Parental Investments and Early Childhood Development: Short and Long Run Evidence from India

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Abstract

The overall impacts of early childhood programs depend on both the direct impacts on exposed cohorts, as well as the indirect impacts that arise due to intra-household reallocation of parental investments. To study these effects, I collected historical administrative data from the rollout of the Integrated Child Development Services program in India, the largest early childhood development program in the world. Children exposed to the program were significantly less likely to be malnourished and more likely to be able to read and do math. Adults exposed to the program when young showed significant improvements in various measures of health. They were also significantly more likely to be literate, employed, and earn a higher wage. However, I show that parents reallocated their investments towards children exposed to an increase in program intensity, as evidenced by negative spillovers on siblings. This crowd-out of investments is particularly severe for girls. Accounting for the negative spillovers on siblings reduces the internal rate of return of the program by approximately 9%.

JEL: I15, O15, I18, I38, D15. **Keywords:** Early childhood development, parental investments, human capital formation, long run impacts, India

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I Introduction

The question of how to build human capital with limited resources remains a key policy problem in developing countries. In India, this problem is particularly salient given the higher rates of stunting and malnutrition in comparison to other developing nations.[1] The Indian government has responded in the form of direct provision of health and education services for children, a strategy also seen in other developing countries. However, little is known about the parental responses to such programs in developing countries. The effects that such programs have on exposed children and their siblings depend on the interaction between investments by the government and parents, in addition to parental preferences and the technology for human capital formation. Parents could reallocate their investments within children over time, or across children. Reallocation of investments by parents so as to reinforce government programs, however, could have spillovers on siblings, leading to important distributional impacts of the policy.

I introduce households to an exogenous source of variation so as to understand the household economics of parental investment responses to investments by the government. I use the rollout of the Integrated Child Development Services (ICDS) program in India, the largest early childhood development program in the world, for this purpose. The Indian government launched the ICDS program in 1975 to help address India's high rates of child malnutrition and has substantially expanded the program in the last decade. The program provides pre-school education and primary healthcare services to children under six years of age. The decades-long expansion since 1975 and detailed administrative data from the ICDS program make it apt for a study of the direct and indirect impacts of early childhood interventions in developing countries.

I study these impacts by constructing a unique dataset merging historical administrative data on the ICDS program that I collected with a large number of household survey datasets. I employ a differences-in-differences strategy that exploits variation in the timing of program expansion across Indian districts. Children exposed to the program showed significant improvements in health and education. They were significantly less likely to be underweight, or have very low weight-for-age, and were more likely to be able to read or do math. These effects are persistent - adults exposed to the program

[1] For example, 38% of children under five years of age are stunted in India, a number larger than many countries with similar incomes (World Bank, 2015).

when young showed significant improvements along a range of health, education, and labor market outcomes. These health measures include objective measures of blood iron deficiency and blood glucose, as well as subjective measures of general health. Adults exposed to the program were also significantly more likely to be literate, have more years of schooling, be employed, and earn a higher wage. They were more likely to engage in healthy behaviors, reflected in lower alcohol consumption, smoking, and tobacco use. In general, women showed greater improvements than men.

Parents can respond to the investments made by the government in the form of intra-household as well as inter-temporal substitution of investments. I use a two period, two child model to illustrate the trade-offs between inequality averse parental preferences and the production technology for human capital that is a function of investments in children over time. Government programs that are complementary to parental investments raise the marginal utility of investing in children. Parents respond by increasing investments in children exposed to an increase in program intensity. However, this crowds out investments for siblings of children exposed to an increase in program intensity.

I provide evidence that parents reallocate their investments towards children exposed to an increase in program intensity, leading to negative sibling spillovers. I show this directly using measures of investments in nutrition and education, as well as indirect measures such as a decrease in adult good consumption. This reallocation of investments leads to a worsening of health and education outcomes for the siblings of children exposed to an increase in program intensity. This crowd-out is particularly severe for girls. To obtain a better understanding of the relative magnitude of the direct and indirect impacts, I conduct a cost-benefit analysis of the program. While taking into account only the direct wage and health benefits of the program yields an internal rate of return (IRR) of 8.8% - 9%, an analysis that accounts for both the direct and indirect impacts yields an IRR of 8% - 8.2%, a 9% decrease.

Furthermore, I show that parents intertemporally respond by front-loading their investments in children exposed to an increase in program intensity. I interpret this as an intertemporal *shift* of resources given that parents do not increase employment in response to the program. In particular, mothers do not increase employment along extensive or intensive margins of employment. This suggests that parents have a fixed household budget over the development cycle of their children. I present evidence showing that parents shift resources to earlier ages of the child by taking on more debt.

This paper contributes to several strands of the literature on parental investments in children in developing countries. First, I contribute to prior work on fetal origins and parental responses.[2] The key question in this literature focuses on how parental investments respond to differential health endowments at birth, primarily in the form of differential birth weight. While a small number of studies (Bharadwaj et al, 2013) present evidence of compensating responses, a large number of studies present evidence of reinforcing responses by parents (Aizer and Cunha, 2012 and Venkataramani, 2012). I complement this work by focusing on how parents respond to large, government-provided early childhood programs in developing countries. I build on the work of two papers that are close to mine in this strand of the literature. Attanasio et al. (2018) show that the effects of a randomized early childhood intervention in Colombia can be explained by increases in parental investments. Furthermore, Adhvaryu and Nyshadham (2014) show that parents respond to an iodine supplementation program in Tanzania by breastfeeding both beneficiary children and their siblings longer.

Second, I contribute to the large body of work that documents favoritism in parental investments towards boys in developing countries. Behrman (1988), Barcellos et al. (2014), and Jayachandran and Pande (2017), for example, document differential allocation of resources by gender. I show that the differential gender impact of a large government program can play an important role in counteracting parents' biases in investments in children. Third, my theoretical framework builds on the models introduced by Cunha et al. (2010), Cunha and Heckman (2007) and Almond and Currie (2011) to study the interaction of a government program with parental investments within the framework of inequality-averse parental preferences and a constant elasticity of substitution production function for human capital formation.

This paper also builds on prior work on short-term impact evaluations of the ICDS program. Jain (2015), Chakravarty (2010), and Lokshin et. al (2005) show short-term impacts of the program on immunizations and stunting. I contribute to this work by collecting historical administrative data on the program to determine program exposure for

[2] Almond and Mazumder (2013) provide an excellent summary of studies in this strand of the literature.

individuals at different ages. The unique dataset I assemble allows me to present the first evidence of long-term impacts of the ICDS program, the largest early childhood development program in the world.[3]

I also contribute to several evaluations of early childhood development programs in developing countries and the U.S. In an important contribution, a psychosocial stimulation experiment in Jamaica with 129 participants was shown to have long-term effects on wages (Gertler et al, 2014). Further evidence from Mexico (Parker and Vogl, 2018), Tanzania (Field et al, 2009), and Guatemala (Hoddinott et al, 2008) show that exposure to early childhood programs have long-term impacts[4]. Evaluations of major early childhood programs in the U.S. have been conducted for the Perry Preschool Program (Heckman et al, 2013), two early childhood randomized trials in North Carolina (Garcia et al, 2016), and the Head Start program (Carneiro and Ginja, 2014). These studies collectively show that early childhood interventions can have significant impacts on adult outcomes including education, employment, earnings, marriage, health, participation in healthy behaviors, and reduced participation in crime. Notably, the ICDS program is significantly larger than other early childhood development programs in the world: while Head Start has served on average 0.55 million children per year, the ICDS served 40 million children in 2010 alone and has been operating for more than 40 years.

The remainder of this paper is organized as follows. I describe the program in greater detail in Section II. Section III presents a two period, two child model to illustrate the trade-offs between parental preferences and the technology of skill formation. Section IV describes the datasets used and presents various summary statistics, trends, and heat maps. Section V discusses the empirical strategy employed and Section VI presents the results from these specifications for cohorts exposed to the program. Section VII discusses the evidence on intra-household reallocation of parental investments, while Section VIII presents evidence of intertemporal reallocation to earlier ages of child development. Section IX studies program impacts on parental employment and wages. Section X describes a cost-benefit analysis of the program, and Section XI concludes.

[3] I do not use household survey data to determine ICDS coverage. Given large measurement error in determining when ICDS centers were opened in the household survey data, an analysis that exploits exposure to the program at particular ages is difficult. In the India Human Development Survey, for example, respondents are asked the number of years that have elapsed since the opening of the nearest ICDS center. A simple plot of the data reveals significant bunching at five-year intervals.

[4] Currie and Vogl (2013) provide an excellent summary of key interventions in developing countries.

II Context and Program Details

The ICDS was launched in 1975 by the Indian government to provide pre-school education and primary healthcare services to children under 6 years of age. There are several health components under the scheme, including immunizations, supplementary nutrition, health checkups, referral services and provision of health information. ICDS centers also provide prenatal services and supplementary nutrition to pregnant mothers. While launched in 1975 primarily with funding from UNICEF, the Indian government has been expanding the program over the last 40 years. Today, the program is large – by 2010, the program reached about 25% of all children in India under the age of 6 (39.7 million children), and during the 2018-19 fiscal year alone, the Indian government is expected to spend Rs. 230 billion (US $3.2 billion) on the program.

ICDS centers are also known as courtyard play centers, given the physical infrastructure of the centers. Centers typically consist of a room for indoor activities, and open space for outdoor activities. ICDS centers are typically run for 3.5 hours a day, after which the ICDS worker conducts two or three home visits for about an hour. Guidelines on typical daily tasks and the corresponding time allocation are given in Table 1.

[Table 1 about here.]

ICDS centers are typically open from morning to the early afternoon, although there is significant variation in the hours of operation across India. In addition to these daily tasks, ICDS workers also conduct health check-ups, immunizations and height and weight growth monitoring on a monthly basis.

The construction of ICDS centers was to be based on population guidelines, which have changed over the years.[5] Prior to 2009, population guidelines stipulated that there should be one ICDS center for every 1,500 people. In 2008, this rule was changed such that for 2009 and later, there should be one ICDS center for every 800 people. The change in the rule was motivated in large part by the Supreme Court ruling of 2006 that called for an expansion of the program. There were no other explicit targeting criteria for construction of ICDS centers. I show in Appendix D that program placement was not correlated

[5] Unfortunately, these guidelines could not be used for identification using a fuzzy RD design, given large heterogeneity across India in the population figures used for funding requests and the time taken for construction of the centers.

with existing village infrastructure.

There has been significant variation in the expansion of the ICDS program over time, as well as across districts in India. I begin by plotting the expansion of the ICDS program in India over time in Figure 1. While the number of centers has been expanding steadily over time, it has also kept up with population growth. Specifically, the number of centers per capita has also been climbing steadily over time. There was approximately one center for every 1,500 people around 1995, 20 years after the start of the program. Given that the target population rule had been achieved, program intensity stayed close to this level until 2006, when the Supreme Court called for a renewed expansion of the program. The new population guidelines of one center for every 800 people were introduced in 2008, after which there was a significant expansion of the program.

Figure 2 presents a number of heat maps showing variation in the number of centers per capita over geographic space and time. The maps illustrate program intensity 10, 20, 30, and 40 years since 1975, the start of program implementation. The heat maps confirm that there is substantial variation in the number of centers per capita across districts in India over time. I note, however, that the number of centers per capita remained consistently low in several states including Gujarat, Maharashtra, and Odisha, over the entire time period. I will explicitly take this into account in my empirical strategy in Section V.

[Figure 1 about here.]

[Figure 2 about here.]

[Figure 3 about here.]

III A Two Period, Two Child Model of Parental Investments

In this section, I set up a two period, two child model of parental investments to illustrate the trade-offs between preferences and the production technology for human capital. After describing parental preferences and the production technology in this environment, I model the introduction of a government program. I then derive theoretical predictions that can empirically be tested in my setting.

III.A Preferences and Production Technology

This section builds on the parental investments framework introduced by Almond and Currie (2011). Within-family investment decisions depend on the tension between parental preferences and the production technology for human capital. For parental preferences, I use a constant elasticity of substitution (CES) function that allows me to flexibly capture inequality aversion among children. I assume that parents only care about the human capital of their two children. $h_a \geq 0$ and $h_b \geq 0$ refer to human capital at the completion of childhood for each child $c \in \{a, b\}$, respectively. $\theta > 0$ implies a low degree of inequality aversion, i.e. parents view their children as substitutes, while $\theta < 0$ implies a high degree of inequality aversion where parents view their children as complements.

$$U_P = [\beta h_a^\theta + (1-\beta) h_b^\theta]^{\frac{1}{\theta}}$$
$$h_c = F_c[\gamma I_{1c}^\phi + (1-\gamma) I_{2c}^\phi]^{\frac{1}{\phi}} \quad \forall\, c \in \{a, b\}$$

As suggested by Cunha and Heckman (2007), I use a constant elasticity of substitution (CES) function that allows me to characterize the production technology for early childhood development in a flexible manner. $I_{1c} \geq 0$ and $I_{2c} \geq 0$ refer to child-specific investments by parents in childhood development periods 1 and 2, respectively. F_c denotes the factor productivity of each child in the production function. $\phi > 0$ implies that substitution of parental investments between period 1 and period 2 is relatively easy, while $\phi < 0$ implies that substitution is relatively difficult. I consider the period of potential exposure to the ICDS program as period 1 (ages -1 to 6), and a later period of childhood (ages 7 to 13) as period 2. In setting up the problem with a CES preference structure and CES production technology, the problem has a nested CES structure.

Parents can take on debt $D \geq 0$ so as to move resources from period 2 to period 1. I assume that income in each period is exogenously determined, and is given by $\bar{y}_1 > 0$ and $\bar{y}_2 > 0$ in periods 1 and 2, respectively (the validity of these assumptions will be tested empirically in Section IX). Denoting by r the interest rate between periods 1 and 2, the household's budget constraint in each period is given by:

$$\text{Period 1: } I_{1a} + I_{1b} \leq \bar{y}_1 + D$$
$$\text{Period 2: } I_{2a} + I_{2b} \leq \bar{y}_2 - \frac{D}{1+r}$$

III.B Introducing Program Exposure

I consider the effect of exogenous positive shocks $\mu_g > 0$ to investments in the first period of childhood - these shocks capture exposure to the ICDS program. In many ways, investments by parents and the ICDS program are complements. For example, ICDS workers perform door-to-door visits and stress the importance of child nutrition to parents, urging them to feed their children adequately, while also doing so in ICDS centers. Furthermore, ICDS workers often deliver pre-school services to children in small groups tailored to their level of language development, motor skills, and cognitive development (Ministry of Women and Child Development, 2017). This allows ICDS centers to build upon pre-school investments made by parents in a complementary manner.

Hence I choose to model investments by parents and the ICDS program as complements - the ICDS shock μ_g enters multiplicatively with period 1 parental investments I_{1c} in period 1. I also allow first period investments I_{1c} to respond to μ_g. Thus, the technology of human capital formation for child a and b is as follows:

$$h_a = F_a[\gamma(I_{1a} * \mu_g)^{\phi} + (1-\gamma)I_{2a}^{\phi}]^{\frac{1}{\phi}}$$
$$h_b = F_b[\gamma I_{1b}^{\phi} + (1-\gamma)I_{2b}^{\phi}]^{\frac{1}{\phi}}$$

Without loss of generality, I normalize the ICDS exposure shock of child b to 1, such that $\mu_g > 1$ captures the *ratio* of ICDS exposure of child a relative to child b. Parents then solve the following maximization problem:

$$\max_{I_{1a}, I_{1b}, I_{2a}, I_{2b}} U_P$$
$$\text{subject to budget constraint: } I_{1a} + I_{1b} + \frac{I_{2a} + I_{2b}}{1+r} \leq \bar{y}_1 + \frac{\bar{y}_2}{1+r}$$

III.C Theoretical Predictions

For $0 < \theta < \phi < 1$, I obtain the following theoretical predictions (see Appendix A for proofs):

1. Human Capital. (a) Human capital at the end of the development cycle will increase as children are exposed to an increase in program intensity, i.e. $\frac{\partial h_a}{\partial \mu_g} > 0$. h_a increases due to (i) the direct effect of relative program exposure μ_g, (ii) the intertemporal reallocation of investments *within* child a, and (iii) the intra-household reallocation of investments away from child b towards child a. **(b)** Human capital at the end of the development cycle will decrease for the siblings of children exposed to an increase in program intensity, i.e.

$\frac{\partial h_b}{\partial \mu_g} < 0$. h_b decreases due to the intra-household reallocation of investments away from child b in both periods.

2. Intra-household Reallocation of Investments. Parents will decrease period 1 and period 2 investments in the sibling of the child that is exposed to an increase in ICDS program intensity: $\frac{\partial I_{1b}}{\partial \mu_g} < 0$ and $\frac{\partial I_{2b}}{\partial \mu_g} < 0$. This result is driven by the fact that when $\theta > 0$, parents view their children as substitutes (recall that $\theta = 1$ represents the case of linear utility, i.e. perfect substitutes). Parents choose to reinforce the government program by investing more in the child exposed to an increase in program intensity.

3. Intertemporal Reallocation of Investments. Parents will increase period 1 investments and decrease period 2 investments as their child is exposed to an increase in ICDS program intensity: $\frac{\partial I_{1a}}{\partial \mu_g} > 0$ and $\frac{\partial I_{2a}}{\partial \mu_g} < 0$. The period 1 result is driven by the fact that period 1 investments and ICDS investments are complements, and substitution of parental investments between period 1 and period 2 is relatively easy ($\phi > 0$). As a result, it would be optimal for parents to increase period 1 investments as their child is exposed to a relative increase in ICDS program intensity. For period 2, there are two opposing effects of relative program exposure on period 2 investment in child a. When θ is high, parents view their children as substitutes and will try to maximize the outcomes for child a. This will have the effect of increasing I_{2a}. However, when ϕ is large, substitution of investments between periods is relatively easy. Consequently, parents will move resources away from period 2 towards period 1, so as to take advantage of the complementarity of their investments with program exposure. This will have the effect of decreasing I_{2a}. When the latter effect dominates, i.e. when $\phi > \theta$, parents will reduce period 2 investments in child a.

4. Debt. Parents will take on more debt in response to an increase in ICDS program intensity: $\frac{\partial D}{\partial \mu_g} > 0$. Parents borrow to cover the difference between period 1 expenses (investments) and income. Since parents decrease investments in both children in period 2, this must imply that parents borrow more in response to an increase in ICDS program exposure, so as to shift resources to period 1.

IV Data

[Table 2 about here.]

In this section, I describe the various datasets that I use for my analysis. To study the various impacts of the program, I put together a large number of datasets as described in Table 2. All datasets are merged at the district sub-division level - by district sub-division, I refer to the distinction between rural and urban areas of a district. This is the finest geographic level at which the datasets, including the administrative ICDS data, are identified. The NFHS, IHDS, NSS, and ASER datasets contain individual-level data. For adults, I exploit data on the location of birth to ensure that individuals are assigned the appropriate level of program exposure according to their location and time of birth. Unfortunately, this data is not available for all children, in particular when using the NSS and ASER datasets. Given the low rates of migration across district sub-divisions by children, however, I include all children in the analysis.

First, and most importantly, I collected historical administrative data on the universe of all ICDS centers since the inception of the program. This data was collected in coordination with the Ministry of Women and Child Development, India. The rich dataset contains details on the location of the centers, the year of opening, and the types of services that each center provides. The dataset has information on the universe of 964,165 centers across India.

The National Family Health Survey (NFHS) is also known as the Demographic Health Survey (DHS) for India. I utilize three rounds of the survey, in particular, rounds 1 (1992-1993), 2 (1998-1999), and 4 (2015-2016). Identifiers below the state level were not released for round 3 of the data due to HIV testing, and hence this round of data was not used. The NFHS is the key source of data for education, health, and healthy behaviors of individuals in my analysis.

The third dataset I use is the India Human Development Survey (IHDS), conducted by the University of Maryland. Although this is a panel dataset with two rounds of data, I only use one round of data so as to avoid inclusion of the same individual more than once in the analysis. I chose to use round 2 of the data so as to include a greater number of exposed cohorts of adults in the analysis. The IHDS dataset is nationally representative, with detailed data on approximately 200,000 individuals households across India. I obtain information on wages and debt from the IHDS and also obtain additional health measures to supplement the NFHS data.

The National Sample Survey (NSS) is the largest nationally representative household survey in India. The employment schedule of the survey details daily employment and hours of work for all working household members in a given week. I use the NSS employment rounds 55 (1999-2000), 60 (2004), 61 (2004-2005), 62 (2005-2006), 64 (2007-2008), 66 (2009-2010), and 68 (2011-2012) to study parents' employment and child labor. The education expenditure schedule of the survey details expenses on education by parents. I use rounds 64 (2007-2008) and 71 (2014) of the survey to study education expenditure.

The Annual Status of Education Report (ASER) is the largest citizen-led survey in India and is facilitated by the Pratham NGO network. This dataset contains reading and math test score data of children in rural areas across India. The dataset is large, with approximately five million observations across the years 2006-2014, and includes both in and out of school children. Each child is asked four questions each in math and reading in their native language. The four math questions are whether the child can recognize numbers 1-9, recognize numbers 10-99, subtract, and divide. The scores are recorded as 1 if the child correctly answers the question, and 0 otherwise. The four reading questions are whether the child can recognize letters, recognize words, read a paragraph, and read a story. I calculate math and reading scores by summing the scores of the four math and reading questions, respectively.

In addition to the survey datasets described above, I use two additional datasets for the analysis. Given the importance of population in determining the number of centers individuals are exposed to, I use population data from the Census of India. The Census of India is conducted every 10 years, and population figures for non-census years are calculated at the district sub-division level by interpolation. I also obtain annual rainfall data from the University of Delaware. The dataset covers all of India between 1900 - 2014. The data is gridded by longitude and latitude lines, so to match these to districts, I use the closest point on the grid to the center of the district and assign that level of rainfall to the district for each year.

Notably, the datasets that I assemble for this project have a number of advantages. First, the location of birth information available in the NFHS and IHDS datasets allows me to determine the program intensity for adults in their location of birth at the time of birth. Consequently, migration of individuals is not a concern when I study long-term program impacts. Second, I use objective measures of health, available from the NFHS biomarker (blood work) data. This helps to overcome interpretation issues that arise when using subjective measures of health. Third, the datasets that I use are large - the ASER dataset, for example, contains more than five million observations. This allows me to obtain precise estimates of the program impact. I do note, however, that the datasets suffer from one disadvantage - the lack of information on program take-up. Consequently, all the analysis that I present will be intent-to-treat (ITT) estimates.

IV.A Summary Statistics

[Table 3 about here.]

Table 3 presents summary statistics for several key variables of interest. Panel A presents summary statistics for child health, education, and labor outcomes. An individual is defined as underweight (stunted) if her weight-for-age (height-for-age) z-score was more than two standard deviations below zero, in accordance with tables developed by the World Health Organization (2007). A large, 32% of children are underweight and 31% of children are stunted. For test scores, I use the ASER dataset. This dataset is large, with approximately five million observations on reading and math test scores. On average, 89% of children can read and do some basic math. From the NSS, I note that 2% of children aged 7-13 are engaged in some form of child labor.[6] For the analysis, I standardize child weight, height, and test scores using the mean and standard deviation of individuals in district sub-divisions without the program.

In Panel B, I present summary statistics for adult health, healthy behaviors, education, and labor. I use two objective measures of health, namely blood hemoglobin and blood glucose levels. A low blood hemoglobin level, or anemia, is an important indicator of iron deficiency. Similarly, hypoglycemia, or a low blood glucose level, is an important indicator of under-nutrition. While a blood glucose level of 70 - 140 mg/dL is considered normal, individuals with blood glucose less than 70 mg/dL are considered hypoglycemic. 14% of individuals suffered from some type of short-term illness including fever, cough, and diarrhea in the last 30 days. Difficulties with activities of daily living (ADL) include difficulties with the ability to speak, hear, or walk normally, and thus reflect long-term

[6] The Child Labor (Prohibition and Regulation) Act of 1986 banned employment of children under the age of 14 (Bharadwaj et al, 2017).

health problems. I supplement these measures with a subjective health score that is self-reported by a sub-group of female IHDS respondents on a scale of 1 to 5. On average, individuals reported a score of 2. Individuals responding with a value of 1 were coded as being in very poor health.

I also consider two common "healthy behaviors" measured in the NFHS - the absence of smoking and drinking alcohol. 21% of individuals in the sample smoke or use tobacco, while 11% report drinking alcohol. 67% of adults on average were literate, with six years of education. On average, individuals earned an hourly wage of 25 rupees (2012 prices), approximately USD 50 cents.

Panel C presents summary statistics on several direct measures of parental investments. Only 18% of parents provided their children with any tuition. 43% of parents participated in Parent-Teacher Associations. Lastly, Panel D presents summary statistics on the employment of parents of children aged 0-6. 97% of fathers are employed, while only 31% of mothers are employed. Mothers on average worked only 1.7 days per week - conditional on being employed, however, they worked an average of 5.5 days per week.

V Empirical Strategy

To exploit the large variation in program intensity across geographic space and time, I use a differences-in-differences empirical strategy. This strategy exploits differences across district sub-divisions in program intensity along with differences across cohorts induced by timing of program arrival in district sub-divisions. In particular, for individual i aged a in district sub-division j of state s and birth year k, I run the following differences-in-differences specification:

$$Y_{ijka} = \alpha + \gamma_j + \lambda_{ks} + \beta P_{jka} + X_{ijk}\delta + \epsilon_{ijka} \tag{1}$$

where Y_{ijka} is the outcome variable of interest, γ_j are district sub-division fixed effects, λ_{ks} are cohort x state fixed effects, P_{jka} refers to the intensity of the program (number of centers per 1,000 children) in the district sub-division of birth at the time of birth, and X_{ijk} are controls including gender, birth order, gender x birth order interaction, a quadratic population polynomial, caste, religion, and mother's education. Although the policy variable based on the population guidelines is the number of centers per capita, I scale this policy variable to the number of centers per 1,000 children to be consistent with the literature and for ease of interpretation. β is the coefficient of interest. Note that by including cohort x state fixed effects, I only compare across district sub-divisions individuals born in the same year and living in the same state. The cohort x state fixed effects effectively control for any state-year level characteristics that might have affected program placement. This is important, given that the series of heat maps presented in Section II showed that program intensity remained consistently low in several states over the time period considered. I present results that augment this baseline specification with village fixed effects in Appendix E and state-specific time trends in Appendix F.

To account for the fact that individuals are treated by the program over 8 years from age -1 (prenatal care) to 6, P_{jka} is constructed as the average program intensity over these ages. $\widetilde{P_{jk}}$ refers to the program intensity in any given year. In cases where the individual is aged a where $a < 6$, $k + a$ is used as the upper bound in the sum above, and the average is taken over the corresponding ages.

$$P_{jka} = \begin{cases} \frac{\sum_{y=k-1}^{k+6} \widetilde{P_{jk}}}{8} \text{ if } a \geq 6 \\ \frac{\sum_{y=k-1}^{k+a} \widetilde{P_{jk}}}{a+1} \text{ if } a < 6 \end{cases}$$

A key assumption for the differences-in-differences strategy is that of parallel trends. This assumption states that absent the program, outcome variables of interest in treatment and control district sub-divisions should have identical trends. While I cannot directly test this counter-factual, I present results from the following placebo test by age of impact to show that this assumption likely holds (see Appendix B):

$$Y_{ijk} = \alpha + \gamma_j + \lambda_{ks} + \sum_{y=k-m}^{k+q} \beta_y \Delta P_{jy} + \mu_k P_{jk} + X_{ijk}\delta + \epsilon_{ijk} \tag{2}$$

where I include m leads and q lags of year-on-year *changes* in program intensity, while controlling for the level of program intensity in the individual's birth year k. This specification is intentionally different from a standard event-study framework with leads and lags, so as to account for the fact that individuals can benefit from centers built before they are born. This is the reason I control for the level of program intensity in the individual's birth year k. Controlling for this level of program intensity, the *differential* number of centers in years before or after birth should only have an effect over the eligible age range for the program, i.e. ages -1 to 6.

Put differently, the idea behind this placebo test is the following: leads and lags of program intensity should not have an impact on the outcome variables of interest outside of the age range over which individuals should be affected by the program, i.e. outside of the age range -1 to 6. This helps to rule out three major types of concerns: (1) systematic placement of centers in district sub-divisions that were getting better (or worse) over time, (2) anticipation effects from knowing that program intensity would increase in the district sub-division in the near future, and (3) confounding programs that might have been introduced in the same district sub-divisions and at the same time as the ICDS.

To illustrate how this placebo test addresses concerns of type (1) outlined above, consider the scenario in which the program has zero impact, but centers are systematically placed in areas where child outcomes are improving over time. If centers are systematically placed, a worry might be that the regressions would simply pick up the trend, and not the impact of the program. If systematic program placement was indeed driving the results, however, I should see significant coefficients for ages 7 to 10. Plots that only exhibit significant impacts in the age range -1 to 6 thus help alleviate this concern.

To illustrate how the test addresses concerns of type (2), consider the scenario in which the program actually has zero impact, but parents anticipate that program intensity is going to increase in their district sub-division in the near future. To be specific, take the case of parents considering having a child five years in the future (child is aged -5). Anticipating the increase in program intensity, parents might choose to set aside fewer funds to invest in child health and education, given that these services will be provided by the ICDS in the future. This represents an "income effect" that might have an effect on outcomes of interest after the child is born. If anticipation effects were indeed driving the results, I should observe significant coefficients for ages -5 to -2. Once again, plots that only exhibit significant impacts in the age range -1 to 6 help alleviate this concern.

With regard to concerns of type (3), consider the specific case of the Mid-day Meal Scheme, a program introduced in 2001 by the Indian government to provide free lunches to primary school children.[7] Given that the nutrition provision component of the pro-

[7] Today, the program is covered by the National Food Security Act of 2013.

gram is similar to the supplemental nutrition component of the ICDS, a potential concern might be that I would pick up the impact of the Mid-day Meal Scheme, instead of the ICDS. However, the Mid-day Meal Scheme only affects primary school children, i.e. those aged 6 and above. As such, plots that only exhibit significant impacts in the age range -1 to 6 would not pick up the impact of the Mid-day Meal Scheme. I present additional robustness checks that explicitly control for the rollout of the Mid-day Meal program in Appendix C.

As seen from the large number of datasets I use, I study a large number of variables in this paper to assess program impacts on health, education, and labor market outcomes. Within these categories, I study all relevant variables available in the datasets for which I expect to see program impacts. Several variables were excluded for three reasons. First, several diseases exhibit large non-response in the NFHS data. Questions on tuberculosis, for example, were only answered by 4% of respondents. Second, I might not expect to see significant program impacts on particular diseases such as cancer, a disease for which the causes are still unclear. Thus I omit the study of particular diseases and instead study long-term health using the measure of difficulties with activities of daily living in the IHDS data, which captures long-term issues relating to speech, hearing, and mobility. Third, I omit variables pertaining to contemporaneous program impacts, so as to focus on short and long-run impacts of the program.

All standard errors are clustered by district sub-division. When studying program impacts for exposed cohorts, I also adjust the p-values for multiple hypothesis testing using a Bonferroni-Sankoh procedure that takes into account both the number of outcomes considered within each "family" of outcomes, as well as the correlation between the outcomes.

VI Direct Impacts: Program Impacts for Exposed Cohorts

Theoretical prediction 1(a) in Section III.C states that the human capital of individuals should increase with an increase in program exposure. In this section, I empirically test this theoretical prediction. The program impacts I estimate in this section can arise due to (i) the direct effect of program exposure, (ii) the intertemporal reallocation of parental investments within children exposed to the program, and (iii) the intra-household reallocation of parental investments away from siblings and towards children exposed to an increase in program intensity.

VI.A Short-run Impacts on Children

In this section, I present the short-run impacts of the program on children. I begin by presenting the health impacts of the program in Panel A of Table 4.

[Table 4 about here.]

Columns (1) and (2) present results on weight and height, respectively. Columns (3) and (4) present results on underweight and stunting, respectively. Children who were exposed to an additional ICDS center per 1,000 children in their district sub-division when aged -1 to 6 were 0.013 standard deviations heavier in their youth. Furthermore, they were 4.5 percentage points less likely to be underweight, on a base of 30.6%. This represents a large, 15% decrease. However, there were no significant impacts on height and stunting.

Panel B of Table 4 presents results on test scores of children. Columns (1) and (2) present results on reading and math scores, respectively. Columns (3) and (4) present results on the ability of children to read and do any math, respectively. While the program did not affect test scores overall, individuals who were exposed to an additional ICDS center per 1,000 children in their district sub-division when aged -1 to 6 were 0.3 percentage points more likely to be able to read and do math, on a base of 90.6%.

Panel C of Table 4 presents results for program impacts on child labor. Children who were exposed to an additional ICDS center per 1,000 children in their district sub-division when aged -1 to 6 were 0.2 percentage points less likely to engage in child labor. On a base of 1.9%, this represents a 9% decrease in child labor.

[Figure 4 about here.]

Figure 4 summarizes impacts of the program for children. All estimates have been standardized using the mean and standard deviation on individuals with no ICDS program exposure. I also display the 95% confidence interval bars corresponding to each point estimate. Overall, the program had significant impacts on underweight and the ability of children to read and do math. While the estimates for underweight and child labor display relatively large standard error bars, the other estimates are relatively precisely estimated. In particular, the estimates for education are very tightly estimated, given the large sample size of more than five million children.

VI.B Long-run Impacts on Adults

In this section, I present the long-term impacts of the program on adults who were exposed to the program when young. I begin by presenting the health impacts of the program in Table 5:

[Table 5 about here.]

