomes, which could also improve educational outcomes, I conduct heterogeneity analyses to try to separate, at least in part, the two channels of improvements in health and increases in income. This section explores heterogeneity by wealth, caste, gender, and father's education. The idea is that, if households facing tighter income constraints are more affected by the program, it is likely that at least some of the impacts are coming through the channel of increased income; if households that are less income constrained are also affected by the program, it is likely that some of the impacts are also coming through the channel of improved health.

### 6.1 Heterogeneity by Wealth

Since the size of the cash transfer relative to the household's income levels will vary across the wealth distribution, it is likely that the impact of these cash transfers on different parts of the wealth distribution will not be constant. To identify potential heterogeneity, I split the ASER sample using a household asset index as a proxy for household wealth. The index consists of six indicator variables: whether a household has a toilet in the house, whether the household has an electricity connection, whether the household has a television, whether the household has newspapers, whether the household has a mobile phone, and whether the house is made of pucca (burnt bricks, stones, cement, timber, corrugated iron roof, tile roof, etc.).<sup>28</sup> To combine the indicators, I create an index following [Kling et al. \(2007\)](#) across the six variables listed above. I compare individuals above and below the 25th percentile of the wealth index.

From Figure 6, it is apparent that the impact on enrollment for young children is stronger for households below the 25th percentile of the distribution. On the other hand, the effect is weaker for households that have a wealth index above the 25th percentile. The differences in impacts between poorer and richer households imply that the poorer households are the ones that experience the largest increase in enrollment, which fits with the theory that poorer households fail to invest in their children because of a resource constraint, and the transfer helps to relax that constraint. It is also apparent that children from poorer households tend to have lower baseline enrollment, meaning that there is more

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<sup>28</sup>Houses made of pucca are the higher-quality houses.

room for the intervention to have an impact for these children.<sup>29</sup> Table A2 shows these results, as well as estimates using the DHS wealth index.

It is worth noting that this wealth index is measured at the time of the survey, meaning that if the treatment has a large effect on values of the wealth index for the household (specifically pushing households above or below the 25th percentile threshold), the distribution of wealth is potentially endogenous. Since the transfers were worth approximately 40 days of minimum wage pay, it is likely not the case that the transfers caused large changes in the wealth distribution. However, if households were moved across the threshold into the poorer or wealthier groups, the estimates could be biased, so these findings should be taken with a grain of salt.

I also find suggestive evidence that take-up of the program may be higher for poorer households. Using data from the IHDS, I compare receipt of maternity benefits across individuals in districts that were and were not included in the IGMSY program. In Table 5, I show the correlation between maternity benefits and treatment by whether households were below or above the median income. For households with below-median income, the point estimate on take-up of maternity benefits is larger and is statistically significant, while take-up for households with above-median income is not statistically significant. This could be one of the reasons why households with different levels of income or wealth respond differently to the program.

## 6.2 Heterogeneity by Caste

For the DHS data, I can split the results along caste lines, which are exogenous to the treatment status since caste is set before the receipt of the transfers. The DHS does not provide detailed data on caste, but it does contain information on whether individuals come from scheduled tribes, scheduled castes, other backward castes, or none of the above (disadvantaged) categories, listed in order from the most to the least disadvantaged from a socio-economic perspective. Figure 7 shows the impacts on enrollment by caste group, with scheduled tribes, scheduled castes, and other backward castes in the first panel and other groups in the second panel. When the sample is split in this way, there is not enough power to capture any effects on either group. Table A3 also shows these results.

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<sup>29</sup>Interestingly, when the same analysis is conducted for the DHS data using the net asset index provided and splitting at the median value, this produces the opposite results shown in Figure B10, where very young children in the wealthier households seem more likely to be currently enrolled in school, while there are no effects for households that are below the median value. This remains true when using the 25th percentile to line up with the ASER estimates. The difference could be due to the fact that the DHS net asset index is constructed differently than the ASER asset index or to the fact that the DHS includes some urban households.

### 6.3 Heterogeneity by Gender

Girls and boys are often valued differently and treated differently in Indian culture ([Congdon Fors and Lindskog, 2023](#), [Singh et al., 2021](#)). As a result, it is potentially interesting to split the sample between boys and girls, since mothers could choose to invest this maternity benefits cash transfer differently in their boy children and girl children. Once again, gender is exogenous to the treatment. When splitting the ASER sample between boys and girls, the effects on enrollment for girls are similar to those for boys (see Figure 8). Table A4 shows the coefficient estimates associated with these results.<sup>30</sup> This suggests that mothers who receive the transfer are not investing differentially in the education of their male vs. female children. Rather, the transfer improves enrollment for girls and boys alike.

### 6.4 Heterogeneity by Father's Education

Parents' education levels may also determine how sensitive children are to the impacts of the program. Additionally, education level of fathers is likely to be correlated with the wealth levels of the household.<sup>31</sup> Figure 9 plots the event study estimates for the ASER sample of children whose fathers attended any school in the first panel and those whose fathers did not attend school in the second panel. I find that the effects are much larger and more statistically significant if the father did not go to school, indicating that children whose father was not educated are more likely to benefit from spillovers to education from the IGMSY program. Notably, among the youngest cohort, the point estimate is nearly four times as large for the children whose fathers did not go to school relative to those whose fathers did go to school. Table A4 shows these results.

This suggests, again, that income constraints may be more binding among households with lower-education fathers. If this is the case, the maternity benefits cash transfer reduces those constraints and may allow parents to invest more in their children. However, it is also important to note that this does not rule out the health channel, since the transfer was conditional on health-related outcomes. This channel could still play a role. What this does indicate is that at least part of the effect seems to be coming through the income channel.

### 6.5 Heterogeneity by Experience of Negative Shocks

Another way that this maternity benefits cash transfer program could impact children's education outcomes is through acting as a form of insurance against risk. This would be

<sup>30</sup>When splitting the sample for the estimates for the impact on the other education-related outcomes, there is not a large enough sample to detect an effect or a difference in effect size between boys and girls.

<sup>31</sup>Education level of the father has the additional benefit that is less likely than the wealth index to be impacted by the receipt of the transfer, meaning that it is not endogenous to the policy.

the case if households faced difficulties smoothing consumption and would sacrifice spending on education when faced with a negative shock. To test this, I look at heterogeneity in effects among children who did and did not experience a year with low rainfall in their first three years of life. Since agriculture is so important for income in this setting, low rainfall shocks reduce household income.

Using rainfall data from the University of Delaware, I fit the data for each district to a gamma distribution, then define a low rainfall year as a year with rainfall below the 15th percentile of that district-specific distribution, following [Burke et al. \(2015\)](#). I find that any year with low rainfall during the first three years of life results in negative impacts on educational outcomes, particularly reading and math (see Table B5).

When I split the sample between individuals who experienced a low rainfall shock in their first three years of life and those who did not, (see Figure B11 and Table B6), I do not find statistically significant differences between the two groups. This suggests that the transfers are not impacting education by acting as a form of insurance.

## 7 Robustness

While the main results show the effects of the IGMSY program on firstborn children, I also estimate the results for the combined sample of first- and second-born children. Figure A1 and in Table B7 present the estimates. These results show that the effects of the program are not exclusive to first-born children and are robust to including their siblings who were also eligible for the program.

Additionally, the program was rolled out fully to the whole country of India in 2017. Using data from the later-treated districts from 2018 and 2022, I compare cohorts of younger and older children, born before and after the program was rolled out, across survey waves. Figure A2 and Table B8 present the impacts on enrollment, reading competency, and math competency. The impacts of the full rollout are similar to the impacts of the initial pilot program, though the point estimates on enrollment for very young children are smaller. Additionally, the pre-trends for the reading and math competency scores are not parallel, so effects on reading and math cannot be interpreted.<sup>32</sup>

To further determine whether the later-life outcomes of these children are affected by the program, I also look at individual-level enrollment for children who are five, six, and seven years old and compare districts who did and did not receive the pilot program across time. These estimates are found in Figure A3 and Table B9. I find that, for five- and seven-year-olds, there are no statistically significant impacts on enrollment for these age groups. There is a statistically significant increase in enrollment for the 6-year-olds, but the point estimate is quite small, less than one percentage point. This supports the

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<sup>32</sup>This is likely due to the fact that the reading and math competency scores are not designed to be compared across survey waves.

idea that the IGMSY program, while it improved enrollment for very young children, did not have large impacts on enrollment for older children.

There might be concerns that other programs rolled out around the same time, such as the NREGA program, might be correlated with the IGMSY program. Using data from the IHDS, I test whether being in a district that received IGMSY predicts receiving income from NREGA in 2011. Table 5 shows these results. I do not find that receipt of NREGA is correlated with the maternity benefits program for either households above or below the median level of income.

## 8 Conclusion

Using a difference-in-differences approach with survey data from the ASER and the DHS, this paper finds that the IGMSY program in India, which provided cash transfers to mothers for their first and second births, did in fact increase enrollment among young children who are of preschool age. However, these enrollment effects do not persist for older children. Additionally, there are no detectable increases in test scores for primary school age children, indicating that learning on the intensive margin is not increasing. This is likely due to the fact that the impacts of the cash transfer slowly fade out as time passes.

The children who are more likely to go to school as a result of the program are those who are poorer and whose fathers did not go to school. The temporary increase in income relaxed constraints that these families faced. This suggests that the program increased education most for the most disadvantaged children, having a smaller effect on those who already were better off relative to their peers. Additionally, girls are not more (or less) likely than boys to be positively impacted.

This research is important because it investigates the long-term outcomes of a large-scale maternity benefits cash transfers program. While the effects of this program did not persist indefinitely, there is evidence that enrollment for preschool-age children was improved as a result of their mothers receiving the cash transfer around the time of their birth. Future research could use information on actual receipt of the maternity benefits to determine the effects on the actual treated population.

One limitation of this project is that the only education-related outcome available for preschool-age children is enrollment, and there is no information on learning. In future research, it would be interesting to investigate the impacts of a maternity benefits program on cognitive or learning-related outcomes for younger children, such as their knowledge of math, reading, or other concepts. This could provide a fuller picture of the impacts of cash transfer maternity benefits on education for young children.

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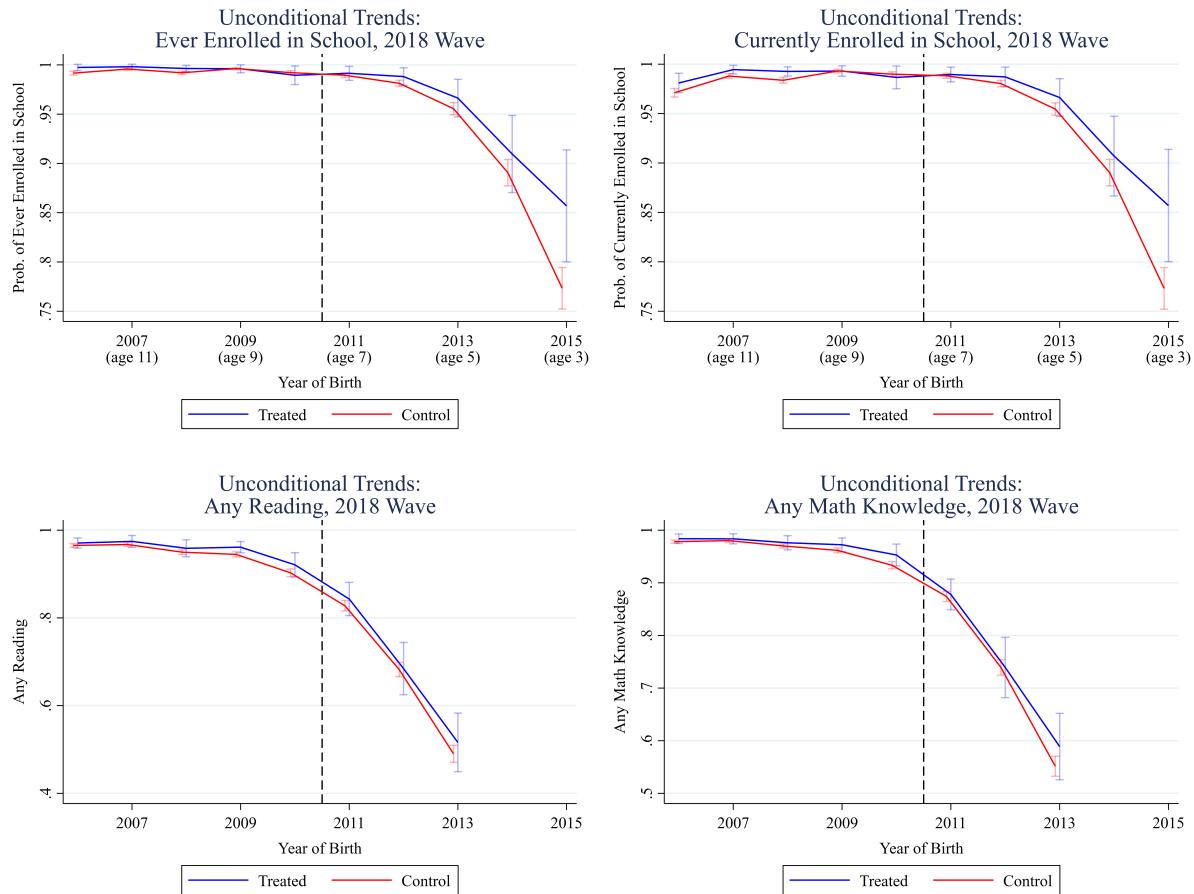
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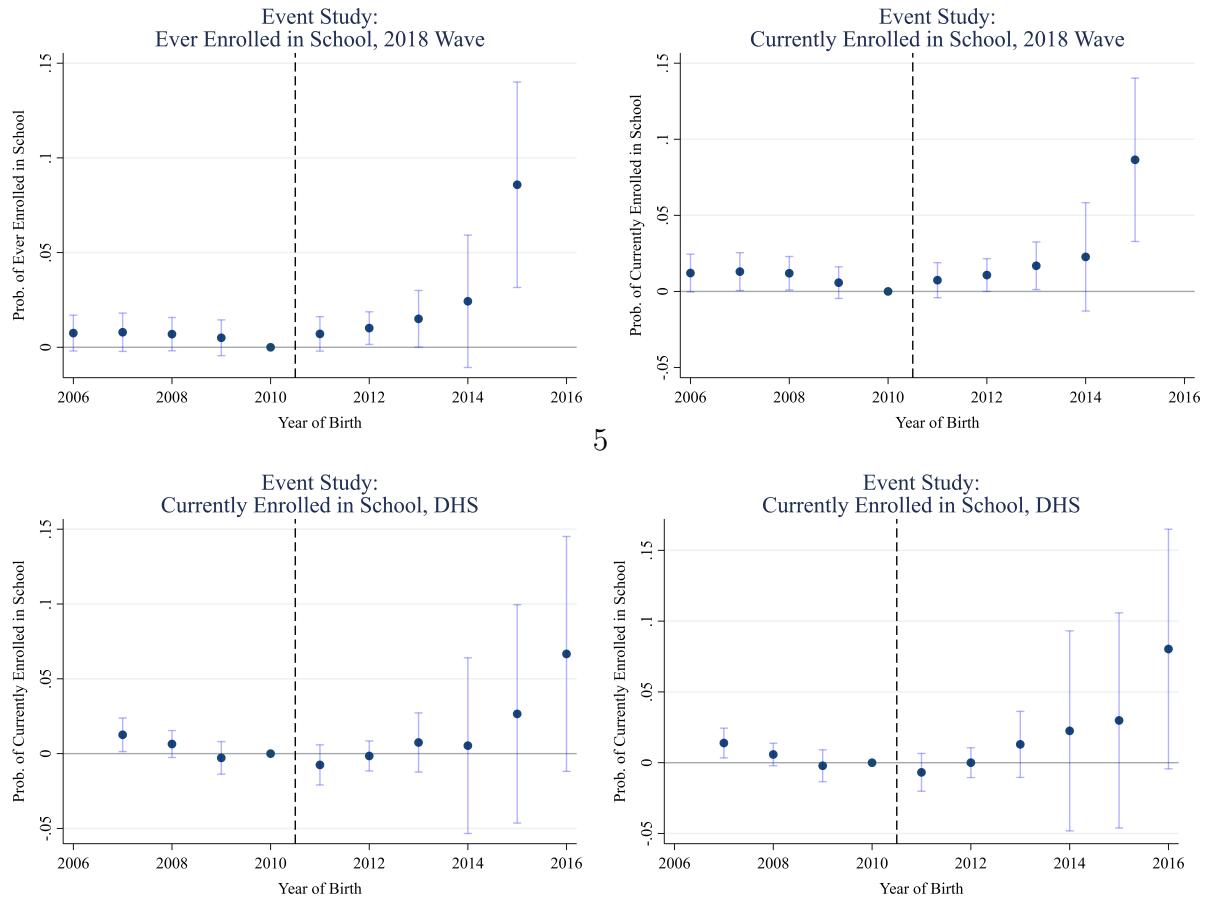
# Exhibits

