



The Effects of Prenatal and Early-Postnatal Exposure to Mexico's Oportunidades on Long-Term Cognitive Achievement

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The Effects of Prenatal and Early-Postnatal Exposure to Mexico's
Oportunidades on Long-term Cognitive Achievement

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A Thesis Presented to the Faculty
of the Graduate School of Education of Harvard University
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for the Degree of Doctor of Education

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To my wife Annie Laurie,
To our daughter Lilia Claire,
And to my parents and my brother.

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The Effects of Prenatal and Early-Postnatal Exposure to Mexico's *Oportunidades* on Long-term Cognitive Achievement

Abstract

It is well established that children's early life environments can have significant consequences on their long-term outcomes. Yet, there is still limited empirical evidence on the effects that being exposed during the prenatal and early postnatal periods to positive shocks, such as conditional cash transfers, has on long-term cognitive function. By exploiting the initial village-level randomization, I estimate the causal effect on long-term cognitive achievement of prenatal and early postnatal exposure to *Oportunidades*, Mexico's conditional cash transfer program. I find that those eligible children born into the program who received its benefits early on have higher standardized assessment scores in mathematics and Spanish in third through sixth grade—up to 15 years after the program began. In line with previous research on CCTs, the effects are largely driven by children living in the poorest villages as measured by a marginalization index. Notably, the effect on these children is large enough to put their cognitive achievement on a par with children from more moderately poor villages.

1. Introduction

Over the last several decades researchers from several disciplines have provided considerable empirical evidence that prenatal and early childhood environments influence brain and concomitant behavioral development that may strongly influence later life outcomes (Shonkoff and Phillips 2000; Heckman 2006; Knudsen et al. 2006; Walker et al. 2007; Shonkoff 2012; Mackey, Raizada, and Bunge 2012). Fox, Levitt, and Nelson (2010) develop a framework to explain how early experience, starting *in utero*, not only interacts with but also shapes brain architecture with lasting influences on cognitive function. Almond and Currie (2011) review how such environmental shocks as infections, pollution, economic downturns, income enhancement and early intervention programs during the prenatal and early childhood stages can have effects on adult outcomes. Importantly, they conclude that although early deficits may have lasting consequences, evaluations of remediation programs suggest that children are quite resilient. Yet, limited or a lack of prenatal and postnatal care, undernutrition, intrauterine growth restriction, deficient health practices, maternal depression, and limited cognitive stimulation are some of the key risk factors in preventing more than 200 million children in the developing world from achieving their developmental potential (Grantham-McGregor et al. 2007; Walker et al. 2011).

One of the most influential social policy experiments in the last two decades in the developing world was the introduction of conditional cash transfers (CCTs) in Mexico and Brazil. These government programs typically provide health, education and nutrition supports to low-income eligible households. In principle, these programs

provide incentives for poor families to invest in their own human capital with the ultimate objective of reducing intergenerational poverty. Yet, despite empirical success from early CCTs and widespread replication, given their recent introduction, there is little evidence on their long-term impacts, particularly among children exposed to the programs early in life. Importantly, CCTs, whose ultimate aim is to reduce intergenerational poverty, are designed precisely to address many of the risk factors to child development highlighted by the literature.

I contribute to the literature on early-life *positive* shocks by assessing the long-term effects of being exposed prenatally and during the early postnatal period to a CCT on long-term cognitive achievement—up to 15 years after the program started—using scores on standardized tests of skills in mathematics and Spanish. My identification strategy exploits the initial village-level randomization of the program, which guaranteed that eligible households in treatment villages were exposed to the program for 18 months before eligible households in the control villages became beneficiaries. I document that early CCT exposure has statistically significant effects in both Spanish and math that are present at third grade for both boys and girls, and that persist at least until the end of the primary education cycle. Consistent with previous research, I show that these impacts are heavily concentrated in the most vulnerable children. Importantly, it suggests that properly targeted CCTs even without an early childhood stimulation or parenting component may be a viable option to support the long-term cognitive development of children in the most disadvantaged and poorest households in similar contexts. I also present suggestive evidence that the monitoring of child growth occurring at visits with medical personnel—henceforth referred to as child growth monitoring—early in the

postnatal period could be an important mechanism behind the effects I find. More speculatively, my findings also suggest the effect is strongest among those exposed prenatally at least during the last trimester of their mother's pregnancy.