Panel A presents outcomes from biomarker data on blood hemoglobin and glucose levels. Panel B presents outcomes from self-reported data on illnesses and general health. Panel C presents two measures of "healthy behaviors" as commonly defined in the literature. In particular, I focus on smoking and tobacco use, as well as consumption of alcohol.

Column (1) of panel A shows that individuals who were exposed to an additional ICDS center per 1,000 children in their district sub-division when aged -1 to 6 had 0.009% higher blood hemoglobin levels as adults. These results are particularly strong at the lower tail of the distribution - column (2) shows that such individuals were 0.2 percentage points less likely to be anemic. Column (3) shows that individuals who were exposed to an additional ICDS center per 1,000 children in their district sub-division when aged -1 to 6 had 0.3% higher blood glucose levels as adults. Furthermore, they were 0.007 percentage points less likely to be hypoglycemic.

While column (1) of panel B shows that individuals exposed to an increase in program intensity were not less likely to experience difficulties with ADL as adults, column (2) shows that these individuals were 0.3 percentage points less likely to suffer from short-term illness. Furthermore, while such individuals were not more likely to report a higher score of general health, they were 0.005 percentage points less likely to report being in very poor health. Taken together, the results on biomarker and self-reported measures of adult health show that the ICDS program had significant long-term impacts on adult health.

Column (1) of panel C shows that individuals exposed to an additional ICDS center per 1,000 children in their district sub-division when aged -1 to 6 were 0.5 percentage points less likely to smoke or use tobacco as adults, on a base of 27.3%. Furthermore, column (2) shows that such individuals were 0.3 percentage points less likely to consume alcohol, on a smaller base of 13.8%. These results show that the ICDS program had significant impacts on long-term healthy behaviors of adults.

[Table 6 about here.]

Columns (1) - (2) and columns (3) - (4) of table 6 presents long-term program impacts on adult education and labor market outcomes, respectively. Column (1) highlights that individuals exposed to an additional ICDS center per 1,000 children in their district subdivision when aged -1 to 6 were 1.2 percentage points more likely to be literate. Furthermore, column (2) shows that such individuals also had 0.06 more years of schooling, on a base of 5.2 years. Column (3) shows that individuals exposed to an additional ICDS center were 0.2 percentage points less likely to be unemployed as adults. They also earned 1.2% more in hourly wages. These results highlight the long-term education and labor market impacts of the program.

[Figure 5 about here.]

Figure 5 summarizes the health, education, and labor market impacts of the ICDS program for adults. All estimates have been standardized using the mean and standard deviation on individuals with no ICDS program exposure. I also display the 95% confidence interval bars corresponding to each point estimate. Overall, the program had significant impacts on health, healthy behaviors, education, and labor market outcomes. While the impacts range from 0.006 - 0.016 standard deviation units for health and labor market outcomes, the impacts on education are slightly larger, in particular 0.024 standard deviation units for literacy.

VI.C Gender Heterogeneity in Program Impacts

[Figure 6 about here.]

[Figure 7 about here.]

Figures 6 and 7 presents results for heterogeneity of program impacts by gender for short-run and long-run outcomes, respectively. In particular, the point estimates represent estimates of the coefficient on an interaction term between female and program intensity. Thus positive point estimates capture additional impacts of the program for females, while negative point estimates represent additional impacts of the program for males. All estimates have been standardized using the mean and standard deviation for individuals with no ICDS program exposure. I also display the 95% confidence interval bars corresponding to each point estimate.

In general, girls and women exposed to an increase in program intensity when young had greater program impacts than boys and men, respectively, across health, education and labor market impacts. For children, the overall program impacts mask important heterogeneity for girls. While estimates for height, reading scores and math scores were not statistically significant for all children, the interaction terms exhibit significant differential impacts for girls. These estimates are also large in magnitude - for adults, in particular, these effect sizes range from 0.009 - 0.037 standard deviation units. Men saw greater program impacts along healthy behaviors of lower alcohol consumption, smoking, and tobacco use, although men were more likely to smoke and consume alcohol than women in general.

Such differential impacts could arise due to (i) differential program take-up by gender, (ii) differences in services provided under the ICDS program by gender, (iii) differences in the production technology for human capital by gender, or (iv) differences in parental preferences by gender. I am not able to cleanly identify these different channels.

VII Indirect Impacts: Intra-Household Reallocation of Investments

Theoretical prediction 1(b) outlined in Section III.C states that siblings of children exposed to an increase in program intensity should have lower human capital. This is because parents shift investments away from siblings and towards the children exposed to an increase in program intensity, as outlined in prediction 2. In this section, I empirically test these predictions. In particular, I consider the impact of having siblings exposed to an increase in program intensity when such siblings were aged -1 to 6 and eligible to receive services from the ICDS program. Owing to difficulties in matching siblings among adults, I choose to focus on child health, education, and labor outcomes in this section.

There are several ways by which parental investments can be measured. These can broadly be categorized into direct and indirect measures of investments. The direct measures of parental investments I use take the form of monetary investments, such as food, education, and tuition on children, or non-monetary time investments, such as time spent with teachers. In general, nutritional investments in children are most important during early ages, i.e. up to six years of age, and educational investments become important when children are of school-going age, i.e. ages 7-13. I supplement these direct measures of investments with an indirect measure of parental investments. Deaton and Subramanian (1991) stress that a household's budget constraint can be exploited to study expenditures on one category of goods based on expenditures on other categories of goods, an idea that dates back to Rothbarth (1943). In particular, the consumption of "adult goods" such as alcohol and tobacco should decrease as investments in children increase. As such, a decrease in adult good consumption can be viewed as an indirect measure suggestive of an increase in investments in children.

To study the impact of having siblings exposed to an increase in program intensity, it is important to control for one's own program exposure. While it is possible to directly control for the program intensity individuals were exposed to, I opt to use district subdivision x cohort x age fixed effects specification that is far more restrictive. The inclusion of these fixed effects absorbs one's own program intensity, rendering it unnecessary to include in the regression. I thus run the following specification for individual i aged a in district sub-division j and birth year k:

$$Y_{ijka} = \alpha + \lambda_{jka} + \beta \tilde{P}_{ijk} + X_{ijk}\delta + \epsilon_{ijka} \tag{3}$$

where Y_{ijka} is the outcome variable of interest, λ_{jka} represent district sub-division x cohort x age fixed effects, $\tilde{P}_{ijk}$ refers to the average program intensity of siblings (number of centers per 1,000 children) in the district sub-division of birth, and X_{ijk} are controls including gender x birth order fixed effects, a quadratic population polynomial, caste, religion, and mother's education. β then captures the impact of having siblings exposed to an increase in ICDS program intensity while controlling for one's own program exposure. The analysis excludes individuals who are the single child of the household. I probe robustness of the results by augmenting this specification with controls for the mid-day meal program in Appendix C, village fixed effects in Appendix E and state-specific time trends in Appendix F.

[Table 7 about here.]

[Table 8 about here.]

Table 7 presents the impacts of an increase in siblings' average program intensity on one's own health, education, and labor market outcomes. Panel A presents the results on child health. There are large negative impacts of an increase in siblings' average program intensity. The point estimates should be interpreted as follows: if hypothetically, it were possible to have all of one's siblings exposed to an additional ICDS center per 1,000 children in the district sub-division at her time of birth, the individual experiences a β standard deviation decrease in outcome variables considered. For example, having all of one's siblings exposed to an additional ICDS center per 1,000 children in the district sub-division at her time of birth is associated with a 0.025 standard deviation decrease in height and weight, as well as a 2 percentage point increase in the probability of stunting. Panel B presents impacts on test scores. There are consistently negative impacts on reading and math test scores, in addition to negative impacts on the ability to read and do math. Furthermore, Panel C shows that individuals with siblings exposed to an increase in program intensity were significantly more likely to be engaged in child labor.

Table 8 shows that these negative impacts can be explained by an intra-household reallocation of parental investments towards children exposed to an increase in program intensity. In Panel A, I present the indirect measure of investments captured through adult goods consumption, as well as debt taken out by households in the past five years. While adult goods consumption increases significantly (thereby suggesting a decrease in investments) for children aged 7-13 with siblings exposed to greater program intensity, the other point estimates are statistically insignificant in this panel. In Panel B, I present the direct measures of investment, broken down by age groups 0-6 and 7-13. There are significant decreases in parental investments in the form of tuition, education expenditure, and participation in Parent-Teacher Associations (PTA) associated with having siblings exposed to an increase in average program intensity.

[Figure 8 about here.]

Figure 8 summarizes the negative impacts of having siblings exposed to an increase in program intensity on child outcomes and parental investments presented in Tables 7 and 8. The direction of all variables has been standardized such that positive point estimates represent an increase in investments or improvement in human capital outcomes. The point estimates capture the term β in specification (3). All estimates have been standardized using the mean and standard deviation of individuals with no ICDS program exposure. I also display the 95% confidence interval bars corresponding to each point estimate. Individuals experience an approximate 0.024 standard deviation decrease in health outcomes and a 0.017 standard deviation decrease in education outcomes. This is driven by a 0.015 - 0.029 standard deviation decrease in investments in children by parents.

VII.A Sibling Age Gap Heterogeneity in Intra-household Reallocation of Investments

While the negative impacts from siblings exposed to an increase in average program intensity presented in Figure 8 considered all siblings, it is important to ask which siblings in particular drive these results. In this section, I break down the analysis on sibling spillovers by the age gap between siblings. The spillovers might be expected to be strongest when siblings are similar in age and competition for parental investments is high. Siblings far apart in age may not compete for parental investments in a similar manner, given that each child may be at a different stage of human capital formation.

[Figure 9 about here.]

Figure 9 presents the results on sibling spillovers by the age gap between siblings. I consider impacts from siblings 4-6 years younger, 1-3 years younger, 1-3 years older, and 4-6 years older in the four panels. While the negative impacts on test scores arise in all panels, the negative health impacts arise primarily when siblings within 1-3 years of age (younger or older) are exposed to an increase in program intensity. These results suggest that the negative sibling spillovers might be driven by siblings that are very similar in age.

VII.B Gender Heterogeneity in Intra-household Reallocation of Investments

[Figure 10 about here.]

Given the large literature on parents' bias towards boys in developing countries, the heterogeneous impacts of this intra-household reallocation of resources by gender are particularly important. Figure 10 presents results by gender of the individual in question, as well as the gender of her siblings. The negative impacts on education arise for all four combinations of the gender of the individual and gender of siblings. However, the negative health impacts from siblings exposed to an increase in program intensity arise only for girls. Interestingly, the gender of the sibling does not seem to matter for girls - girls with male and female siblings experience negative health impacts of similar magnitudes. Thus while Section VI.C showed that the positive direct impacts of the program were larger for women, it is also important to note that the negative indirect impacts of the program, particularly in health, are also larger for women.

VIII Mechanisms: Intertemporal Reallocation of Parental Investments

In this section, I empirically test theoretical prediction 3 outlined in Section III.C, which states that parents intertemporally reallocate their investments in children exposed to an increase in program intensity. Specifically, the prediction states that parents should increase their investments during ages 0-6 while decreasing investments during ages 7-13. Furthermore, I empirically test theoretical prediction 4, which states that parents should take on more debt so as to intertemporally reallocate resources to earlier ages.

[Table 9 about here.]

Panel A of table 9 presents results on adult good consumption, the indirect measure of parental investments, and debt. Columns (1) and (2) consider consumption and borrowing by parents when their children are aged 0-6. Column (1) presents results on per capita consumption of adult goods. Although the coefficient is not statistically significant, its direction indicates that parents reduce consumption of adult goods when their child aged 0-6 is exposed to an increase in ICDS program intensity. This, in turn, is indicative of an increase in investments in the child. Column (2) presents results on borrowing by parents of young children aged 0-6 in the past five years. Parents in district sub-divisions with an additional ICDS center per 1,000 children are 1.8 percentage points more likely to have taken out a loan in the past five years, on a base of 52.1%.

Columns (3) and (4) study consumption and borrowing by parents when their children are aged 7-13. Column (3) shows that parents in district sub-divisions with an additional ICDS center per 1,000 children increase consumption of adult goods by 3.2%. This result, statistically significant at the 5% level, suggests that parents decrease their investments in children aged 7-13 when exposed to an increase in ICDS program intensity. Furthermore, column (4) shows that parents decrease borrowing of such children aged 7-13. The magnitude of the estimate is not very different from that obtained in column (2). It is thus possible that the decrease in loans taken out when children were older is a reflection of parents paying off the increase in debt taken out when their children were younger. These results are in line with theoretical predictions (1) and (3) outlined in Section III.C.

Panel B of table 9 presents results using direct measures of parental investments. Column (1) presents results on consumption of a list of nutritious food and drink by the child when aged 0-6. Children in district sub-divisions with an additional ICDS center per 1,000 children increased their consumption of nutritious food and drink by 0.2 percentage points, on a base of 83.3%. However, since the survey question did not explicitly ask “did you feed your child nutritious food and drink”, but rather “did your child consume nutritious food and drink”, this result could also capture direct feeding by ICDS centers and should hence be treated with caution.

Columns (2) - (3) capture monetary investments, while column (4) captures non-monetary time investments when children are aged 7-13. Column (2) shows that children in district sub-divisions with an additional ICDS center per 1,000 children were 0.3 percentage points less likely to receive any tuition, on a base of 15.8%, i.e. a 2% decrease. This result is statistically significant at the 5% level. Column (3) shows that parents also spend less overall on educational expenses when children are exposed to an increase in ICDS program intensity, although this result is not statistically significant. Column (4) also shows that parents are less likely to spend time participating in parent-teacher association meetings when their children are exposed to an increase in ICDS program intensity.

[Figure 11 about here.]

[Figure 12 about here.]

Figure 11 summarizes the results on intertemporal reallocation of parental investments. The direction of all variables has been standardized such that positive point estimates represent an increase in investments. All estimates have been standardized using the mean and standard deviation of individuals with no ICDS program exposure. I also display 90% confidence interval bars corresponding to each point estimate. The results on direct measures of investments, when combined with the indirect measure of adult good consumption and borrowing, show that parents respond to the ICDS program by intertemporally reallocating parental investments. Figure 12 shows that there is little if any, heterogeneity by gender in the intertemporal reallocation of parental investments.

IX Mechanisms: Ruling out Program Impacts on Parental Employment & Wages

In this section, I empirically test a key assumption made in the theoretical model presented in Section III.A, namely that of exogenous parental income. In particular, I test whether parental employment and wages respond to an increase in program exposure for children, on both the extensive and intensive margins. Such parental labor supply responses to early childhood programs have been shown to be important in other contexts.

For example, Baker et al (2008) provide evidence of women increasing their labor force participation in response to a universal child care program in Quebec, Canada.

[Table 10 about here.]

Table 10 presents program impacts on parental employment and wages. Panel A focuses on children aged 0-6, given that children older than 6 are no longer eligible to avail services from the ICDS program. Columns (1) - (3) and (4) - (6) present outcomes for mothers and fathers, respectively. For each parent, I consider the extensive margin of employment, intensive margin (days worked), and the daily wage. Overall, I find no significant impacts of the program on parental employment and wages. This result may not be too surprising, given that ICDS centers are typically only open for about three hours a day, as shown in Table 1. As a result, parents may not be able to respond to the presence of the program in a meaningful way along dimensions of employment and wages. However, it may be difficult for parents to drop off young babies at the center so as to work longer hours. I investigate this by studying the program impact on the employment and wages of parents of children aged 3-6 in panel B. Again, I find no significant impacts of the program on parental employment and wages.

[Figure 13 about here.]

Figure 13 summarizes the results on parental employment and wages. Notably, these estimates are relatively precisely estimated, in particular for children aged 0-6, where the sample sizes are larger. This shows that I can rule out a mechanism by which the ICDS program enables parents to work and earn more, thereby leading to an income effect that affects child outcomes. This allows me to take the household budget constraint as fixed and interpret the changes in investment across siblings and time as *reallocations* of parental investments in response to an increase in program exposure.

X Cost-Benefit Analysis

In this section, I present a cost-benefit analysis of the ICDS program, taking into account the direct impacts of the program, as well as the indirect impacts on siblings due to the intra-household reallocation of resources. The benefits from the program stem from (i) the increase in wages, and (ii) the health impacts that arise due to a reduction in lost days of work due to illness.

The direct wage benefits from the program are calculated as follows:

$$B_w = \frac{1}{N}\sum_{i=1}^{N}\Big(\beta_k * P_{jk} * NPV_{ijk}\Big) \tag{4}$$

where β_k refers to the percentage increase in wages accruing to cohort k that arises from an additional center per 1000 children in their district sub-division when aged -1 to 6, P_{jk} refers to the average program intensity that cohort k in district sub-division j was exposed to, and NPV_{ijk} refers to the net present value of individual i's lifetime income stream. An average is then taken across all individuals. The estimates of β_k are obtained from the following regression similar to equation (1):

$$Y_{ijk} = \alpha + \gamma_j + \lambda_{ks} + \sum_k \beta_k P_{jk} + X_{ijk}\delta + \epsilon_{ijk} \tag{5}$$

where γ_j represents district sub-division fixed effects and λ_{ks} represents cohort x state fixed effects. Estimates of β_k are obtained by interacting program intensity P_{jk} with the full set of cohort dummies. In computing the benefits of the program, I only include estimates of β_k that are statistically significant at the 10% level - all other estimates are set to zero.

The direct health impacts of the program are calculated as follows:

$$B_h = \frac{1}{N}\sum_{i=1}^{N}\Big(P_{jk} * NPV_i(\beta_h)\Big) \tag{6}$$

where β_h refers to the reduction in lost income due to lost days of work that arises from an additional center per 1000 children in their district sub-division when aged -1 to 6. The estimate of β_h is obtained from regression specification (1) and is statistically significant at the 5% level. $NPV_i(\beta_h)$ refers to the net present value of these health benefits for each individual i. A program exposure-weighted average is then taken over all individuals. Total program benefits are then calculated as the sum: $B = B_w + B_h$.

The indirect costs from the program are calculated as follows:

$$C_I = \frac{1}{N}\sum_{i=1}^{N}\sum_{l=1}^{L}\Big(\tilde{\theta}_k * P_{jk} * \widetilde{NPV}_{iljk}\Big) \tag{7}$$

where $\tilde{\theta}_k$ refers to the percentage decrease in wages accruing to sibling l of an individual i in cohort k, where individual i was exposed to an additional center per 1000 children in their district sub-division when aged -1 to 6. P_{jk} refers to the average program intensity that cohort k in district sub-division j was exposed to, and $\widetilde{NPV}_{ijk}$ refers to the net present value of sibling l's lifetime income stream. A sum is taken over all siblings for each individual, after which an average is taken across all individuals. Estimates of $\tilde{\theta}_k = \frac{\theta_k}{L}$ are obtained from the following regression similar to equation (3):

$$Y_{ijk} = \alpha + \lambda_{jka} + \sum_k \theta_k \tilde{P}_{ijk} + X_{ijk}\delta + \epsilon_{ijk} \tag{8}$$

where λ_{jka} represents district sub-division x cohort x age fixed effects. Estimates of θ_k are obtained by interacting average sibling program intensity $\tilde{P}_{jk}$ with the full set of cohort dummies. In computing the indirect costs of the program, I only include estimates of θ_k that are statistically significant at the 10% level - all other estimates are set to zero. I do not include the indirect health costs of the program, as these estimates are not statistically significant at the 10% level.

I stress an important caveat to the indirect cost analysis: matching of siblings among adults is difficult. While the IHDS data has some information on adult siblings, this may not include siblings who have left the household. The omission of one's siblings has two key impacts on the analysis: first, it may change the estimated value of $\tilde{\theta}_k$, the impact of higher program intensity on siblings. Second, a value of L lower than the true value would lead to an under-estimate of the indirect costs of the program, as per equation (7).

The direct cost of the program depends on the annual cost of the program to the government, as well as each individual's exposure to the program. The annual cost of the program is estimated by noting that in 2012, the Indian government spent Rs. 159 billion on the program on a population of 159 million kids aged 0-6. As such, the annual cost of the program per kid is an estimated Rs. 1,000 (2012 prices). Note that in order to be consistent with the computation of the benefits, the costs are calculated using an intent-to-treat (ITT) methodology, rather than the treatment-on-treated (TOT). Each individual's exposure to the program in number of years is calculated using their birth year and the arrival of the program in their district sub-division. The direct cost is then estimated as a program exposure-weighted average over all individuals. Total program costs are calculated as the sum: $C = C_I + C_D$.

$$C_D = \frac{1}{N}\sum_{i=1}^{N}\Big(AnnualCost * Exposure_i\Big) \tag{9}$$

Two key assumptions are made in the cost-benefit analysis.[8] First, an assumption has to be made on the age until which individuals work. As such, I present the analysis for three scenarios corresponding to retirement at ages 50, 55, and 60. Second, an assumption has to be made on the discount rate used. I present calculations using a discount rate of 8%, but also calculate the internal rate of return (IRR), i.e. the discount rate that gives the program a net present value of zero.[9] Table 11 presents results from the cost-benefit analysis.

[Table 11 about here.]

I start by considering only the direct costs and benefits of the program. The total direct benefits of the program per person range from Rs. 5,538 to Rs. 5,714 (2012 rupees). Decomposing the direct benefits into wage and health benefits, I note that 16% of the direct benefits arise due to health impacts of the program. A large 84% of the benefits arise due to wage impacts. The direct program cost is calculated to be Rs. 4,531 per person. This indicates that at a discount rate of 8%, the direct benefits of the program outweigh the direct costs. The internal rate of return (IRR) of the program, defined as the discount rate such that the net present value of the program is zero, ranges from 8.8% to 9% under the most and least conservative scenarios, respectively.

Next, I include the indirect costs of the program that arise due to the intra-household reallocation of resources. At a discount rate of 8%, these costs range from an estimated Rs. 359 to Rs. 371 (2012 rupees) per person. As a result, the IRR that takes into account the direct and indirect impacts of the program decreases slightly to 8% to 8.2%, i.e. by approximately 9%. These returns calculated are in the range of other IRR estimates for education interventions in developing countries. For example, the range of IRR obtained is very comparable to that obtained by Duflo (2001) of 8.8% to 12% for a large primary school construction program in Indonesia. However, they are notably smaller than the internal rates of return to health interventions such as deworming. For example, Baird et

[8] I do not assume any wage growth over the lifetime, as the estimated coefficients on age and age^2 are not significantly different from zero in the wage regressions.

[9] While a discount rate of 8% may be considered high, it is important to note that the discount rate for India has ranged from 6% - 12% over the time period considered. (Source: IMF).

al. (2016) estimate an internal rate of return of 32% for a school-based deworming experiment in Kenya.

XI Conclusion

This paper shows that the overall impacts of early childhood programs depend on both the direct impacts on exposed cohorts, as well as the indirect impacts that arise due to the reallocation of parental investments. Cohorts exposed to the ICDS program had significant impacts along various dimensions of health, education, and economic well-being, in both the short and long run. However, parents reallocate their investments towards children exposed to an increase in program intensity, as evidenced by the crowd-out of program impacts from exposed siblings. Parents also reallocate their investments in children towards earlier ages by taking on more debt.

The results on intra-household and intertemporal reallocation by parents have two important policy implications. First, the results highlight that the aggregate effects of early childhood programs might be lower than the direct impacts on exposed cohorts due to negative spillovers on siblings. Second, the results caution against short-term evaluations of such programs. Intertemporal reallocation of investments by parents might mean that the reduction in investment might not yet have taken place at the time of evaluation, thereby leading to an overestimation of program impacts.

While this paper takes an important step in presenting a particular type of indirect impacts, namely that of intra-household reallocation, little is known about other indirect impacts of such programs. Specifically, inter-generational impacts of program exposure might lead to persistent impacts across generations. Furthermore, general equilibrium impacts within the village might mean that wages of unexposed cohorts are lower relative to wages of exposed cohorts within villages. I leave these questions for future research.

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Figure 1: Rollout of the ICDS Program over Time

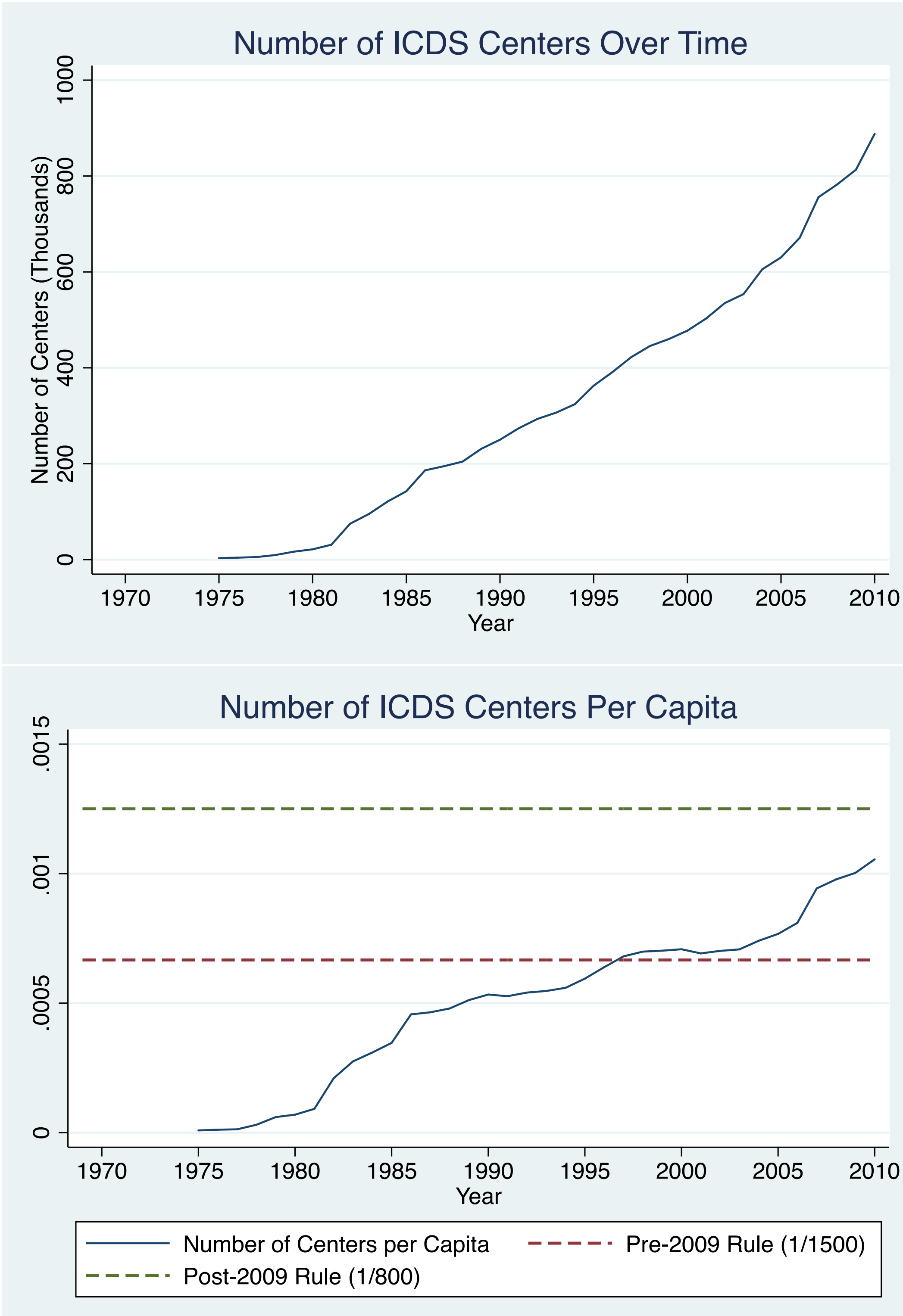


Figure 2: Rollout of the ICDS Program across India over Time

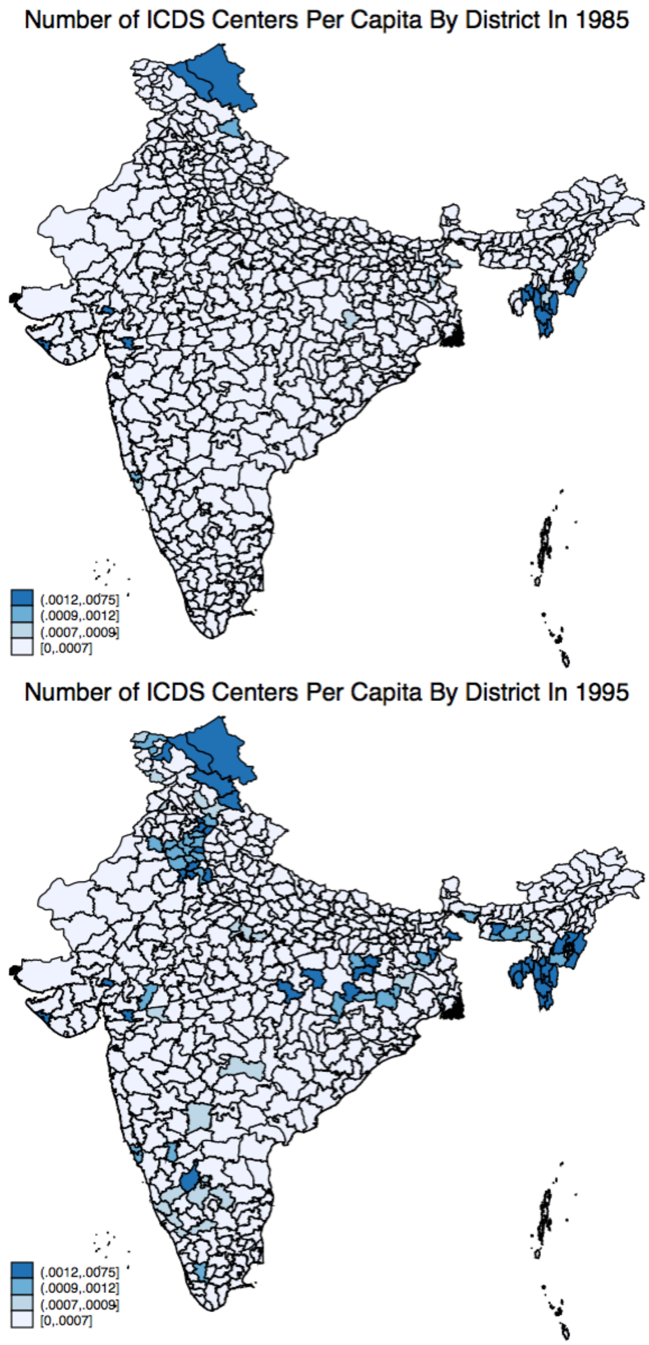


Figure 3: Rollout of ICDS Program across India over Time (continued)