Figure 1. Trends in ASER Outcomes by District Type



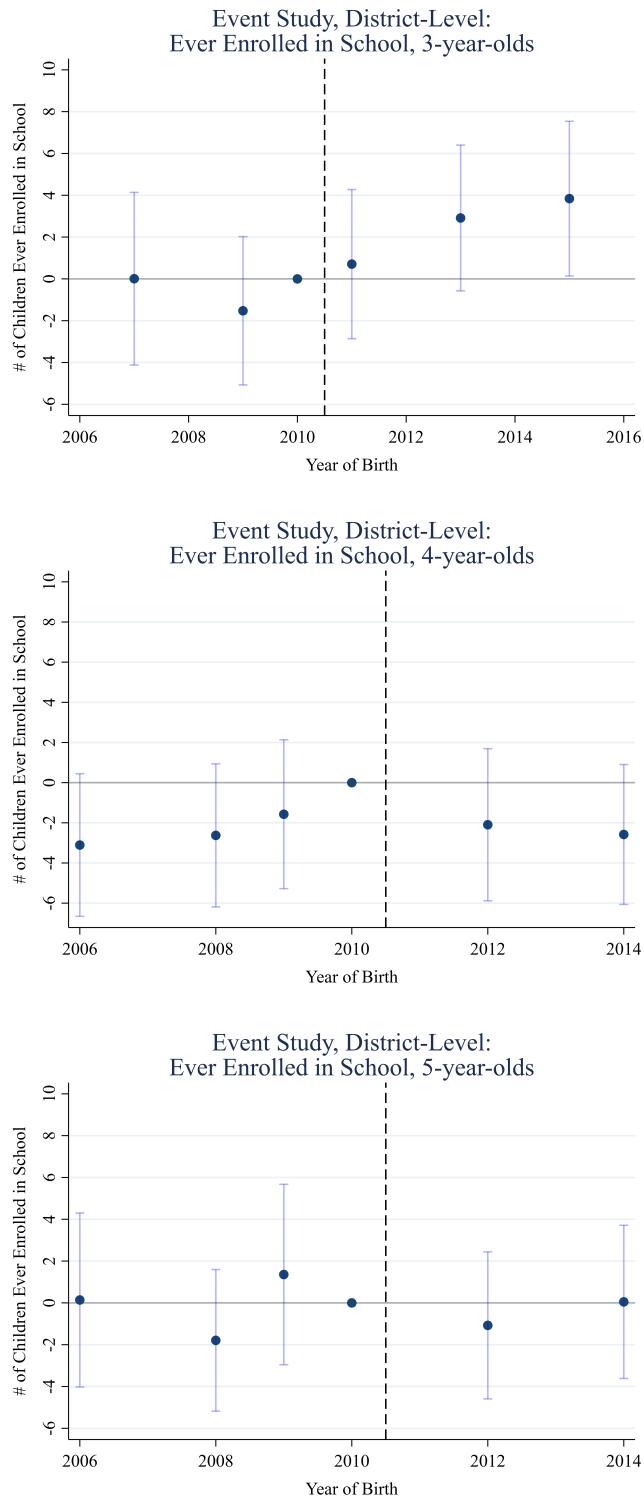
Notes: This figure shows the raw trends for probability of ever being enrolled in school, probability of currently being enrolled in school, any math competency, and any reading competency by birth year for the first child, separately for the treated and control districts. Standard errors are clustered at the district level. Data comes from the 2018 ASER. Confidence intervals are at the 95% level.

Figure 2. Impacts of Program on Individual-Level Enrollment



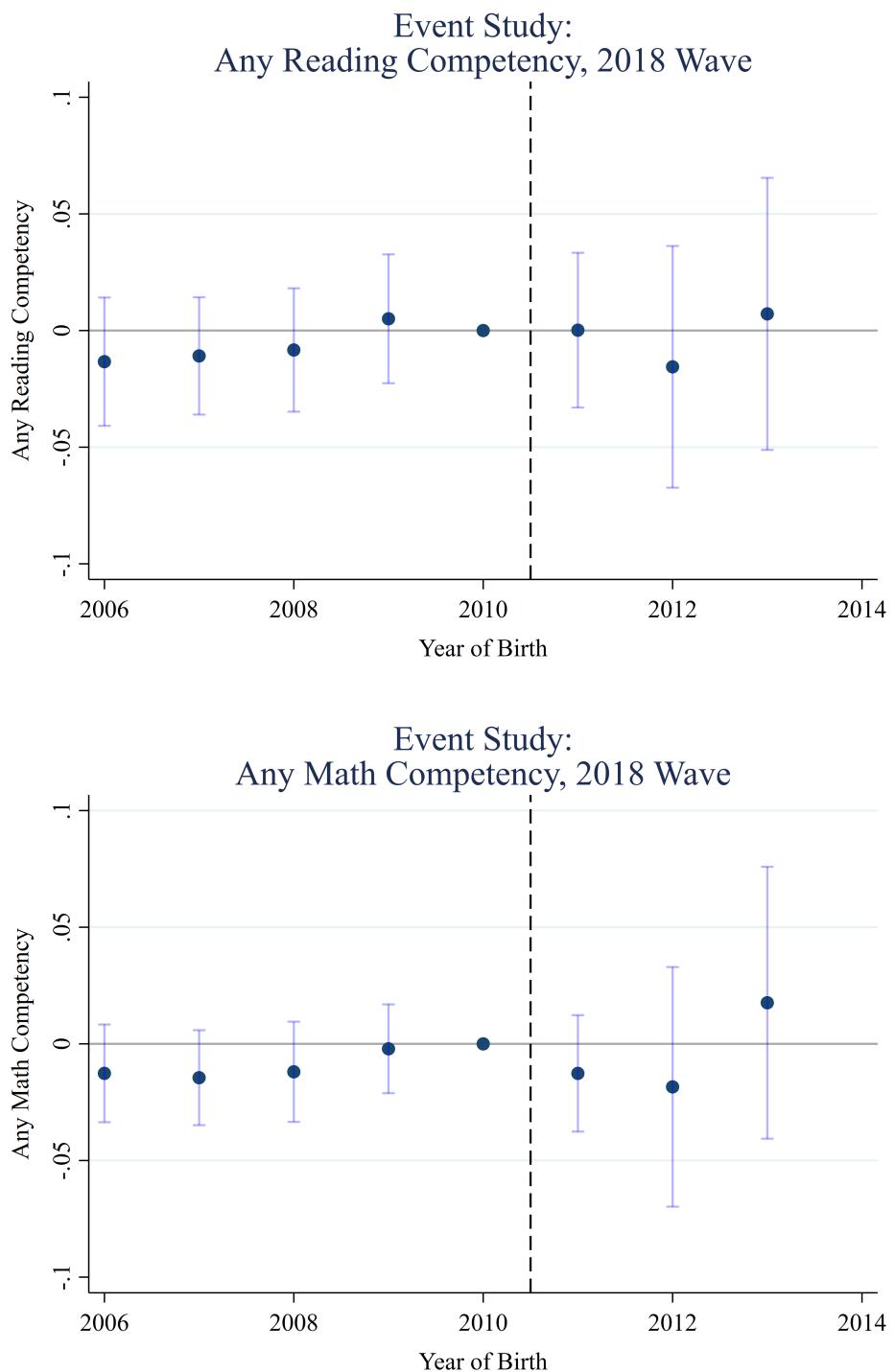
Notes: This figure shows the event study impacts on probability of being enrolled in school and probability of being currently enrolled in school by birth year for the first child. District fixed effects are included, and standard errors are clustered at the district level. Data for the first two panels comes from the 2018 ASER. Data from the third and fourth panels comes from the 2019-2021 DHS. The third panel does not include age fixed effects, and the fourth panel does include age fixed effects. Confidence intervals are at the 95% level.

Figure 3. Impacts of Program on District-Level Enrollment



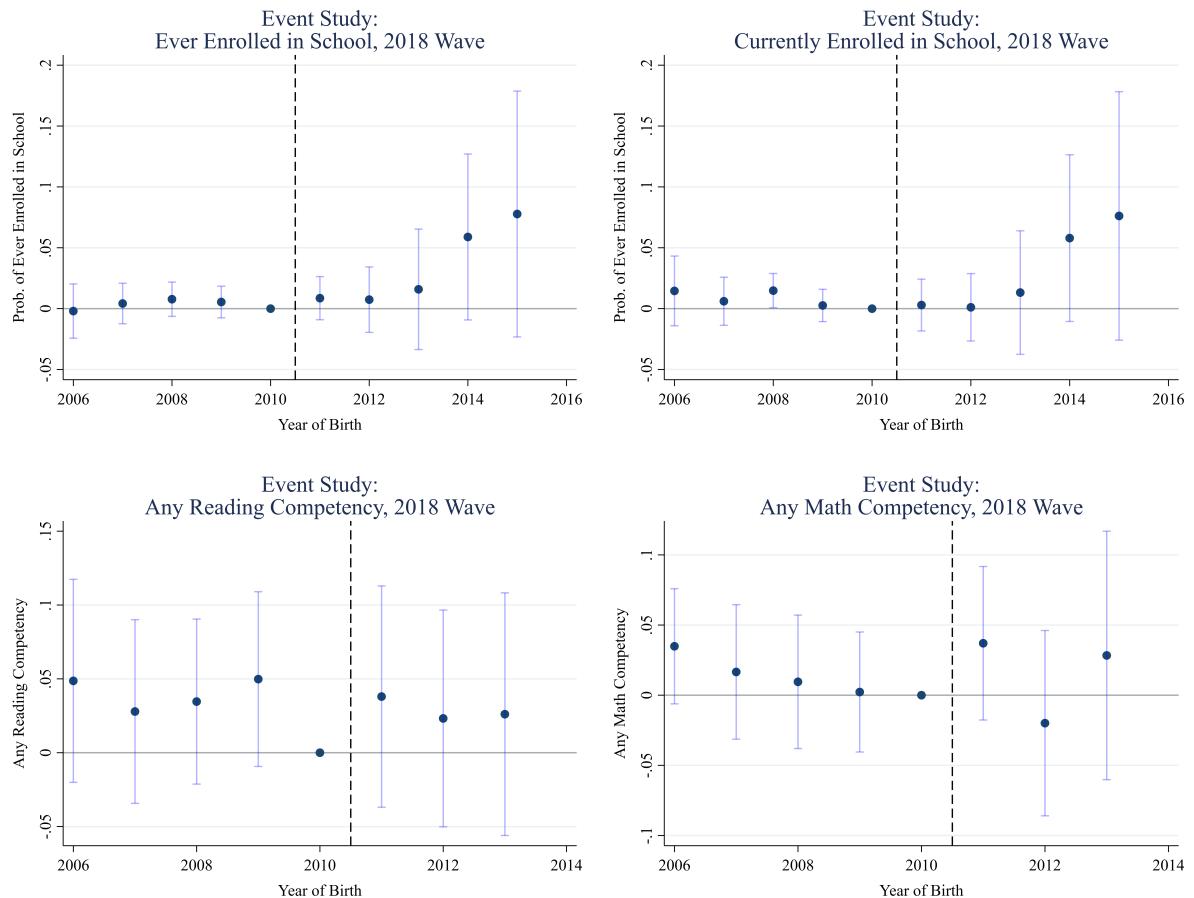
Notes: This figure shows the event study impacts on school/pre-school enrollment at the district level. District and survey wave fixed effects are included, and standard errors are clustered at the district level. Confidence intervals are at the 95% level. Data comes from the 2010, 2012, 2013, 2014, 2016, and 2018 waves of the ASER.