The rest of the paper is organized as follows. In the next section, I discuss the basics of the *Oportunidades* program and what we know about the effects of CCTs on child development—particularly on long-term cognitive functioning. In section 3, I describe my data and sample. Section 4 covers my empirical strategy. I proceed to discuss my results in Section 5, which includes a discussion of potential mechanisms. I assess the robustness of my findings in Section 6, and conclude in Section 7.

2. Background on *Oportunidades*¹ and Early-Life Exposure to CCTs on Child Development

2A. *Oportunidades*

Many governments in developing countries over the last two decades have experimented widely with CCTs. Generally, these safety net programs provide cash to eligible low-income households on the condition that they carry out certain actions leading to improvements in their own health, nutrition and education levels. This experimentation and rapid expansion were at least in part a direct result of the early

¹ The program has undergone several name changes throughout its life. It was originally called PROGRESA, which stood for *Programa de Educación, Salud y Alimentación* or Program of Education, Health and Nutrition. In 2002, under a different presidential administration, its name was changed to *Oportunidades*. Once again, in 2013, after a change in government, the program was rebranded as *Prospera*. As the program's name has been *Oportunidades* during most of its lifetime, I use this term to refer to it throughout.

success of Mexico's CCT, *Oportunidades*.² This program broke ground in social policy because it coupled a multidimensional approach to poverty reduction with a specific targeting on poor households in marginalized rural villages.³ Moreover, in order to rigorously evaluate the effects of the program, the government rolled out the program in a manner that supported a large-scale randomized controlled trial. The results from this impact evaluation, which was carried out by an external independent organization, helped validate the short-term results of the program and paved the way for its extensive replication.

The Mexican government began implementation of *Oportunidades* in 1997 through a separate bureaucracy supported by the Ministries of Social Development, Health, and Education, as well as the Social Security Institute. Since its inception, the program has provided supports in the areas of health, nutrition and education. Originally, it targeted poor households only in marginalized rural villages. At its core, *Oportunidades* provides eligible households with cash and in-kind transfers through the mother, the title beneficiary. These transfers are provided only as long as households invest in their own human capital by carrying out certain actions, such as getting periodic health check-ups for each of their members (pregnant women, and lactating mothers and their young children are supposed to attend check-ups much more regularly than other members), having school-age children enroll in and regularly attend school, and having

² The International Food Policy Research Institute (IFPRI) was hired to be the external evaluator of the program. More than a dozen of papers and reports are available at <http://www.ifpri.org/category/progres>. Please see Skoufias (2001) for a summary of this literature. In addition to the wide dissemination of these results, international agencies like the World Bank and the Inter-American Development Bank also promoted the success of *Oportunidades* with its client governments and hosted various international conferences to showcase the program.

³ This targeting on poor households in disadvantaged rural communities coupled with a multidimensional approach to poverty reduction was a way to improve on a previously fragmented approach to social policy that was also deemed inefficient. See Schultz (2004) and Levy (2006) for in-depth discussions.

mothers attend health and nutrition education talks on a regular basis. Since the initial rural implementation, the program has expanded and now includes urban and semi-urban areas throughout the country. Today, roughly one in five households in Mexico is a beneficiary.

The components of the program most relevant to my analysis are as follows:

a) primary health care services for pregnant women, mothers and children up to 2 years old, which include prenatal care visits, immunizations, height, weight and nutrition monitoring for children, and illness prevention;

b) daily nutrition supplements for pregnant and lactating mothers, as well as for children aged 4 to 24 months;

c) a requirement to attend health and nutrition lectures held regularly by doctors and nurses covering nutrition, hygiene and health designed primarily for mothers with an emphasis on health-risk reduction and prevention, recognition of signs of malnutrition and illness, and follow-up primary care provision;

d) a requirement that school-age children from third grade to ninth grade enroll in and attend school regularly;

e) cash transfers to the mother in eligible families equivalent to 25 percent of pre-program household expenditures, on average;

f) social marketing campaigns that emphasize: i) household responsibility, which refers to being an active participant in one's own development, and ii) the use of cash transfers to prioritize buying nutritious foods and school supplies.⁴

⁴ For a more detailed description of the program and its origins see Skoufias (2001) and Levy (2006).