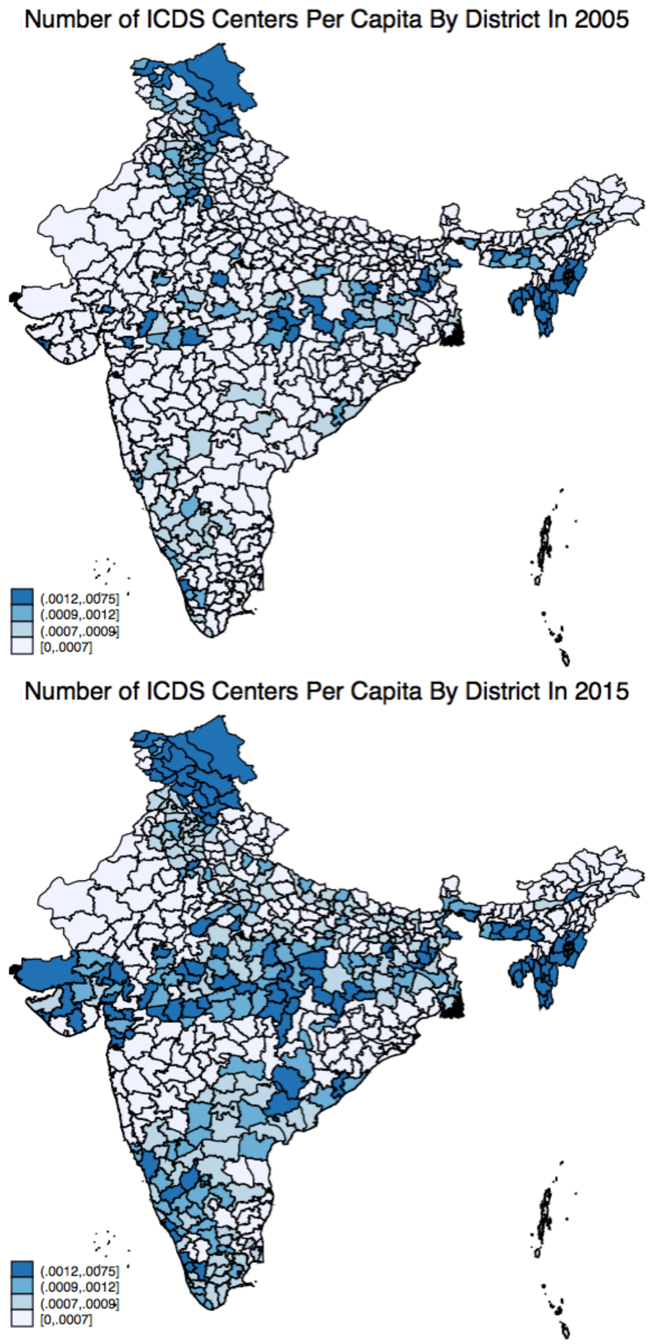
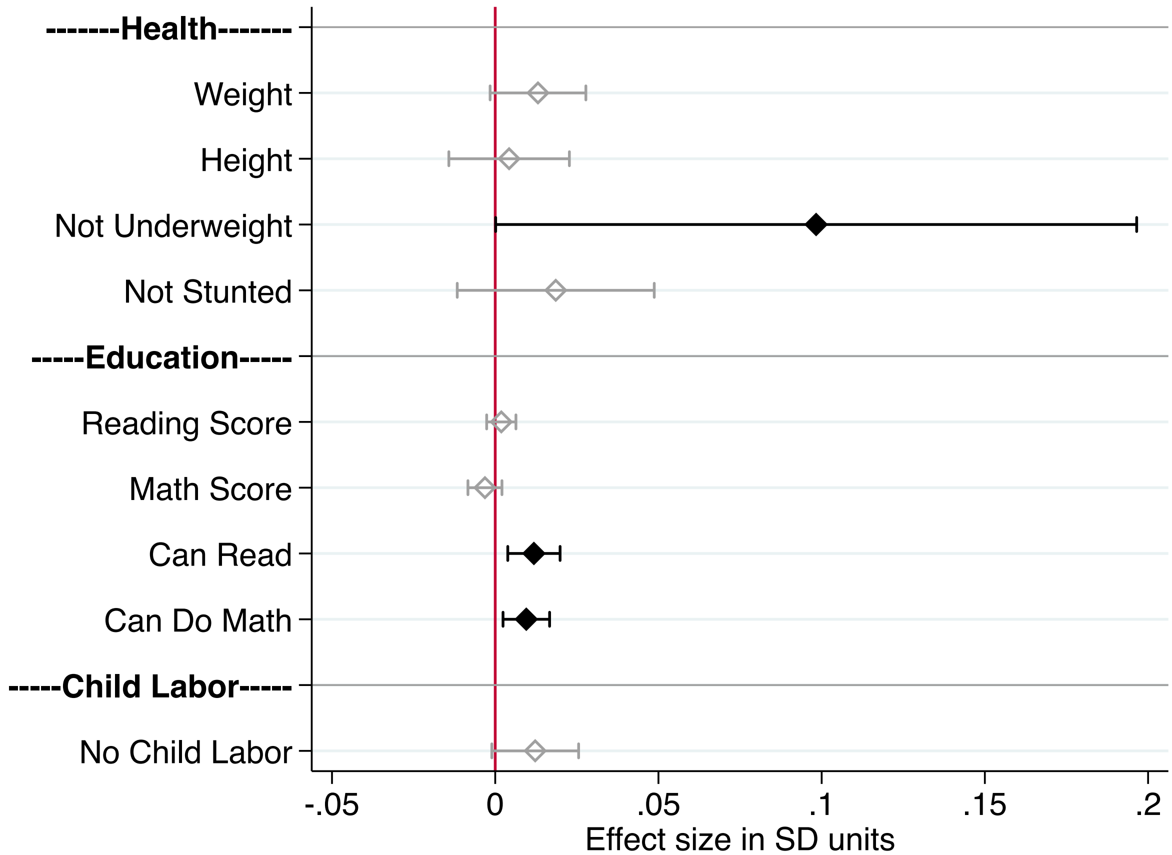
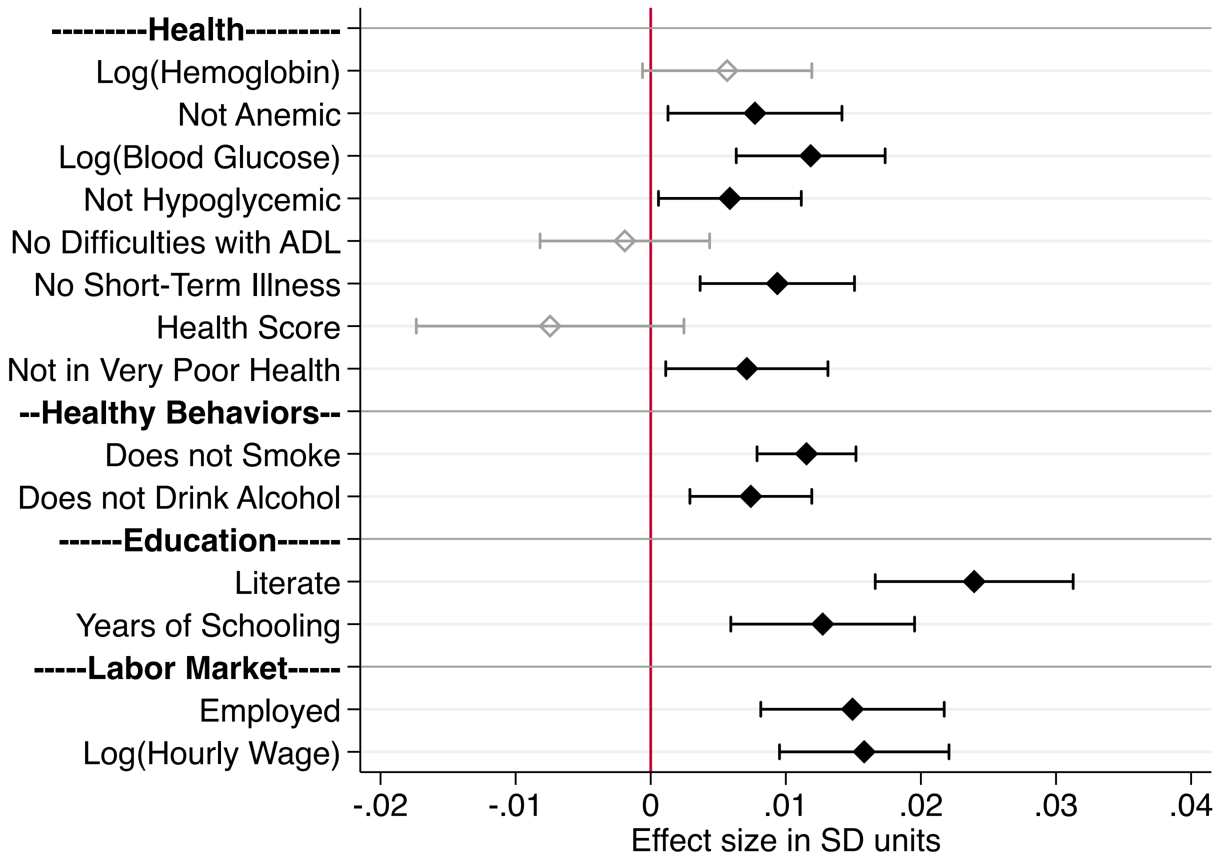


Figure 4: Short-Term Program Impacts



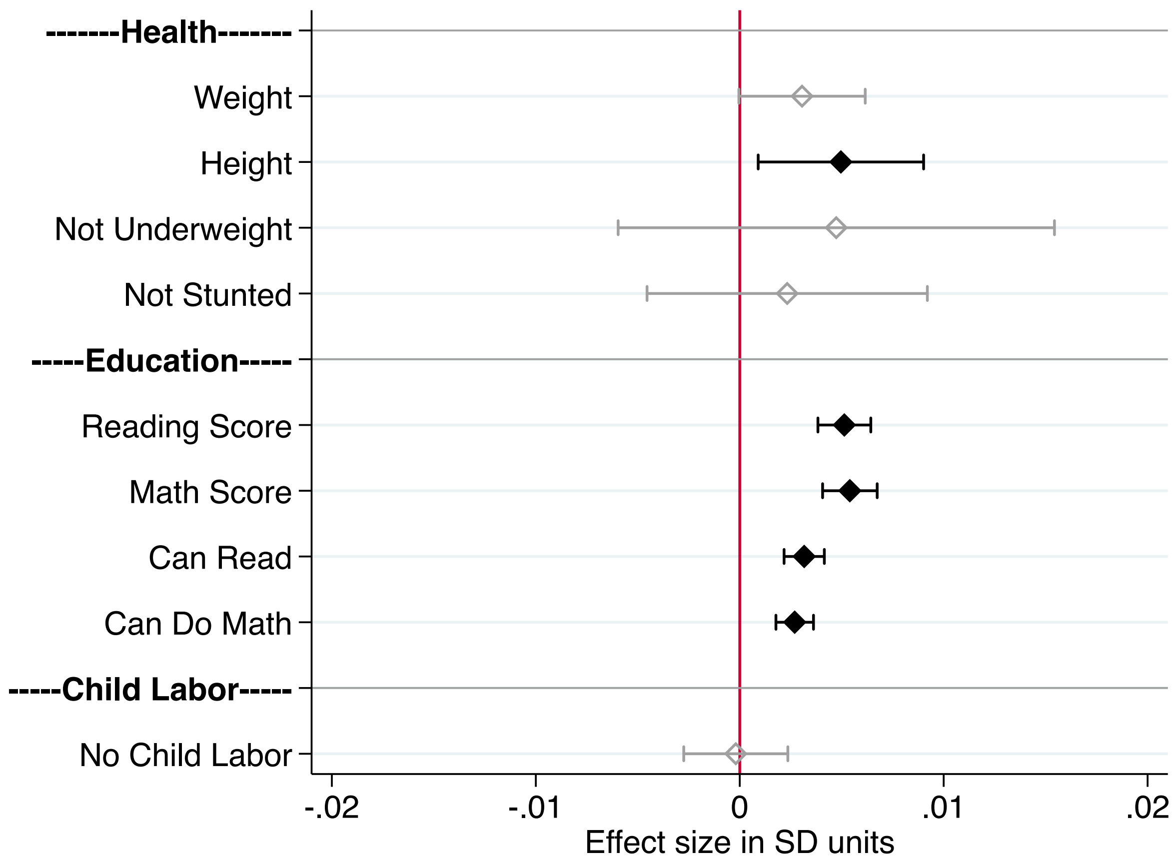
Notes: Each diamond plots the point estimate for β estimated using specification (1). The line corresponding to each point estimate reflects the 95% confidence interval for the outcome. Each outcome variable has been standardized using the mean and standard deviation of individuals in district sub-divisions without the program. The regressions include district sub-division fixed effects, cohort x state fixed effects, controls for gender, birth order, gender x birth order interaction, quadratic population polynomial, caste, religion, and mother's education. All standard errors are clustered by district sub-division.

Figure 5: Long-Term Program Impacts



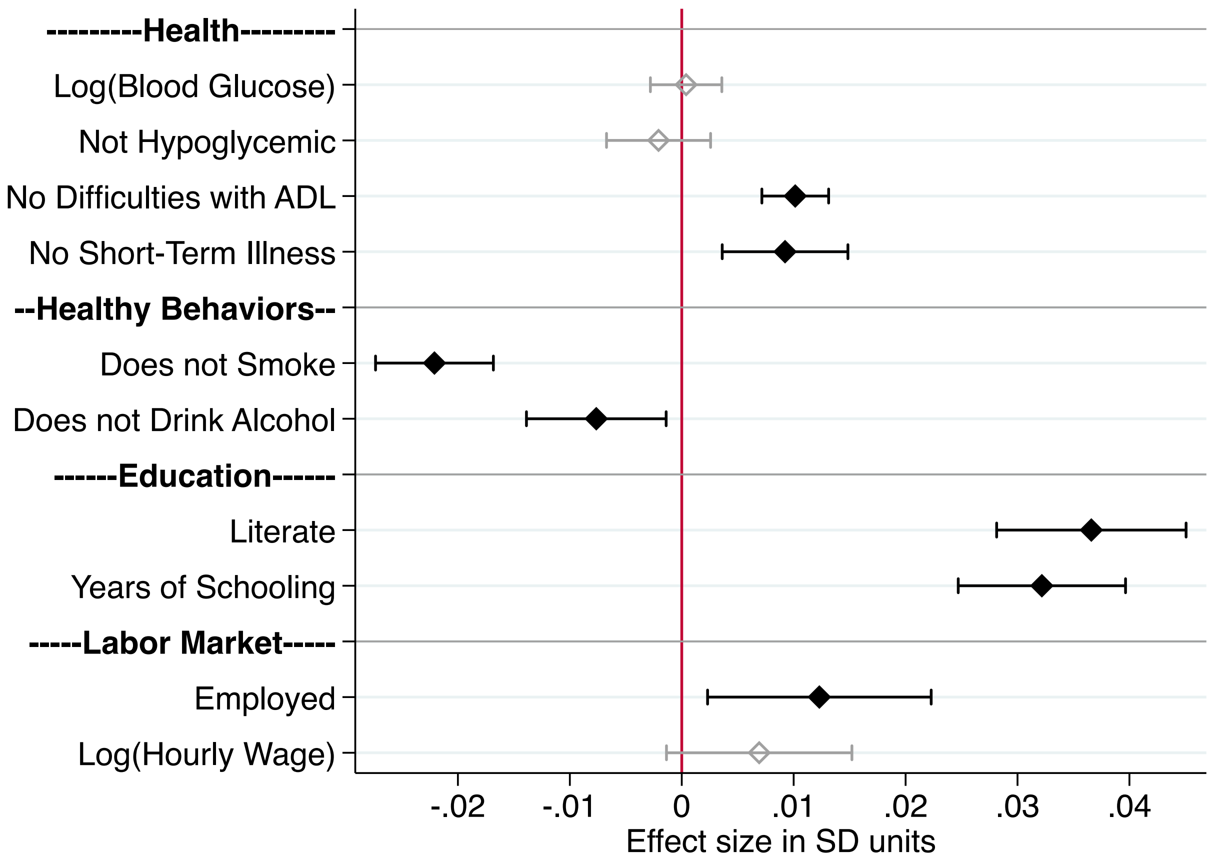
Notes: Each diamond plots the point estimate for β estimated using specification (1). The line corresponding to each point estimate reflects the 95% confidence interval for the outcome. Each outcome variable has been standardized using the mean and standard deviation of individuals in district sub-divisions without the program. The regressions include district sub-division fixed effects, cohort x state fixed effects, controls for gender, quadratic population polynomial, caste, and religion. All standard errors are clustered by district sub-division.

Figure 6: Short-Term Program Impacts: Heterogeneity by Gender



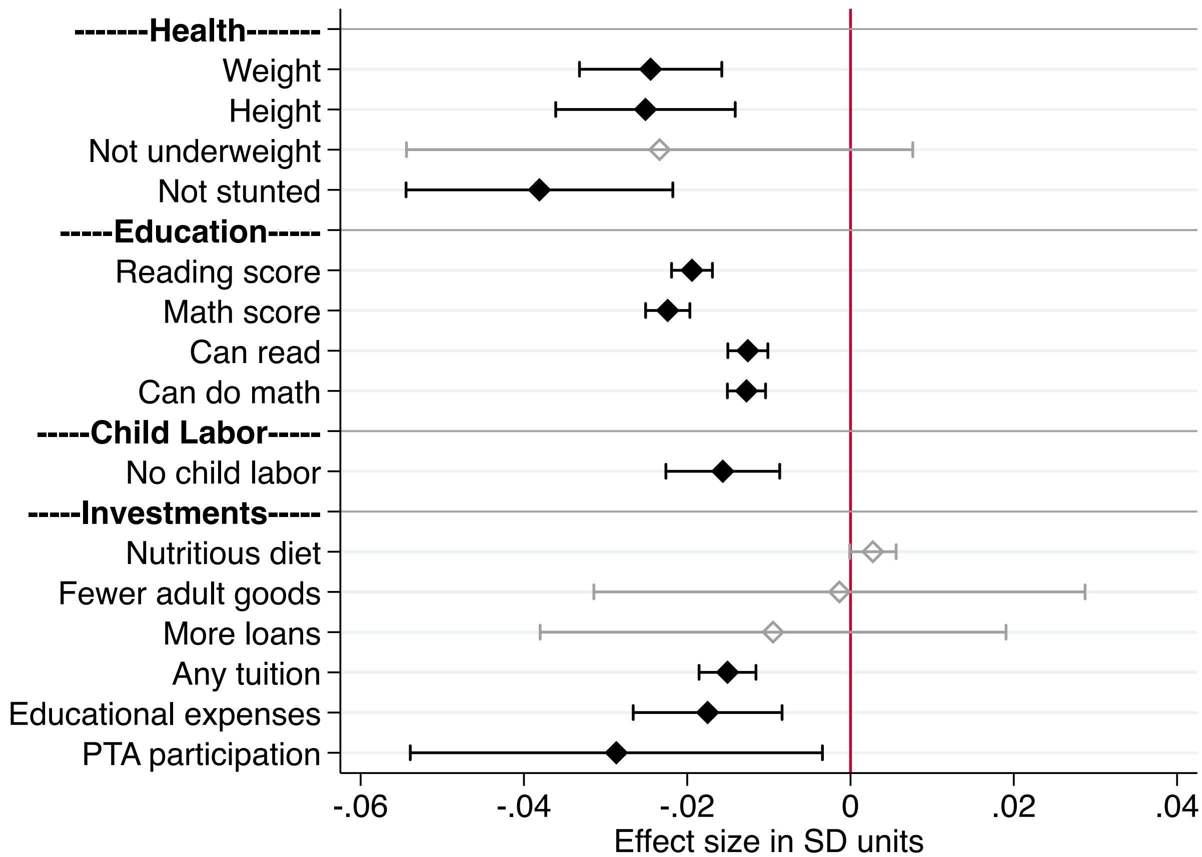
Notes: Each diamond plots the point estimate for the coefficient on an interaction term between program intensity and gender. The regressions estimated augment specification (1) with the interaction term. The line corresponding to each point estimate reflects the 95% confidence interval for the outcome. Each outcome variable has been standardized using the mean and standard deviation of individuals in district sub-divisions without the program. The regressions include district sub-division fixed effects, cohort x state fixed effects, controls for gender, birth order, gender x birth order interaction, quadratic population polynomial, caste, religion, and mother's education. All standard errors are clustered by district sub-division.

Figure 7: Long-Term Program Impacts: Heterogeneity by Gender



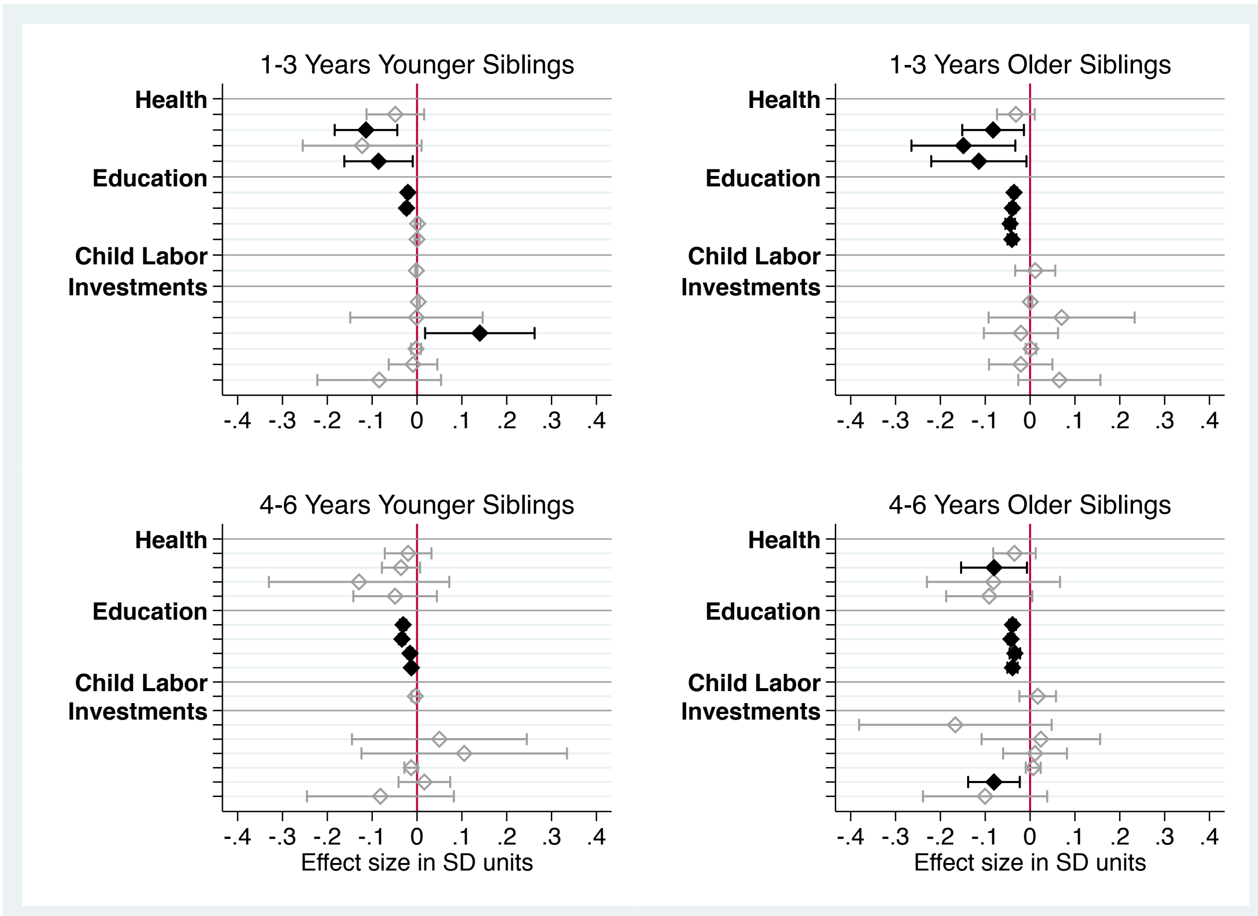
Notes: Each diamond plots the point estimate for the coefficient on an interaction term between program intensity and gender. The regressions estimated augment specification (1) with the interaction term. The line corresponding to each point estimate reflects the 95% confidence interval for the outcome. Each outcome variable has been standardized using the mean and standard deviation of individuals in district sub-divisions without the program. The regressions include district sub-division fixed effects, cohort x state fixed effects, controls for gender, quadratic population polynomial, caste, and religion. All standard errors are clustered by district sub-division.

Figure 8: Intra-household Reallocation of Investments



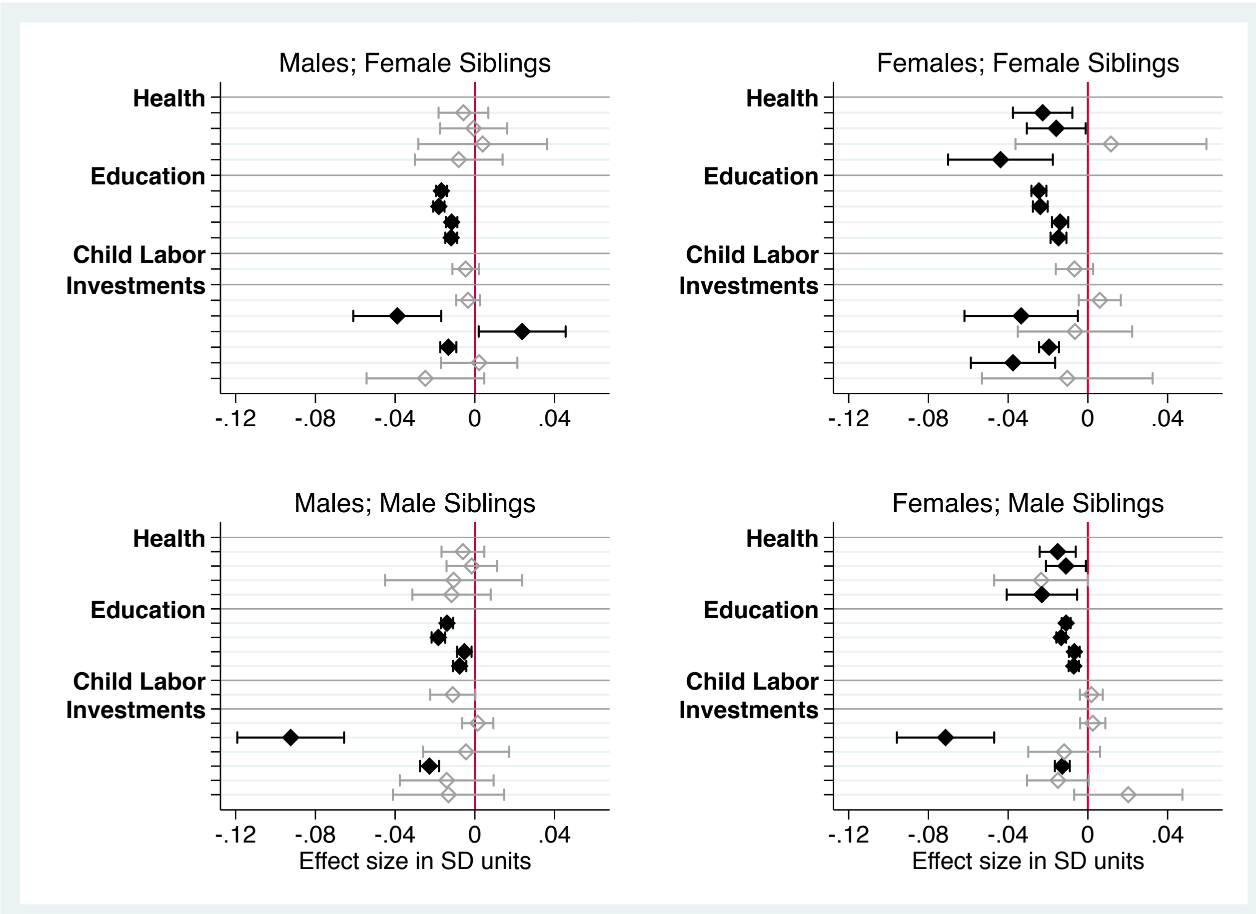
Notes: Each diamond plots the point estimate for β estimated using specification (3). The line corresponding to each point estimate reflects the 95% confidence interval for the outcome. Each outcome variable has been standardized using the mean and standard deviation of individuals in district sub-divisions without the program. The regressions include district sub-division x cohort x age fixed effects, gender x birth order fixed effects, and controls for quadratic population polynomial, caste, religion, and mother's education. All standard errors are clustered by district sub-division.

Figure 9: Intra-household Reallocation of Investments: Heterogeneity by Age Gap



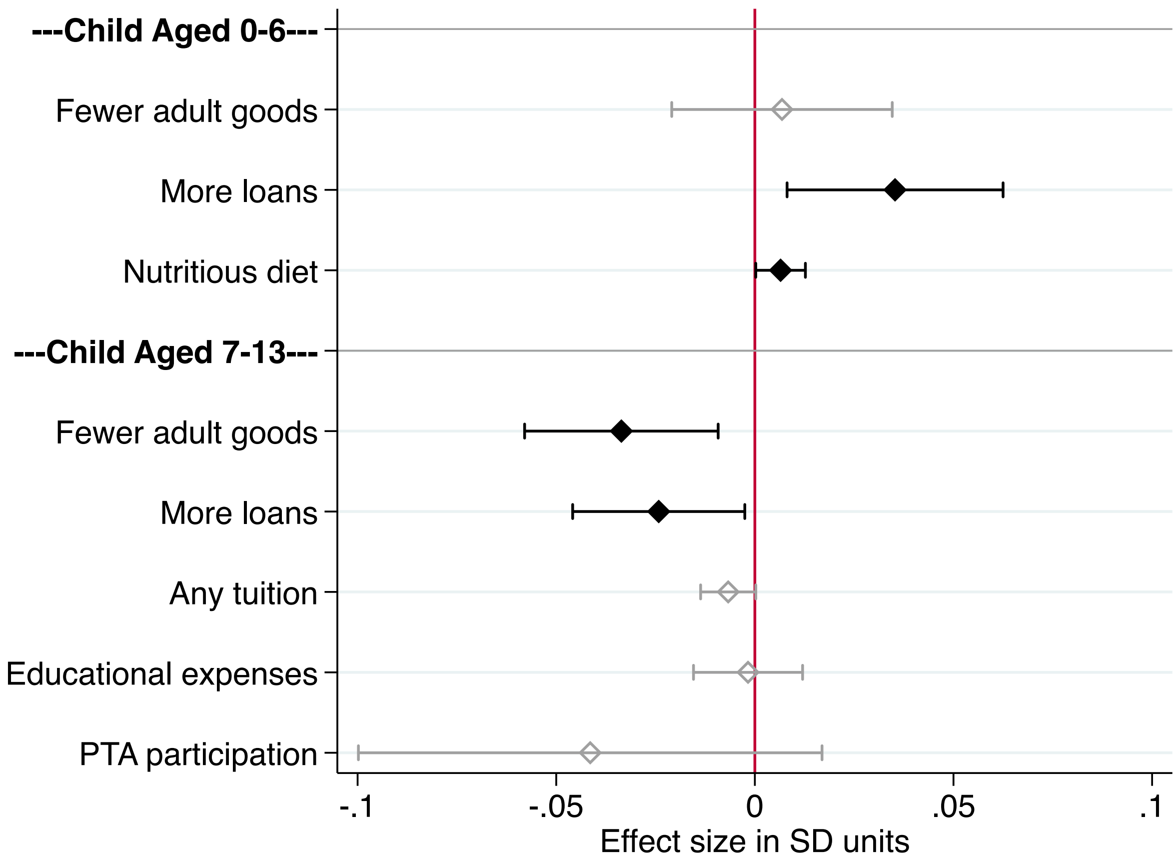
Notes: Each panel presents results using only siblings that are 1-3 years younger, 4-6 years younger, 1-3 years older, or 4-6 years older. Within each panel, each diamond plots the point estimate for β estimated using specification (3). The line corresponding to each point estimate reflects the 95% confidence interval for the outcome. Each outcome variable has been standardized using the mean and standard deviation of individuals in district sub-divisions without the program. The regressions include district sub-division x cohort x age fixed effects, gender x birth order fixed effects, and controls for quadratic population polynomial, caste, religion, and mother's education. All standard errors are clustered by district sub-division.

Figure 10: Intra-household Reallocation of Investments: Heterogeneity by Gender



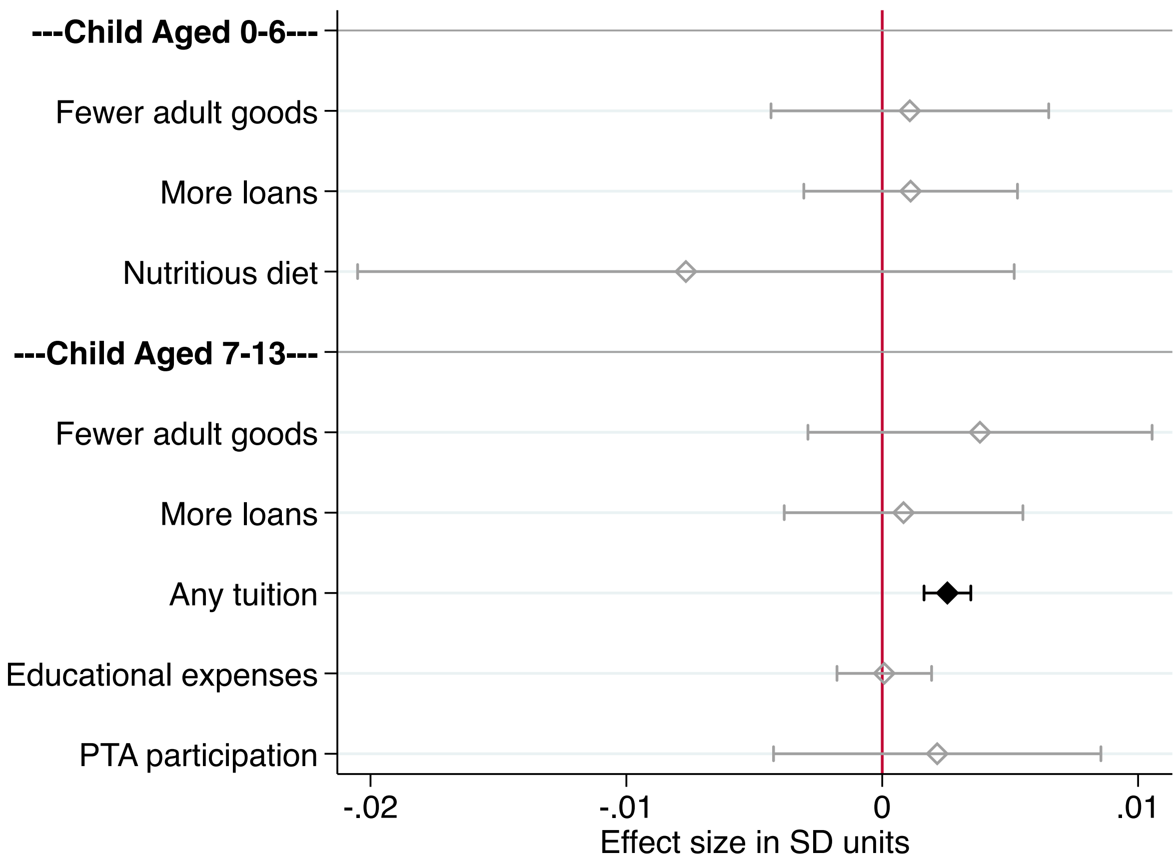
Notes: Each panel presents results from one of the four combinations of gender (boy or girl) x siblings' gender (boy siblings or girl siblings). For example, *Males; Female Siblings* refers to the impacts on males, estimated using only the program intensity of female siblings. Within each panel, each diamond plots the point estimate for β estimated using specification (3). The line corresponding to each point estimate reflects the 95% confidence interval for the outcome. Each outcome variable has been standardized using the mean and standard deviation of individuals in district sub-divisions without the program. The regressions include district sub-division x cohort x age fixed effects, gender x birth order fixed effects, and controls for quadratic population polynomial, caste, religion, and mother's education. All standard errors are clustered by district sub-division.