Figure 4. Impacts of Program on Individual-Level ASER Math & Reading Competency



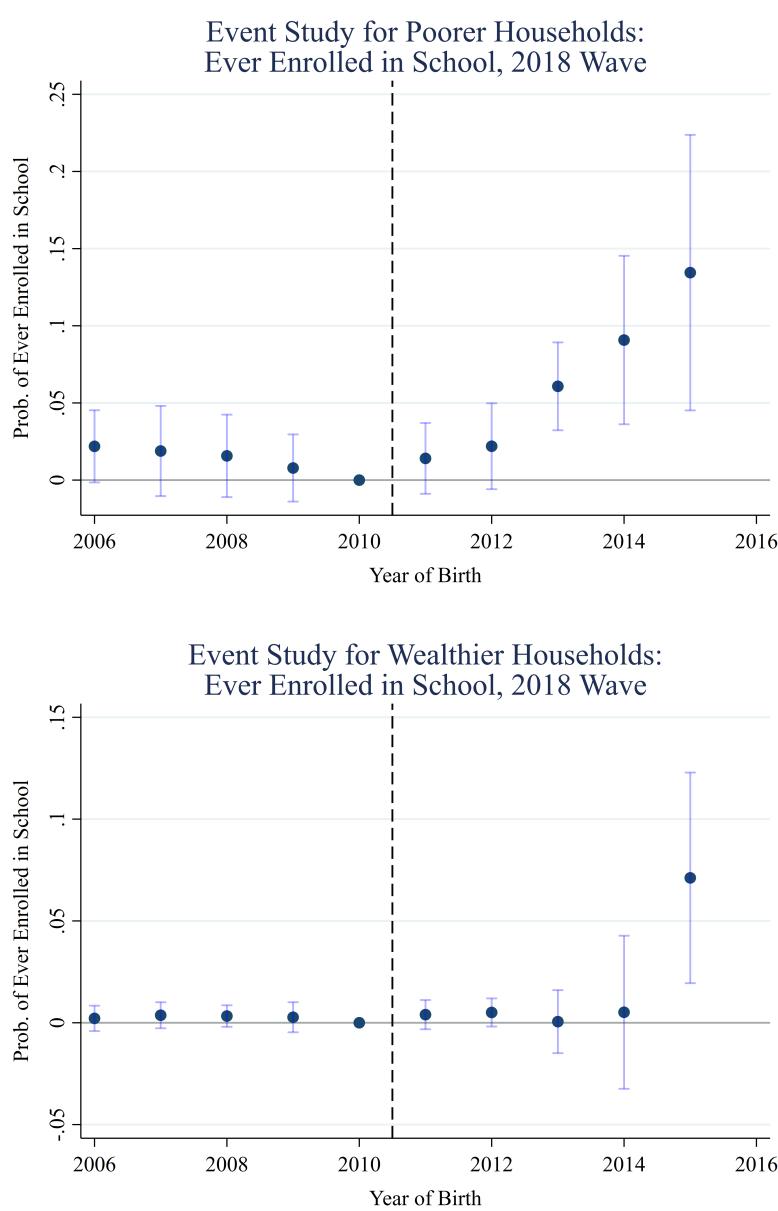
Notes: This figure shows the event study impacts on reading competency and math competency by birth year for the first child. District fixed effects are included, and standard errors are clustered at the district level. Data comes from the 2018 ASER. Confidence intervals are at the 95% level.

Figure 5. Impacts of Program on Individual-Level ASER Outcomes, Sibling Spillovers



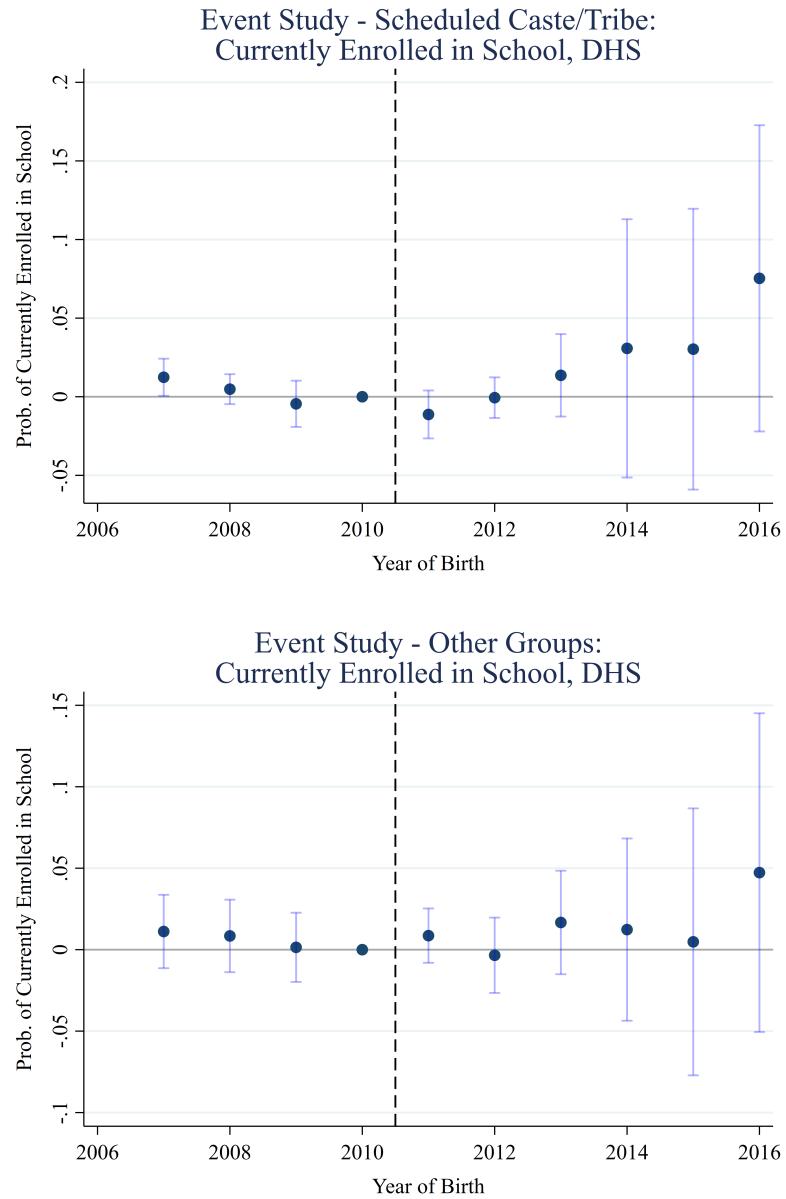
Notes: This figure shows the event study impacts on probability of being enrolled in school, probability of being currently enrolled in school, reading competency, and math competency by birth year for the third and fourth children. District fixed effects are included, and standard errors are clustered at the district level. Data comes from the 2018 ASER. Confidence intervals are at the 95% level.

Figure 6. Impacts of Program on Individual Enrollment by Wealth



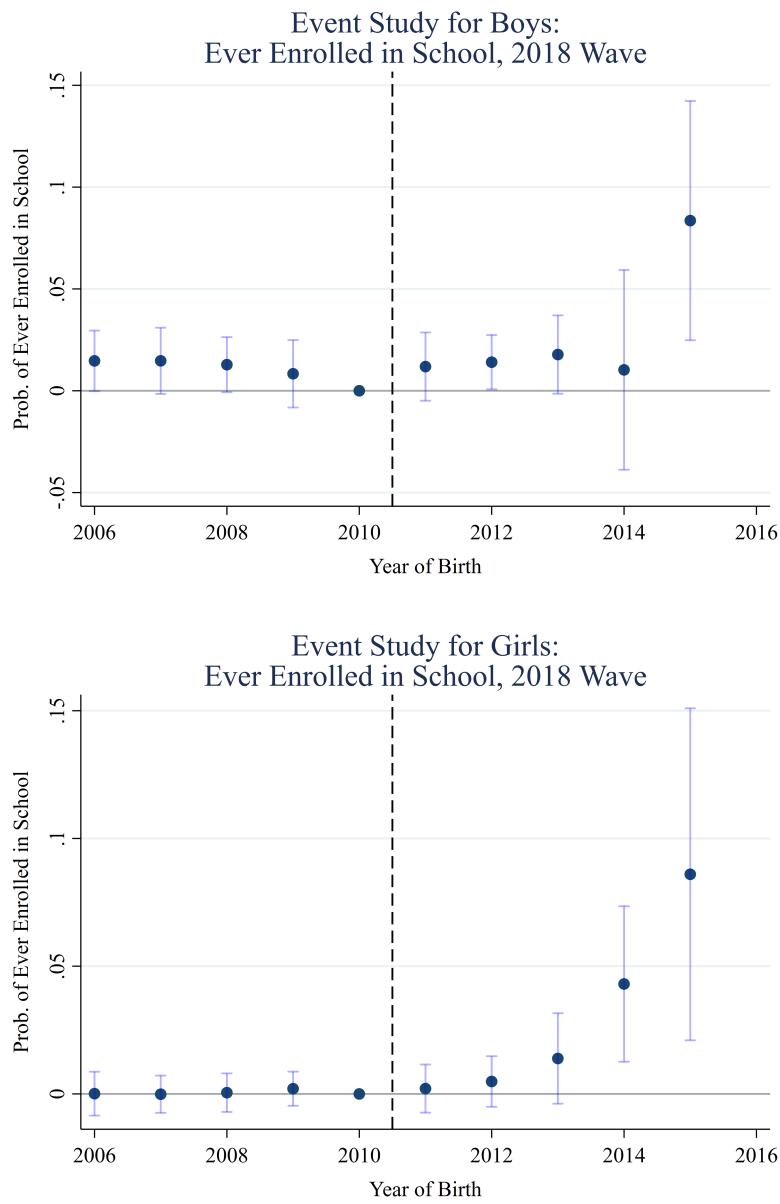
Notes: This figure shows the event study impacts on probability of being enrolled in school by birth year for the first child. District fixed effects are included, and standard errors are clustered at the district level. The first panel shows the effects for the bottom 25th percentile of wealth, and the second panel shows the effects for the top 25th percentile of wealth. Data comes from the 2018 ASER. Confidence intervals are at the 95% level.

Figure 7. Impacts of Program on Individual Enrollment by Caste



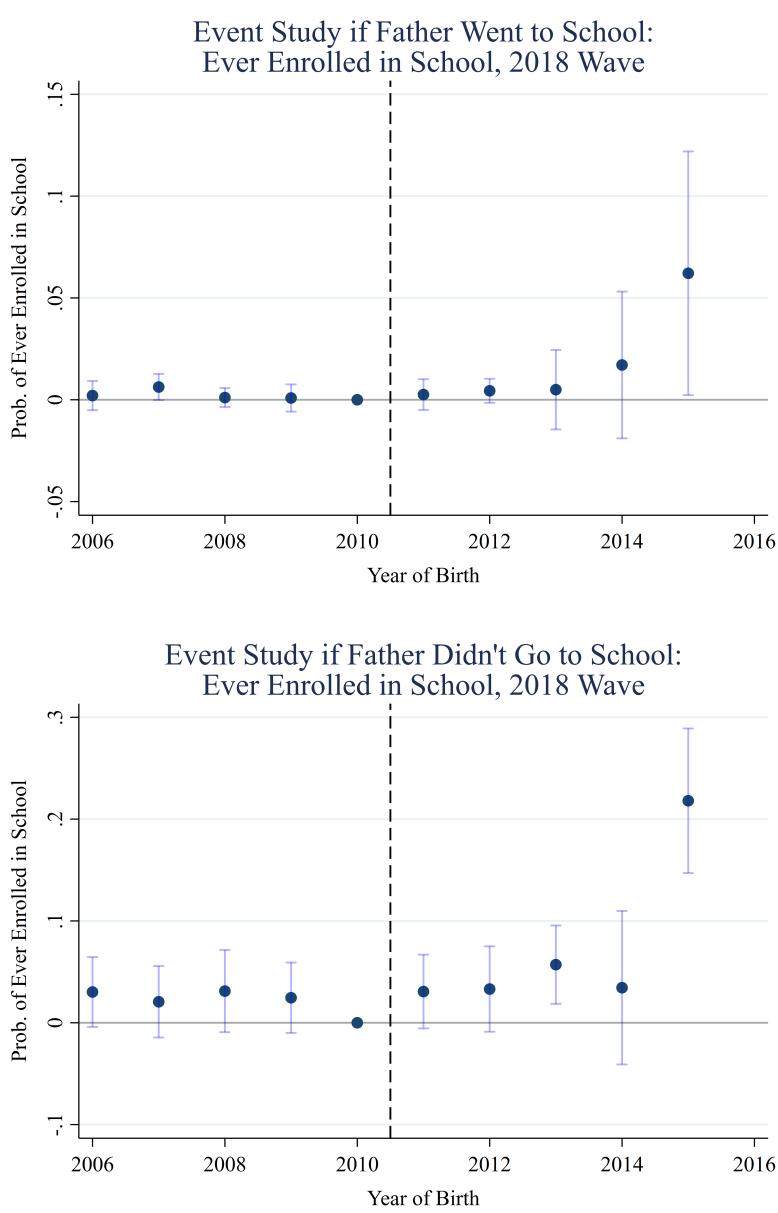
Notes: This figure shows the event study impacts on probability of being enrolled in school by birth year for the first child. District fixed effects are included, and standard errors are clustered at the district level. The first panel shows the effects for scheduled tribes, scheduled castes, and other backward castes, and the second panel shows the effects for other groups. Data comes from the 2019-2021 DHS. Confidence intervals are at the 95% level.

Figure 8. Impacts of Program on Individual Enrollment by Gender



Notes: This figure shows the event study impacts on probability of being enrolled in school by birth year for the first child. District fixed effects are included, and standard errors are clustered at the district level. The first panel shows the effects for boys, and the second panel shows the effects for girls. Data comes from the 2018 ASER. Confidence intervals are at the 95% level.

Figure 9. Impacts of Program on Individual Enrollment by Father's Education



Notes: This figure shows the event study impacts on probability of being enrolled in school by birth year for the first child. District fixed effects are included, and standard errors are clustered at the district level. The first panel shows the effects for children whose fathers are educated, and the second panel shows the effects for children whose fathers are not educated. Data comes from the 2018 ASER. Confidence intervals are at the 95% level.