2B. Early-life Exposure to CCTs and Cognitive Achievement into Early Adolescence

While there is considerable evidence on the impacts of CCTs on the health, education and nutrition of children⁵, there are only a few rigorous studies on the impact on cognitive development of prenatal and early-life exposure to a CCT.⁶ Using the randomized introduction of the *Bono de Desarrollo Humano* (BDH), a CCT in Ecuador, Paxson and Schady (2010) document that there were no treatment effects on development outcomes for their overall sample of children aged 3 to 7 after 17 months of exposure.⁷ Yet, the poorest quartile of children, as measured by imputed log per capita expenditure, do observe positive, although modest, effects on cognitive, behavioral, and physical outcomes. Fernald and Hidrobo (2011) assess the impact of the BDH on younger children—those 12 to 35 months when program benefits began. They find no effects of the program on language and physical outcomes in the overall sample. Yet, they do find that those children in rural areas, who have mothers with much lower levels of schooling, observed an effect on the number of words a child was speaking and on the probability of putting together two or more words. Macours, Schady, and Vakis (2012) rely on an experimental evaluation to assess the impacts of *Atención a Crisis*, a small CCT in Nicaragua. These authors find that the CCT had a positive effect on the cognitive, socio-

⁵ See Fiszbein and Schady (2009) for an extensive review of this literature, Baird et al. (2014) for a review of the impacts of conditional and unconditional cash transfers on education outcomes, and Lagarde, Haynes, and Palmer (2007) for a review of the effects of CCTs on health outcomes in Latin America and Africa.

⁶ Cognitive development refers to how a child's ability to think and reason evolves as he ages. Cognitive function, while related to the former concept, refers to cognitive processes but does not necessarily concern itself with their growth. Cognitive achievement refers to a form of *observed* cognitive function. Attention, memory, perception, information-processing, problem-solving, language, and metacognition are all cognitive processes.

⁷ Although the social marketing campaign did emphasize that beneficiary households had to enroll their children in school and take them to clinics for preventive health check-ups, the Ecuadorian program did not formally required conditions to be met in order to receive a payment (Fiszbein and Schady 2009). As could be expected, compliance has not been monitored. Thus, strictly speaking, the program could be considered an unconditional cash transfer program.

emotional, and physical development of young children (0 to 7 years of age at baseline) after only nine months of receiving benefits. Furthermore, these effects were sustained, as children in treated households continued to score higher in all developmental domains two years after the program was eliminated and beneficiaries stopped receiving benefits.

As CCTs have only been operating over the last two decades, there is even less evidence on the long-term effects of being exposed to these programs during early childhood or *in utero*. Behrman et al. (2008), find that, nine years after the program began, *Oportunidades* had a positive effect on socio-emotional development (i.e. reduced behavioral problems) and language development among children who were less than 36 months of age at baseline.⁸ There were, however, no effects on cognitive ability, schooling or height and weight indicators. Similarly, Fernald, Gertler, and Neufeld (2009), who use the same 2007 follow-up survey as Behrman et al., find that *Oportunidades* also reduced behavioral problems on a somewhat younger sample of children. Yet, Fernald et al., find there were no effects on cognitive assessment scores, language development, height and weight indicators.

Only Barham, Macours, and Maluccio (2013) have assessed effects on long-term outcomes to being exposed to a CCT starting in the prenatal period and up to age two. This study found that, at around age 10, boys exposed *in utero* and up to age two to Nicaragua's *Red de Protección Social* scored higher on a battery of age-appropriate cognitive tests—covering processing speed, memory, vocabulary and visual integration—than children in the original control group, which received program benefits from age two to about five. Barham et al., however, also document that exposure to the CCT did not

⁸ While language development is part of cognitive development, it is often assessed (and discussed) separately because of its higher-order nature and complexity.

lead to impacts on boys' anthropometric outcomes at age 10. Analysis of the boys height-for-age z-scores three and four years after the intervention began for those who were intervened early suggests the null effect on the anthropometric outcomes can be attributed to catch-up growth among those boys in the original control group, whose families received CCTs when there were two to five years of age. Yet, this latter study is limited since it doesn't assess whether this long-term impact on cognition remains stable over time, whether it varies by gender, and how it manifests in schooling outcomes.

These six studies provide limited and mixed evidence on the effects of early-life exposure to CCTs on cognitive and child development outcomes. One way to reconcile these mixed findings is to realize that these studies have assessed children who have had different base levels of poverty and other risk factors. The current *Oportunidades* literature assessing effects on children during early life has only found consistent effects on behavioral outcomes. The effects of the Ecuadorian CCT are only observed among the most vulnerable—the poorest quartile of children, as measured by imputed log per capita expenditures, or those children in rural areas. Only the Nicaraguan children have shown consistent effects on a variety of child development outcomes to being exposed to a CCT prenatally and during early childhood. Yet, Nicaraguan children are likely considerably poorer than those in Ecuador and in Mexico (Paxson and Schady 2010; Macours, Schady, and Vakis (2012); Schady et al. 2015). Thus, it is possible that the vulnerable population of children in Ecuador, where the studies do find effects, have important similarities with the overall children population in Nicaragua. For this reason and the fact that the broader literature on risk and protective factors to child development (Walker et al. 2007; Grantham-McGregor et al. 2007; Schady et al. 2015) also finds links between levels of