Figure 11: Intertemporal Reallocation of Parental Investments



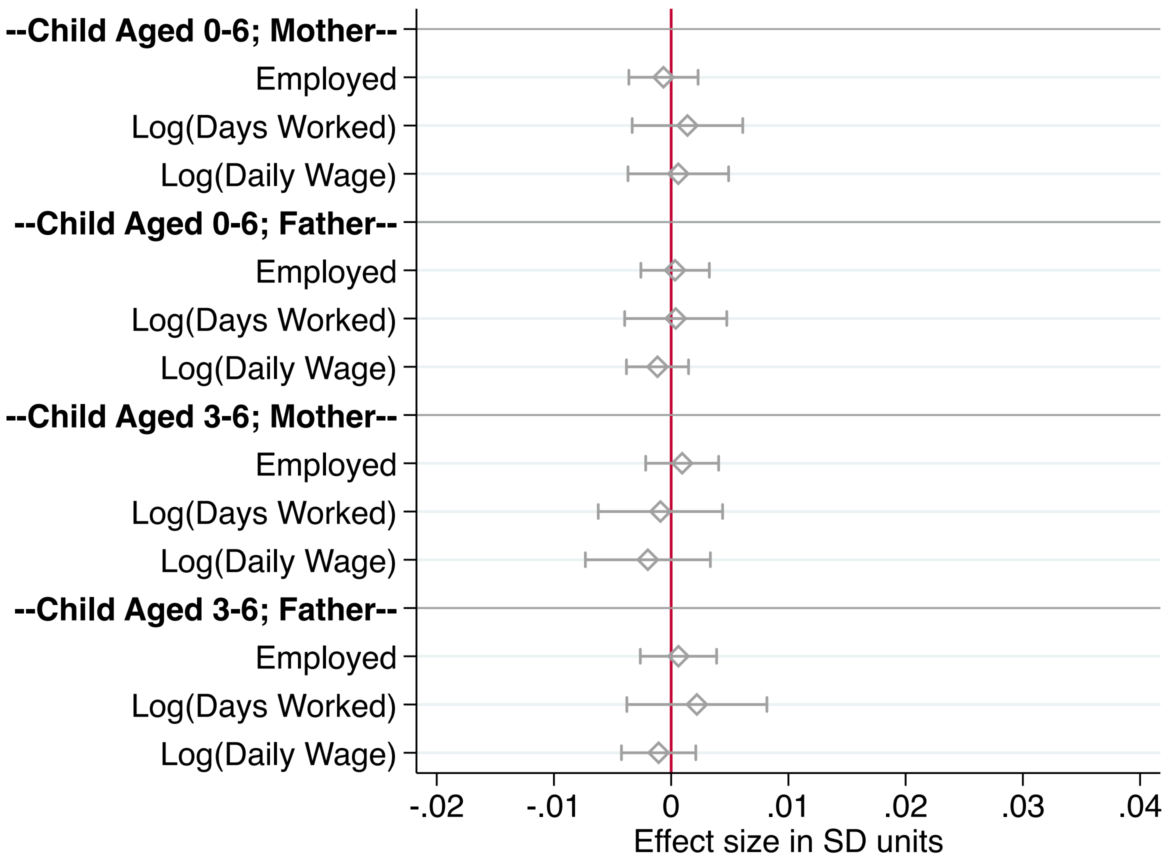
Notes: Each diamond plots the point estimate for β estimated using specification (1). The line corresponding to each point estimate reflects the 90% confidence interval for the outcome. Each outcome variable has been standardized using the mean and standard deviation of individuals in district sub-divisions without the program. The regressions include district sub-division fixed effects, cohort x state fixed effects, controls for gender, birth order, gender x birth order interaction, quadratic population polynomial, caste, religion, and mother's education. All standard errors are clustered by district sub-division.

Figure 12: Intertemporal Reallocation: Heterogeneity by Gender



Notes: Each diamond plots the point estimate for the coefficient on an interaction term between program intensity and gender. The regressions estimated augment specification (1) with the interaction term. The line corresponding to each point estimate reflects the 90% confidence interval for the outcome. Each outcome variable has been standardized using the mean and standard deviation of individuals in district sub-divisions without the program. The regressions include district sub-division fixed effects, cohort x state fixed effects, controls for gender, birth order, gender x birth order interaction, quadratic population polynomial, caste, religion, and mother's education. All standard errors are clustered by district sub-division.

Figure 13: Program Impacts on Parental Employment & Wages



Notes: Each diamond plots the point estimate for β estimated using specification (1). The line corresponding to each point estimate reflects the 95% confidence interval for the outcome. Each outcome variable has been standardized using the mean and standard deviation of individuals in district sub-divisions without the program. The regressions include district sub-division fixed effects, cohort x state fixed effects, controls for gender, birth order, gender x birth order interaction, quadratic population polynomial, caste, religion, and mother's education. All standard errors are clustered by district sub-division.

Table 1: Guidelines on Time Allocation During Operational Hours

Daily Tasks	Expected Time
Preschool Education	2 Hours
Preparation and Distribution of Supplementary Nutrition	30 Minutes
Treatment of Common Childhood Illnesses & Referral	30 Minutes
Filling up Records and Registers	30 Minutes
Total	**3.5 Hours**

Source: Handbook for Anganwadi Workers, National Institute for Public Cooperation and Child Development (2006).

Table 2: Summary of Key Datasets Used

Dataset	Years Covered	Geography Covered	Key Variables
ICDS Administrative Data	1975 - 2016	Rural + Urban	Location of centers, date of opening
National Family Health Survey (NFHS) Rounds 1, 2, and 4	1992 - 2016 (with gaps)	Rural + Urban	Health, healthy behaviors
India Human Development Survey (IHDS) Round 2	2011 - 2012	Rural + Urban	Education, health, wages, employment
National Sample Survey (NSS) Employment Rounds 55, 60, 61, 62, 64, 66, 68	1999 - 2012 (with gaps)	Rural + Urban	Parents' employment, child labor
National Sample Survey (NSS) Education Expenditure Rounds 64, 71	2007 - 2014 (with gaps)	Rural + Urban	Education expenditure
Annual Status of Education Report (ASER)	2006 - 2014	Rural only	Test scores
Census of India	1901 - 2016	Rural + Urban	Population
University of Delaware Rainfall Data	1900 - 2014	Rural + Urban	Rainfall

Table 3: Summary Statistics

Category	Variable	Mean	Std. Dev.	N
Panel A: Child Outcomes				
Health	Weight, Age 7-17 (kg)	32.85	11.8	34,032
	Height, Age 7-17 (m)	1.38	0.19	33,989
	Underweight	0.32	0.47	12,604
	Stunted	0.31	0.46	33,989
Education	Reading test score (0-4)	2.66	1.44	4,709,681
	Math test score (0-4)	2.47	1.36	4,688,733
	Can read	0.89	0.31	4,709,681
	Can do math	0.89	0.31	4,688,733
Labor	Any child labor	0.02	0.13	493,894
Panel B: Adult Outcomes				
Health	Blood hemoglobin (g/dL)	11.77	1.66	182,930
	Anemic	0.12	0.33	182,929
	Blood glucose (mg/dL)	103.26	25.95	254,532
	Hypoglycemic	0.02	0.13	254,532
	Any difficulties with ADL	0.07	0.26	117,414
	Any short-term illness	0.14	0.35	117,414
	Subjective health score (1-5)	2.08	0.84	34,092
	Very poor health	0.004	0.061	34,092
Healthy Behaviors	Drinks alcohol	0.11	0.31	272,039
	Smokes or uses tobacco	0.21	0.4	272,036
Education	Literate	0.67	0.47	117,267
	Years of education	6.09	5.14	117,244
Labor	Hourly wage (Rs.)	25.14	30.8	45,298
	Unemployed	0.02	0.14	111,396
Panel C: Parental Investments				
Monetary	Child ate nutritious food	0.83	0.38	182,202
	Annual educational expenses (Rs.)	12,561	30,023	186,675
	Any tuition	0.18	0.39	3,260,790
Time	Parents participated in PTA	0.43	0.5	13,023
Panel D: Parental Employment				
Mother	Mother employed	0.31	0.46	220,712
	Mother weekly days worked	1.72	2.73	220,712
Father	Father employed	0.97	0.17	209,283
	Father weekly days worked	6.43	1.5	209,283

Table 4: Short-Run Program Impacts on Children

Panel A: Child Health (IHDS)	(1) Weight (Z-Score)	(2) Height (Z-Score)	(3) Underweight (Z<-2)	(4) Stunted (Z<-2)
Program Intensity	0.0131*	0.00425	-0.0453**	-0.00872
	(0.00746)	(0.00940)	(0.0230)	(0.00722)
	[0.159]	[0.901]	[0.100]	[0.416]
Adjusted R^2	0.681	0.624	0.072	0.068
District sub-division FEs	Yes	Yes	Yes	Yes
Cohort x State FEs	Yes	Yes	Yes	Yes
Mean in district sub-divisions without program	0	0	0.306	0.329
Observations	31,538	31,506	11,815	31,506

Panel B: Test Scores (ASER)	Reading Score (Z-Score)	Math Score (Z-Score)	Can Read	Can Do Math
Program Intensity	0.00187	-0.00315	0.00345***	0.00279***
	(0.00228)	(0.00265)	(0.00119)	(0.00106)
	[0.574]	[0.346]	[0.006]	[0.014]
Adjusted R^2	0.451	0.442	0.201	0.186
District sub-division FEs	Yes	Yes	Yes	Yes
Cohort x State FEs	Yes	Yes	Yes	Yes
Mean in district sub-divisions without program	0	0	0.906	0.905
Observations	4,459,291	4,440,879	4,459,291	4,440,879

Panel C: Child Labor (NSS)	Any Child Labor
Program Intensity	-0.00167*
	(0.000925)
	[0.071]
Adjusted R^2	0.053
District sub-division FEs	Yes
Cohort x State FEs	Yes
Mean in district sub-divisions without program	0.019
Observations	346,963

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parentheses and clustered by district sub-division.

P-values adjusted for multiple hypothesis testing via Bonferroni-Sankoh procedure in brackets.

Notes: Sample consists of children aged 7-17. Program intensity is measured as number of ICDS centers per 1,000 children aged 0-6. All regressions include controls for gender, birth order, gender x birth order interaction, quadratic population polynomial, caste, religion, and mother's education.

Table 5: Program Impact on Adult Health & Healthy Behaviors

Panel A: Biomarker Data (NFHS)	(1) Log (Hemoglobin)	(2) Anemic	(3) Log(Blood Glucose)	(4) Hypoglycemic
Program Intensity	0.000894*	-0.00244**	0.00269***	-0.000737**
	(0.000505)	(0.00104)	(0.000638)	(0.000339)
	[0.246]	[0.065]	[0.003]	[0.087]
Adjusted R^2	0.063	0.033	0.086	0.024
District sub-division FEs	Yes	Yes	Yes	Yes
Cohort x State FEs	Yes	Yes	Yes	Yes
Mean in district sub-divisions without program	4.767	0.112	4.635	0.016
Observations	174,329	174,329	241,478	241,478

Panel B: Self-Reported Data (IHDS)	Any Difficulties with ADL	Any Short-Term Illness	Health Score	Very Poor Health
Program Intensity	0.000577	-0.00335***	-0.00653	-0.000460**
	(0.000961)	(0.00104)	(0.00442)	(0.000198)
	[0.797]	[0.002]	[0.260]	[0.040]
Adjusted R^2	0.222	0.085	0.227	0.018
District sub-division FEs	Yes	Yes	Yes	Yes
Cohort x State FEs	Yes	Yes	Yes	Yes
Mean in district sub-divisions without program	0.101	0.151	2.106	0.004
Observations	113,807	113,807	29,613	29,613

Panel C: Healthy Behaviors (NFHS)	Smokes or Uses Tobacco	Consumes Alcohol
Program Intensity	-0.00514***	-0.00255***
	(0.000831)	(0.000793)
	[0.002]	[0.002]
Adjusted R^2	0.376	0.264
District sub-division FEs	Yes	Yes
Cohort x State FEs	Yes	Yes
Mean in district sub-divisions without program	0.273	0.138
Observations	258,408	258,411

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parentheses and clustered by district sub-division.

P-values adjusted for multiple hypothesis testing via Bonferroni-Sankoh procedure in brackets.

Notes: Sample for columns (3) - (4) of panel B consists only of adult women. Program intensity is measured as number of ICDS centers per 1,000 children aged 0-6. All regressions include controls for quadratic population polynomial, caste, and religion.

Table 6: Program Impact on Education & Labor Market Outcomes

	(1) Literate	(2) Years of Schooling	(3) Unemployed	(4) Log(Hourly Wage)
Program Intensity	0.0117***	0.0646***	-0.00194***	0.0115***
	(0.00182)	(0.0176)	(0.000449)	(0.00233)
	[0.002]	[0.002]	[0.002]	[0.002]
Adjusted R^2	0.339	0.429	0.064	0.382
District sub-division FEs	Yes	Yes	Yes	Yes
Cohort x State FEs	Yes	Yes	Yes	Yes
Mean in district sub-divisions without program	0.604	5.193	0.017	2.952
Dataset	IHDS	IHDS	IHDS	IHDS
Observations	113,690	113,669	108,002	43,834

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parentheses and clustered by district sub-division.

P-values adjusted for multiple hypothesis testing via Bonferroni-Sankoh procedure in brackets.

Notes: Program intensity is measured as number of ICDS centers per 1,000 children aged 0-6. All regressions include controls for gender, quadratic population polynomial, caste, and religion.

Table 7: Intra-household Reallocation - Impact on Child Outcomes

Panel A: Impact on Child Health				
	(1) Weight (Z-Score)	(2) Height (Z-Score)	(3) Underweight (Z<-2)	(4) Stunted (Z<-2)
Average Program Intensity of Siblings	-0.0247***	-0.0253***	0.0106	0.0179***
	(0.00444)	(0.00557)	(0.00730)	(0.00394)
	[0.002]	[0.002]	[0.282]	[0.002]
Adjusted R^2	0.693	0.636	0.075	0.092
District sub-division x Cohort x Age FEs	Yes	Yes	Yes	Yes
Mean in district sub-divisions without program	0	0	0.306	0.329
Observations	28,707	28,677	10,834	28,677

Panel B: Impact on Test Scores				
	Reading Score (Z-Score)	Math Score (Z-Score)	Can Read	Can Do Math
Average Program Intensity of Siblings	-0.0194***	-0.0224***	-0.00366***	-0.00374***
	(0.00127)	(0.00138)	(0.000363)	(0.000349)
	[0.002]	[0.002]	[0.002]	[0.002]
Adjusted R^2	0.488	0.486	0.271	0.272
District sub-division x Cohort x Age FEs	Yes	Yes	Yes	Yes
Mean in district sub-divisions without program	0	0	0.906	0.905
Observations	3,751,114	3,735,250	3,751,114	3,735,250

Panel C: Impact on Child Labor	
	Any Child Labor
Average Program Intensity of Siblings	0.00214***
	(0.000485)
	[0.000]
Adjusted R^2	0.193
District sub-division x Cohort x Age FEs	Yes
Mean in district sub-divisions without program	0.019
Observations	335,059

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parentheses and clustered by district sub-division.

P-values adjusted for multiple hypothesis testing via Bonferroni-Sankoh procedure in brackets.

Notes: Average program intensity of siblings is measured as number of ICDS centers per 1,000 children aged 0-6. All regressions include gender x birth order fixed effects and controls for quadratic population polynomial, caste, religion, and mother's education.

Table 8: Intra-household Reallocation - Impact on Parental Investments

Panel A: Adult Goods Consumption and Debt				
	Child Aged 0-6:		Child Aged 7-13:	
	(1) Log(Per Capita Consumption on Adult Goods)	(2) Any Loan in Past Five Years	(3) Log(Per Capita Consumption on Adult Goods)	(4) Any Loan in Past Five Years
Average Program Intensity of Siblings	0.00140	-0.00473	0.0255***	-0.00367
	(0.0151)	(0.00725)	(0.00980)	(0.00452)
	[0.995]	[0.765]	[0.020]	[0.659]
Adjusted R^2	0.297	0.181	0.302	0.214
District sub-division x Cohort x Age FEs	Yes	Yes	Yes	Yes
Mean in district sub-divisions without program	3.834	0.471	3.982	0.466
Dataset	IHDS	IHDS	IHDS	IHDS
Observations	11,847	17,638	15,301	23,350

Panel B: Direct Measures of Investment				
	Child Aged 0-6:	Child Aged 7-13:		
	Nutritious Diet	Any Tuition	Log(Educational Expenditure)	Parents Participated in PTA
Average Program Intensity of Siblings	0.00102*	-0.00550***	-0.0268***	-0.0143**
	(0.000541)	(0.000648)	(0.00661)	(0.00638)
	[0.060]	[0.000]	[0.000]	[0.025]
Adjusted R^2	0.182	0.244	0.715	0.286
District sub-division x Cohort x Age FEs	Yes	Yes	Yes	Yes
Mean in district sub-divisions without program	0.833	0.158	7.678	0.447
Dataset	NFHS	ASER	NSS	IHDS
Observations	105,237	2,579,239	61,191	11,114

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parentheses and clustered by district sub-division.

P-values adjusted for multiple hypothesis testing via Bonferroni-Sankoh procedure in brackets.

Notes: Average program intensity of siblings is measured as number of ICDS centers per 1,000 children aged 0-6. All regressions include gender x birth order fixed effects and controls for quadratic population polynomial, caste, religion, and provision of any free textbooks, stationery, meals, or education.

Table 9: Intertemporal Reallocation of Investments

Panel A: Adult Goods Consumption and Debt				
	Child Aged 0-6:		Child Aged 7-13:	
	(1) Log(Per Capita Consumption on Adult Goods)	(2) Any Loan in Past Five Years	(3) Log(Per Capita Consumption on Adult Goods)	(4) Any Loan in Past Five Years
Program Intensity	-0.00675	0.0176**	0.0322**	-0.0121*
	(0.0166)	(0.00824)	(0.0142)	(0.00656)
Adjusted R^2	0.301	0.188	0.301	0.217
District sub-division FEs	Yes	Yes	Yes	Yes
Cohort x State FEs	Yes	Yes	Yes	Yes
Mean in district sub-divisions without program	3.834	0.471	3.982	0.466
Dataset	IHDS	IHDS	IHDS	IHDS
Observations	15,679	23,239	16,742	25,301

Panel B: Direct Measures of Investment				
	Child Aged 0-6:	Child Aged 7-13:		
	Nutritious Diet	Any Tuition	Log(Educational Expenditure)	Parents Participated in PTA
Program Intensity	0.00242*	-0.00250**	-0.00239	-0.0206
	(0.00142)	(0.00120)	(0.0116)	(0.0176)
Adjusted R^2	0.192	0.225	0.683	0.294
District sub-division FEs	Yes	Yes	Yes	Yes
Cohort x State FEs	Yes	Yes	Yes	Yes
Mean in district sub-divisions without program	0.833	0.158	7.678	0.447
Dataset	NFHS	ASER	NSS	IHDS
Observations	177,116	3,105,805	93,884	12,102

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parentheses and clustered by district sub-division.

Notes: Program intensity is measured as number of ICDS centers per 1,000 children aged 0-6. All regressions include controls for gender, birth order, gender x birth order interaction, quadratic population polynomial, caste, religion, and provision of any free textbooks, stationery, meals, or education.

Table 10: Impact on Parental Employment & Wages

	Mother			Father		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Child Aged 0-6 (NSS)	Employed	Log(Days Worked)	Log(Daily Wage)	Employed	Log(Days Worked)	Log(Daily Wage)
Program Intensity	-0.000310	0.000571	0.000533	0.0000581	0.0000760	-0.000974
	(0.000713)	(0.000988)	(0.00191)	(0.000259)	(0.000433)	(0.00113)
Adjusted R^2	0.193	0.197	0.376	0.031	0.069	0.431
District sub-division FEs	Yes	Yes	Yes	Yes	Yes	Yes
Cohort x State FEs	Yes	Yes	Yes	Yes	Yes	Yes
Mean in district sub-divisions without program	0.341	1.663	4.505	0.969	1.89	5.24
Observations	190,290	59,456	22,391	180,054	174,530	85,879
Panel B: Child Aged 3-6 (NSS)	Employed	Log(Days Worked)	Log(Daily Wage)	Employed	Log(Days Worked)	Log(Daily Wage)
Program Intensity	0.000450	-0.000374	-0.00171	0.000107	0.000426	-0.000899
	(0.000760)	(0.00110)	(0.00233)	(0.000286)	(0.000591)	(0.00136)
Adjusted R^2	0.202	0.195	0.371	0.031	0.072	0.431
District sub-division FEs	Yes	Yes	Yes	Yes	Yes	Yes
Cohort x State FEs	Yes	Yes	Yes	Yes	Yes	Yes
Mean in district sub-divisions without program	0.36	1.668	4.479	0.969	1.89	5.25
Observations	133,259	44,131	16,722	125,704	121,898	58,964

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parentheses and clustered by district sub-division.

Notes: Program intensity is measured as number of ICDS centers per 1,000 children aged 0-6. All regressions include controls for gender, birth order, gender x birth order interaction, quadratic population polynomial, caste, religion, and dummies for the quarter of the year in which the survey was conducted, to account for seasonality in employment.

Table 11: Cost-Benefit Analysis

	Direct Benefits (Rs.)		Costs (Rs.)		IRR (%)	
	Wage	Health	Direct	Indirect	Direct	Direct & Indirect
Retirement at Age 50	4,628	910	4,531	359	8.8	8.0
Retirement at Age 55	4,714	929	4,531	366	8.9	8.1
Retirement at Age 60	4,772	942	4,531	371	9.0	8.2

Notes: A discount rate of 8% is used to value the direct and indirect impacts of the program.

A Theoretical Appendix

Parents solve the following maximization problem:

$$\max_{I_{1a},I_{1b},I_{2a},I_{2b}} U_P$$
$$\text{subject to budget constraint: } I_{1a}+I_{1b}+\frac{I_{2a}+I_{2b}}{1+r}\leq \bar{y}$$

where $\bar{y}=\bar{y}_1+\frac{\bar{y}_2}{1+r}$.

A.A Solution to CES Problem with Two Goods

Before solving the nested CES problem, it is helpful to first recall solutions to a simple CES problem with two goods. Consider a consumer maximizing utility over two goods, c_1 and c_2 as follows:

$$\max_{c_1,c_2} U=(Ac_1^{\theta}+Bc_2^{\theta})^{\frac{1}{\theta}}$$
$$\text{subject to budget constraint: } c_1p_1+c_2p_2\leq \bar{y}$$

Define a price index $\bar{p}=\left(A^{\frac{1}{1-\theta}}p_1^{\frac{\theta}{\theta-1}}+B^{\frac{1}{1-\theta}}p_2^{\frac{\theta}{\theta-1}}\right)^{\frac{\theta-1}{\theta}}$. Then I can write c_1 as follows:

$$c_1=\bar{y}\cdot\frac{A^{\frac{1}{1-\theta}}p_1^{\frac{1}{\theta-1}}}{\bar{p}^{\frac{\theta}{\theta-1}}} \tag{10}$$

The intuition behind this expression is as follows:

$$c_1=\underbrace{\bar{y}}_{\text{income}}\cdot\underbrace{\frac{A^{\frac{1}{1-\theta}}p_1^{\frac{\theta}{\theta-1}}}{\bar{p}^{\frac{\theta}{\theta-1}}}}_{\substack{\text{fraction of income}\\ \text{spent on good 1}}}\cdot\underbrace{p_1^{-1}}_{\substack{\text{dividing by price}\\ \text{gives quantity of}\\ \text{good 1 consumed}}}$$

Similarly, the expression for c_2 is as follows:

$$c_2=\bar{y}\cdot\frac{B^{\frac{1}{1-\theta}}p_2^{\frac{1}{\theta-1}}}{\bar{p}^{\frac{\theta}{\theta-1}}} \tag{11}$$

A.B Solution to Nested CES Problem of Parents

To solve the nested CES problem of parents, I will first reduce the problem to the simple CES problem with two goods. I can then apply expressions (10) and (11) derived above to solve the problem.

Recall that parents solve the following problem:

$$\max_{I_{1a},I_{1b},I_{2a},I_{2b}} \left(\beta h_a^\theta + (1-\beta)h_b^\theta\right)^{\frac{1}{\theta}}$$

$$\text{such that: } h_a = F_a\left[\gamma(I_{1a}\mu_g)^\phi + (1-\gamma)I_{2a}^\phi\right]^{\frac{1}{\phi}}$$

$$h_b = F_b\left[\gamma I_{1b}^\phi + (1-\gamma)I_{2b}^\phi\right]^{\frac{1}{\phi}}$$

$$I_{1a} + I_{1b} + \frac{I_{2a}+I_{2b}}{1+r} \leq \bar{y}$$

To simplify notation, let $A = \beta F_a^\theta$ and $B = (1-\beta)F_b^\theta$. Also, let $p = \frac{1}{1+r}$. Then the problem can be written as follows:

$$\max_{I_{1a},I_{1b},I_{2a},I_{2b}} \left(Ah_a^\theta + Bh_b^\theta\right)^{\frac{1}{\theta}}$$

$$\text{such that: } h_a = \left[\gamma(I_{1a}\mu_g)^\phi + (1-\gamma)I_{2a}^\phi\right]^{\frac{1}{\phi}}$$

$$h_b = \left[\gamma I_{1b}^\phi + (1-\gamma)I_{2b}^\phi\right]^{\frac{1}{\phi}}$$

$$I_{1a} + I_{1b} + pI_{2a} + pI_{2b} \leq \bar{y}$$

Before proceeding, it is helpful to note the following:

$$\frac{\partial U}{\partial h_a} = A(Ah_a^\theta + Bh_b^\theta)^{\frac{1-\theta}{\theta}} h_a^{\theta-1}$$

$$= Ah_a^{\theta-1}U^{1-\theta}$$

$$\frac{\partial U}{\partial h_a} = A\left(\frac{h_a}{U}\right)^{\theta-1} \tag{12}$$

Similarly,

$$\frac{\partial U}{\partial h_b} = B\left(\frac{h_b}{U}\right)^{\theta-1} \tag{13}$$

Furthermore, I note that:

$$\frac{\partial h_a}{\partial I_{1a}} = \mu_g^{\phi}\gamma h_a^{1-\phi} I_{1a}^{\phi-1}$$

$$\frac{\partial h_a}{\partial I_{1a}} = \mu_g^{\phi}\gamma\left(\frac{I_{1a}}{h_a}\right)^{\phi-1} \tag{14}$$

Similarly,

$$\frac{\partial h_a}{\partial I_{2a}} = (1-\gamma)\left(\frac{I_{2a}}{h_a}\right)^{\phi-1} \tag{15}$$

$$\frac{\partial h_b}{\partial I_{1b}} = \gamma\left(\frac{I_{1b}}{h_b}\right)^{\phi-1} \tag{16}$$

$$\frac{\partial h_b}{\partial I_{2b}} = (1-\gamma)\left(\frac{I_{2b}}{h_b}\right)^{\phi-1} \tag{17}$$

Setting up the Lagrangean for the problem, I have:

$$\mathcal{L} = U - \lambda(I_{1a} + I_{1b} + pI_{2a} + pI_{2b} - \bar{y})$$

Taking first-order conditions, I obtain the following:

$$\frac{\partial \mathcal{L}}{\partial I_{1a}} = 0: \ \frac{\partial U}{\partial h_a}\cdot\frac{\partial h_a}{\partial I_{1a}} = \lambda \tag{18}$$

$$\frac{\partial \mathcal{L}}{\partial I_{2a}} = 0: \ \frac{\partial U}{\partial h_a}\cdot\frac{\partial h_a}{\partial I_{2a}} = p\lambda \tag{19}$$

$$\frac{\partial \mathcal{L}}{\partial I_{1b}} = 0: \ \frac{\partial U}{\partial h_b}\cdot\frac{\partial h_b}{\partial I_{1b}} = \lambda \tag{20}$$

$$\frac{\partial \mathcal{L}}{\partial I_{2b}} = 0: \ \frac{\partial U}{\partial h_b}\cdot\frac{\partial h_b}{\partial I_{2b}} = p\lambda \tag{21}$$

Plugging equations (12) and (14) into (18), I have:

$$A\left(\frac{h_a}{U}\right)^{\theta-1}\mu_g^{\phi}\gamma\left(\frac{I_{1a}}{h_a}\right)^{\phi-1} = \lambda \tag{22}$$

Plugging equations (12) and (15) into (19), I have:

$$A\left(\frac{h_a}{U}\right)^{\theta-1}(1-\gamma)\left(\frac{I_{2a}}{h_a}\right)^{\phi-1} = \lambda p \tag{23}$$

Taking $\frac{(22)}{(23)}$, I obtain:

$$\frac{\mu_g^{\phi}\gamma}{1-\gamma}\cdot\left(\frac{I_{1a}}{I_{2a}}\right)^{\phi-1} = \frac{1}{p}$$

$$I_{1a} = I_{2a}\cdot\left[\frac{1-\gamma}{\mu_g^{\phi}\gamma}\cdot\frac{1}{p}\right]^{\frac{1}{\phi-1}} \tag{24}$$

Similarly, for child b, I have:

$$I_{1b} = I_{2b}\cdot\left[\frac{1-\gamma}{\gamma}\cdot\frac{1}{p}\right]^{\frac{1}{\phi-1}} \tag{25}$$

Our goal is to re-write I_{1a}, I_{2a}, I_{1b}, and I_{2b} in terms of h_a and h_b. This would allow me to re-write the budget constraint in a way that reduces the nested CES problem to a simple CES problem over two goods.

Plugging equation (24) into the definition of h_a, I have:

$$\begin{aligned}
h_a &= \left[\gamma\mu_g^{\phi}I_{1a}^{\phi} + (1-\gamma)\left(\frac{1-\gamma}{p}\right)^{\frac{\phi}{1-\phi}}\left(\gamma\mu_g^{\phi}\right)^{\frac{\phi}{\phi-1}}I_{1a}^{\phi}\right]^{\frac{1}{\phi}} \\
&= I_{1a}\cdot\left[\gamma\mu_g^{\phi} + (1-\gamma)^{\frac{1}{1-\phi}}p^{\frac{\phi}{\phi-1}}\gamma^{\frac{\phi}{\phi-1}}\left(\mu_g^{\phi}\right)^{\frac{\phi}{\phi-1}}\right]^{\frac{1}{\phi}} \\
&= I_{1a}\cdot\gamma^{\frac{1}{\phi-1}}\mu_g^{\frac{\phi}{\phi-1}}\left[\gamma^{\frac{1}{1-\phi}}\mu_g^{\frac{\phi}{1-\phi}} + (1-\gamma)^{\frac{1}{1-\phi}}p^{\frac{\phi}{\phi-1}}\right]^{\frac{1}{\phi}}
\end{aligned}$$

$$h_a = I_{1a}\cdot\gamma^{\frac{1}{\phi-1}}\mu_g^{\frac{\phi}{\phi-1}}p_a^{\frac{1}{\phi-1}} \tag{26}$$

where the price index p_a is defined as $p_a = \left(\gamma^{\frac{1}{1-\phi}}\mu_g^{\frac{\phi}{1-\phi}} + (1-\gamma)^{\frac{1}{1-\phi}}p^{\frac{\phi}{\phi-1}}\right)^{\frac{\phi-1}{\phi}}$.