Table 1. Impact of Program on Education at the Individual Level

	ASER Data					DHS Data
	Ever Enrolled	Currently Enrolled	Any Reading Competency	Any Math Competency	Pays Tuition	Currently Enrolled
	(1)	(2)	(3)	(4)	(5)	(6)
Born 2016 x Treated						0.080*
						(0.043)
Born 2015 x Treated	0.086*** (0.028)	0.087*** (0.027)			-0.003 (0.034)	0.030 (0.039)
Born 2014 x Treated	0.024 (0.018)	0.023 (0.018)			-0.018 (0.027)	0.022 (0.036)
Born 2013 x Treated	0.015** (0.008)	0.017** (0.008)	0.007 (0.030)	0.018 (0.030)	-0.005 (0.023)	0.013 (0.012)
Born 2012 x Treated	0.010** (0.004)	0.011* (0.005)	-0.016 (0.026)	-0.018 (0.026)	-0.033 (0.023)	0.000 (0.005)
Born 2011 x Treated	0.007 (0.005)	0.007 (0.006)	0.000 (0.017)	-0.013 (0.013)	-0.007 (0.022)	-0.007 (0.007)
Born 2009 x Treated	0.005 (0.005)	0.006 (0.005)	0.005 (0.014)	-0.002 (0.010)	-0.014 (0.019)	-0.002 (0.006)
Born 2008 x Treated	0.007 (0.004)	0.012** (0.006)	-0.008 (0.013)	-0.012 (0.011)	-0.012 (0.020)	0.006 (0.004)
Born 2007 x Treated	0.008 (0.005)	0.013** (0.006)	-0.011 (0.013)	-0.015 (0.010)	-0.018 (0.018)	0.014*** (0.005)
Born 2006 x Treated	0.008 (0.005)	0.012* (0.006)	-0.013 (0.014)	-0.013 (0.011)	0.005 (0.020)	
District FE	X	X	X	X	X	X
Birth Year FE	X	X	X	X	X	X
Age FE						X
Control Group DV Mean	0.965	0.959	0.857	0.888	0.215	0.889
Observations	145,653	145,653	107,430	107,100	135,336	170,244

Notes: The data used for the estimates in columns (1)-(4) comes from the 2018 ASER and contains first born children ages 3-12. The data used in column (5) comes from the 2019-2021 DHS and contains first born children born between 2007 and 2016 (approx. ages 3-12). Standard errors are clustered at the district level. Ever enrolled is an indicator for ever being enrolled in school or preschool, currently enrolled is an indicator for currently being enrolled in school or preschool, any reading competency is an indicator for a child having any non-zero level of reading competency (only for children ages 5-12), any math competency is an indicator for a child having any non-zero level of math competency (only for children ages 5-12), and current enrollment in the DHS is an indicator for being currently enrolled in school or preschool. The control group mean includes children across all age cohorts, born both before and after the program started. \*p<0.1, \*\*p<0.05, \*\*\*p<0.01.

Table 2. Impact of Program on Enrollment at the District Level

	Ever Enrolled: ASER Data		
	3-year-olds	4-year-olds	5-year-olds
	(1)	(2)	(3)
Surveyed 2018 x Treated	3.840** (1.887)	-1.008 (2.011)	-0.697 (1.673)
Surveyed 2016 x Treated	2.913 (1.778)	-0.525 (2.162)	-1.819 (1.582)
Surveyed 2014 x Treated	0.707 (1.820)	1.573 (1.891)	-0.744 (1.747)
Surveyed 2012 x Treated	-1.525 (1.809)	-1.053 (2.031)	-2.536* (1.502)
Surveyed 2010 x Treated	0.008 (2.107)	-1.534 (2.051)	-0.604 (1.969)
District FE	X	X	X
Survey Year FE	X	X	X
Control Group DV Mean	33.49	36.35	40.58
Observations	3,440	3,441	4,004

Notes: The data used comes from the 2010-2018 ASER and contains first born children ages 3-12. Standard errors are clustered at the district level. The outcome variable is the number of children in each age group who were ever enrolled in school at the district level. District and survey year fixed effects are included in the regression. \*p<0.1, \*\*p<0.05, \*\*\*p<0.01.

Table 3. Impact of Program on Education at the Individual Level, Sibling Spillovers

	ASER Data				
	Ever Enrolled	Currently Enrolled	Any Reading Competency	Any Math Competency	Birth Parity
	(1)	(2)	(3)	(4)	(5)
Born 2016 x Treated					
Born 2015 x Treated	0.078 (0.051)	0.076 (0.052)			-0.002 (0.035)
Born 2014 x Treated	0.059* (0.035)	0.058* (0.035)			-0.015 (0.030)
Born 2013 x Treated	0.016 (0.025)	0.013 (0.026)	0.026 (0.042)	0.028 (0.045)	-0.003 (0.022)
Born 2012 x Treated	0.007 (0.014)	0.001 (0.014)	0.023 (0.037)	-0.020 (0.034)	0.003 (0.024)
Born 2011 x Treated	0.009 (0.009)	0.003 (0.011)	0.038 (0.038)	0.037 (0.028)	0.005 (0.024)
Born 2009 x Treated	0.005 (0.007)	0.003 (0.007)	0.050* (0.030)	0.002 (0.022)	0.024 (0.027)
Born 2008 x Treated	0.008 (0.007)	0.015** (0.007)	0.035 (0.028)	0.010 (0.024)	-0.018 (0.022)
Born 2007 x Treated	0.004 (0.008)	0.006 (0.010)	0.028 (0.032)	0.017 (0.024)	-0.022 (0.028)
Born 2006 x Treated	-0.002 (0.011)	0.015 (0.015)	0.049 (0.035)	0.035* (0.021)	0.021 (0.035)
District FE	X	X	X	X	X
Birth Year FE	X	X	X	X	X
Age FE					
Control Group DV Mean	0.917	0.912	0.715	0.773	1.876
Observations	72,044	72,044	44,896	44,670	322,008

Notes: The data used for the estimates in columns (1)-(4) comes from the 2018 ASER and contains third- and fourth-born children ages 3-12. Standard errors are clustered at the district level. Ever enrolled is an indicator for ever being enrolled in school or preschool, currently enrolled is an indicator for currently being enrolled in school or preschool, any reading competency is an indicator for a child having any non-zero level of reading competency (only for children ages 5-12), and any math competency is an indicator for a child having any non-zero level of math competency (only for children ages 5-12). The data used for the estimates in column (5) comes from the 2018 ASER and contains first through fourth born children. Birth parity is a measure of the birth order. The control group mean includes children across all age cohorts, born both before and after the program started. \*p<0.1, \*\*p<0.05, \*\*\*p<0.01.

Table 4. Take-up & Mothers' Employment, IHDS

	2011 IHDS Only		2011 vs. 2005 IHDS	
	Maternity Benefits		Maternity Benefits	
	(1)	(2)	(3)	(4)
Treated x Post			0.083 (0.052)	0.100** (0.041)
Treated	0.083* (0.049)	0.039 (0.055)		
Control Group DV Mean	0.189	0.233	0.0818	0.438
Observations	3,315	3,315	8,207	8,207

Notes: Data comes from the IHDS. Outcomes are reported at the household level, for any women (for whom data was provided) in the household receiving maternity benefits or currently working, respectively. Columns (1) and (2) use only data from the 2011 wave of the IHDS, comparing treated and untreated districts. Columns (3) and (4) use data from both the 2011 and 2005 waves of the IHDS, allowing for a difference-in-differences analysis. Note that in the 2005 wave, all women were asked about their employment. In the 2011 wave, only a subset of women were asked. This is why the control group mean is much higher in column (4) than in column (2). Standard errors are clustered at the district level.  
 \*p<0.1, \*\*p<0.05, \*\*\*p<0.01.

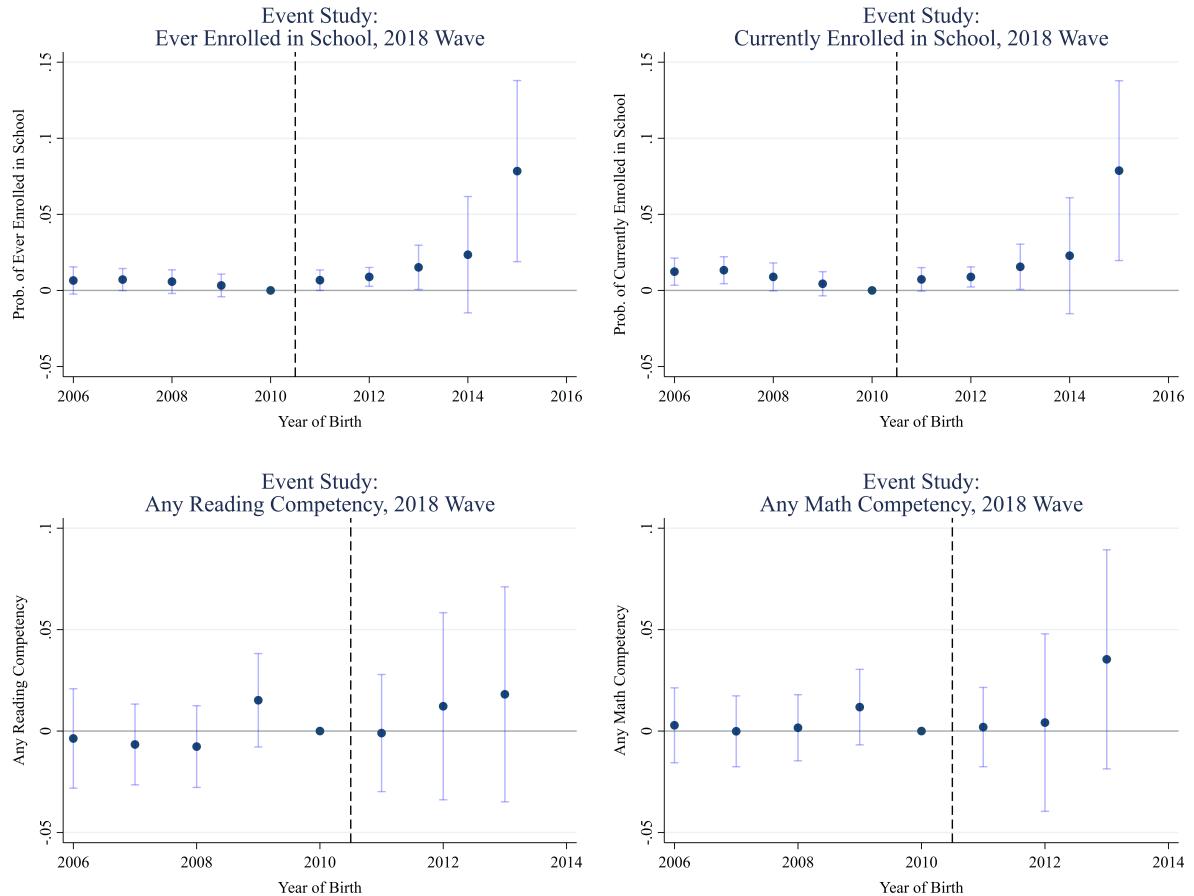
Table 5. Take-up by Household Income & Correlation with NREGA

	<b>Below Median Income</b>		<b>Above Median Income</b>	
	<b>Maternity Benefits</b>		<b>Maternity Benefits</b>	
	(1)	(2)	(3)	(4)
Treated	0.123** (0.057)	0.053 (0.064)	0.042 (0.045)	-0.040 (0.031)
Control Group DV Mean	0.189	0.148	0.189	0.148
Observations	1,657	1,657	1,658	1,658

Notes: Data comes from the 2011 IHDS. Outcomes are reported at the household level, for anyone in the household receiving maternity benefits or receiving income through NREGA. Columns (1) and (2) show the differences between treated and untreated districts for households with below-median income. Columns (3) and (4) show differences between treated and untreated districts for households with above-median income. Standard errors are clustered at the district level. \*p<0.1, \*\*p<0.05, \*\*\*p<0.01.