poverty and child development, it will be important to account for the intensity of poverty in my analysis. This approach is also supported by the broader CCT literature, which has consistently documented there is heterogeneity in treatment effects by poverty (Rivera et al. 2004; De Janvry, Finan and Sadoulet 2005; Behrman, Sengupta and Todd 2005; Maluccio and Flores 2005; Djebbari and Smith 2008; Leroy et al. 2008; Filmer and Schady 2008; Gaer, Vandenbossche, Dammert 2009; Figueroa 2013; and Galiani and McEwan 2013).

3. Description of Data

I rely on a matched dataset that links survey data from the original *Oportunidades* evaluation sample with individual student records on standardized assessments (called ENLACE) in 3rd through 6th grades from 2007 to 2012. I first identify young children born between January 1998 and December 2000 in the program’s evaluation sample—see details below. These records are then matched to the student assessment data using names (including first name, paternal last name and maternal last name), dates of birth, sex, and a government-issued individual identifier. 89.9% of all the matches are exact. The rest of the matches are conducted using an algorithm that allows for minor differences in the records or matching with fewer variables when there is incomplete information. The partial or fuzzy matches do not affect my findings, as I obtained virtually identical results using only the exact matches.

Table 1 provides details of the match rate—the proportion of those in the original *Oportunidades* evaluation sample that are matched to student achievement records. My

match rate is 71.7% overall and 72% for my analytic sample.⁹ I also breakdown the matches by children born in treatment (column 1) and control (column 2) villages, which show the match rates are remarkably similar. Across all panels, I can't reject that the match rates between treatment and control are equal (column 4). I also show in Figures 2A and 2B plots of nonparametric locally-weighted regressions of the match rate on the poverty index. The figures show that the match rate between children in control and treatment villages across various levels of poverty is the same across almost the entire distribution. I conclude there is no differential attrition from the sample between those in treatment and control villages.

3A. Initial Program Evaluation and Survey Data

In order to rigorously evaluate the effectiveness of the newly established *Oportunidades* program, the government, from the very beginning, designed a randomized controlled trial at the village level. Specifically, the government had plans to roll out the program in all of the rural villages in the country with high levels of marginalization, where large numbers of poor households would be located. It identified such villages by constructing a marginalization index that is based on education, housing and labor market characteristics taken from census and intercensal population data. Yet, given it was logistically and financially infeasible to roll out the program to all villages

⁹ This number is somewhat lower but very much in line with the match rate in Figlio et al. (2015). The match rate in that study, which links neonatal health outcomes with student achievement records, was 80.7%.

with high levels of marginalization at once, it did so in several phases.¹⁰ During one of the first phases of program rollout, it randomized 506 rural villages with high levels of marginalization into treatment (320 villages) and control (186 villages) groups.

To determine household eligibility for the program, administrators conducted a baseline survey of all households in these 506 villages to construct a poverty index. Of the roughly 24,000 surveyed households in these 506 villages that made up the evaluation sample, 52 percent of them were deemed eligible based on poverty status. Moreover, program take up in treatment villages was very high, and averaged 93 percent (Skoufias 2001). Households in treatment villages began receiving benefits in May 1998. In November 1999, after roughly 18 months, the program was rolled out in the control villages. In addition to the 1997 baseline survey and another one in March 1998, the government fielded post-treatment follow-ups in all 506 treatment and control villages in November 1998, June and November 1999, May and November 2000, and in October 2003.¹¹ Most analyses take advantage of this randomized and phased incorporation into *Oportunidades* and use the program evaluation survey datasets to assess effects on a variety of human development outcomes.

3B. Student Achievement Scores

¹⁰ The government was forced to exclude a small percentage of very small villages without the necessary health and schooling infrastructure to operate the program.

¹¹ There was also a 2007 follow-up, but it excluded the smallest villages—those with less than 20 beneficiary households—based on cost implications. For further technical details on the baseline and follow-up surveys, see INSP, 2006 and INSP, 2008.