Re-arranging, I have that:

$$I_{1a} = h_a \cdot \gamma^{\frac{1}{1-\phi}} \mu_g^{\frac{\phi}{1-\phi}} p_a^{\frac{1}{1-\phi}} \tag{27}$$

Plugging equation (27) into (24), I have that:

$$\begin{aligned} I_{2a} &= I_{1a} \cdot \left[\frac{1-\gamma}{\mu_g^{\phi}\gamma} \cdot \frac{1}{p}\right]^{\frac{1}{1-\phi}} \\ &= h_a \cdot \gamma^{\frac{1}{1-\phi}} \mu_g^{\frac{\phi}{1-\phi}} p_a^{\frac{1}{1-\phi}} \cdot \left[\frac{1-\gamma}{\mu_g^{\phi}\gamma} \cdot \frac{1}{p}\right]^{\frac{1}{1-\phi}} \end{aligned}$$

Therefore:

$$I_{2a} = h_a p_a^{\frac{1}{1-\phi}} (1-\gamma)^{\frac{1}{1-\phi}} p^{\frac{1}{\phi-1}} \tag{28}$$

Note then, that:

$$\begin{aligned} I_{1a} + pI_{2a} &= h_a p_a^{\frac{1}{1-\phi}} \left[\gamma^{\frac{1}{1-\phi}} \mu_g^{\frac{\phi}{1-\phi}} + (1-\gamma)^{\frac{1}{1-\phi}} p^{\frac{\phi}{\phi-1}}\right] \\ &= h_a p_a^{\frac{1}{1-\phi}} \cdot p_a^{\frac{\phi}{\phi-1}} \\ &= h_a p_a \end{aligned}$$

Similarly, for child b, I have that:

$$p_b = \left(\gamma^{\frac{1}{1-\phi}} + (1-\gamma)^{\frac{1}{1-\phi}} p^{\frac{\phi}{\phi-1}}\right)^{\frac{\phi-1}{\phi}} \tag{29}$$

$$h_b = I_{1b} \cdot \gamma^{\frac{1}{\phi-1}} p_b^{\frac{1}{\phi-1}} \tag{30}$$

$$I_{1b} = h_b \cdot \gamma^{\frac{1}{1-\phi}} p_b^{\frac{1}{1-\phi}} \tag{31}$$

$$I_{2b} = h_b p_b^{\frac{1}{1-\phi}} (1-\gamma)^{\frac{1}{1-\phi}} p^{\frac{1}{\phi-1}} \tag{32}$$

$$I_{1b} + pI_{2b} = h_b p_b \tag{33}$$

Thus I can now re-write the problem as a simple CES problem over two goods:

$$\max_{h_a, h_b} \left(Ah_a^{\theta} + Bh_b^{\theta}\right)^{\frac{1}{\theta}} \text{ such that: } h_a p_a + h_b p_b \leq \bar{y}$$

Then applying the solution derived in equation (10), I have that:

$$h_a = \bar{y} \cdot \frac{A^{\frac{1}{1-\theta}} p_a^{\frac{1}{\theta-1}}}{\bar{p}^{\frac{\theta}{\theta-1}}} \text{, where } \bar{p} = \left(A^{\frac{1}{1-\theta}} p_a^{\frac{\theta}{\theta-1}} + B^{\frac{1}{1-\theta}} p_b^{\frac{\theta}{\theta-1}} \right)^{\frac{\theta-1}{\theta}}$$

Then using equation (27), I have that:

$$I_{1a} = \bar{y} \cdot \frac{A^{\frac{1}{1-\theta}} p_a^{\frac{1}{\theta-1}}}{\bar{p}^{\frac{\theta}{\theta-1}}} \cdot p_a \cdot \frac{\gamma^{\frac{1}{1-\phi}} \mu_g^{\frac{\phi}{1-\phi}}}{p_a^{\frac{\phi}{\phi-1}}}$$

Therefore:

$$I_{1a} = \bar{y} \cdot \frac{A^{\frac{1}{1-\theta}} p_a^{\frac{\theta}{\theta-1}}}{\bar{p}^{\frac{\theta}{\theta-1}}} \cdot \frac{\gamma^{\frac{1}{1-\phi}} \left(\mu_g^{-1}\right)^{\frac{\phi}{\phi-1}}}{p_a^{\frac{\phi}{\phi-1}}} \tag{34}$$

where relative program exposure μ_g acts as a negative price (subsidy) on period 1 investments in child a. The intuition behind this expression is as follows:

$$\underbrace{I_{1a}\mu_g}_{\substack{\text{combined quantity of}\\ \text{period 1 investments}\\ \text{in child } a}} = \underbrace{\bar{y}}_{\text{income}} \cdot \underbrace{\frac{A^{\frac{1}{1-\theta}} p_a^{\frac{\theta}{\theta-1}}}{\bar{p}^{\frac{\theta}{\theta-1}}}}_{\substack{\text{fraction of income}\\ \text{spent on child } a}} \cdot \underbrace{\frac{\gamma^{\frac{1}{1-\phi}} \left(\mu_g^{-1}\right)^{\frac{\phi}{\phi-1}}}{p_a^{\frac{\phi}{\phi-1}}}}_{\substack{\text{fraction within income}\\ \text{spent on child } a\\ \text{allocated to period 1}}} \cdot \underbrace{\left(\mu_g^{-1}\right)^{-1}}_{\substack{\text{dividing by price}\\ \text{gives quantity of } I_{1a}\mu_g}}$$

Plugging in equation (24) into (34), I have that:

$$\underbrace{I_{2a}}_{\substack{\text{quantity of}\\ \text{period 2 investments}\\ \text{in child } a}} = \underbrace{\bar{y}}_{\text{income}} \cdot \underbrace{\frac{A^{\frac{1}{1-\theta}} p_a^{\frac{\theta}{\theta-1}}}{\bar{p}^{\frac{\theta}{\theta-1}}}}_{\substack{\text{fraction of income}\\ \text{spent on child } a}} \cdot \underbrace{\frac{(1-\gamma)^{\frac{1}{1-\phi}} p^{\frac{\phi}{\phi-1}}}{p_a^{\frac{\phi}{\phi-1}}}}_{\substack{\text{fraction within income}\\ \text{spent on child } a\\ \text{allocated to period 2}}} \cdot \underbrace{p^{-1}}_{\substack{\text{dividing by price}\\ \text{gives quantity of } I_{2a}}} \tag{35}$$

Similarly, for child b, I have that:

$$I_{1b} = \bar{y} \cdot \frac{B^{\frac{1}{1-\theta}} p_b^{\frac{\theta}{\theta-1}}}{\bar{p}^{\frac{\theta}{\theta-1}}} \cdot \frac{\gamma^{\frac{1}{1-\phi}}}{p_b^{\frac{\phi}{\phi-1}}} \tag{36}$$

$$I_{2b} = \bar{y} \cdot \frac{B^{\frac{1}{1-\theta}} p_b^{\frac{\theta}{\theta-1}}}{\bar{p}^{\frac{\theta}{\theta-1}}} \cdot \frac{(1-\gamma)^{\frac{1}{1-\phi}} p^{\frac{1}{\phi-1}}}{p_b^{\frac{\phi}{\phi-1}}} \tag{37}$$

We have thus solved the model for I_{1a}, I_{2a}, I_{1b}, and I_{2b}.

A.C Comparative Statics

Having solved for the child and period-specific investments, I can now study the comparative statics of these investments with respect to relative program exposure, μ_g.

Since

$$\text{sign}\left[\frac{\partial x}{\partial \mu_g}\right] = \text{sign}\left[\frac{\partial \log x}{\partial \log \mu_g}\right],$$

I choose to work with logs for the comparative statics.

Before proceeding, it is helpful to first compute the following derivatives:

$$\log p_a = \frac{\phi - 1}{\phi} \cdot \log\left(\gamma^{\frac{1}{1-\phi}} \mu_g^{\frac{\phi}{1-\phi}} + (1-\gamma)^{\frac{1}{1-\phi}} p^{\frac{\phi}{\phi-1}}\right)$$

$$\frac{\partial \log p_a}{\partial \log \mu_g} = \frac{\phi - 1}{\phi} \cdot \frac{\gamma^{\frac{1}{1-\phi}} (\frac{\phi}{1-\phi}) \mu_g^{\frac{\phi}{1-\phi}-1} \mu_g}{\gamma^{\frac{1}{1-\phi}} \mu_g^{\frac{\phi}{1-\phi}} + (1-\gamma)^{\frac{1}{1-\phi}} p^{\frac{\phi}{\phi-1}}}$$

Therefore:

$$\frac{\partial \log p_a}{\partial \log \mu_g} = -\frac{\gamma^{\frac{1}{1-\phi}} \mu_g^{\frac{\phi}{1-\phi}}}{p_a^{\frac{\phi}{\phi-1}}} < 0 \tag{38}$$

Intuitively, I should expect the price index p_a to decrease with an increase in relative program exposure μ_g, since μ_g acts as a price subsidy for child a.

For child b, I note that:

$$\log p_b = \frac{\phi - 1}{\phi} \cdot \log\left(\gamma^{\frac{1}{1-\phi}} + (1-\gamma)^{\frac{1}{1-\phi}} p^{\frac{\phi}{\phi-1}}\right)$$

$$\frac{\partial \log p_b}{\partial \log \mu_g} = 0 \tag{39}$$

Once again, this result arises because μ_g acts as a price subsidy only for child a. Finally, for the overall price index $\bar{p}$, I have that:

$$\log \bar{p} = \frac{\theta-1}{\theta} \cdot \log\left(A^{\frac{1}{1-\theta}} p_a^{\frac{\theta}{\theta-1}} + B^{\frac{1}{1-\theta}} p_b^{\frac{\theta}{\theta-1}}\right)$$

$$\frac{\partial \log \bar{p}}{\partial \log \mu_g} = \frac{\theta-1}{\theta} \cdot \frac{A^{\frac{1}{1-\theta}}\left(\frac{\theta}{\theta-1}\right) p_a^{\frac{\theta}{\theta-1}-1} \frac{\partial p_a}{\partial \log \mu_g}}{\bar{p}^{\frac{\theta}{\theta-1}}} \tag{40}$$

Note that $\frac{\partial p_a}{\partial \log \mu_g} = \frac{\partial p_a}{\partial \mu_g} \cdot \mu_g$. Furthermore, I have that:

$$\begin{aligned}\frac{\partial p_a}{\partial \mu_g} &= \frac{\phi-1}{\phi} p_a^{-\frac{1}{\phi-1}} \gamma^{\frac{1}{1-\phi}} \left(\frac{\phi}{1-\phi}\right) \mu_g^{\frac{\phi}{1-\phi}-1} \\ &= -p_a^{\frac{1}{1-\phi}} \gamma^{\frac{1}{1-\phi}} \mu_g^{\frac{\phi}{1-\phi}-1}\end{aligned}$$

Therefore,

$$\frac{\partial p_a}{\partial \log \mu_g} = -p_a^{\frac{1}{1-\phi}} \gamma^{\frac{1}{1-\phi}} \mu_g^{\frac{\phi}{1-\phi}}$$

Thus I have that:

$$\frac{\partial \log \bar{p}}{\partial \log \mu_g} = -\frac{A^{\frac{1}{1-\theta}} p_a^{\frac{1}{\theta-1}} p_a^{\frac{1}{1-\phi}} \gamma^{\frac{1}{1-\phi}} \mu_g^{\frac{\phi}{1-\phi}}}{\bar{p}^{\frac{\theta}{\theta-1}}} < 0 \tag{41}$$

Intuitively, the overall price index $\bar{p}$ is an index with respect to p_a and p_b. Since the elasticity of p_a with respect to μ_g is negative and the elasticity of p_b with respect to μ_g is zero, it must be that the elasticity of $\bar{p}$ with respect to μ_g is negative.

We are now ready to analyze the comparative statics of I_{1a}, I_{2a}, I_{1b}, and I_{2b}. Taking logs on (34), I have that:

$$\begin{aligned}\log I_{1a} &= \log \bar{y} + \log A^{\frac{1}{1-\theta}} + \log p_a^{\frac{\theta}{\theta-1}} - \log \bar{p}^{\frac{\theta}{\theta-1}} + \log \gamma^{\frac{1}{1-\theta}} + \log \mu_g^{\frac{\phi}{1-\phi}} - \log p_a^{\frac{\phi}{\phi-1}} \\ &= \log \bar{y} + \log A^{\frac{1}{1-\theta}} + \log \gamma^{\frac{1}{1-\theta}} + \left(\frac{\theta}{\theta-1} - \frac{\phi}{\phi-1}\right)\log p_a - \left(\frac{\theta}{\theta-1}\right)\log \bar{p} + \left(\frac{\phi}{1-\phi}\right)\log \mu_g\end{aligned}$$

Then I have that:

$$\frac{\partial \log I_{1a}}{\partial \log \mu_g} = \Big(\frac{\theta}{\theta-1} - \frac{\phi}{\phi-1}\Big)\Bigg(-\frac{\gamma^{\frac{1}{1-\phi}}\mu_g^{\frac{\phi}{1-\phi}}}{p_a^{\frac{\phi}{\phi-1}}}\Bigg) - \Big(\frac{\theta}{\theta-1}\Big)\Bigg(-\frac{A^{\frac{1}{1-\theta}}p_a^{\frac{1}{\theta-1}}p_a^{\frac{1}{1-\phi}}\gamma^{\frac{1}{1-\phi}}\mu_g^{\frac{\phi}{1-\phi}}}{\bar{p}^{\frac{\theta}{\theta-1}}}\Bigg) + \frac{\phi}{1-\phi}$$

$$= \Big(\frac{\phi}{\phi-1} - \frac{\theta}{\theta-1}\Big)\Bigg(\frac{\gamma^{\frac{1}{1-\phi}}\mu_g^{\frac{\phi}{1-\phi}}}{p_a^{\frac{\phi}{\phi-1}}}\Bigg) + \Big(\frac{\theta}{\theta-1}\Big)\Bigg(\frac{A^{\frac{1}{1-\theta}}p_a^{\frac{1}{\theta-1}}p_a^{\frac{1}{1-\phi}}\gamma^{\frac{1}{1-\phi}}\mu_g^{\frac{\phi}{1-\phi}}}{\bar{p}^{\frac{\theta}{\theta-1}}}\Bigg) + \frac{\phi}{1-\phi}$$

$$= \Big(\frac{\phi}{\phi-1}\Big)\Bigg(\frac{\gamma^{\frac{1}{1-\phi}}\mu_g^{\frac{\phi}{1-\phi}}}{p_a^{\frac{\phi}{\phi-1}}} - 1\Bigg) + \Big(\frac{\theta}{\theta-1}\Big)\Bigg(\frac{\gamma^{\frac{1}{1-\phi}}\mu_g^{\frac{\phi}{1-\phi}}}{p_a^{\frac{\phi}{\phi-1}}}\Bigg)\Bigg(\frac{A^{\frac{1}{1-\theta}}p_a^{\frac{1}{\theta-1}}p_a}{\bar{p}^{\frac{\theta}{\theta-1}}} - 1\Bigg)$$

Therefore, I have that:

$$\frac{\partial \log I_{1a}}{\partial \log \mu_g} = \Big(\frac{\phi}{1-\phi}\Big)\underbrace{\Bigg(\frac{(1-\gamma)^{\frac{1}{1-\phi}}p^{\frac{\phi}{\phi-1}}}{p_a^{\frac{\phi}{\phi-1}}}\Bigg)}_{\text{positive}} + \Big(\frac{\theta}{1-\theta}\Big)\underbrace{\Bigg(\frac{\gamma^{\frac{1}{1-\phi}}\mu_g^{\frac{\phi}{1-\phi}}}{p_a^{\frac{\phi}{\phi-1}}}\Bigg)}_{\text{positive}}\underbrace{\Bigg(\frac{B^{\frac{1}{1-\theta}}p_b^{\frac{\theta}{\theta-1}}}{\bar{p}^{\frac{\theta}{\theta-1}}}\Bigg)}_{\text{positive}}$$

Thus a sufficient condition for $\frac{\partial \log I_{1a}}{\partial \log \mu_g} > 0$ is: $0 < \phi < 1$ and $0 < \theta < 1$.
Taking logs on equation (24), I have that:

$$\log I_{2a} = \log I_{1a} + \frac{\phi}{1-\phi}\log\Big(\frac{1-\gamma}{\gamma}\cdot\frac{1}{p}\Big) - \frac{\phi}{1-\phi}\log \mu_g \tag{42}$$

Then I have that:

$$\frac{\partial \log I_{2a}}{\partial \log \mu_g} = \frac{\partial \log I_{1a}}{\partial \log \mu_g} - \frac{\phi}{1-\phi}$$

$$= \Big(\frac{\phi}{\phi-1}\Big)\Bigg(\frac{\gamma^{\frac{1}{1-\phi}}\mu_g^{\frac{\phi}{1-\phi}}}{p_a^{\frac{\phi}{\phi-1}}}\Bigg) + \Big(\frac{\theta}{\theta-1}\Big)\Bigg(\frac{\gamma^{\frac{1}{1-\phi}}\mu_g^{\frac{\phi}{1-\phi}}}{p_a^{\frac{\phi}{\phi-1}}}\Bigg)\Bigg(\frac{A^{\frac{1}{1-\theta}}p_a^{\frac{1}{\theta-1}}p_a}{\bar{p}^{\frac{\theta}{\theta-1}}} - 1\Bigg)$$

$$= \underbrace{\Bigg(\frac{\gamma^{\frac{1}{1-\phi}}\mu_g^{\frac{\phi}{1-\phi}}}{p_a^{\frac{\phi}{\phi-1}}}\Bigg)}_{\text{positive}}\Bigg(\frac{\phi}{\phi-1} - \Big(\frac{\theta}{\theta-1}\Big)\Bigg(\frac{B^{\frac{1}{1-\theta}}p_b^{\frac{\theta}{\theta-1}}}{\bar{p}^{\frac{\theta}{\theta-1}}}\Bigg)\Bigg)$$

Then $\frac{\partial \log I_{2a}}{\partial \log \mu_g} < 0$ if and only if:

$$\frac{\phi}{\phi-1} - \Big(\frac{\theta}{\theta-1}\Big)\left(\frac{B^{\frac{1}{1-\theta}} p_b^{\frac{\theta}{\theta-1}}}{\bar{p}^{\frac{\theta}{\theta-1}}}\right) < 0$$

$$\frac{\phi}{1-\phi} > \frac{\theta}{1-\theta} \cdot \left(\frac{B^{\frac{1}{1-\theta}} p_b^{\frac{\theta}{\theta-1}}}{\bar{p}^{\frac{\theta}{\theta-1}}}\right)$$

Since $\frac{B^{\frac{1}{1-\theta}} p_b^{\frac{\theta}{\theta-1}}}{\bar{p}^{\frac{\theta}{\theta-1}}} < 1$, a sufficient condition for $\frac{\partial \log I_{2a}}{\partial \log \mu_g} < 0$ is: $0 < \theta < \phi < 1$. Intuitively, there are two opposing effects of relative program exposure on period 2 investment in child a. When θ is high, parents view their children as substitutes, and will try to maximize the outcomes for child a. This will have the effect of increasing I_{2a}. However, when ϕ is large, substitution of investments between periods is relatively easy. Consequently, parents will move resources away from period 2 towards period 1, so as to take advantage of the complementarity of their investments with program exposure. This will have the effect of decreasing I_{2a}. The comparative statics show that when the latter effect dominates, i.e. when $\phi > \theta$, parents will reduce period 2 investments in child a.

For child b, I can proceed in a similar manner by taking logs on equation (36):

$$\begin{aligned}
\log I_{1b} &= \log \bar{y} + \log B^{\frac{1}{1-\theta}} + \log p_b^{\frac{\theta}{\theta-1}} - \log \bar{p}^{\frac{\theta}{\theta-1}} + \log \gamma^{\frac{1}{1-\phi}} - \log p_b^{\frac{\phi}{\phi-1}} \\
&= \log \bar{y} + \log B^{\frac{1}{1-\theta}} + \log \gamma^{\frac{1}{1-\phi}} + \Big(\frac{\theta}{\theta-1} - \frac{\phi}{\phi-1}\Big)\log p_b - \frac{\theta}{\theta-1}\log \bar{p}
\end{aligned}$$

Then I have that:

$$\begin{aligned}
\frac{\partial \log I_{1b}}{\partial \log \mu_g} &= -\frac{\theta}{\theta-1} \cdot \frac{\partial \log \bar{p}}{\partial \log \mu_g} \\
&= \frac{\theta}{1-\theta} \cdot \underbrace{\frac{\partial \log \bar{p}}{\partial \log \mu_g}}_{\text{negative}}
\end{aligned}$$

Therefore, $\frac{\partial \log I_{1b}}{\partial \log \mu_g} < 0$ if and only if $\frac{\theta}{1-\theta} > 0$, i.e. $0 < \theta < 1$.
Furthermore, taking logs on equation (25), I have that:

$$\log I_{2b} = \log I_{1b} + \frac{1}{1-\phi}\log\Big(\frac{1-\gamma}{\gamma} \cdot \frac{1}{p}\Big)$$

Therefore, $\frac{\partial \log I_{2b}}{\partial \log \mu_g} = \frac{\partial \log I_{1b}}{\partial \log \mu_g} < 0$ if and only if $0 < \theta < 1$.

To summarize the comparative statics on parental investments, for $0 < \theta < \phi < 1$, I have the following theoretical predictions: $\frac{\partial I_{1a}}{\partial \mu_g} > 0$, $\frac{\partial I_{2a}}{\partial \mu_g} < 0$, $\frac{\partial I_{1b}}{\partial \mu_g} < 0$, $\frac{\partial I_{2b}}{\partial \mu_g} < 0$.

To solve for debt D, I use the period 2 budget constraint:

$$\begin{aligned} I_{2a} + I_{2b} &= \bar{y}_2 - pD \\ D &= \frac{\bar{y}_2}{p} - \frac{I_{2a} + I_{2b}}{p} \end{aligned}$$

Then I have that for $0 < \theta < \phi < 1$:

$$\frac{\partial D}{\partial \mu_g} = \underbrace{-\frac{1}{p}}_{\text{negative}} \cdot \underbrace{\left(\frac{\partial I_{2a}}{\partial \mu_g} + \frac{\partial I_{2b}}{\partial \mu_g}\right)}_{\text{negative}} > 0$$

Lastly, I can derive comparative statics for the human capital of child a and b at the end of their development cycles. For child a, taking logs on equation (26), I note that:

$$\log h_a = \log I_{1a} + \frac{1}{\phi - 1}\log \gamma + \frac{\phi}{\phi - 1}\log \mu_g + \frac{1}{\phi - 1}\log p_a$$

Then I have that:

$$
\begin{aligned}
\frac{\partial \log h_a}{\partial \log \mu_g} &= \frac{\partial \log I_{1a}}{\partial \log \mu_g} + \frac{\phi}{\phi - 1} + \frac{1}{\phi - 1} \cdot \frac{\partial \log p_a}{\partial \log \mu_g} \\
&= \Big(\frac{\phi}{1-\phi}\Big)\left(\frac{(1-\gamma)^{\frac{1}{1-\phi}} p^{\frac{\phi}{\phi-1}}}{p_a^{\frac{\phi}{\phi-1}}}\right) + \Big(\frac{\theta}{1-\theta}\Big)\left(\frac{\gamma^{\frac{1}{1-\phi}} \mu_g^{\frac{\phi}{1-\phi}}}{p_a^{\frac{\phi}{\phi-1}}}\right)\left(\frac{B^{\frac{1}{1-\theta}} p_b^{\frac{\theta}{\theta-1}}}{\bar{p}^{\frac{\theta}{\theta-1}}}\right) \\
&+ \frac{\phi}{\phi-1} + \Big(\frac{1}{\phi-1}\Big)\left(-\frac{\gamma^{\frac{1}{1-\phi}} \mu_g^{\frac{\phi}{1-\phi}}}{p_a^{\frac{\phi}{\phi-1}}}\right) \\
&= \Big(\frac{\phi}{1-\phi}\Big)\left(\frac{(1-\gamma)^{\frac{1}{1-\phi}} p^{\frac{\phi}{\phi-1}}}{p_a^{\frac{\phi}{\phi-1}}} - 1\right) + \Big(\frac{\theta}{1-\theta}\Big)\left(\frac{\gamma^{\frac{1}{1-\phi}} \mu_g^{\frac{\phi}{1-\phi}}}{p_a^{\frac{\phi}{\phi-1}}}\right)\left(\frac{B^{\frac{1}{1-\theta}} p_b^{\frac{\theta}{\theta-1}}}{\bar{p}^{\frac{\theta}{\theta-1}}}\right) \\
&+ \Big(\frac{1}{1-\phi}\Big)\left(\frac{\gamma^{\frac{1}{1-\phi}} \mu_g^{\frac{\phi}{1-\phi}}}{p_a^{\frac{\phi}{\phi-1}}}\right) \\
&= \Big(\frac{\phi}{1-\phi}\Big)\left(-\frac{\gamma^{\frac{1}{1-\phi}} \mu_g^{\frac{\phi}{1-\phi}}}{p_a^{\frac{\phi}{\phi-1}}}\right) + \Big(\frac{\theta}{1-\theta}\Big)\left(\frac{\gamma^{\frac{1}{1-\phi}} \mu_g^{\frac{\phi}{1-\phi}}}{p_a^{\frac{\phi}{\phi-1}}}\right)\left(\frac{B^{\frac{1}{1-\theta}} p_b^{\frac{\theta}{\theta-1}}}{\bar{p}^{\frac{\theta}{\theta-1}}}\right) \\
&+ \Big(\frac{1}{1-\phi}\Big)\left(\frac{\gamma^{\frac{1}{1-\phi}} \mu_g^{\frac{\phi}{1-\phi}}}{p_a^{\frac{\phi}{\phi-1}}}\right)
\end{aligned}
$$

Therefore:

$$
\begin{aligned}
\frac{\partial \log h_a}{\partial \log \mu_g} &= \frac{\gamma^{\frac{1}{1-\phi}} \mu_g^{\frac{\phi}{1-\phi}}}{p_a^{\frac{\phi}{\phi-1}}} + \Big(\frac{\theta}{1-\theta}\Big)\left(\frac{\gamma^{\frac{1}{1-\phi}} \mu_g^{\frac{\phi}{1-\phi}}}{p_a^{\frac{\phi}{\phi-1}}}\right)\left(\frac{B^{\frac{1}{1-\theta}} p_b^{\frac{\theta}{\theta-1}}}{\bar{p}^{\frac{\theta}{\theta-1}}}\right) \\
&= \left(\frac{\gamma^{\frac{1}{1-\phi}} \mu_g^{\frac{\phi}{1-\phi}}}{p_a^{\frac{\phi}{\phi-1}}}\right)\left(1 + \frac{\theta}{1-\theta} \cdot \frac{B^{\frac{1}{1-\theta}} p_b^{\frac{\theta}{\theta-1}}}{\bar{p}^{\frac{\theta}{\theta-1}}}\right) > 0
\end{aligned}
$$

Intuitively, h_a increases due to (i) the direct effect of relative program exposure μ_g, (ii) the intertemporal reallocation of investments *within* child a, and (iii) the intra-household reallocation of investments away from child b towards child a.

For child b, taking logs on equation (30) yields the following:

$$\log h_b = \log I_{1b} + \frac{1}{\phi - 1} \log \gamma + \frac{1}{\phi - 1} \log p_b$$

Therefore:

$$\frac{\partial \log h_b}{\partial \log \mu_g} = \frac{\partial \log I_{1b}}{\partial \log \mu_g} < 0$$

Intuitively, h_b decreases due to the intra-household reallocation of investments away from child b in both periods. To summarize the results on human capital, $\frac{\partial h_a}{\partial \mu_g} > 0$ and $\frac{\partial h_b}{\partial \mu_g} < 0$.

B Robustness Checks: Placebo Tests

In this section, I present results from the specification outlined in equation (2). Overall, program impacts arise in the age range over which individuals are eligible to avail services from the program (ages -1 to 6), and not before or after.

[Figure B.1 about here.]

[Figure B.2 about here.]

[Figure B.3 about here.]

[Figure B.4 about here.]

[Figure B.5 about here.]

[Figure B.6 about here.]

[Figure B.7 about here.]

[Figure B.8 about here.]

[Figure B.9 about here.]

[Figure B.10 about here.]

[Figure B.11 about here.]

[Figure B.12 about here.]

[Figure B.13 about here.]

[Figure B.14 about here.]

C Robustness Checks: Mid-Day Meal Program

In this section, I repeat my analysis, explicitly controlling for a large government program aimed at improving health and nutrition of children of primary school going age. As Chakraborty and Jayaraman (2016) note, India implemented a free school lunch program known as the mid-day meal program, in large part following a 2001 Indian Supreme Court Directive. The implementation of the program did not take place immediately or all at once, but over the next five years states across India implemented the program until, by 2006, every Indian state had instituted a free school lunch in primary schools. One possible concern might be that the rollout of the mid-day meal program was correlated with the rollout of the ICDS program. If this were the case, the impacts that I attribute to the ICDS program may in fact be contaminated by impacts from the mid-day meal program.

I present several pieces of evidence to argue that this is not a concern. First, the ICDS program and the mid-day meal program do not share the same infrastructure or budget allocations. Meals for children under six years of age are prepared in ICDS centers, while meals for primary-school going children are prepared in schools. Furthermore, the budget allocations, including the division of funding between the central and state governments, are different for the two programs. Second, the placebo tests I present by age show that there are no impacts from the ICDS program arising from primary school going ages. On the contrary, the impacts arise over the age range for which children are eligible to avail services from the ICDS program, i.e. ages -1 to 6. Third, and in this section, I explicitly control for the rollout of the mid-day meal program and re-run my analysis. I show that my results are robust to the inclusion of the mid-day meal controls. I construct mid-day meal exposure as per Chakraborty and Jayaraman (2016). However, data on mid-day meal program rollout is only available for 24 out of 36 states and union territories in India. Despite the reduction in sample sizes and power, I show that my results are robust to the inclusion of the control.

[Figure C.1 about here.]

[Figure C.2 about here.]

[Figure C.3 about here.]

[Figure C.4 about here.]

Figures C.1 and C.2 present short and long-term program impacts controlling for the mid-day meal program, while figures C.3 and C.4 present the results on intra-household and intertemporal reallocation of parental investments with the mid-day meal control. Overall, the results are robust to explicitly controlling for the mid-day meal program.

D Robustness Checks: Program Placement & Village Infrastructure

In this section, I check whether program placement was correlated to existing village infrastructure. If it were the case that ICDS centers were systematically built in areas with better or worse infrastructure, a potential concern could be that the results may be biased due to non-random program placement. To test this hypothesis, I use village-level data from rounds 1 and 2 of the NFHS and run the following empirical specification for village v in district sub-division j of state s and time t:[10]

$$Y_{vjst} = \alpha + \gamma_j + \lambda_{st} + \beta P_{jst} + X_{vjst}\delta + \epsilon_{vjst} \tag{43}$$

where Y_{vjst} is the outcome variable of interest, γ_j are district sub-division fixed effects, λ_{st} are state x year fixed effects, P_{jst} refers to the intensity of the program (number of centers per 1,000 children) in the district sub-division at time t, and X_{vjst} are village-level controls for a quadratic population polynomial. β is the coefficient of interest. The inclusion of district sub-division fixed effects and state x year fixed effects closely mimics the main empirical specification (1) that includes district sub-division fixed effects and state x cohort fixed effects with age controls.

[Table D.1 about here.]

Table D.1 presents the results using specification (43) to study the relationship between program placement and existing village infrastructure. Overall, there are no statistically significant correlations between village infrastructure and program intensity across education, health, and other infrastructure. Thus it is unlikely that endogenous program placement by existing village infrastructure is a concern in this setting.

[10] Village-level data is not yet available for the fourth wave of the NFHS (2015-2016).

E Robustness Checks: Village Fixed Effects

In this section, I repeat my analysis, including in my regressions village fixed effects. Despite using this specification that is far more restrictive in comparison to the baseline specifications, I show that my results are robust to the inclusion of village fixed effects.

[Figure E.1 about here.]

[Figure E.2 about here.]

[Figure E.3 about here.]

[Figure E.4 about here.]

Figures E.1 and E.2 present short and long-term program impacts with village fixed effects, while figures E.3 and E.4 present the results on intra-household and intertemporal reallocation of parental investments with village fixed effects. Notably, the standard errors are slightly larger with the inclusion of village fixed effects. For example, three results on the intertemporal reallocation of parental investments - loans for children aged 0-6, adult goods for children aged 7-13, and tuition for children aged 7-13 - are statistically significant at p-values 0.125, 0.107, and 0.126, respectively. Overall, the results are robust to the inclusion of village fixed effects.

F Robustness Checks: State-Specific Time Trends

In this section, I repeat my analysis, including in my regressions state-specific linear time trends. This addresses the concern that program placement might be correlated with the evolution of health, education, or labor market outcomes in states over time. I show that my results are robust to the inclusion of state-specific linear time trends.

[Figure F.1 about here.]

[Figure F.2 about here.]

[Figure F.3 about here.]

[Figure F.4 about here.]

Figures F.1 and F.2 present short and long-term program impacts with state-specific linear time trends, while figures F.3 and F.4 present the results on intra-household and intertemporal reallocation of parental investments with state-specific linear time trends. The results are robust to the inclusion of state-specific linear time trends.

G Empirical Tests for Dynamic Complementarities

In an important contribution to the literature on human capital formation, Cunha and Heckman (2007) introduce the idea of *dynamic complementarities*, where skills produced at one age raise the productivity of investment at subsequent ages. This implies that levels of skill investments at different ages bolster each other. Empirical evidence of dynamic complementarities, however, remains limited.[11]

Several recent papers have exploited a "shock-shock" methodology to investigate dynamic complementarities in early childhood skill formation. Malamud et al. (2016) do not find evidence of dynamic complementarities in human capital formation when studying access to abortion and access to better schools in Romania. However, Adhvaryu et al. (2016) show that children whose families were randomized to receive conditional cash transfers through the Mexican government's Progresa policy experiment experienced a smaller decline in education and employment outcomes than control group children who experienced adverse rainfall in the year of birth. Johnson and Jackson (2018) show that the benefits of Head Start spending were larger when followed by access to better-funded public K12 schools in the U.S., while Gilraine (2018) presents evidence of dynamic complementarities in a major federal accountability scheme in North Carolina.

In this section, I employ a similar methodology and ask: do rainfall shocks at birth raise the productivity of ICDS program exposure? Rainfall shocks are particularly important in countries that are primarily agricultural since rainfed agricultural productivity decreases in drought years (Shah and Steinberg, 2017). As a result of the negative income effect, families have fewer resources to spend on human capital production, in the form of educational and nutritional inputs.

To study the interaction between rainfall shocks and the ICDS program, I construct rainfall shocks in a similar manner to Shah and Steinberg (2017). I first define district-year-level positive rainfall shock variables equal to one if rainfall in the district in the given year exceeded the 80th percentile of historical rainfall for the district. Negative rainfall shock variables were constructed in a similar manner if rainfall fell below the

[11] The use of this term has evolved over time, particularly in the empirical literature. I use the term *dynamic complementarities* to refer to the interaction between shocks, rather than the interaction between a shock and ability. Notably, there is an overlap in timing between the shocks I consider.

20th percentile of historical rainfall for the district. I then define an individual to be hit with a positive (negative) rainfall shock if her district received a positive (negative) rainfall shock in either (i) her year of birth, (ii) the year preceding her birth, or (iii) the year after her birth. The inclusion of years before and after birth reduces noise in the estimates that might arise due to misreporting of age in the household survey data.