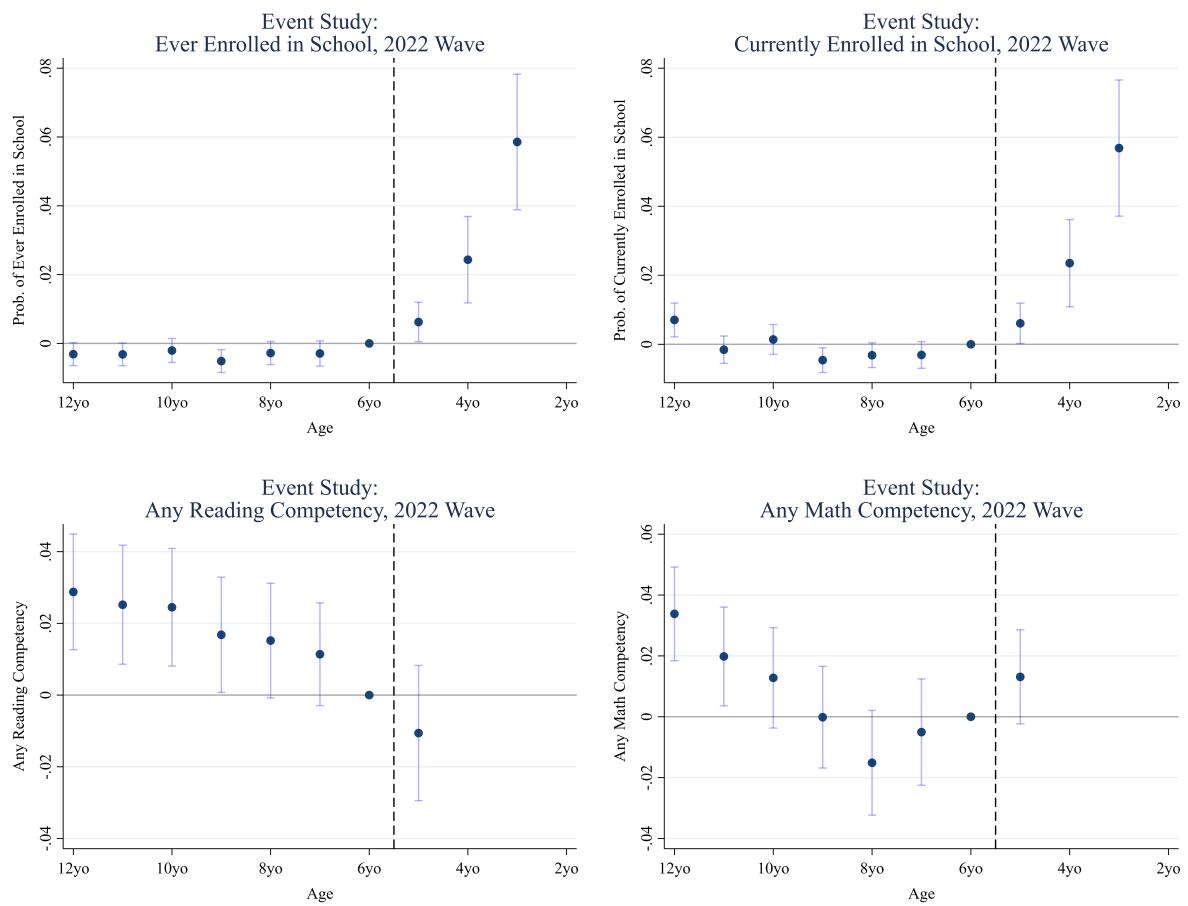
# Appendix

Figure A1. Impacts on Individual-Level ASER Outcomes, First & Second-Born



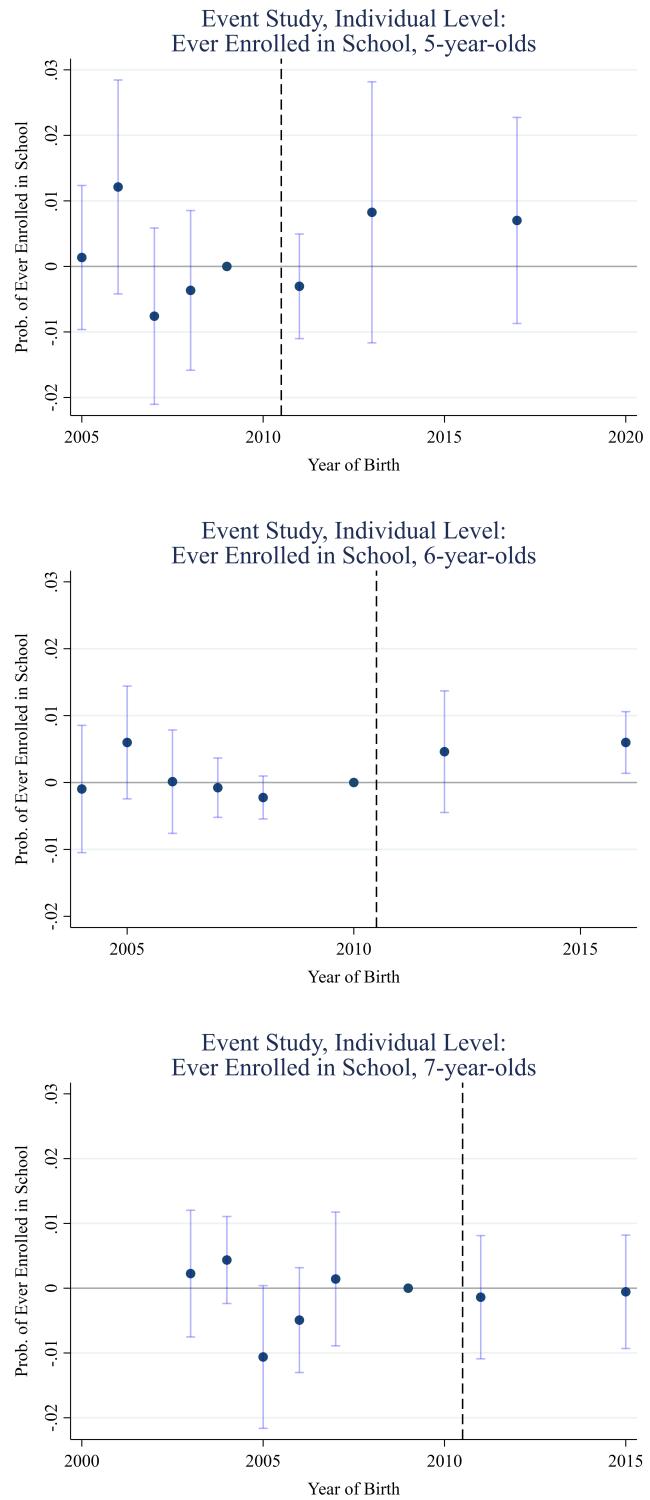
Notes: This figure shows the event study impacts on probability of being enrolled in school, probability of being currently enrolled in school, reading competency, and math competency by birth year for the first and second child. District fixed effects are included, and standard errors are clustered at the district level. Data comes from the 2018 ASER. Confidence intervals are at the 95% level.

Figure A2. Impacts on Individual-Level ASER Outcomes, Nationwide Rollout



Notes: This figure shows the event study impacts on probability of being enrolled in school, probability of being currently enrolled in school, reading competency, and math competency by birth year for the first and second child. District fixed effects are included, and standard errors are clustered at the district level. Data comes from the 2018 ASER. Confidence intervals are at the 95% level.

Figure A3. Impacts of Program on Individual-Level Enrollment Across Waves



Notes: This figure shows the event study impacts on probability of being enrolled in school for 5, 6, and 7-year-olds. District fixed effects are included, and standard errors are clustered at the district level. Data comes from the 2010-2022 ASER. Confidence intervals are at the 95% level.

Table A1. Descriptive Statistics

	<b>Mean</b>	<b>Standard Deviation</b>
<b>Panel A: ASER Sample</b>	(1)	(2)
Ever Enrolled in School	0.97	0.18
Currently Enrolled in School	0.96	0.20
Any Math Competency	0.89	0.31
Any Reading Competency	0.86	0.35
Pays Tuition	0.21	0.41
Child Age	8.00	2.85
Fraction of Girls	0.49	0.50
Father Went to School	0.81	0.39
Wealthy	0.75	0.43
Total Number of Observations	145,653	
<b>Panel B: DHS Sample</b>	<b>Mean</b>	<b>Standard Deviation</b>
	(1)	(2)
Currently Enrolled in School	0.89	0.31
Age	8.17	2.94
Wealthy	0.51	0.50
Lower Caste	0.80	0.40
Total Number of Observations	170,254	

Notes: Panel A contains the sample of first-born children from the 2018 ASER. Panel B contains the sample of first-born children from the 2019-2021 DHS.

Table A2. Heterogeneity by Wealth at the Individual Level

	Ever Enrolled: ASER Data		Currently Enrolled: DHS Data	
	Poor	Wealthy	Poor	Wealthy
	(1)	(2)	(3)	(4)
Born 2016 x Treated			0.051 (0.050)	0.086** (0.033)
Born 2015 x Treated	0.134*** (0.045)	0.071*** (0.026)	0.008 (0.046)	0.023 (0.033)
Born 2014 x Treated	0.091*** (0.028)	0.005 (0.019)	0.016 (0.036)	0.016 (0.034)
Born 2013 x Treated	0.061*** (0.014)	0.001 (0.008)	0.015 (0.015)	0.006 (0.013)
Born 2012 x Treated	0.022 (0.014)	0.005 (0.004)	0.003 (0.009)	-0.003 (0.005)
Born 2011 x Treated	0.014 (0.012)	0.004 (0.004)	0.001 (0.009)	-0.011 (0.009)
Born 2009 x Treated	0.008 (0.011)	0.003 (0.004)	0.002 (0.010)	-0.005 (0.006)
Born 2008 x Treated	0.016 (0.014)	0.003 (0.003)	0.007 (0.007)	0.003 (0.005)
Born 2007 x Treated	0.019 (0.015)	0.004 (0.003)	0.018* (0.010)	0.007 (0.004)
Born 2006 x Treated	0.022* (0.012)	0.002 (0.003)		
District FE	X	X	X	X
Birth Year FE	X	X	X	X
Age FE			X	X
Control Group DV Mean	0.945	0.972	0.856	0.922
Observations	35,986	109,663	82,641	87,602

Notes: The data for columns (1)-(2) comes from the 2018 ASER and contains first born children ages 3-12. Poor and wealthy households are split at the 25th percentile of a [Kling et al. \(2007\)](#) index across indicators for whether the household has a toilet, electricity connection, television, newspapers, and mobile phone and whether the house is made of pucca. The data for columns (3)-(4) comes from the 2019-2021 DHS and contains first born children born between 2007 and 2016. Poor and wealthy households are split by the median net asset index. Standard errors are clustered at the district level. Ever enrolled, which is the outcome variable for the ASER, is an indicator for ever being enrolled in school or preschool. Currently enrolled, which is the outcome variable for the DHS, is an indicator for currently being enrolled in school or preschool. The control group mean includes children across all age cohorts, born both before and after the program started. \*p<0.1, \*\*p<0.05, \*\*\*p<0.01.

Table A3. Heterogeneity by Caste at the Individual Level

	<b>Currently Enrolled: DHS Data</b>	
	<b>Low Caste</b>	<b>High Caste</b>
	(1)	(2)
Born 2016 x Treated	0.075 (0.050)	0.047 (0.050)
Born 2015 x Treated	0.030 (0.045)	0.005 (0.042)
Born 2014 x Treated	0.031 (0.042)	0.012 (0.028)
Born 2013 x Treated	0.014 (0.013)	0.017 (0.016)
Born 2012 x Treated	-0.001 (0.007)	-0.003 (0.012)
Born 2011 x Treated	-0.011 (0.008)	0.009 (0.008)
Born 2009 x Treated	-0.004 (0.007)	0.001 (0.011)
Born 2008 x Treated	0.005 (0.005)	0.008 (0.011)
Born 2007 x Treated	0.012** (0.006)	0.011 (0.011)
Born 2006 x Treated		
District FE	X	X
Birth Year FE	X	X
Age FE	X	X
Control Group DV Mean	0.883	0.918
Observations	127,824	32,947

Notes: Data comes from the 2019-2021 DHS and contains first born children born between 2007 and 2016. The first column contains individuals from scheduled tribes, scheduled castes, and other backward castes. The second column contains individuals from all other groups. Standard errors are clustered at the district level. Currently enrolled, which is the outcome variable, is an indicator for currently being enrolled in school or preschool. The control group mean includes children across all age cohorts, born both before and after the program started. \*p<0.1, \*\*p<0.05, \*\*\*p<0.01.

Table A4. Heterogeneity by Gender and Father's Schooling at the Individual Level

	Ever Enrolled: ASER Data			
	Girls Only	Boys Only	Father Went to School	Father Did Not Go to School
	(1)	(2)	(3)	(4)
Born 2016 x Treated				
Born 2015 x Treated	0.086*** (0.033)	0.084*** (0.030)	0.062** (0.030)	0.218*** (0.036)
Born 2014 x Treated	0.043*** (0.016)	0.010 (0.025)	0.017 (0.018)	0.034 (0.038)
Born 2013 x Treated	0.014 (0.009)	0.018* (0.010)	0.005 (0.010)	0.057*** (0.020)
Born 2012 x Treated	0.005 (0.005)	0.014** (0.007)	0.004 (0.003)	0.033 (0.021)
Born 2011 x Treated	0.002 (0.005)	0.012 (0.009)	0.003 (0.004)	0.031* (0.018)
Born 2009 x Treated	0.002 (0.003)	0.008 (0.008)	0.001 (0.003)	0.025 (0.018)
Born 2008 x Treated	0.000 (0.004)	0.013* (0.007)	0.001 (0.002)	0.031 (0.021)
Born 2007 x Treated	-0.000 (0.004)	0.015* (0.008)	0.006* (0.003)	0.021 (0.018)
Born 2006 x Treated	0.000 (0.004)	0.015* (0.008)	0.002 (0.004)	0.030* (0.017)
District FE	X	X	X	X
Birth Year FE	X	X	X	X
Age FE				
Control Group DV Mean	0.965	0.965	0.970	0.949
Observations	71,208	73,896	112,685	26,093

Notes: The data comes from the 2018 ASER and contains first born children ages 3-12. Standard errors are clustered at the district level. Ever enrolled, which is the outcome variable, is an indicator for ever being enrolled in school or preschool. The control group mean includes children across all age cohorts, born both before and after the program started. \*p<0.1, \*\*p<0.05, \*\*\*p<0.01.

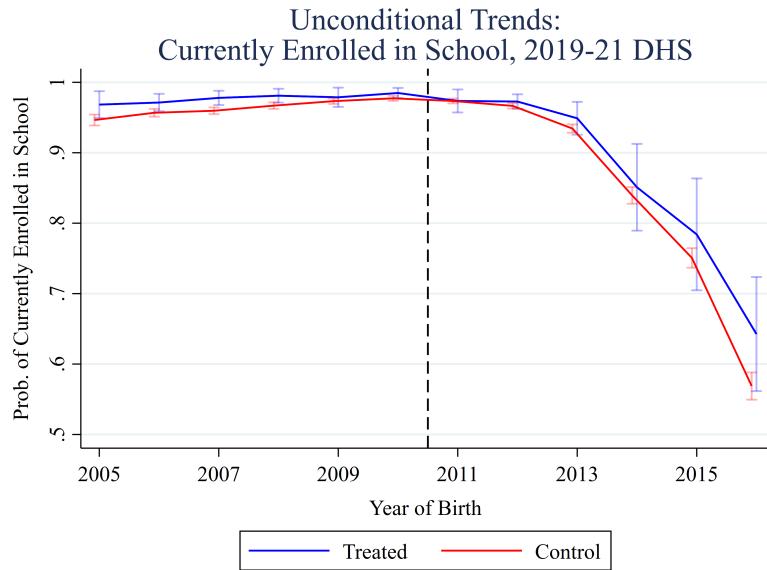
## Supplementary Material

Figure B1. Policy Conditions & Details from [Ministry of Women and Child Development \(2011\)](#)

Cash Transfer	Conditions	Amount (In Rs.)	Means of Verification
<b>First (at the end of second trimester)</b>	<ul style="list-style-type: none"> <li>• Registration of Pregnancy at AWC / health centres within 4 months of pregnancy</li> <li>• At least one ANC with IFA tablets and TT</li> <li>• Attended at least one counselling session at AWC / VHND</li> </ul>	1500	Mother & Child Protection Card reflecting registration of pregnancy by relevant AWC/ Health Centres and counter signed by AWW
<b>Incentive under JSY</b>	<ul style="list-style-type: none"> <li>• JSY package for institutional delivery including early initiation of breastfeeding and ensure colostrum feed.</li> </ul>	As per JSY norms	
<b>Second (3 months after delivery)</b>	<ul style="list-style-type: none"> <li>• The birth of the child is registered.</li> <li>• The child has received: <ul style="list-style-type: none"> <li>▪ OPV and BCG at birth</li> <li>▪ OPV and DPT at 6 weeks</li> <li>▪ OPV and DPT at 10 weeks</li> </ul> </li> <li>• Attended at least 2 growth monitoring and IYCF counselling sessions within 3 months of delivery.</li> </ul>	1500	<p>Mother &amp; Child Protection Card, Growth Monitoring Chart and Immunization Register</p> <p>*would also be available for still births and infant mortality.</p>
<b>Third (6 months after delivery)</b>	<ul style="list-style-type: none"> <li>• Exclusive breastfeeding for six months and introduction of complimentary feeding as certified by the mother</li> <li>• The child has received OPV and third dose of DPT</li> <li>• Attended at least 2 growth monitoring and IYCF counselling sessions between 3rd and 6th months of delivery.</li> </ul>	1000	Self certification, Mother & Child Protection Card, Growth Monitoring Chart and Immunization Register