For individual i in district sub-division j of district d of state s and birth year k, I then run the following specification:

$$Y_{ijdsk} = \alpha + \gamma_j + \lambda_{ks} + \beta_1 P_{jk} + \beta_2 Shock_{dk} + \beta_3 P_{jk} * Shock_{dk} + X_{ijk}\delta + \epsilon_{ijdsk} \tag{44}$$

where Y_{ijk} is the outcome variable of interest, γ_j represent district sub-division fixed effects, λ_{ks} represent cohort x state fixed effects, P_{jk} refers to the intensity of the program (number of centers per 1,000 children) in the district sub-division of birth at the time of birth, $Shock_{dk}$ refers to the rainfall shock (positive or negative), and X_{ijk} are controls including gender, birth order, gender x birth order interaction, a quadratic population polynomial, caste, religion, and mother's education. The interaction term β_3 then captures dynamic complementarities that might arise due to the interaction of rainfall shocks with ICDS program exposure.

[Table G.1 about here.]

[Table G.2 about here.]

[Figure G.1 about here.]

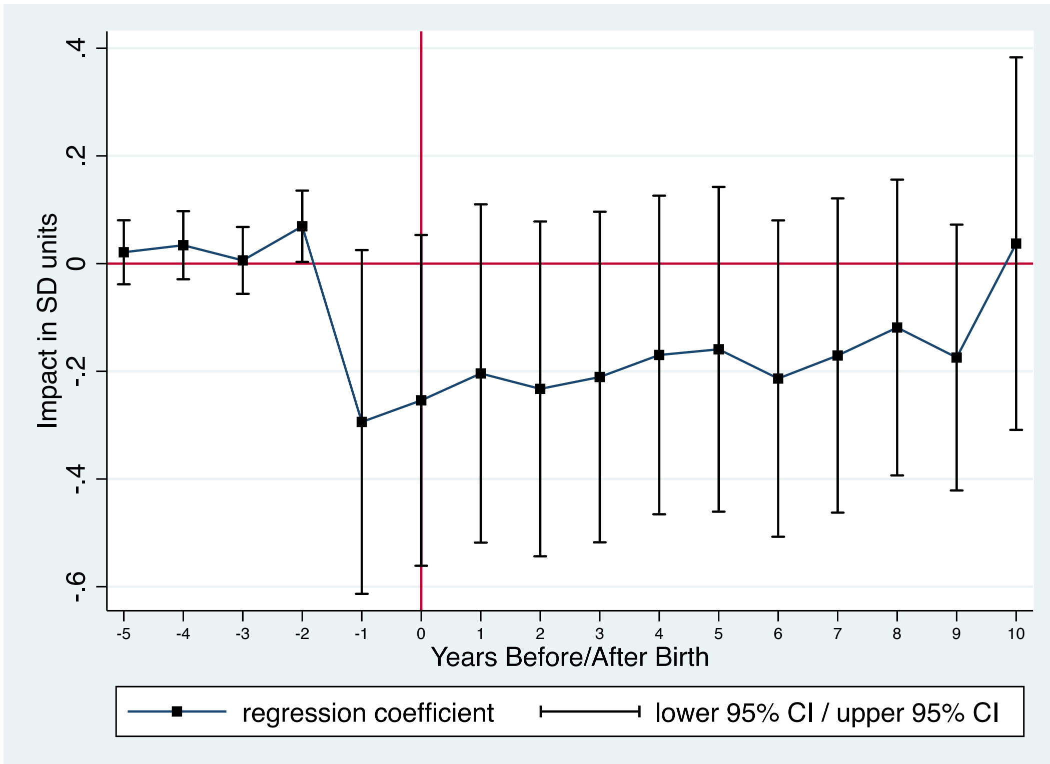
Figure G.1 presents results separately for dynamic complementarities with negative rainfall shocks and positive rainfall shocks. The point estimates capture the interaction term β_3 in specification (44). All estimates have been standardized using the mean and standard deviation of individuals with no ICDS program exposure. I also display the 90% confidence interval bars corresponding to each point estimate. I present all child outcome variables considered, comprising child health, education, and labor. Tables G.1 and G.2 present point estimates for coefficients β_1 and β_3 in specification (44), showing that the program impacts are robust to controlling for negative and positive rainfall shocks, respectively.

I do not find evidence of dynamic complementarities that arise due to positive rainfall shocks. The zero point estimates on education, in particular, are very tightly estimated due to the large sample size. I observe weak evidence of dynamic complementarities due to negative rainfall shocks along the dimensions of child height as well as reading and math test scores. However, I do not observe any statistically significant interactions with the malnourishment, stunting, the ability to read and do math, and child labor.

These results suggest that dynamic complementarities do not seem to play an important role in the context of the ICDS program. This is consistent with the result on the intertemporal reallocation of parental investments to earlier ages for children exposed to an increase in program intensity. If indeed it were the case that there are strong dynamic complementarities in this setting, parents should increase investments at later ages. This is because subsequent investments would be more productive as a result of earlier investments by the government. Thus the weak if any, evidence of dynamic complementarities is consistent with the result on the intertemporal reallocation of parental investments to earlier ages.

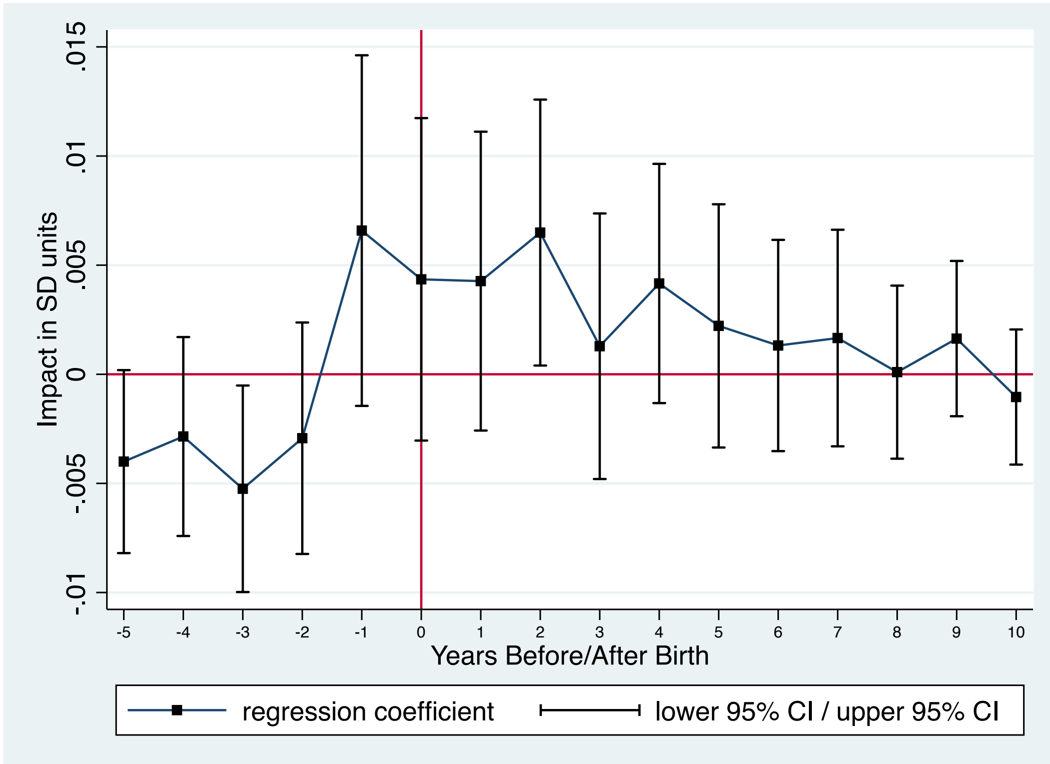
I do note that program take-up might respond to rainfall shocks. In particular, individuals exposed to positive rainfall shocks might reduce take-up of the program, while individuals exposed to negative rainfall shocks might increase program take-up. In this case, I would not expect to see a significant additional impact of program exposure when interacted with rainfall shocks, regardless of the presence of dynamic complementarities. In the absence of data on program take-up, it is difficult to assess whether individuals view the program and rainfall shocks as substitutes.

Figure B.1: Placebo Test: Underweight



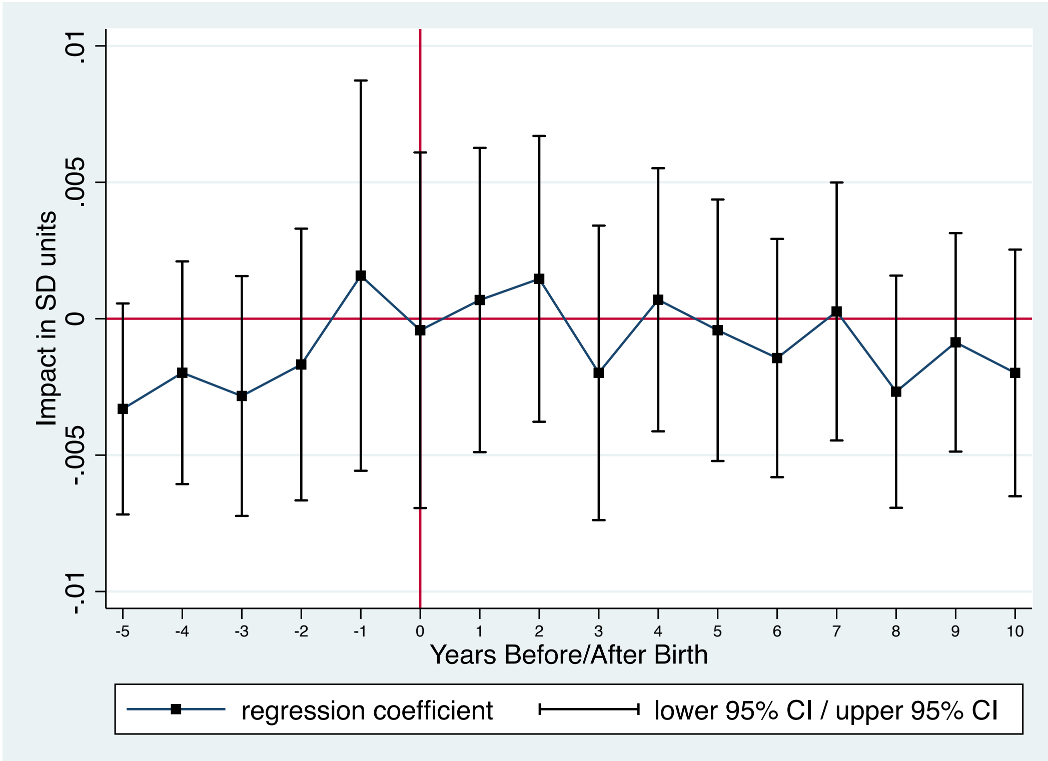
Notes: I plot in this figure the point estimates for β_y estimated using specification (2). The line corresponding to each point estimate reflects the 95% confidence interval for the outcome. The outcome variable has been standardized using the mean and standard deviation of individuals in district sub-divisions without the program. The regression includes district sub-division fixed effects, cohort x state fixed effects, controls for gender, birth order, gender x birth order interaction, quadratic population polynomial, caste, religion, mother's education, and program intensity in the individual's birth year. All standard errors are clustered by district sub-division.

Figure B.2: Placebo Test: Can Read



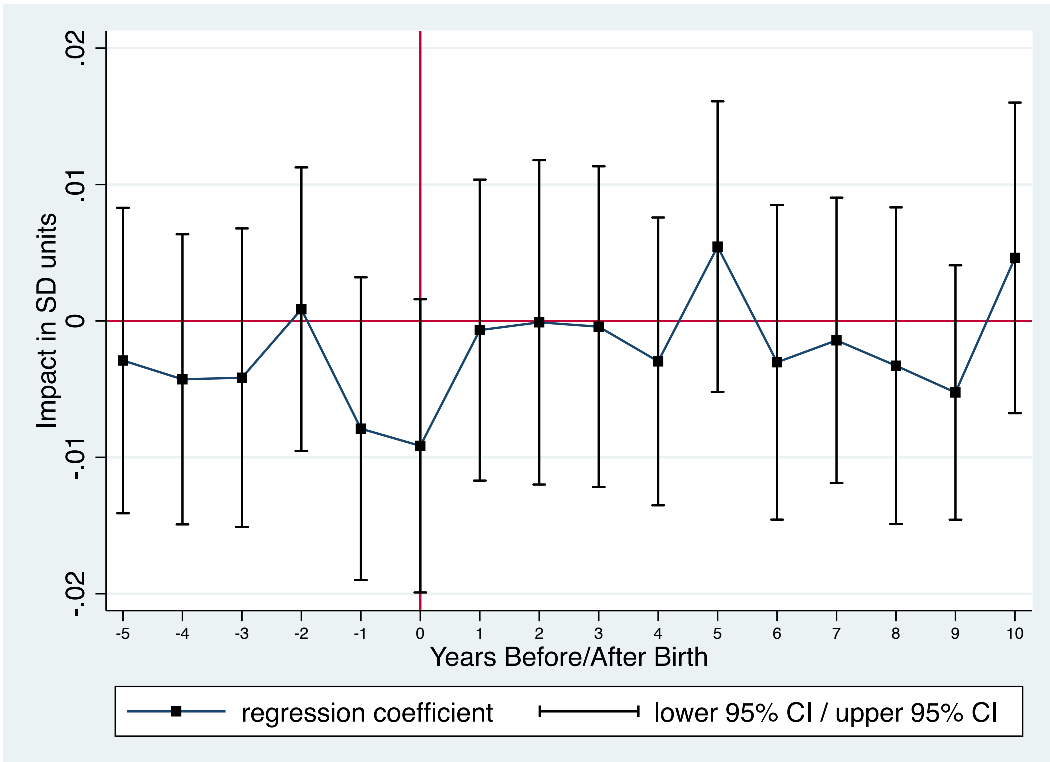
Notes: I plot in this figure the point estimates for β_y estimated using specification (2). The line corresponding to each point estimate reflects the 95% confidence interval for the outcome. The outcome variable has been standardized using the mean and standard deviation of individuals in district sub-divisions without the program. The regression includes district sub-division fixed effects, cohort x state fixed effects, controls for gender, birth order, gender x birth order interaction, quadratic population polynomial, mother's education, and program intensity in the individual's birth year. All standard errors are clustered by district sub-division.

Figure B.3: Placebo Test: Can Do Math



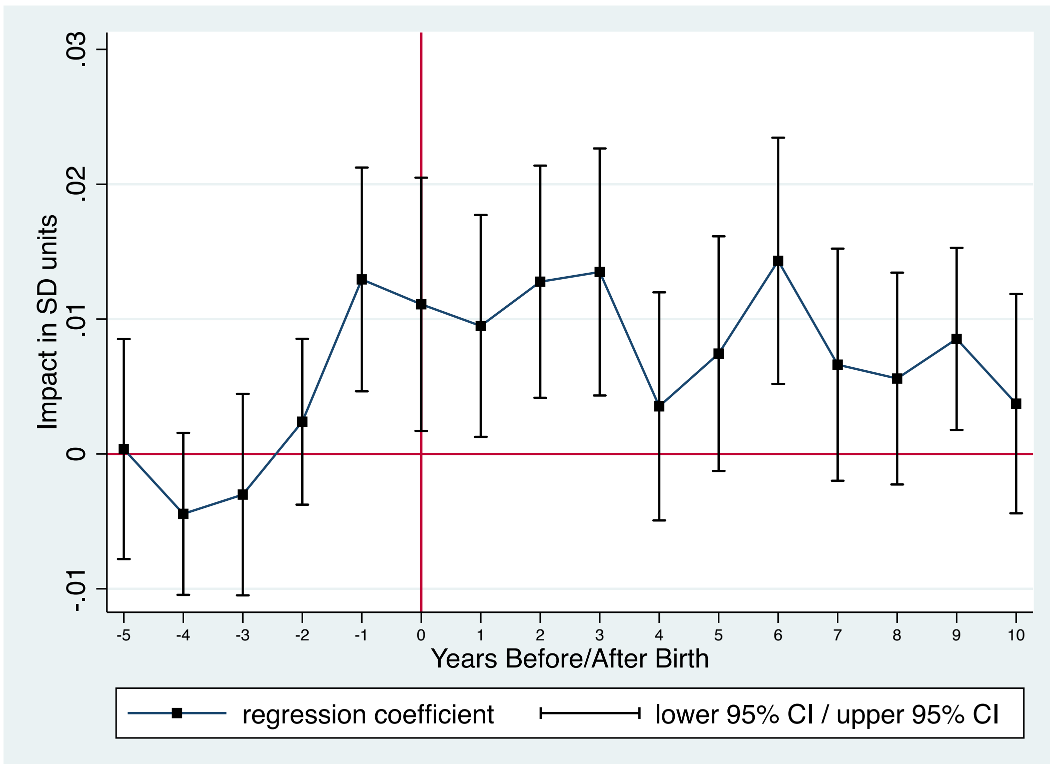
Notes: I plot in this figure the point estimates for β_y estimated using specification (2). The line corresponding to each point estimate reflects the 95% confidence interval for the outcome. The outcome variable has been standardized using the mean and standard deviation of individuals in district sub-divisions without the program. The regression includes district sub-division fixed effects, cohort x state fixed effects, controls for gender, birth order, gender x birth order interaction, quadratic population polynomial, mother's education, and program intensity in the individual's birth year. All standard errors are clustered by district sub-division.

Figure B.4: Placebo Test: Anemic



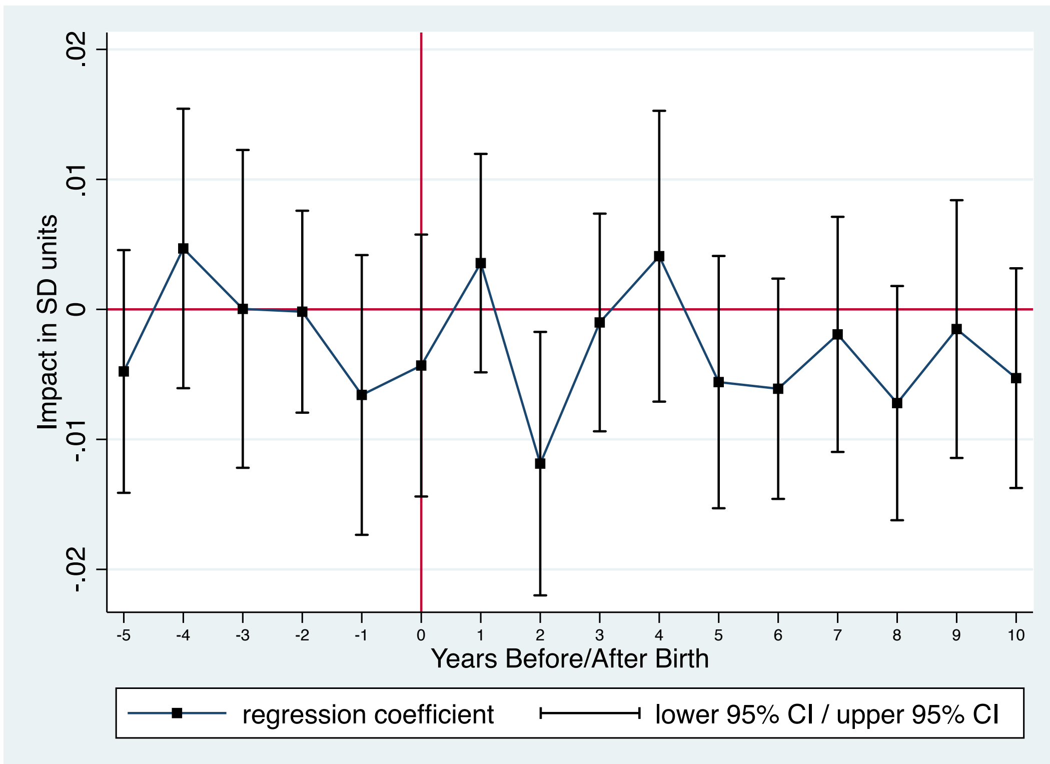
Notes: I plot in this figure the point estimates for β_y estimated using specification (2). The line corresponding to each point estimate reflects the 95% confidence interval for the outcome. The outcome variable has been standardized using the mean and standard deviation of individuals in district sub-divisions without the program. The regression includes district sub-division fixed effects, cohort x state fixed effects, controls for gender, quadratic population polynomial, caste, religion, and program intensity in the individual's birth year. All standard errors are clustered by district sub-division.

Figure B.5: Placebo Test: Log(Blood Glucose)



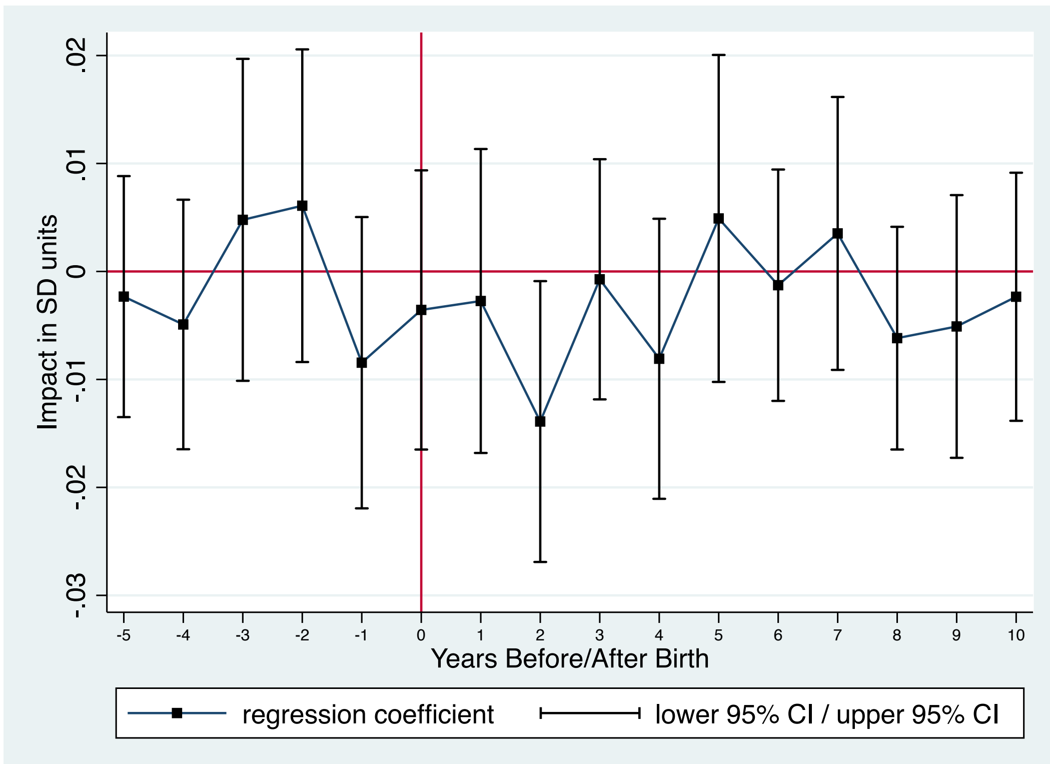
Notes: I plot in this figure the point estimates for β_y estimated using specification (2). The line corresponding to each point estimate reflects the 95% confidence interval for the outcome. The outcome variable has been standardized using the mean and standard deviation of individuals in district sub-divisions without the program. The regression includes district sub-division fixed effects, cohort x state fixed effects, controls for gender, quadratic population polynomial, caste, religion, and program intensity in the individual's birth year. All standard errors are clustered by district sub-division.

Figure B.6: Placebo Test: Hypoglycemia



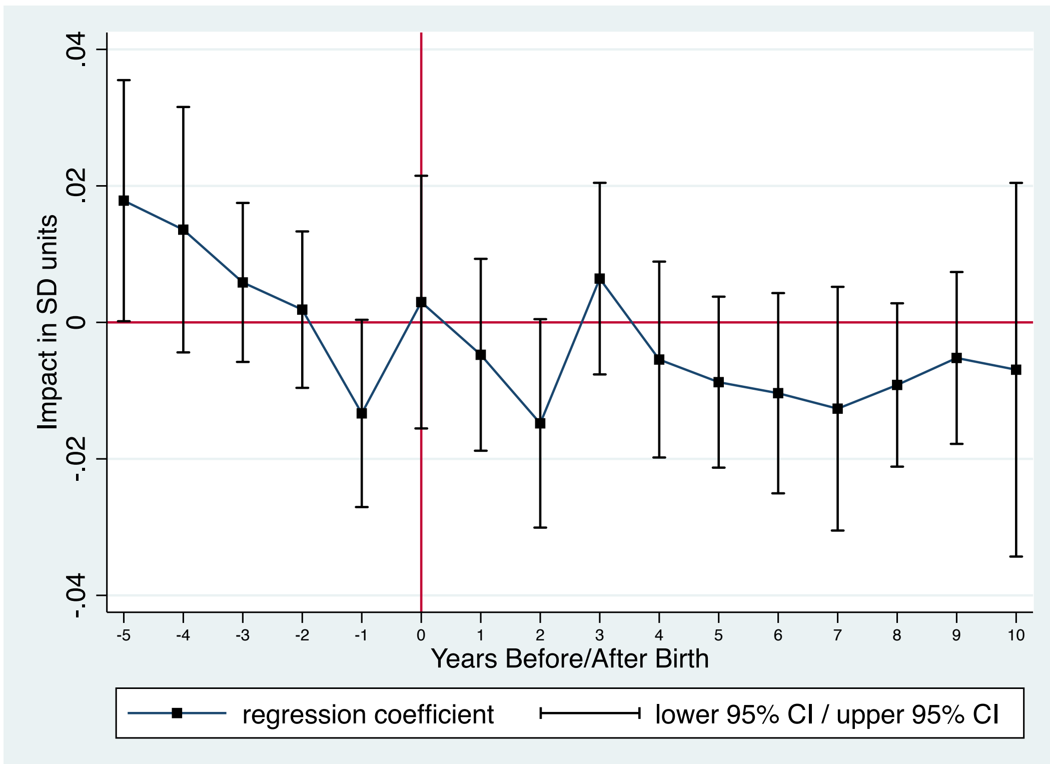
Notes: I plot in this figure the point estimates for β_y estimated using specification (2). The line corresponding to each point estimate reflects the 95% confidence interval for the outcome. The outcome variable has been standardized using the mean and standard deviation of individuals in district sub-divisions without the program. The regression includes district sub-division fixed effects, cohort x state fixed effects, controls for gender, quadratic population polynomial, caste, religion, and program intensity in the individual's birth year. All standard errors are clustered by district sub-division.

Figure B.7: Placebo Test: Any Short-Term Illness



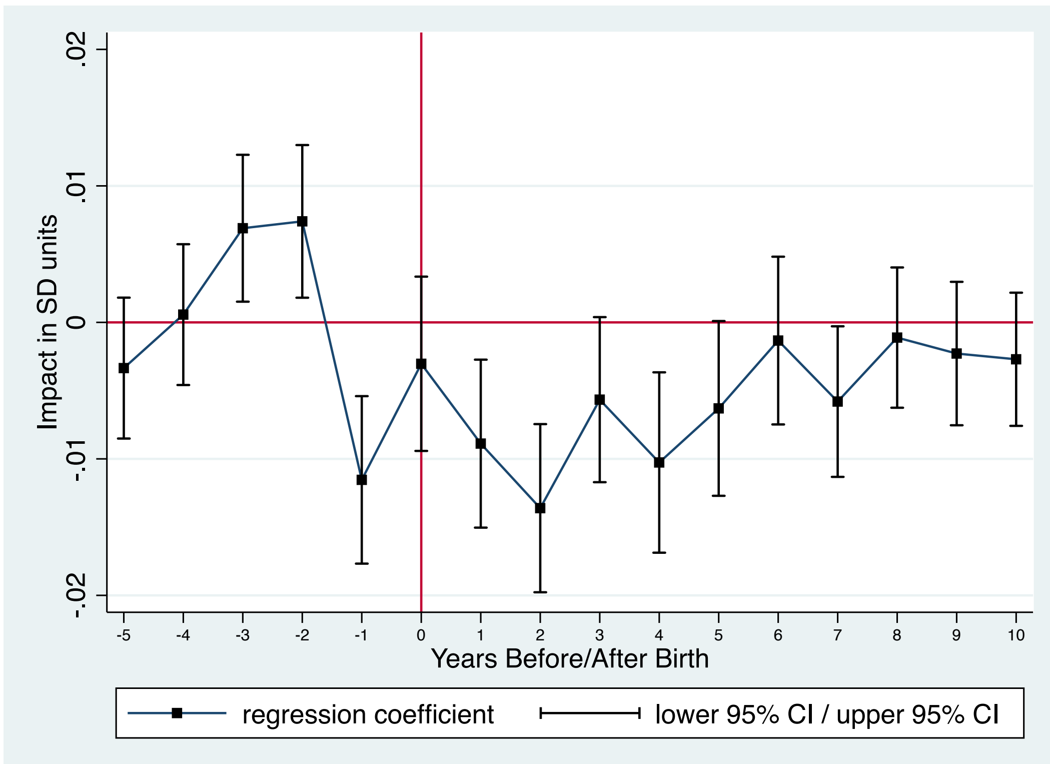
Notes: I plot in this figure the point estimates for β_y estimated using specification (2). The line corresponding to each point estimate reflects the 95% confidence interval for the outcome. The outcome variable has been standardized using the mean and standard deviation of individuals in district sub-divisions without the program. The regression includes district sub-division fixed effects, cohort x state fixed effects, controls for gender, quadratic population polynomial, caste, religion, and program intensity in the individual's birth year. All standard errors are clustered by district sub-division.

Figure B.8: Placebo Test: Very Poor Health



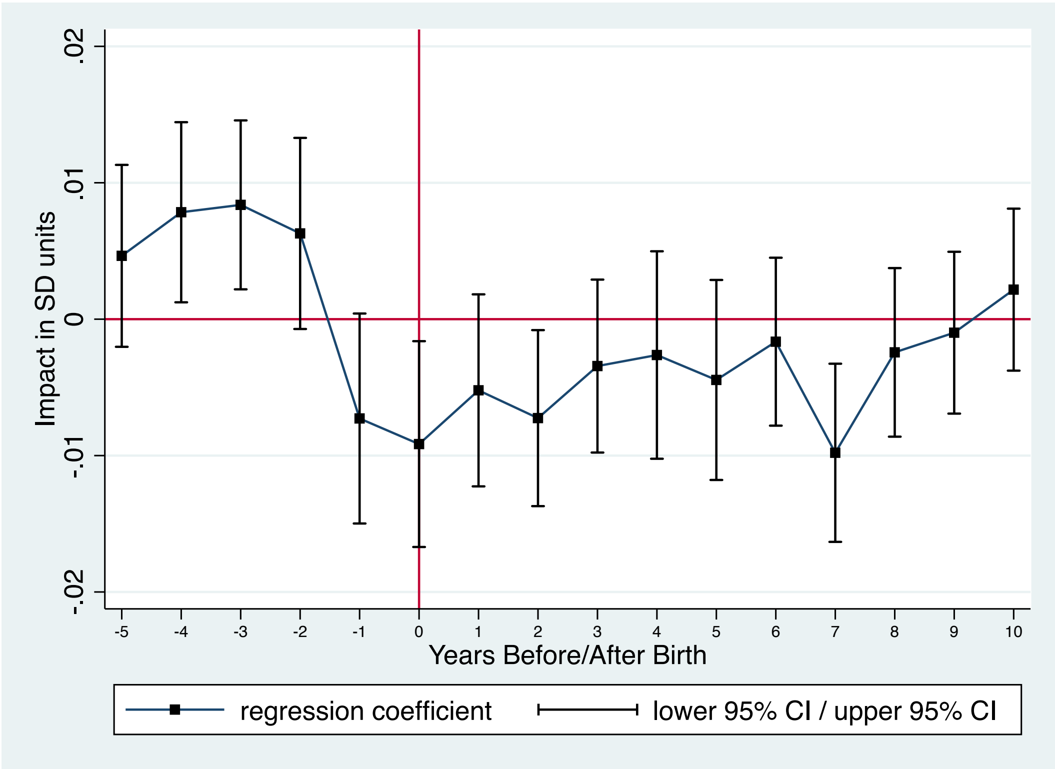
Notes: I plot in this figure the point estimates for β_y estimated using specification (2). The line corresponding to each point estimate reflects the 95% confidence interval for the outcome. The outcome variable has been standardized using the mean and standard deviation of individuals in district sub-divisions without the program. The regression includes district sub-division fixed effects, cohort x state fixed effects, controls for gender, quadratic population polynomial, caste, religion, and program intensity in the individual's birth year. All standard errors are clustered by district sub-division.