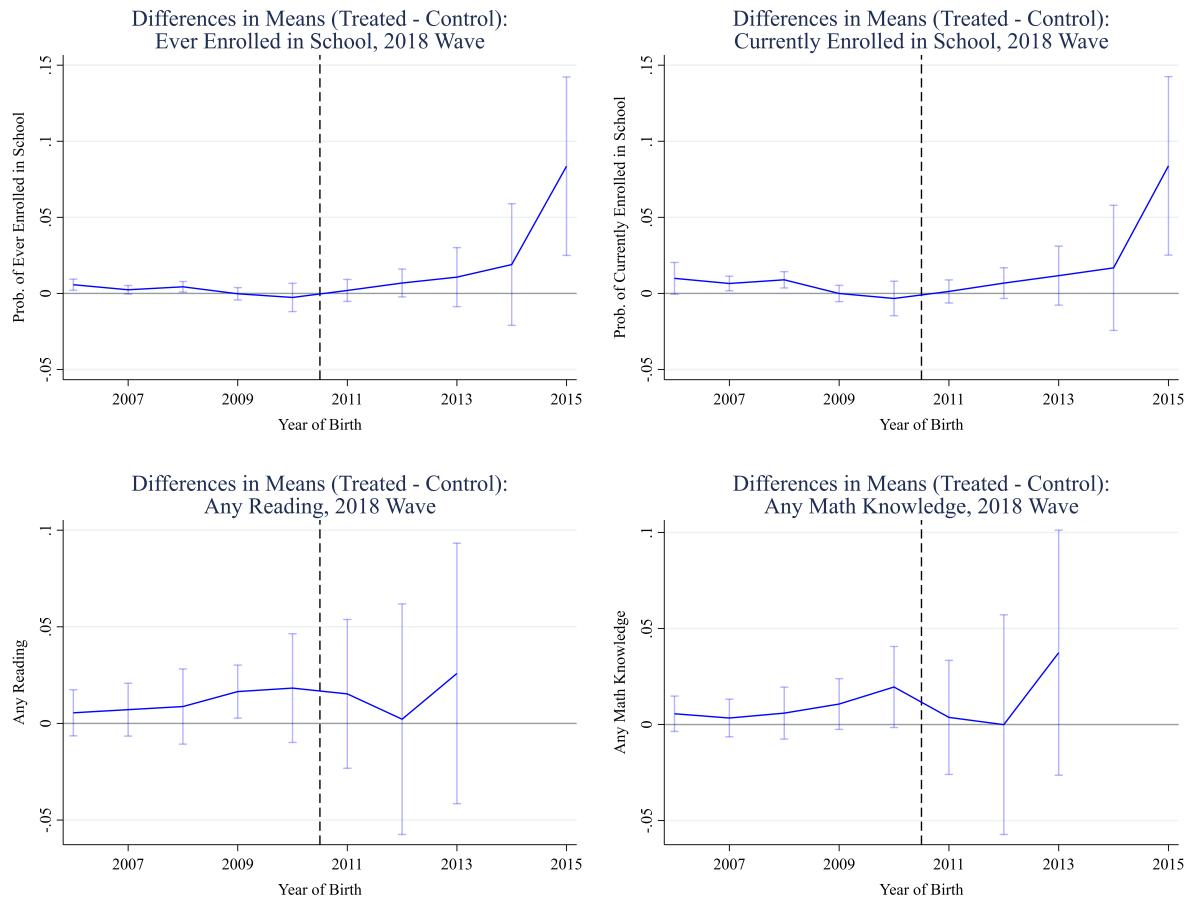
Notes: This table includes the conditionality details for the pilot of the IGMSY program initially implemented in 2011. Note that the JSY incentive in the second row was distributed through a separate program.

Figure B2. Trends in DHS Current Enrollment by District Type



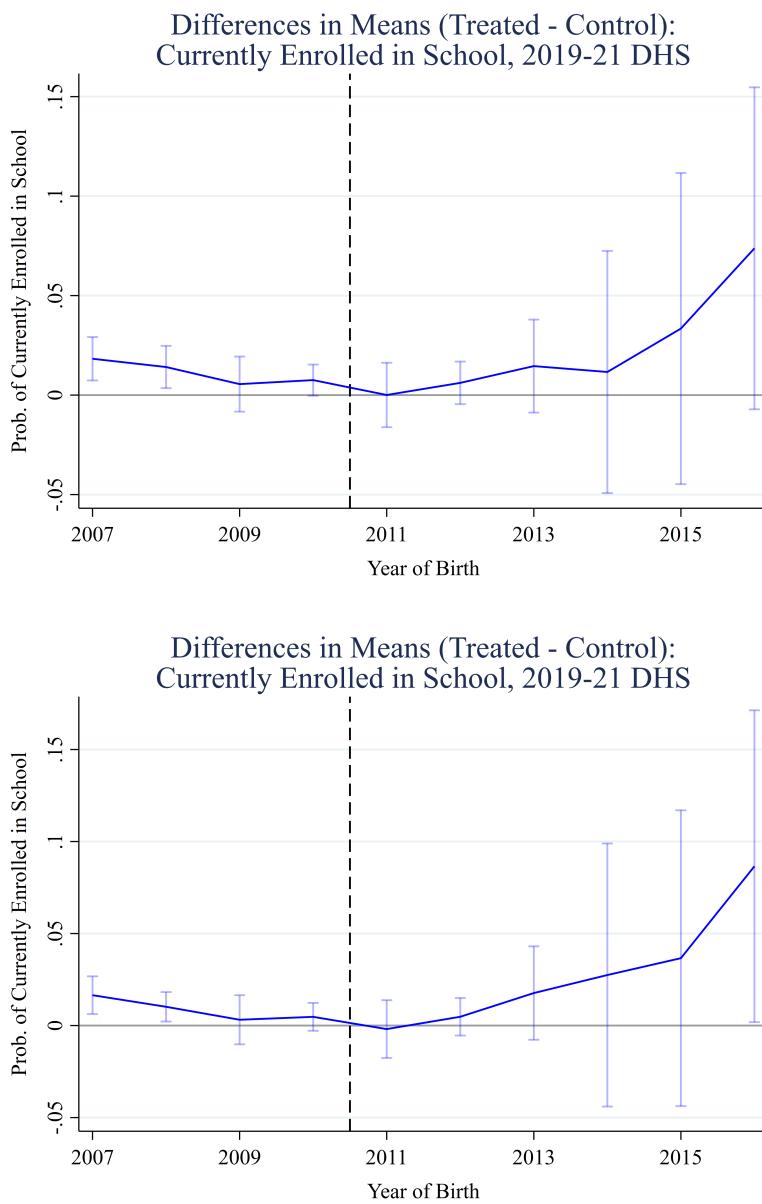
Notes: This figure shows the raw trends for probability of currently being enrolled in school by birth year for the first child, separately for the treated and control districts. Standard errors are clustered at the district level. Data comes from the 2019-2021 DHS. Confidence intervals are at the 95% level.

Figure B3. Difference in Means in ASER Outcomes by District Treatment Status



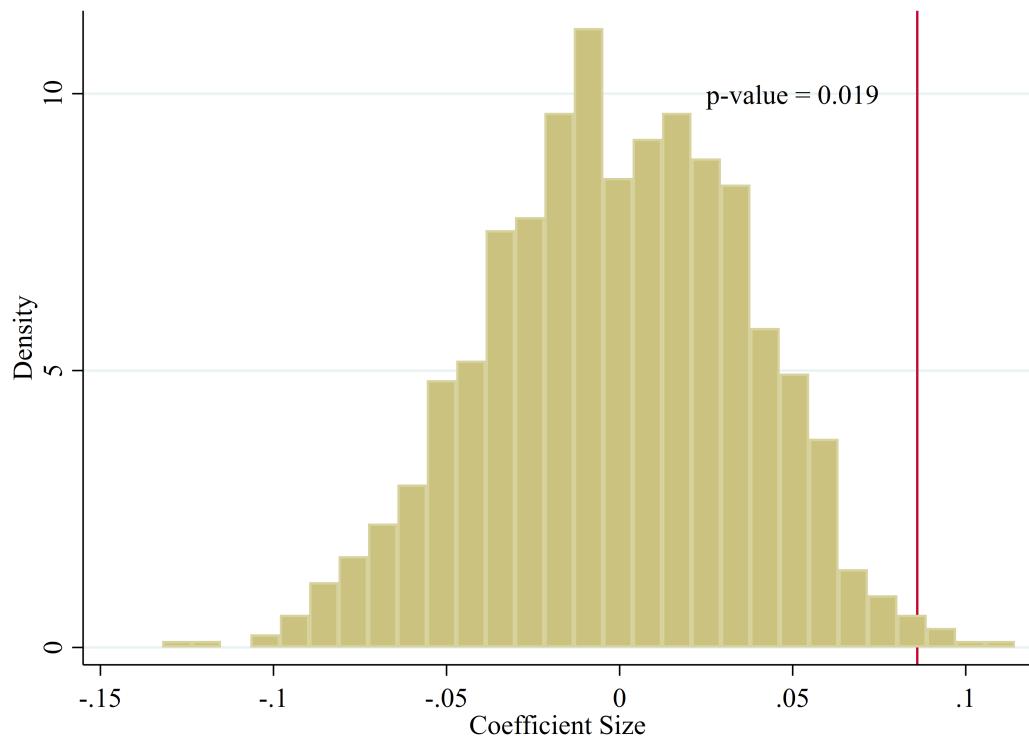
Notes: This figure shows the differences in means for probability of ever being enrolled in school, probability of currently being enrolled in school, any math competency, and any reading competency by birth year for the first child. Standard errors are clustered at the district level. Data comes from the 2018 ASER. Confidence intervals are at the 95% level.

Figure B4. Difference in Means in DHS Current Enrollment by District Treatment Status



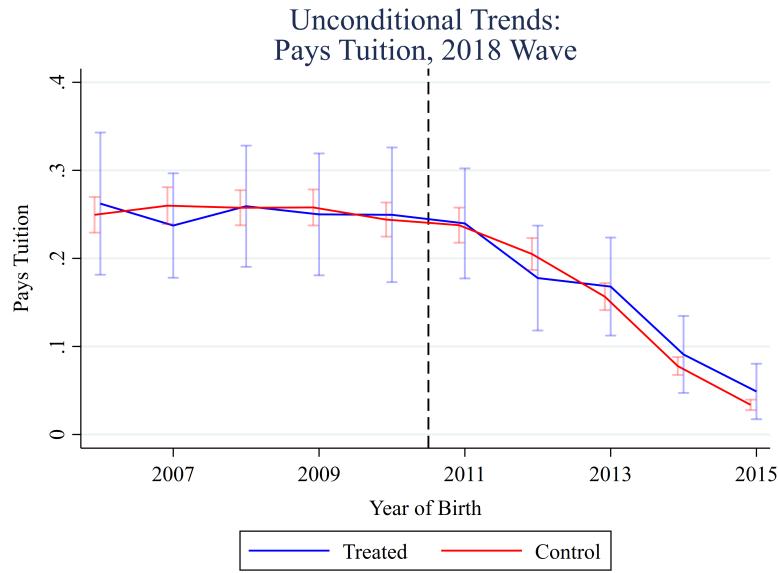
Notes: This figure shows the differences in means for probability of currently being enrolled in school by birth year for the first child. The first panel does not include age fixed effects, and the second panel does include age fixed effects. Standard errors are clustered at the district level. Data comes from the 2019-2021 DHS. Confidence intervals are at the 95% level.

Figure B5. Randomization Inference: Individual-Level Enrollment Effect on 3-Year-Olds



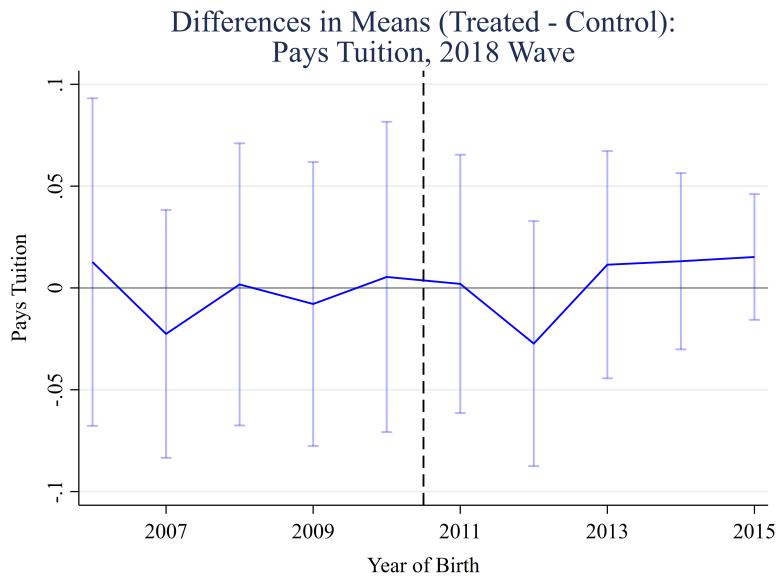
Notes: This figure plots the regression coefficients for the 2015 birth cohort from 1000 replications of randomly-assigned treatment status. The red line represents the actual estimated coefficient from the main results. The p-value is 0.019, which is the probability of estimating a larger treatment effect by random chance.

Figure B6. Trends in ASER Tuition Payment by District Type



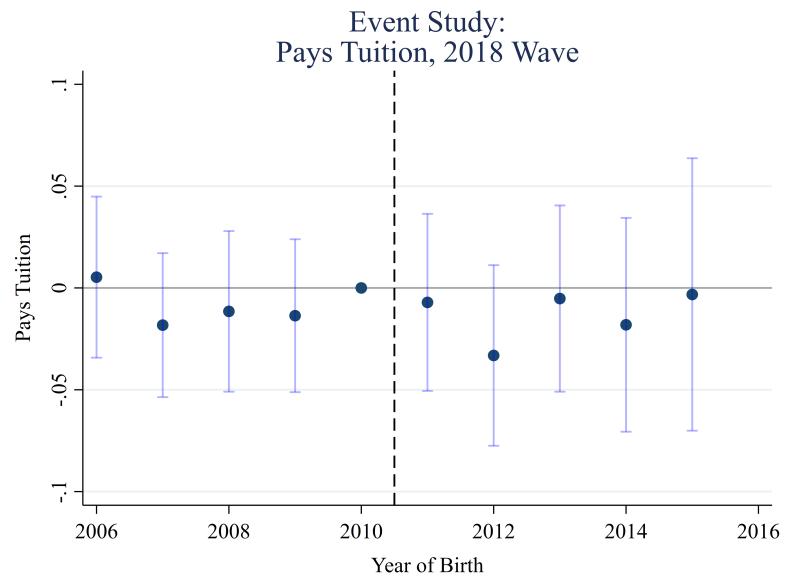
Notes: This figure shows the raw trends for probability of paying any tuition by birth year for the first child, separately for the treated and control districts. Standard errors are clustered at the district level. Data comes from the 2018 ASER. Confidence intervals are at the 95% level.