Figure B.9: Placebo Test: Smokes or Consumes Tobacco



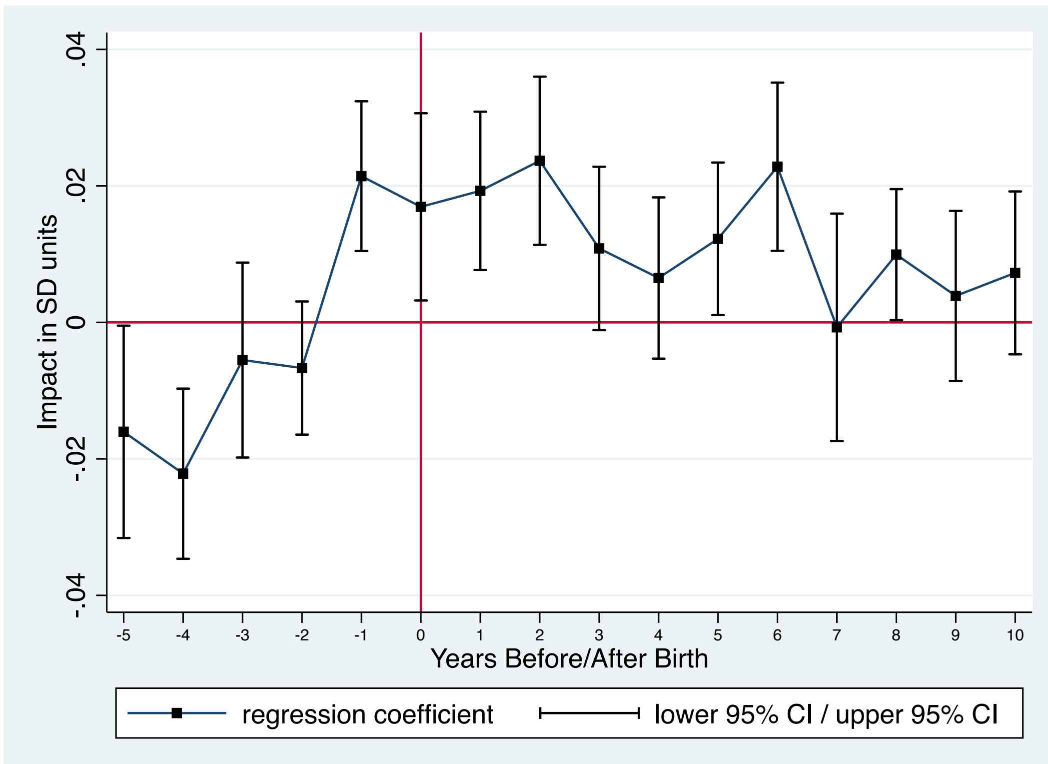
Notes: I plot in this figure the point estimates for β_y estimated using specification (2). The line corresponding to each point estimate reflects the 95% confidence interval for the outcome. The outcome variable has been standardized using the mean and standard deviation of individuals in district sub-divisions without the program. The regression includes district sub-division fixed effects, cohort x state fixed effects, controls for gender, quadratic population polynomial, caste, religion, and program intensity in the individual's birth year. All standard errors are clustered by district sub-division.

Figure B.10: Placebo Test: Consumes Alcohol



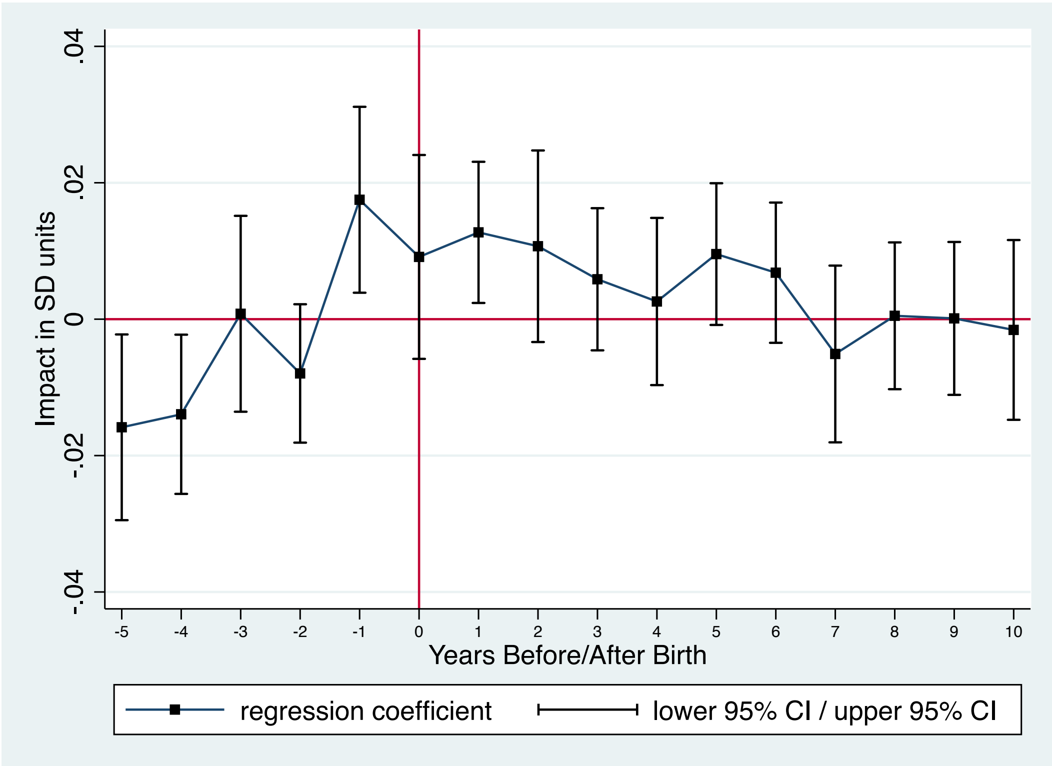
Notes: I plot in this figure the point estimates for β_y estimated using specification (2). The line corresponding to each point estimate reflects the 95% confidence interval for the outcome. The outcome variable has been standardized using the mean and standard deviation of individuals in district sub-divisions without the program. The regression includes district sub-division fixed effects, cohort x state fixed effects, controls for gender, quadratic population polynomial, caste, religion, and program intensity in the individual's birth year. All standard errors are clustered by district sub-division.

Figure B.11: Placebo Test: Literate



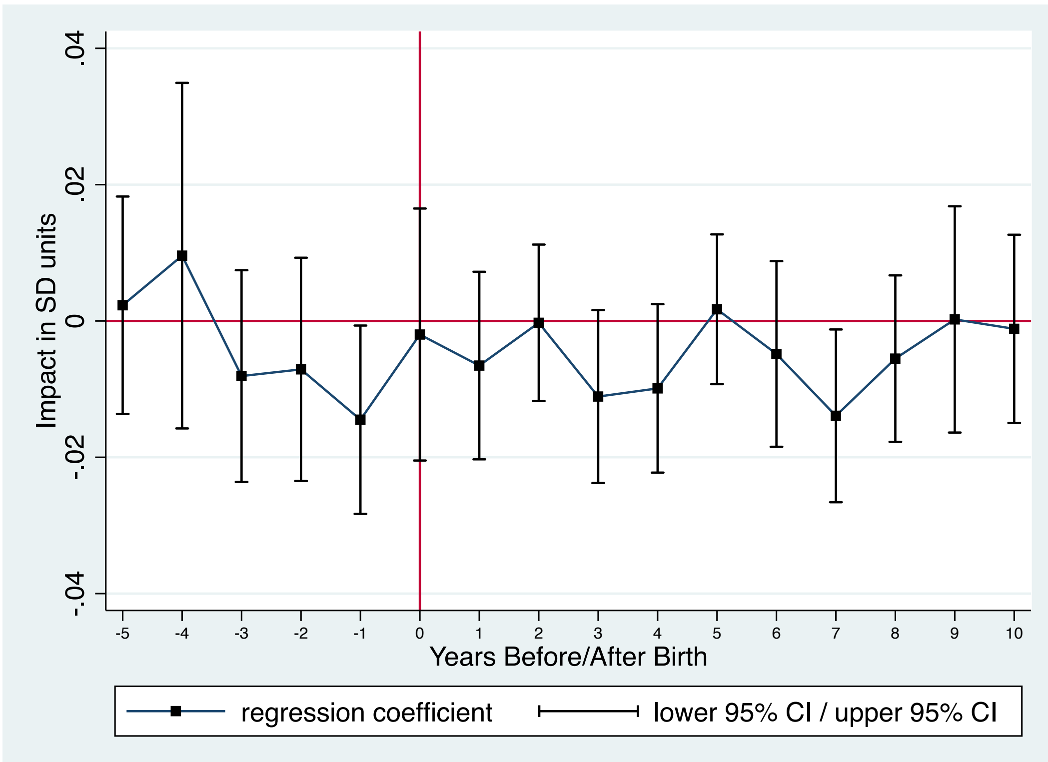
Notes: I plot in this figure the point estimates for β_y estimated using specification (2). The line corresponding to each point estimate reflects the 95% confidence interval for the outcome. The outcome variable has been standardized using the mean and standard deviation of individuals in district sub-divisions without the program. The regression includes district sub-division fixed effects, cohort x state fixed effects, controls for gender, quadratic population polynomial, caste, religion, and program intensity in the individual's birth year. All standard errors are clustered by district sub-division.

Figure B.12: Placebo Test: Years of Education



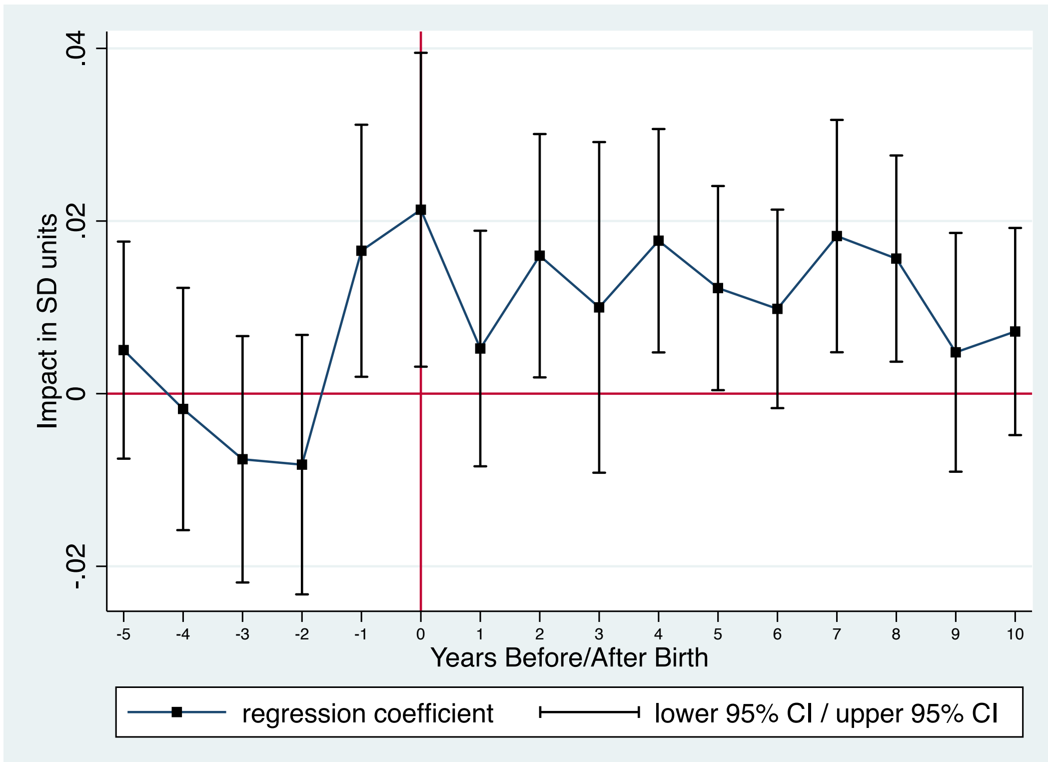
Notes: I plot in this figure the point estimates for β_y estimated using specification (2). The line corresponding to each point estimate reflects the 95% confidence interval for the outcome. The outcome variable has been standardized using the mean and standard deviation of individuals in district sub-divisions without the program. The regression includes district sub-division fixed effects, cohort x state fixed effects, controls for gender, quadratic population polynomial, caste, religion, and program intensity in the individual's birth year. All standard errors are clustered by district sub-division.

Figure B.13: Placebo Test: Unemployed



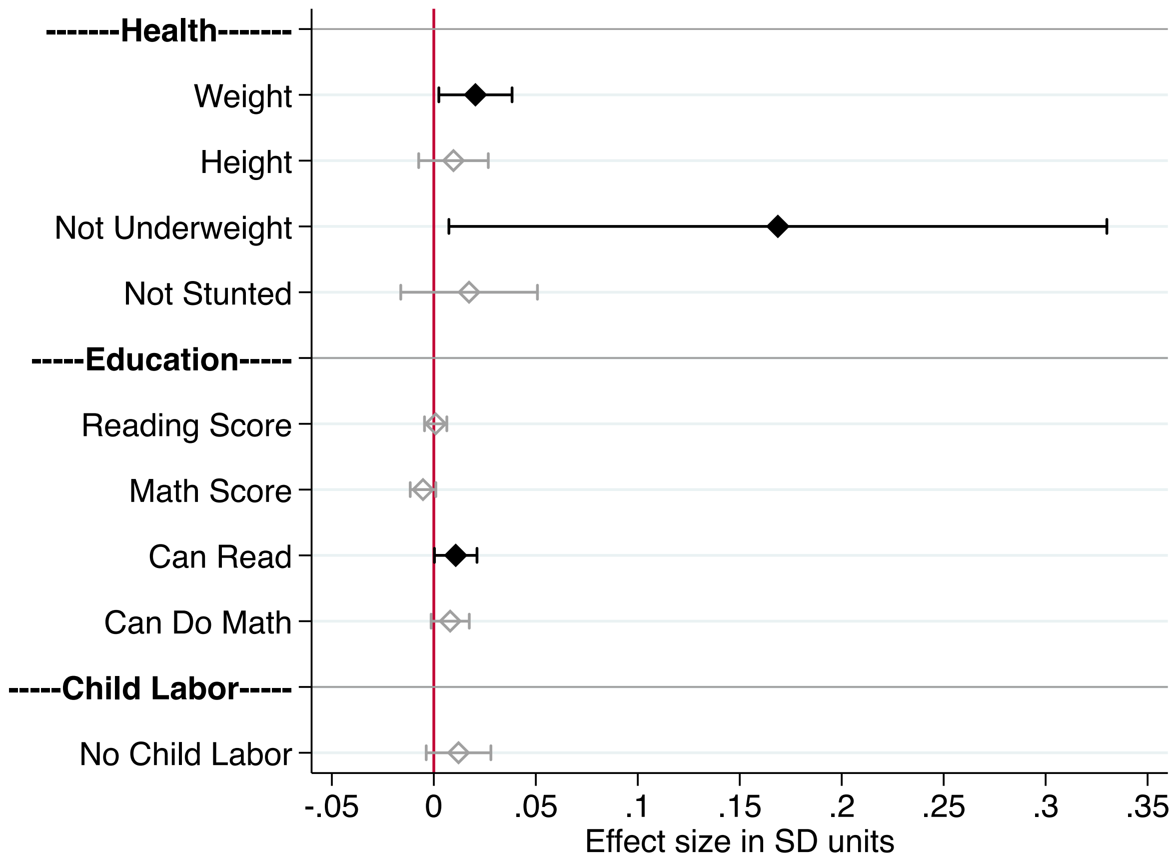
Notes: I plot in this figure the point estimates for β_y estimated using specification (2). The line corresponding to each point estimate reflects the 95% confidence interval for the outcome. The outcome variable has been standardized using the mean and standard deviation of individuals in district sub-divisions without the program. The regression includes district sub-division fixed effects, cohort x state fixed effects, controls for gender, quadratic population polynomial, caste, religion, and program intensity in the individual's birth year. All standard errors are clustered by district sub-division.

Figure B.14: Placebo Test: Log(Hourly Wage)



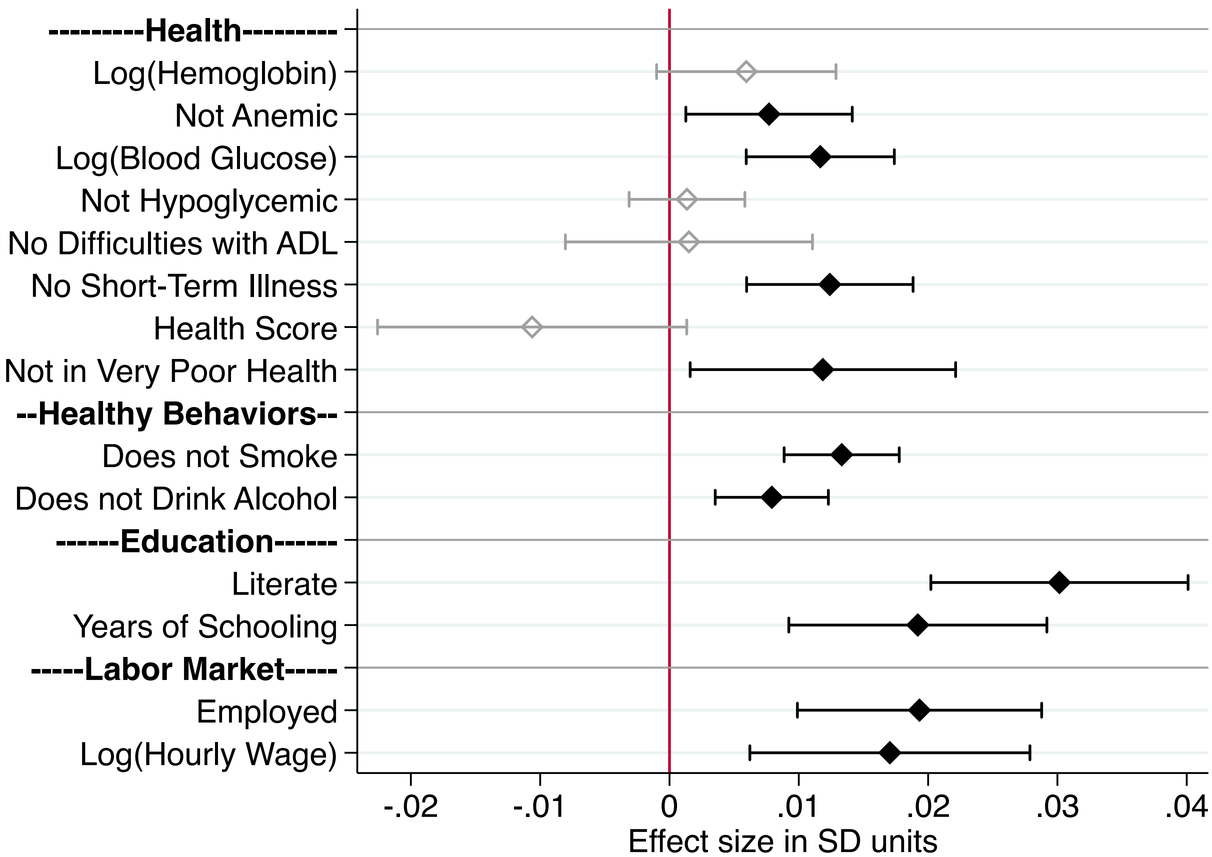
Notes: I plot in this figure the point estimates for β_y estimated using specification (2). The line corresponding to each point estimate reflects the 95% confidence interval for the outcome. The outcome variable has been standardized using the mean and standard deviation of individuals in district sub-divisions without the program. The regression includes district sub-division fixed effects, cohort x state fixed effects, controls for gender, quadratic population polynomial, caste, religion, and program intensity in the individual's birth year. All standard errors are clustered by district sub-division.

Figure C.1: Short-Term Impacts; Mid-Day Meal Controls



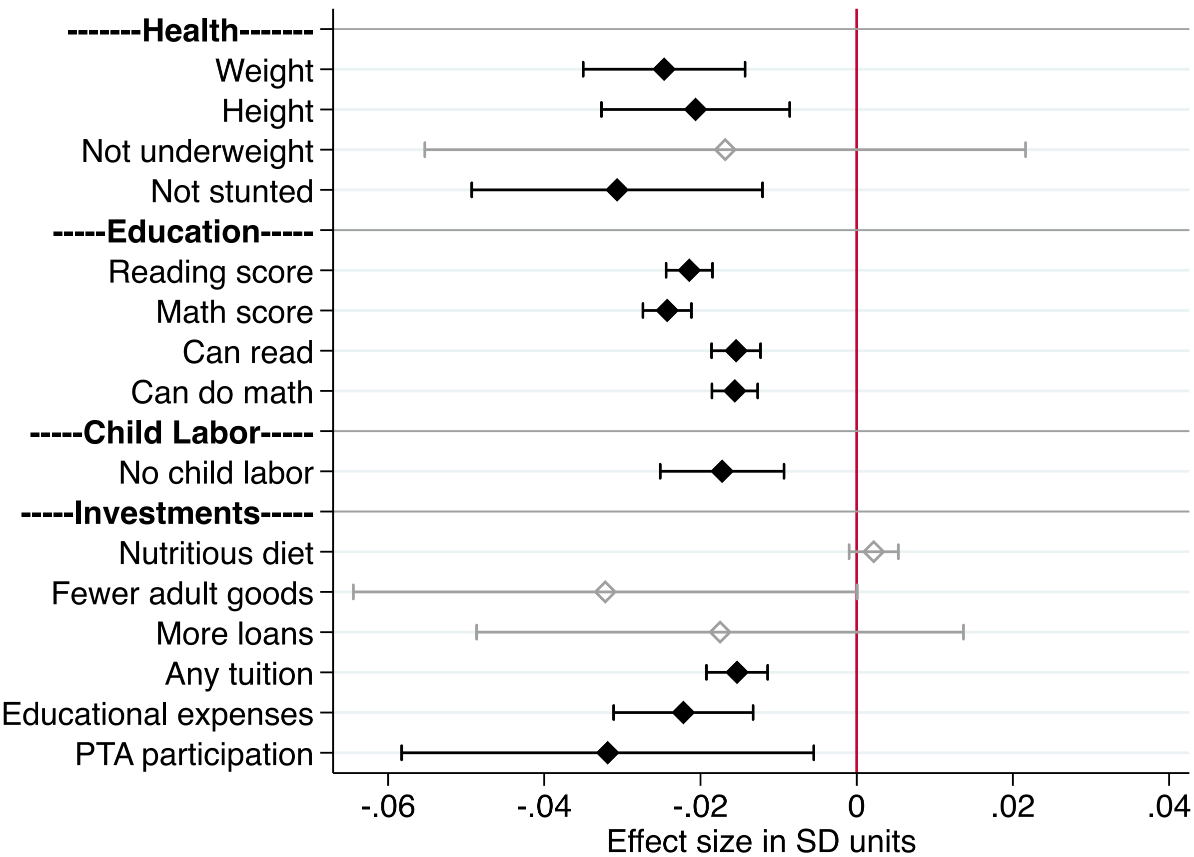
Notes: Each diamond plots the point estimate for β estimated using specification (1). The line corresponding to each point estimate reflects the 95% confidence interval for the outcome. Each outcome variable has been standardized using the mean and standard deviation of individuals in district sub-divisions without the program. The regressions include district sub-division fixed effects, cohort x state fixed effects, controls for gender, birth order, gender x birth order interaction, quadratic population polynomial, caste, religion, mother's education, and exposure to the mid-day meal program. All standard errors are clustered by district sub-division.

Figure C.2: Long-Term Impacts; Mid-Day Meal Controls



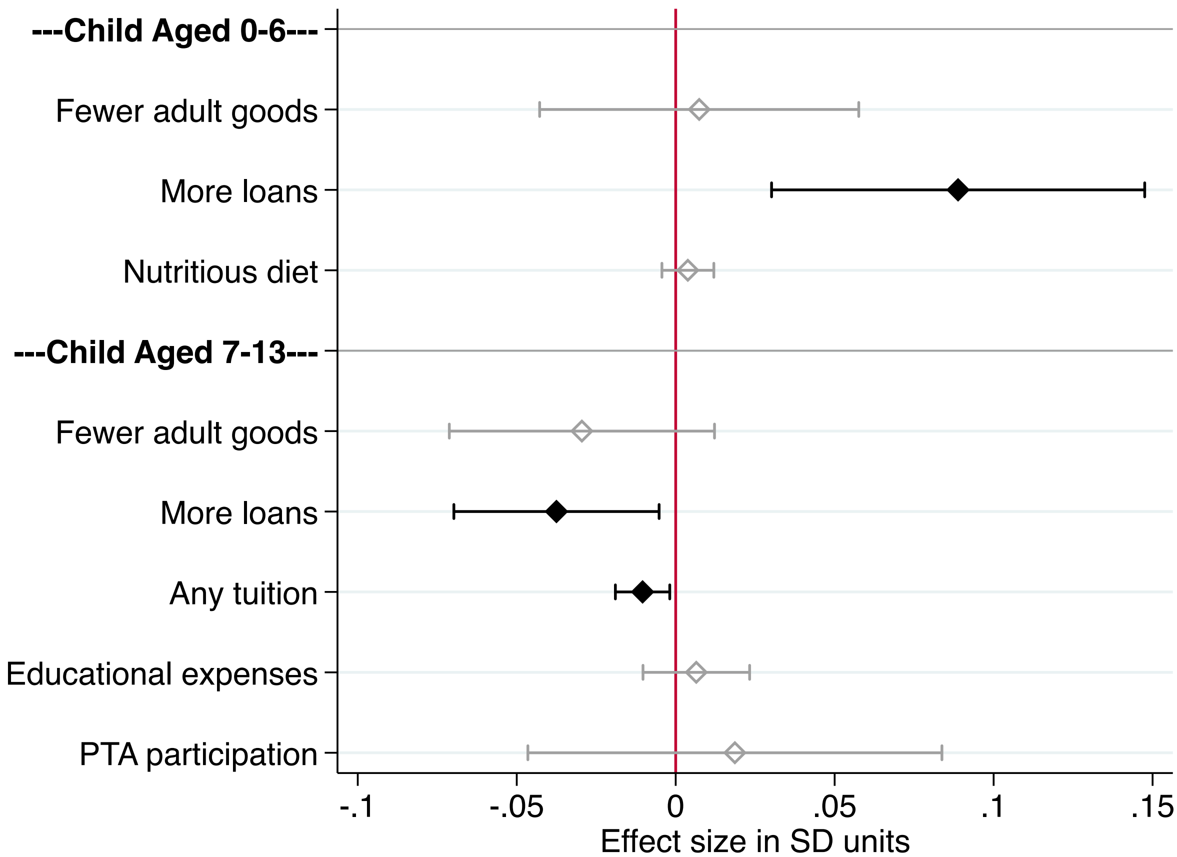
Notes: Each diamond plots the point estimate for β estimated using specification (1). The line corresponding to each point estimate reflects the 95% confidence interval for the outcome. Each outcome variable has been standardized using the mean and standard deviation of individuals in district sub-divisions without the program. The regressions include district sub-division fixed effects, cohort x state fixed effects, controls for gender, quadratic population polynomial, caste, religion, and exposure to the mid-day meal program. All standard errors are clustered by district sub-division.

Figure C.3: Intra-household Reallocation; Mid-Day Meal Controls



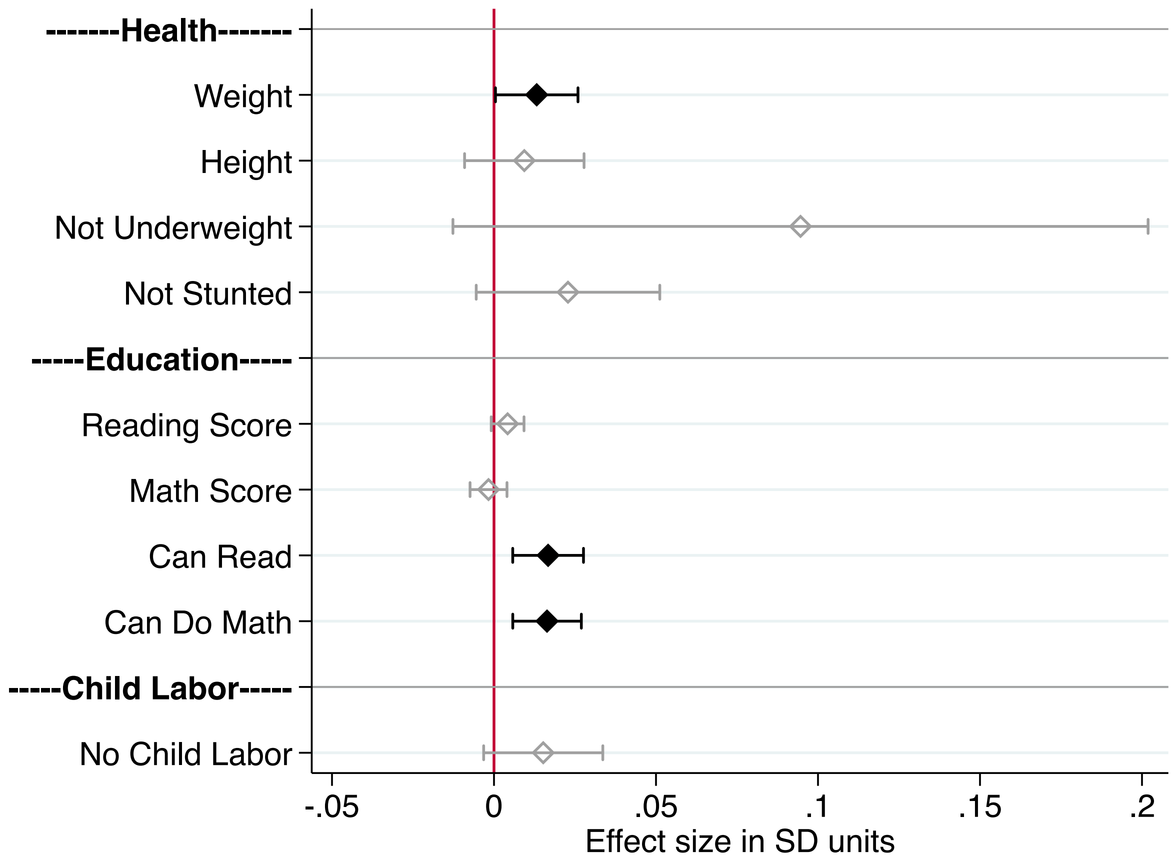
Notes: Each diamond plots the point estimate for β estimated using specification (3). The line corresponding to each point estimate reflects the 95% confidence interval for the outcome. Each outcome variable has been standardized using the mean and standard deviation of individuals in district sub-divisions without the program. The regressions include district sub-division x cohort x age fixed effects, gender x birth order fixed effects, and controls for quadratic population polynomial, caste, religion, mother's education, and exposure to the mid-day meal program. All standard errors are clustered by district sub-division.

Figure C.4: Intertemporal Reallocation; Mid-Day Meal Controls



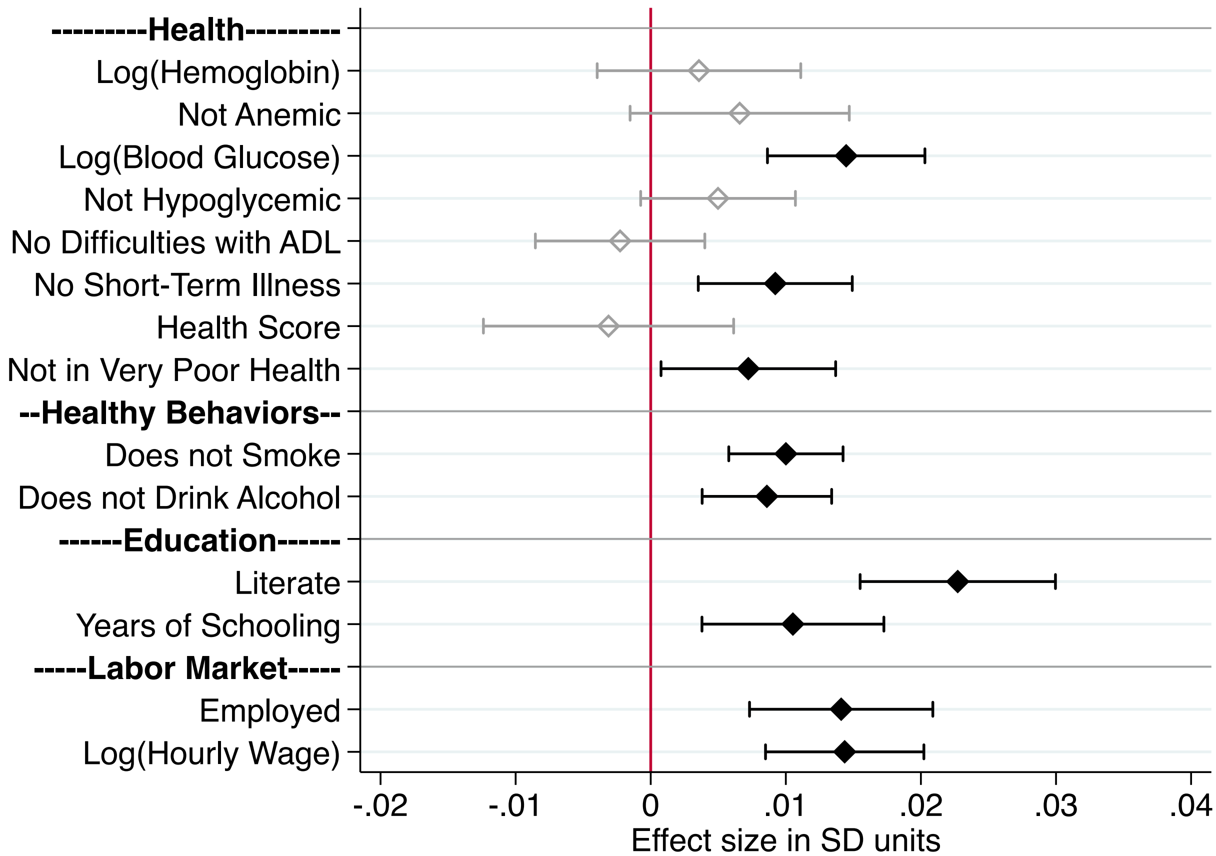
Notes: Each diamond plots the point estimate for β estimated using specification (1). The line corresponding to each point estimate reflects the 90% confidence interval for the outcome. Each outcome variable has been standardized using the mean and standard deviation of individuals in district sub-divisions without the program. The regressions include district sub-division fixed effects, cohort x state fixed effects, controls for gender, birth order, gender x birth order interaction, quadratic population polynomial, caste, religion, mother's education, and exposure to the mid-day meal program. All standard errors are clustered by district sub-division.

Figure E.1: Short-Term Impacts; Village Fixed Effects



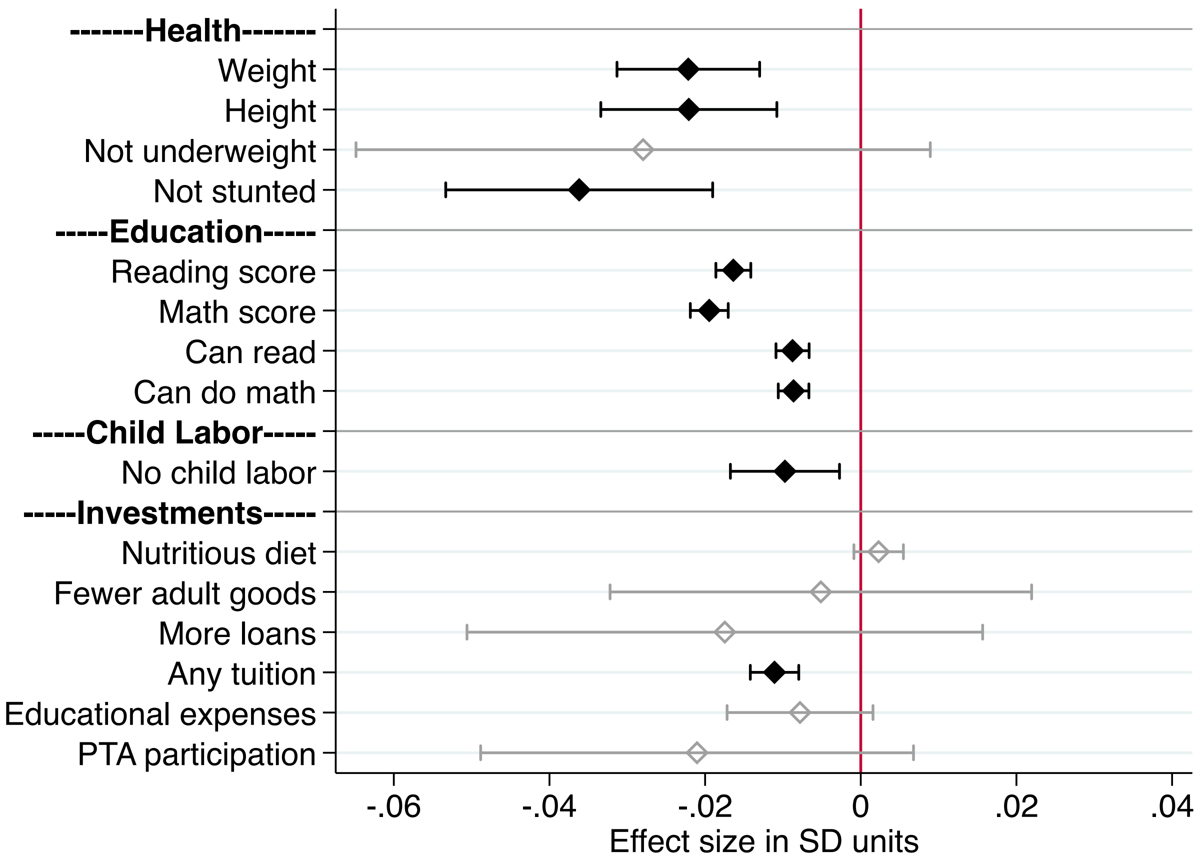
Notes: Each diamond plots the point estimate for β estimated using specification (1) augmented with village fixed effects. The line corresponding to each point estimate reflects the 95% confidence interval for the outcome. Each outcome variable has been standardized using the mean and standard deviation of individuals in district sub-divisions without the program. The regressions include village fixed effects, cohort x state fixed effects, controls for gender, birth order, gender x birth order interaction, quadratic population polynomial, caste, religion, and mother's education. All standard errors are clustered by district sub-division.

Figure E.2: Long-Term Impacts; Village Fixed Effects



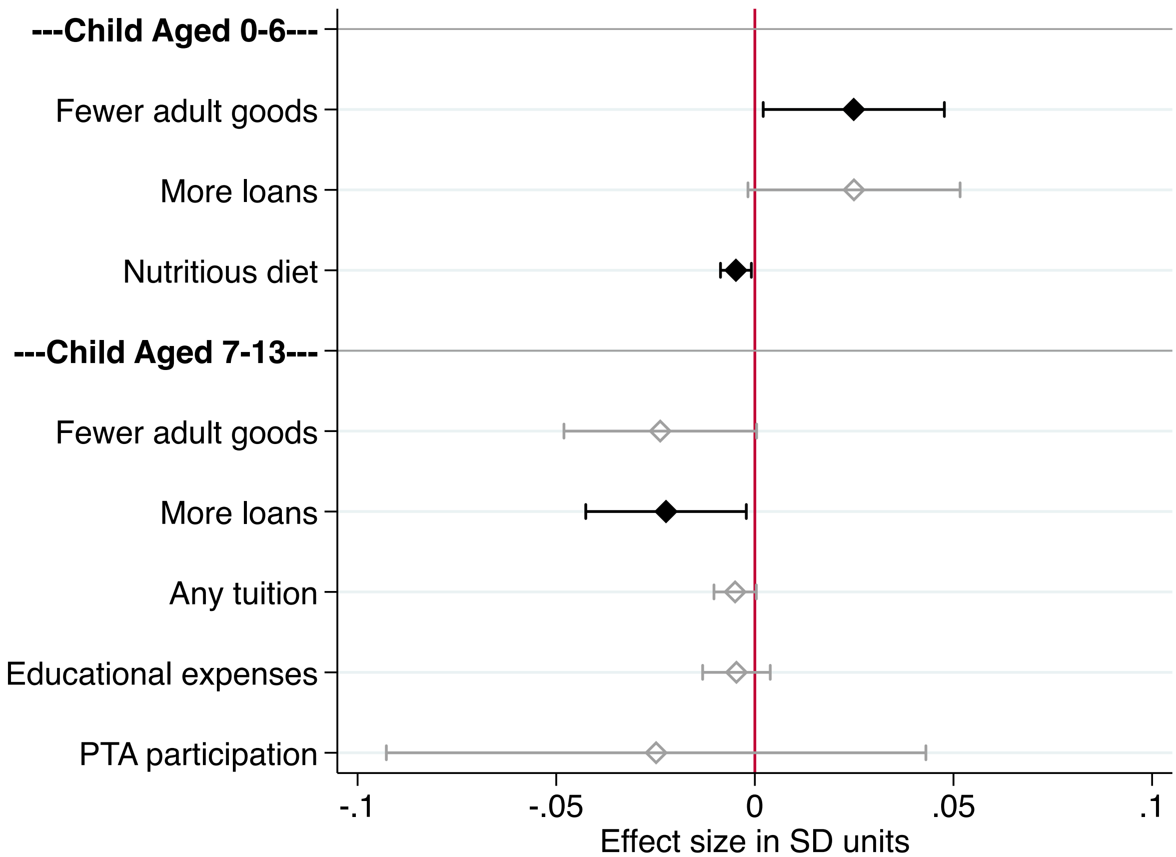
Notes: Each diamond plots the point estimate for β estimated using specification (1) augmented with village fixed effects. The line corresponding to each point estimate reflects the 95% confidence interval for the outcome. Each outcome variable has been standardized using the mean and standard deviation of individuals in district sub-divisions without the program. The regressions include village fixed effects, cohort x state fixed effects, controls for gender, quadratic population polynomial, caste, and religion. All standard errors are clustered by district sub-division.

Figure E.3: Intra-household Reallocation; Village Fixed Effects



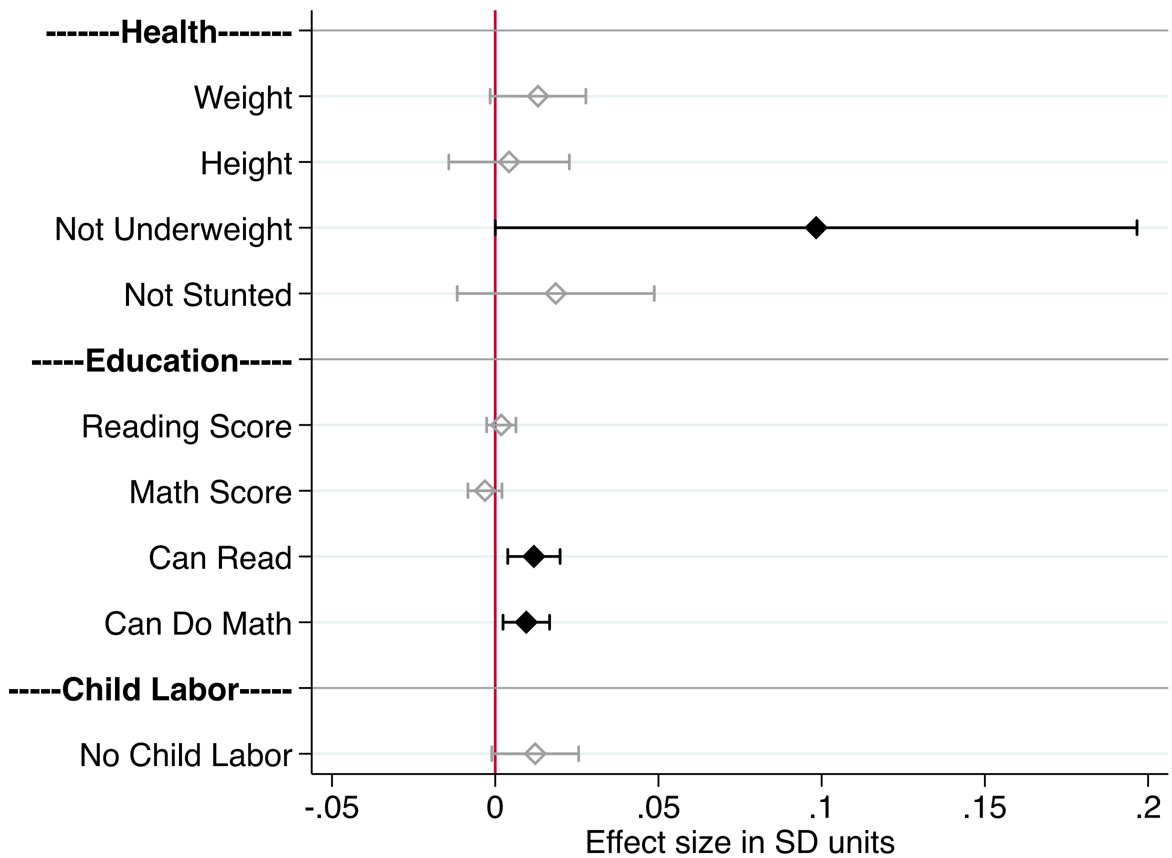
Notes: Each diamond plots the point estimate for β estimated using specification (3) augmented with village fixed effects. The line corresponding to each point estimate reflects the 95% confidence interval for the outcome. Each outcome variable has been standardized using the mean and standard deviation of individuals in district sub-divisions without the program. The regressions include village fixed effects, district sub-division x cohort x age fixed effects, gender x birth order fixed effects, and controls for quadratic population polynomial, caste, religion, and mother's education. All standard errors are clustered by district sub-division.

Figure E.4: Intertemporal Reallocation; Village Fixed Effects



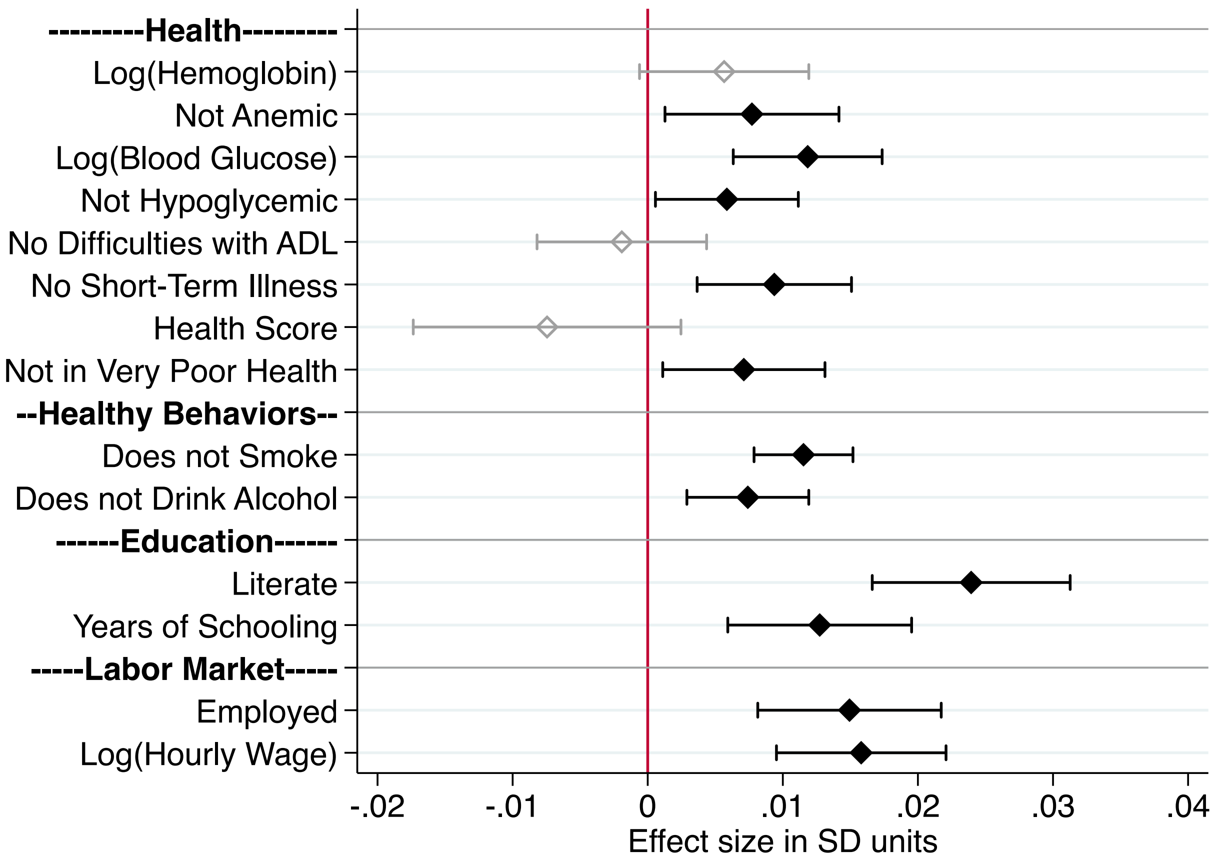
Notes: Each diamond plots the point estimate for β estimated using specification (1) augmented with village fixed effects. The line corresponding to each point estimate reflects the 90% confidence interval for the outcome. Each outcome variable has been standardized using the mean and standard deviation of individuals in district sub-divisions without the program. The regressions include village fixed effects, cohort x state fixed effects, controls for gender, birth order, gender x birth order interaction, quadratic population polynomial, caste, religion, and mother's education. All standard errors are clustered by district sub-division.

Figure F.1: Short-Term Impacts; State-Specific Time Trends



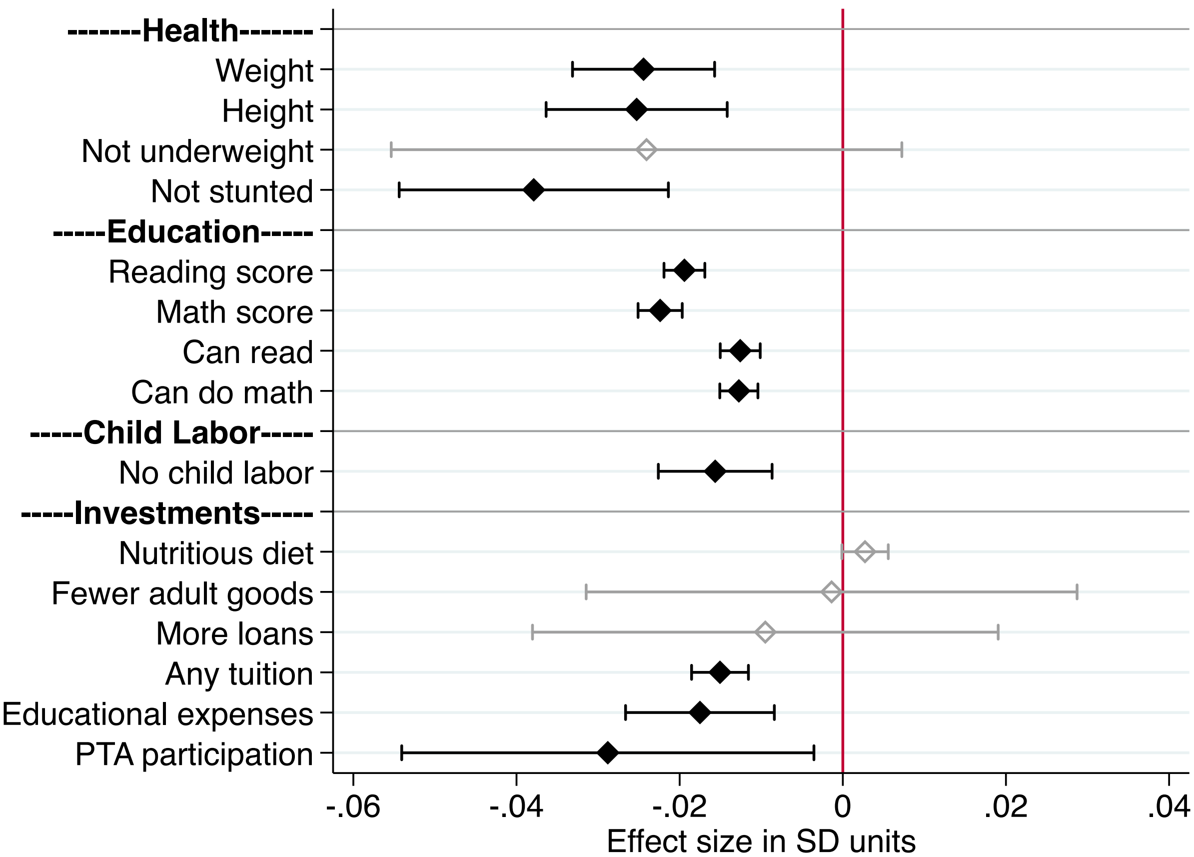
Notes: Each diamond plots the point estimate for β estimated using specification (1) augmented with state-specific linear time trends. The line corresponding to each point estimate reflects the 95% confidence interval for the outcome. Each outcome variable has been standardized using the mean and standard deviation of individuals in district sub-divisions without the program. The regressions include district sub-division fixed effects, cohort x state fixed effects, state-specific linear time trends, controls for gender, birth order, gender x birth order interaction, quadratic population polynomial, caste, religion, and mother's education. All standard errors are clustered by district sub-division.

Figure F.2: Long-Term Impacts; State-Specific Time Trends



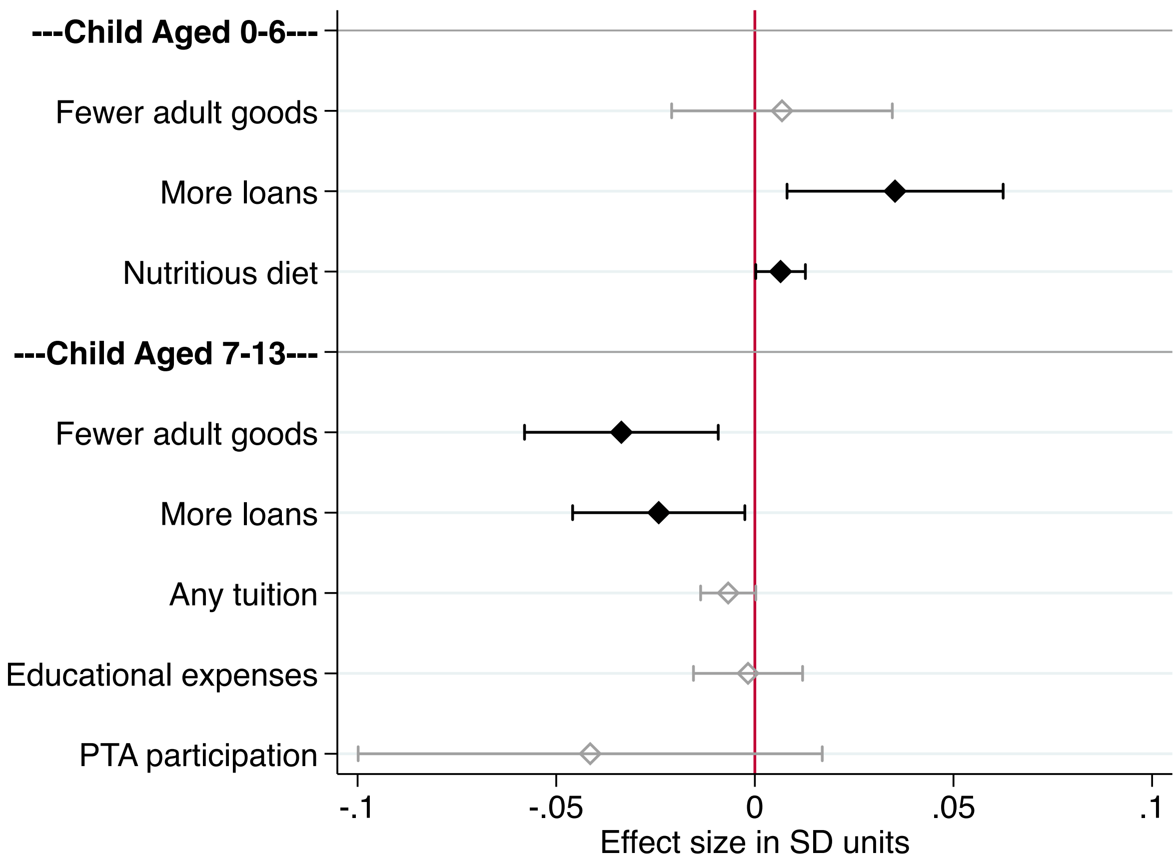
Notes: Each diamond plots the point estimate for β estimated using specification (1) augmented with state-specific linear time trends. The line corresponding to each point estimate reflects the 95% confidence interval for the outcome. Each outcome variable has been standardized using the mean and standard deviation of individuals in district sub-divisions without the program. The regressions include district sub-division fixed effects, cohort x state fixed effects, state-specific linear time trends, controls for gender, quadratic population polynomial, caste, and religion. All standard errors are clustered by district sub-division.

Figure F.3: Intra-household Reallocation; State-Specific Time Trends



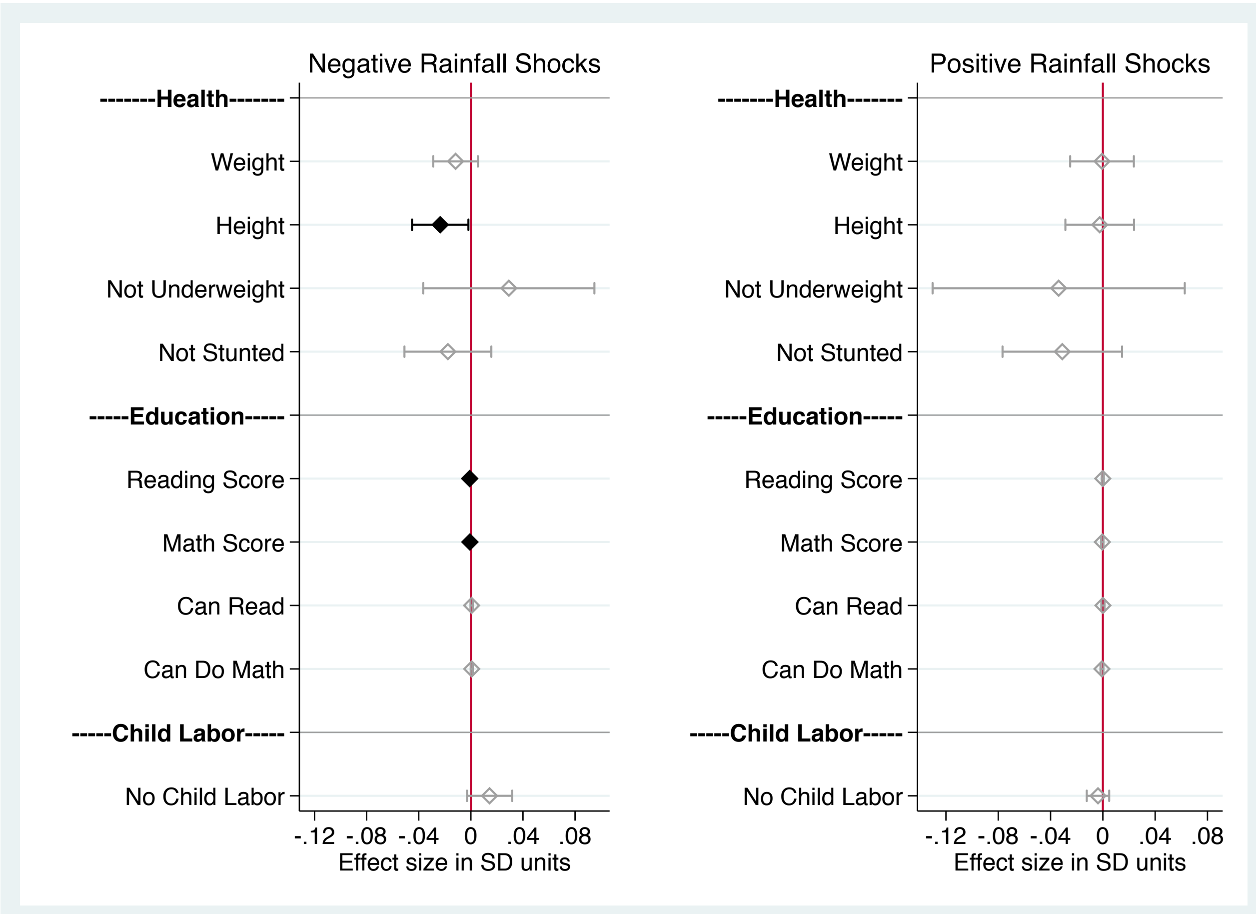
Notes: Each diamond plots the point estimate for β estimated using specification (3) augmented with state-specific linear time trends. The line corresponding to each point estimate reflects the 95% confidence interval for the outcome. Each outcome variable has been standardized using the mean and standard deviation of individuals in district sub-divisions without the program. The regressions include district sub-division x cohort x age fixed effects, gender x birth order fixed effects, state-specific linear time trends, and controls for quadratic population polynomial, caste, religion, and mother's education. All standard errors are clustered by district sub-division.

Figure F.4: Intertemporal Reallocation; State-Specific Time Trends



Notes: Each diamond plots the point estimate for β estimated using specification (1) augmented with state-specific linear time trends. The line corresponding to each point estimate reflects the 90% confidence interval for the outcome. Each outcome variable has been standardized using the mean and standard deviation of individuals in district sub-divisions without the program. The regressions include district sub-division fixed effects, cohort x state fixed effects, state-specific linear time trends, controls for gender, birth order, gender x birth order interaction, quadratic population polynomial, caste, religion, and mother's education. All standard errors are clustered by district sub-division.

Figure G.1: Dynamic Complementarities with Rainfall Shocks



Notes: Each diamond plots the point estimate for β_3 estimated using specification (44). The line corresponding to each point estimate reflects the 90% confidence interval for the outcome. Each outcome variable has been standardized using the mean and standard deviation of individuals in district sub-divisions without the program. The regressions include district sub-division fixed effects, cohort x state fixed effects, controls for gender, birth order, gender x birth order interaction, quadratic population polynomial, caste, religion, and mother's education. All standard errors are clustered by district sub-division.

Table D.1: Correlation between Program Placement and Village Infrastructure

Panel A: Village Education Infrastructure	(1) Pri. School in Village	(2) Middle School in Village	(3) Sec. School in Village	(4) Higher Sec. School in Village
Program Intensity	-0.00722	-0.00827	-0.00391	-0.00187
	(0.0110)	(0.0152)	(0.0109)	(0.00822)
Adjusted R^2	0.217	0.220	0.188	0.149
District sub-division FEs	Yes	Yes	Yes	Yes
State x Year FEs	Yes	Yes	Yes	Yes
Mean in district sub-divisions without program	0.887	0.507	0.256	0.096
Observations	4,060	4,056	4,067	4,061

Panel B: Village Health Infrastructure	Pri. Health Center in Village	Health Sub-Center in Village	Hospital in Village	Clinic in Village
Program Intensity	-0.00196	-0.0130	-0.00236	-0.0139
	(0.00860)	(0.0125)	(0.00601)	(0.0163)
Adjusted R^2	0.137	0.200	0.116	0.223
District sub-division FEs	Yes	Yes	Yes	Yes
State x Year FEs	Yes	Yes	Yes	Yes
Mean in district sub-divisions without program	0.131	0.329	0.141	0.312
Observations	4,064	4,046	4,053	3,945

Panel C: Other Village Infrastructure	Village has Electricity	Village Connected to Roads	Bank in Village	Post Office in Village
Program Intensity	0.00664	0.00809	0.00407	0.0106
	(0.0173)	(0.0228)	(0.0108)	(0.0150)
Adjusted R^2	0.350	0.254	0.157	0.261
District sub-division FEs	Yes	Yes	Yes	Yes
State x Year FEs	Yes	Yes	Yes	Yes
Mean in district sub-divisions without program	0.851	0.338	0.175	0.417
Observations	4,059	4,052	4,067	4,004

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parentheses and clustered by district sub-division.

Notes: Program intensity is measured as number of ICDS centers per 1,000 children aged 0-6.

All regressions include controls for a quadratic polynomial in village-level population.

Table G.1: Dynamic Complementarities - Negative Rainfall Shocks

Panel A: Impact on Child Health				
	(1) Weight (Z-Score)	(2) Height (Z-Score)	(3) Underweight (Z<-2)	(4) Stunted (Z<-2)
Program Intensity	0.116*	0.0597	-0.356**	-0.0781
	(0.0604)	(0.0725)	(0.180)	(0.0567)
Program Intensity ∗ Negative Rainfall Shock	-0.0117	-0.0235*	-0.0134	0.00828
	(0.0104)	(0.0131)	(0.0184)	(0.00951)
Adjusted R^2	0.681	0.624	0.072	0.068
Mean in district sub-divisions without program	0	0	0.306	0.329
Observations	31,538	31,506	11,815	31,506

Panel B: Impact on Test Scores				
	Reading Score (Z-Score)	Math Score (Z-Score)	Can Read	Can Do Math
Program Intensity	0.00247	-0.00241	0.00321***	0.00256**
	(0.00228)	(0.00265)	(0.00120)	(0.00107)
Program Intensity ∗ Negative Rainfall Shock	-0.000878**	-0.000735*	0.000175	0.000196
	(0.000392)	(0.000414)	(0.000158)	(0.000145)
Adjusted R^2	0.451	0.442	0.201	0.186
Mean in district sub-divisions without program	0	0	0.906	0.905
Observations	4,459,291	4,440,879	4,459,291	4,440,879

Panel C: Impact on Child Labor	
	Any Child Labor
Program Intensity	-0.0126*
	(0.00726)
Program Intensity ∗ Negative Rainfall Shock	-0.00196
	(0.00144)
Adjusted R^2	0.053
Mean in district sub-divisions without program	0.019
Observations	346,963

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parentheses and clustered by district sub-division.

Notes: All regressions include district sub-division fixed effects and cohort x state fixed effects. Program intensity is measured as number of ICDS centers per 1,000 children aged 0-6. All regressions include controls for gender, birth order, gender x birth order interaction, quadratic population polynomial, caste, religion, mother's education, and a negative rainfall shock.

Table G.2: Dynamic Complementarities - Positive Rainfall Shocks

Panel A: Impact on Child Health

	(1) Weight (Z-Score)	(2) Height (Z-Score)	(3) Underweight (Z<-2)	(4) Stunted (Z<-2)
Program Intensity	0.104*	0.0350	-0.368**	-0.0764
	(0.0608)	(0.0772)	(0.185)	(0.0593)
Program Intensity ∗ Positive Rainfall Shock	-0.000647	-0.00243	0.0156	0.0146
	(0.0148)	(0.0159)	(0.0270)	(0.0130)
Adjusted R^2	0.681	0.624	0.072	0.068
Mean in district sub-divisions without program	0	0	0.306	0.329
Observations	31,538	31,506	11,815	31,506

Panel B: Impact on Test Scores

	Reading Score (Z-Score)	Math Score (Z-Score)	Can Read	Can Do Math
Program Intensity	0.00187	-0.00300	0.00344***	0.00287***
	(0.00228)	(0.00267)	(0.00121)	(0.00108)
Program Intensity ∗ Positive Rainfall Shock	-0.0000686	-0.000414	0.0000175	-0.000207
	(0.000509)	(0.000539)	(0.000221)	(0.000205)
Adjusted R^2	0.451	0.442	0.201	0.186
Mean in district sub-divisions without program	0	0	0.906	0.905
Observations	4,459,291	4,440,879	4,459,291	4,440,879

Panel C: Impact on Child Labor

	Any Child Labor
Program Intensity	-0.0139*
	(0.00746)
Program Intensity ∗ Positive Rainfall Shock	0.000510
	(0.000712)
Adjusted R^2	0.053
Mean in district sub-divisions without program	0.019
Observations	346,963

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parentheses and clustered by district sub-division.

Notes: All regressions include district sub-division fixed effects and cohort x state fixed effects. Program intensity is measured as number of ICDS centers per 1,000 children aged 0-6. All regressions include controls for gender, birth order, gender x birth order interaction, quadratic population polynomial, caste, religion, mother's education, and a positive rainfall shock.