Figure B7. Difference in Trends in ASER Tuition Payment by District Type



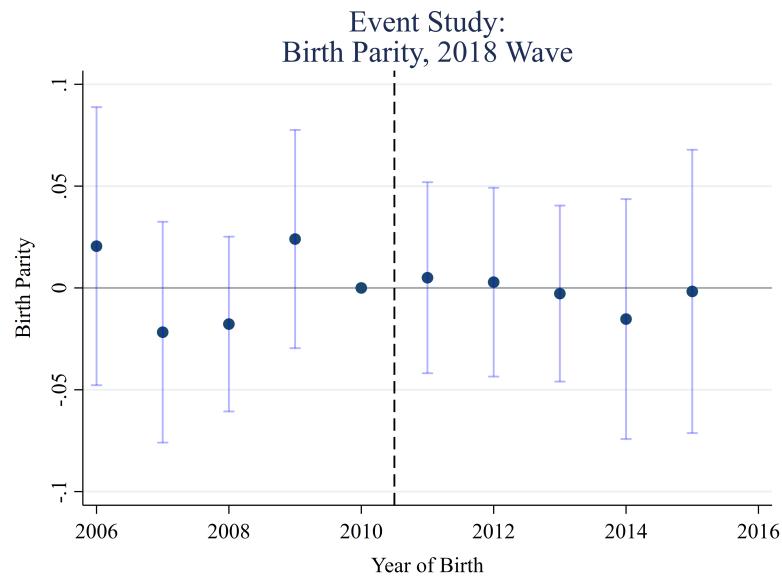
Notes: This figure shows the differences in means for probability of paying any tuition by birth year for the first child. Standard errors are clustered at the district level. Data comes from the 2018 ASER. Confidence intervals are at the 95% level.

Figure B8. Impacts on Individual Tuition Payment



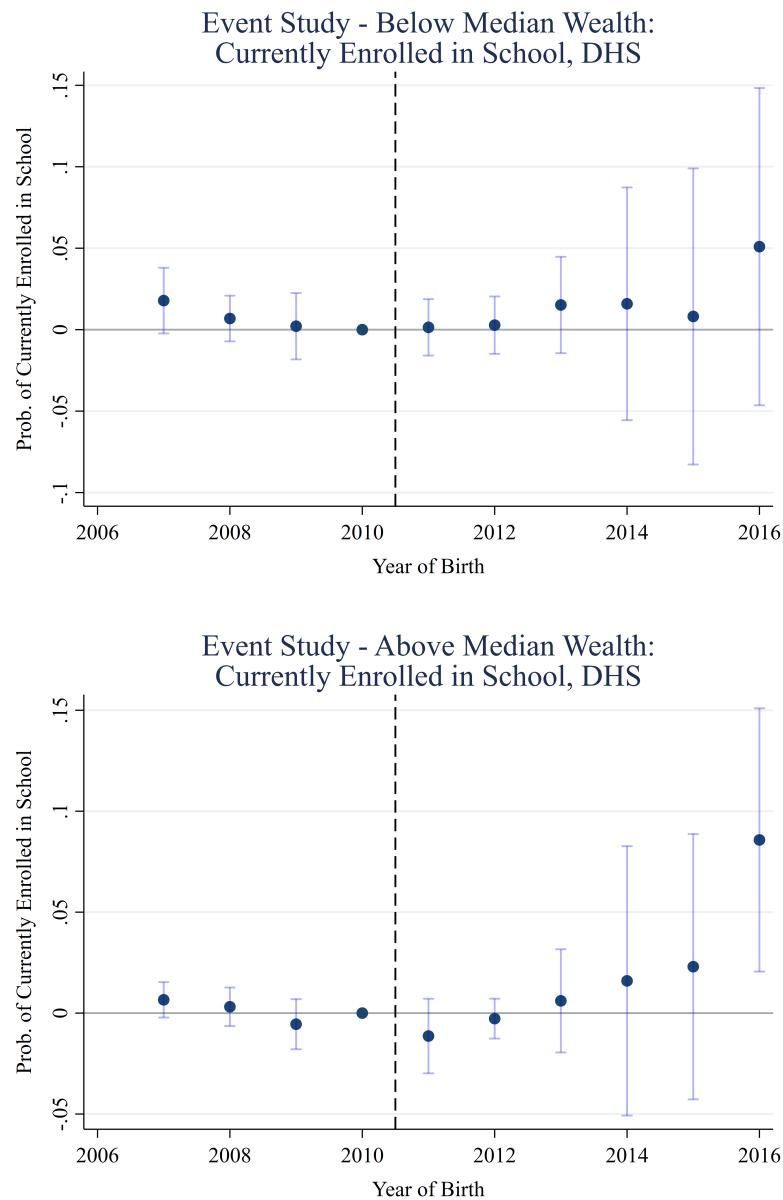
Notes: This figure shows the event study impacts on probability of paying any tuition by birth year for the first child. District fixed effects are included, and standard errors are clustered at the district level. Data comes from the 2018 ASER. Confidence intervals are at the 95% level.

Figure B9. Impacts on Birth Parity



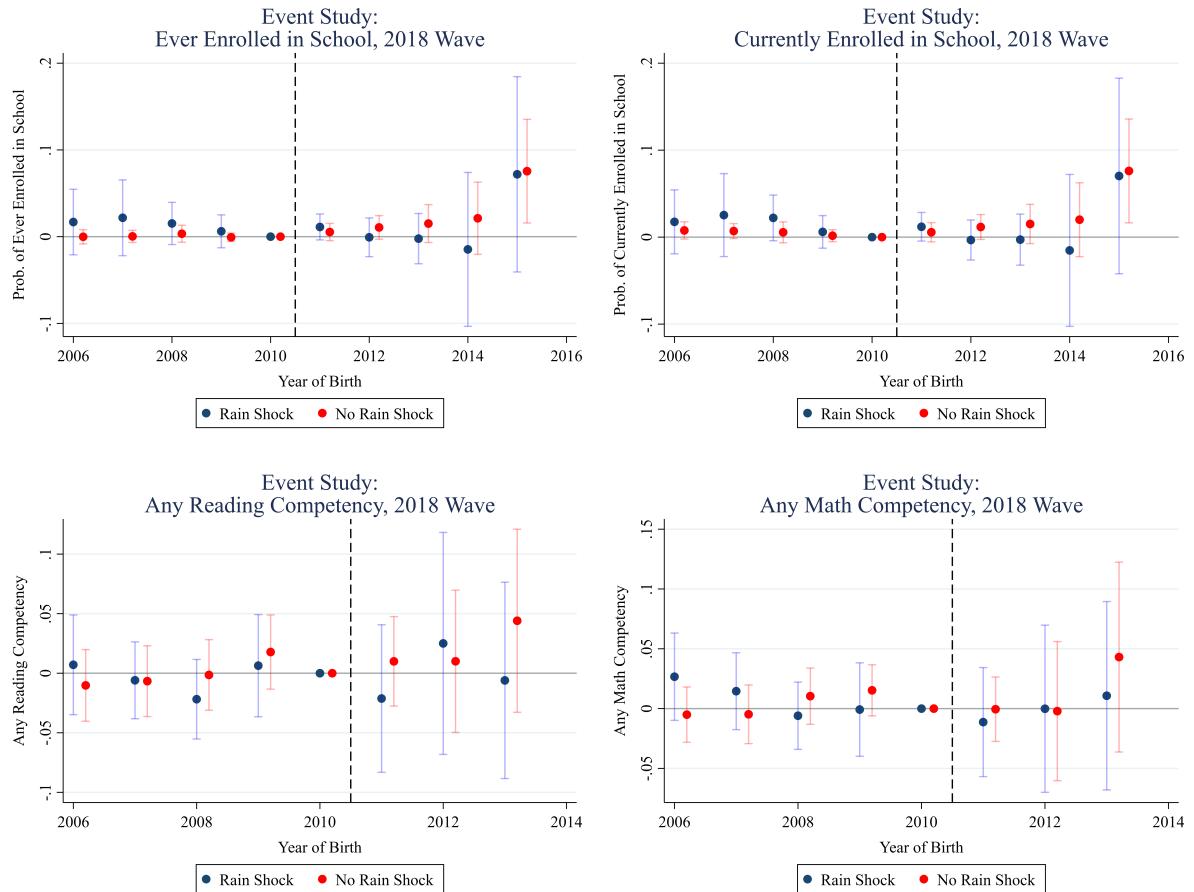
Notes: This figure shows the event study impacts on birth parity. District fixed effects are included, and standard errors are clustered at the district level. Data comes from the 2018 ASER. Confidence intervals are at the 95% level.

Figure B10. Impacts on Current Enrollment by Wealth, DHS



Notes: This figure shows the event study impacts on probability of being enrolled in school by birth year for the first child. District fixed effects are included, and standard errors are clustered at the district level. The first panel shows the effects for households below the median for wealth, and the second panel shows the effects for households above the median for wealth. Data comes from the 2019-2021 DHS. Confidence intervals are at the 95% level.

Figure B11. Impacts on Individual-Level ASER Outcomes, Negative Shocks



Notes: This figure shows the event study impacts on probability of being enrolled in school, probability of being currently enrolled in school, reading competency, and math competency by birth year for the first and second child. The sample is split across individuals who did and did not experience a negative rainfall shock during their first three years of life. District fixed effects are included, and standard errors are clustered at the district level. Data comes from the 2018 ASER and the University of Delaware. Confidence intervals are at the 95% level.

Table B1. Impact of Program on Education, Logistic Regression

	ASER Data					DHS Data
	Ever Enrolled	Currently Enrolled	Any Reading Competency	Any Math Competency	Pays Tuition	Currently Enrolled
	(1)	(2)	(3)	(4)	(5)	(6)
Born 2016 x Treated						0.022 (0.243)
Born 2015 x Treated	1.015*** (0.350)	1.008*** (0.340)			0.339 (0.303)	-0.116 (0.257)
Born 2014 x Treated	0.713** (0.353)	0.649* (0.346)			0.001 (0.167)	-0.114 (0.306)
Born 2013 x Treated	0.747** (0.380)	0.755** (0.384)	-0.057 (0.177)	-0.160 (0.197)	0.012 (0.157)	0.017 (0.266)
Born 2012 x Treated	0.866** (0.392)	0.813* (0.416)	-0.183 (0.179)	-0.347* (0.206)	-0.245 (0.165)	-0.119 (0.253)
Born 2011 x Treated	0.664 (0.452)	0.559 (0.457)	-0.059 (0.177)	-0.289 (0.193)	-0.049 (0.150)	-0.369 (0.283)
Born 2009 x Treated	0.338 (0.617)	0.359 (0.458)	0.206 (0.227)	0.058 (0.236)	-0.098 (0.130)	-0.144 (0.289)
Born 2008 x Treated	1.124** (0.547)	1.135** (0.489)	-0.029 (0.249)	-0.129 (0.329)	-0.081 (0.136)	0.163 (0.241)
Born 2007 x Treated	1.282 (0.872)	1.223** (0.602)	0.024 (0.258)	-0.158 (0.337)	-0.116 (0.122)	0.326 (0.286)
Born 2006 x Treated	1.515** (0.627)	0.771* (0.429)	-0.071 (0.252)	-0.048 (0.338)	0.038 (0.137)	
District FE	X	X	X	X	X	X
Birth Year FE	X	X	X	X	X	X
Age FE						X
Control Group DV Mean	0.965	0.959	0.857	0.888	0.215	0.889
Observations	126,286	131,971	107,297	107,100	134,949	170,054

Notes: The data used for the estimates in columns (1)-(4) comes from the 2018 ASER and contains first born children ages 3-12. The data used in column (5) comes from the 2019-2021 DHS and contains first born children born between 2007 and 2016 (approx. ages 3-12). Standard errors are clustered at the district level. Ever enrolled is an indicator for ever being enrolled in school or preschool, currently enrolled is an indicator for currently being enrolled in school or preschool, any reading competency is an indicator for a child having any non-zero level of reading competency (only for children ages 5-12), any math competency is an indicator for a child having any non-zero level of math competency (only for children ages 5-12), and current enrollment in the DHS is an indicator for being currently enrolled in school or preschool. A logistic regression model is used to estimate the coefficients in this table, so the coefficients should be interpreted as changes in the odds ratio. The control group mean includes children across all age cohorts, born both before and after the program started. \*p<0.1, \*\*p<0.05, \*\*\*p<0.01.

Table B2. Impact of Program on Education, Probit Regression

	ASER Data					DHS Data
	Ever Enrolled	Currently Enrolled	Any Reading Competency	Any Math Competency	Pays Tuition	Currently Enrolled
	(1)	(2)	(3)	(4)	(5)	(6)
Born 2016 x Treated						0.065 (0.127)
Born 2015 x Treated	0.487*** (0.156)	0.529*** (0.152)			0.138 (0.165)	-0.022 (0.139)
Born 2014 x Treated	0.301** (0.149)	0.305** (0.149)			-0.017 (0.094)	-0.028 (0.166)
Born 2013 x Treated	0.300* (0.162)	0.325** (0.163)	-0.020 (0.093)	-0.043 (0.100)	-0.001 (0.089)	0.028 (0.129)
Born 2012 x Treated	0.367** (0.173)	0.363** (0.179)	-0.092 (0.094)	-0.157 (0.107)	-0.138 (0.092)	-0.053 (0.109)
Born 2011 x Treated	0.234 (0.207)	0.187 (0.203)	-0.030 (0.092)	-0.143 (0.095)	-0.025 (0.085)	-0.140 (0.123)
Born 2009 x Treated	0.101 (0.257)	0.106 (0.206)	0.070 (0.115)	0.015 (0.111)	-0.049 (0.075)	-0.035 (0.128)
Born 2008 x Treated	0.382* (0.221)	0.423** (0.196)	-0.028 (0.116)	-0.078 (0.142)	-0.045 (0.078)	0.085 (0.111)
Born 2007 x Treated	0.441 (0.337)	0.461* (0.247)	-0.001 (0.118)	-0.085 (0.150)	-0.058 (0.070)	0.158 (0.119)
Born 2006 x Treated	0.582** (0.241)	0.308* (0.177)	-0.068 (0.115)	-0.057 (0.147)	0.023 (0.077)	
District FE	X	X	X	X	X	X
Birth Year FE	X	X	X	X	X	X
Age FE						X
Control Group DV Mean	0.965	0.959	0.857	0.888	0.215	0.889
Observations	126,286	131,971	107,297	107,100	134,949	170,054

Notes: The data used for the estimates in columns (1)-(4) comes from the 2018 ASER and contains first born children ages 3-12. The data used in column (5) comes from the 2019-2021 DHS and contains first born children born between 2007 and 2016 (approx. ages 3-12). Standard errors are clustered at the district level. Ever enrolled is an indicator for ever being enrolled in school or preschool, currently enrolled is an indicator for currently being enrolled in school or preschool, any reading competency is an indicator for a child having any non-zero level of reading competency (only for children ages 5-12), any math competency is an indicator for a child having any non-zero level of math competency (only for children ages 5-12), and current enrollment in the DHS is an indicator for being currently enrolled in school or preschool. A probit regression model is used to estimate the coefficients in this table. The control group mean includes children across all age cohorts, born both before and after the program started. \*p<0.1, \*\*p<0.05, \*\*\*p<0.01.

Table B3. Two-by-two DiD Estimates for Individual-Level Outcomes

	ASER Data				DHS Data
	Ever Enrolled	Currently Enrolled	Any Reading Competency	Any Math Competency	Currently Enrolled
	(1)	(2)	(3)	(4)	(5)
Treated x Post	0.019** (0.009)	0.017* (0.009)	0.003 (0.020)	0.003 (0.019)	-0.007 (0.014)
District FE	X	X	X	X	X
Birth Year FE	X	X	X	X	X
Age FE					X
Control Group DV Mean	0.965	0.959	0.857	0.888	0.889
Observations	145,653	145,653	107,430	107,100	170,244

Notes: The data used for the estimates in columns (1)-(4) comes from the 2018 ASER and contains first born children ages 3-12. The data used in column (5) comes from the 2019-2021 DHS and contains first born children born between 2007 and 2016 (approx. ages 3-12). Standard errors are clustered at the district level. Ever enrolled is an indicator for ever being enrolled in school or preschool, currently enrolled is an indicator for currently being enrolled in school or preschool, any reading competency is an indicator for a child having any non-zero level of reading competency (only for children ages 5-12), any math competency is an indicator for a child having any non-zero level of math competency (only for children ages 5-12), and current enrollment in the DHS is an indicator for being currently enrolled in school or preschool. The control group mean includes children across all age cohorts, born both before and after the program started. \*p<0.1, \*\*p<0.05, \*\*\*p<0.01.

Table B4. Estimates for Individual-Level Outcomes, Entire Pre-period Aggregated

	ASER Data				DHS Data
	Ever Enrolled	Currently Enrolled	Any Reading Competency	Any Math Competency	Currently Enrolled
	(1)	(2)	(3)	(4)	(5)
Born 2016 x Treated					0.076* (0.042)
Born 2015 x Treated	0.080*** (0.030)	0.077*** (0.029)			0.025 (0.038)
Born 2014 x Treated	0.019 (0.020)	0.014 (0.020)			0.018 (0.035)
Born 2013 x Treated	0.009 (0.009)	0.008 (0.009)	0.013 (0.031)	0.026 (0.031)	0.009 (0.011)
Born 2012 x Treated	0.004 (0.004)	0.002 (0.004)	-0.009 (0.028)	-0.010 (0.027)	-0.004 (0.005)
Born 2011 x Treated	0.001 (0.003)	-0.002 (0.003)	0.006 (0.015)	-0.004 (0.013)	-0.011* (0.006)
District FE	X	X	X	X	X
Birth Year FE	X	X	X	X	X
Age FE					X
Control Group DV Mean	0.965	0.959	0.857	0.888	0.889
Observations	145,653	145,653	107,430	107,100	170,244

Notes: The data used for the estimates in columns (1)-(4) comes from the 2018 ASER and contains first born children ages 3-12. The data used in column (5) comes from the 2019-2021 DHS and contains first born children born between 2007 and 2016 (approx. ages 3-12). Standard errors are clustered at the district level. Ever enrolled is an indicator for ever being enrolled in school or preschool, currently enrolled is an indicator for currently being enrolled in school or preschool, any reading competency is an indicator for a child having any non-zero level of reading competency (only for children ages 5-12), any math competency is an indicator for a child having any non-zero level of math competency (only for children ages 5-12), and current enrollment in the DHS is an indicator for being currently enrolled in school or preschool. The control group mean includes children across all age cohorts, born both before and after the program started. \*p<0.1, \*\*p<0.05, \*\*\*p<0.01.

Table B5. Impact of Rainfall Shocks on Education at the Individual Level

	ASER Data			
	Ever Enrolled	Currently Enrolled	Any Reading Competency	Any Math Competency
		(1)	(2)	(3)
Rainfall Shock in First 3 Years of Life	-0.005 (0.004)	-0.005 (0.004)	-0.011** (0.005)	-0.017*** (0.006)
District FE	X	X	X	X
Birth Year FE	X	X	X	X
Age FE				
Control Group DV Mean	0.970	0.970	0.894	0.866
Observations	145,653	145,653	107,100	107,430

Notes: The data used for the estimates in columns (1)-(4) comes from the 2018 ASER and contains firstborn children ages 3-12. Rainfall data comes from the University of Delaware. A rainfall shock is defined as a year with rainfall below the 15th percentile of the district-specific gamma distribution. Standard errors are clustered at the district level. Ever enrolled is an indicator for ever being enrolled in school or preschool, currently enrolled is an indicator for currently being enrolled in school or preschool, any reading competency is an indicator for a child having any non-zero level of reading competency (only for children ages 5-12), and any math competency is an indicator for a child having any non-zero level of math competency (only for children ages 5-12). The control group mean includes children across all age cohorts, born both before and after the program started. \*p<0.1, \*\*p<0.05, \*\*\*p<0.01.

Table B6. Impact of Program on Education at the Individual Level, by Rainfall Shocks

<b>PANEL A: ASER Data - No Rainfall Shock in First 1000 Days of Life</b>				
	<b>Ever Enrolled</b>	<b>Currently Enrolled</b>	<b>Any Reading</b>	<b>Any Math</b>
	(1)	(2)	(3)	(4)
Born 2015 x Treated	0.077** (0.034)	0.078** (0.034)		
Born 2014 x Treated	0.012 (0.025)	0.009 (0.027)		
Born 2013 x Treated	0.014 (0.012)	0.016 (0.013)	0.037 (0.042)	0.033 (0.040)
Born 2012 x Treated	0.009 (0.007)	0.009 (0.008)	-0.019 (0.035)	-0.033 (0.035)
Born 2011 x Treated	0.006 (0.006)	0.005 (0.007)	0.012 (0.021)	-0.002 (0.014)
Born 2009 x Treated	-0.003 (0.003)	-0.002 (0.004)	0.006 (0.019)	-0.006 (0.012)
Born 2008 x Treated	0.003 (0.005)	0.005 (0.005)	0.002 (0.019)	-0.005 (0.014)
Born 2007 x Treated	-0.002 (0.004)	0.003 (0.004)	-0.004 (0.019)	-0.017 (0.015)
Born 2006 x Treated	-0.000 (0.003)	0.006 (0.006)	-0.016 (0.018)	-0.023* (0.013)
District FE	X	X	X	X
Birth Year FE	X	X	X	X
Age FE				
Control Group DV Mean	0.969	0.963	0.864	0.893
Observations	96,599	96,599	73,006	72,774
<b>PANEL B: ASER Data - Rainfall Shock in First 1000 Days of Life</b>				
Born 2015 x Treated	0.081 (0.050)	0.080 (0.050)		
Born 2014 x Treated	0.022 (0.030)	0.022 (0.029)		
Born 2013 x Treated	0.010 (0.012)	0.011 (0.012)	-0.032 (0.045)	-0.032 (0.044)
Born 2012 x Treated	0.020 (0.013)	0.021 (0.014)	-0.012 (0.053)	-0.034 (0.050)
Born 2011 x Treated	0.019* (0.011)	0.020 (0.013)	-0.009 (0.034)	-0.043 (0.028)
Born 2009 x Treated	0.013 (0.012)	0.013 (0.012)	0.001 (0.022)	0.006 (0.018)
Born 2008 x Treated	0.022 (0.013)	0.029* (0.016)	-0.036* (0.019)	-0.020 (0.019)
Born 2007 x Treated	0.031 (0.020)	0.034 (0.025)	-0.025 (0.019)	-0.012 (0.021)
Born 2006 x Treated	0.023 (0.018)	0.020 (0.020)	-0.015 (0.022)	-0.002 (0.019)
District FE	X	X	X	X
Birth Year FE	X	X	X	X
Age FE				
Control Group DV Mean	0.957	0.952	0.842	0.876
Observations	49,054	49,054	34,424	34,326

Notes: The data used for the estimates in columns (1)-(4) comes from the 2018 ASER and contains firstborn children ages 3-12. Rainfall data comes from the University of Delaware, and the sample is split based on whether the child experienced a year with rainfall below the 15th percentile of the district-specific gamma distribution within the first three years of life. Standard errors are clustered at the district level. Ever enrolled is an indicator for ever being enrolled in school or preschool, currently enrolled is an indicator for currently being enrolled in school or preschool, any reading competency is an indicator for a child having any non-zero level of reading competency (only for children ages 5-12), and any math competency is an indicator for a child having any non-zero level of math competency (only for children ages 5-12). The control group mean includes children across all age cohorts, born both before and after the program started. \*p<0.1, \*\*p<0.05 <sup>B17</sup>\*p<0.01.

Table B7. Impact of Program on Education at the Individual Level, First & Second Born

	ASER Data				
	Ever Enrolled	Currently Enrolled	Any Reading Competency	Any Math Competency	Birth Parity
	(1)	(2)	(3)	(4)	(5)
Born 2016 x Treated					
Born 2015 x Treated	0.078 (0.051)	0.076 (0.052)			-0.002 (0.035)
Born 2014 x Treated	0.059* (0.035)	0.058* (0.035)			-0.015 (0.030)
Born 2013 x Treated	0.016 (0.025)	0.013 (0.026)	0.026 (0.042)	0.028 (0.045)	-0.003 (0.022)
Born 2012 x Treated	0.007 (0.014)	0.001 (0.014)	0.023 (0.037)	-0.020 (0.034)	0.003 (0.024)
Born 2011 x Treated	0.009 (0.009)	0.003 (0.011)	0.038 (0.038)	0.037 (0.028)	0.005 (0.024)
Born 2009 x Treated	0.005 (0.007)	0.003 (0.007)	0.050* (0.030)	0.002 (0.022)	0.024 (0.027)
Born 2008 x Treated	0.008 (0.007)	0.015** (0.007)	0.035 (0.028)	0.010 (0.024)	-0.018 (0.022)
Born 2007 x Treated	0.004 (0.008)	0.006 (0.010)	0.028 (0.032)	0.017 (0.024)	-0.022 (0.028)
Born 2006 x Treated	-0.002 (0.011)	0.015 (0.015)	0.049 (0.035)	0.035* (0.021)	0.021 (0.035)
District FE	X	X	X	X	X
Birth Year FE	X	X	X	X	X
Age FE					
Control Group DV Mean	0.917	0.912	0.715	0.773	1.876
Observations	72,044	72,044	44,896	44,670	322,008

Notes: The data used for the estimates in columns (1)-(4) comes from the 2018 ASER and contains first and second born children ages 3-12. Standard errors are clustered at the district level. Ever enrolled is an indicator for ever being enrolled in school or preschool, currently enrolled is an indicator for currently being enrolled in school or preschool, any reading competency is an indicator for a child having any non-zero level of reading competency (only for children ages 5-12), and any math competency is an indicator for a child having any non-zero level of math competency (only for children ages 5-12). The data used for the estimates in column (5) comes from the 2018 ASER and contains first through fourth born children. Birth parity is a measure of the birth order. The control group mean includes children across all age cohorts, born both before and after the program started. \*p<0.1, \*\*p<0.05, \*\*\*p<0.01.

Table B8. Impact of Program on Education at the Individual Level, Nationwide Rollout

	ASER Data			
	Ever Enrolled	Currently Enrolled	Any Reading Competency	Any Math Competency
	(1)	(2)	(3)	(4)
3 years old x Post	0.059*** (0.010)	0.057*** (0.010)		
4 years old x Post	0.024*** (0.006)	0.024*** (0.006)		
5 years old x Post	0.006** (0.003)	0.006** (0.003)	-0.011 (0.010)	0.000 (0.010)
7 years old x Post	-0.003 (0.002)	-0.003 (0.002)	0.011 (0.007)	0.014** (0.007)
8 years old x Post	-0.003 (0.002)	-0.003* (0.002)	0.015* (0.008)	0.021*** (0.008)
9 years old x Post	-0.005*** (0.002)	-0.005** (0.002)	0.017** (0.008)	0.026*** (0.008)
10 years old x Post	-0.002 (0.002)	0.001 (0.002)	0.024*** (0.008)	0.028*** (0.008)
11 years old x Post	-0.003* (0.002)	-0.002 (0.002)	0.025*** (0.008)	0.029*** (0.008)
12 years old x Post	-0.003* (0.002)	0.007*** (0.002)	0.029*** (0.008)	0.031*** (0.008)
District FE	X	X	X	X
Year FE	X	X	X	X
Child Age FE	X	X	X	X
Control Group DV Mean	0.965	0.959	0.857	0.888
Observations	326,105	326,105	251,942	251,652

Notes: The data used for the estimates in columns (1)-(4) comes from the 2018 and 2022 waves of the ASER and contains first born children ages 3-12. Standard errors are clustered at the district level. Ever enrolled is an indicator for ever being enrolled in school or preschool, currently enrolled is an indicator for currently being enrolled in school or preschool, any reading competency is an indicator for a child having any non-zero level of reading competency (only for children ages 5-12), and any math competency is an indicator for a child having any non-zero level of math competency (only for children ages 5-12). The control group mean includes children across all age cohorts, born both before and after the program started. \*p<0.1, \*\*p<0.05, \*\*\*p<0.01.

Table B9. Impact of Program on School Enrollment at the Individual Level

	Ever Enrolled: ASER Data		
	5-year-olds	6-year-olds	7-year-olds
	(1)	(2)	(3)
Surveyed 2022 x Treated	0.007 (0.008)	0.006** (0.002)	-0.001 (0.004)
Surveyed 2018 x Treated	0.008 (0.010)	0.005 (0.005)	-0.001 (0.005)
Surveyed 2016 x Treated	-0.003 (0.004)		
Surveyed 2014 x Treated		-0.002 (0.002)	0.001 (0.005)
Surveyed 2013 x Treated	-0.004 (0.006)	-0.001 (0.002)	-0.005 (0.004)
Surveyed 2012 x Treated	-0.008 (0.007)	0.000 (0.004)	-0.011* (0.006)
Surveyed 2011 x Treated	0.012 (0.008)	0.006 (0.004)	0.004 (0.003)
Surveyed 2010 x Treated	0.001 (0.006)	-0.001 (0.005)	0.002 (0.005)
District FE	X	X	X
Survey Year FE	X	X	X
Control Group DV Mean	0.980	0.990	0.989
Observations	102,369	108,596	113,461

Notes: The data comes from the 2010-2022 rounds of the ASER and contains first born children ages 5, 6, and 7, respectively. Standard errors are clustered at the district level. The outcome is an indicator for the individual being enrolled in school, where enrollment is an indicator for ever being enrolled in school or preschool. \*p<0.1, \*\*p<0.05, \*\*\*p<0.01.