From 2006 to 2013 the Ministry of Education (SEP) implemented an annual student achievement assessment program called ENLACE for primary and lower secondary schools in the entire country. All children in public and private schools in grades third through ninth took mathematics and Spanish standardized tests at the end of each school year. To analyze the program's long-term effect on cognitive achievement, I use the matched end-of-grade student achievement scores from 3rd through 6th grade—my dataset contains four observations (rows) of data per child. While ENLACE scores are horizontally equatable (i.e. allow for across-year comparisons within grade), for ease of interpretation, I transform all scale scores into standard scores. That is, for each grade, from each individual score I subtract the population (i.e. of the entire country) mean and divide it by the population standard deviation. Moreover, while I will analyze the effect of early exposure to the program on children's mathematical and Spanish skills at each grade level, my main outcome of interest will be the overall effect across all grades by combining the grade-specific standard scores into one single measure.

3C. Sample

I have matched records for 1,867 children born between May 1998 and October 1999 who belong to eligible households based on poverty. This 18-month birth span corresponds to the delay in incorporation to the program between the treatment and control villages. Consequently, as illustrated in Figure 1, this setup focuses my analysis on: a) eligible children who were born right around the time eligible households in treatment villages began receiving benefits in May 1998 (i.e. eligible children in treatment villages in this group had virtually no prenatal exposure to the program, but received program benefits during their first 18 months before other eligible children in

control villages were also incorporated), b) eligible children who were born right around the time eligible households in the control villages were incorporated into the program in November 1999 (i.e. eligible children in treatment villages had a full prenatal exposure to the program preceded by a 9-month period in which their households had been beneficiaries), and c) all those eligible children born in between.

I show in Table 2 the differences in average characteristics between all of the children born between May 1998 and October 1999 (column 1) and those born in that same period but to poor, and thus eligible, households (column 2). Poor children have mothers that are less educated and that are more likely to speak an indigenous language when compared to those in the overall sample. These poor children also come from households that are headed by a slightly younger individual that is more likely to speak an indigenous language than the children in the overall sample. Column 3 shows the poor children I have been able to match. Upon inspection, the characteristics of those who are matched are virtually the same as the entire sample of poor children. This is further evidence that, although I am only able to match close to 3 out of 4 poor children born in the 18-month period, those matched are not a selected group.

3D. Village Poverty and Child Development

Although all households eligible to receive *Oportunidades* were poor, there is considerable variation in the level of poverty among beneficiaries. The intensity, not just the condition, of poverty has been associated with a number of important measures of well-being including education and child development outcomes (Walker et al. 2007,

Grantham-McGregor et al. 2007; Schady et al. 2015).¹² Table 2 provides village-level characteristics in the *Oportunidades* evaluation sample by quintiles of the village marginalization index. As mentioned earlier, this index was used by the designers of *Oportunidades* to identify locations with large numbers of poor households—and has been documented to be particularly successful at identifying the extreme poor (Skoufias, Davis, and Behrman 1999).

There is variation across quintiles by construction, but what is most stark is that conditions in the bottom quintile appear to be considerably different than in the rest of the population—the top four quintiles. In this bottom quintile of villages, the marginalization and poverty indexes on average are 1.53 and 0.96 standard deviations lower than the mean. There is also almost twice the percentage of poor households and more than two-and-a-half times the percentage of extremely poor households than there are in the top 4 quintiles. Additionally, women of childbearing age living in very high-poverty villages have almost two years of schooling less than those in the other quintiles. These large differences are important, as poverty and maternal education have been firmly established as a risk factor and a protective factor, respectively, to child development (Walker et al 2007, 2011).

As previously argued in *Section 2B*, and given the differential risk to their development, extremely poor children may be more susceptible to a social policy program like *Oportunidades* than the more moderately poor. Behrman and Hoddinot (2005) suggest the impact of the program on child height is larger in poorer households

¹² This is also the reason why governments have defined different indexes to reflect the intensity of poverty, and have used them to differentially intervene these populations. The Mexican government, for instance, has used since the mid 2000s three different measures of poverty, *pobreza alimentaria* (based on a basket of basic foods), *pobreza de capacidades* (adds health and education expenditures to the previous measure) y *pobreza de patrimonio* (adds housing, dress and transport expenditures to the previous measure).

and poorer villages, and Djebbari and Smith (2008) find the impacts of *Oportunidades* on consumption are stronger among those in more marginalized villages. For these reasons, I assess program effects by quintiles of village marginalization, as a way to take into account the intensity of poverty.¹³ As shown in Tables 2 and 3 (Columns 4 and 5), this particular breakdown captures considerable variation precisely in co-occurring factors important to child development—i.e. poverty and maternal education. Moreover, by using quintiles of village marginalization in the original set of villages, which I henceforth refer to as village-poverty or poverty quintiles, I avoid splitting my sample by using a measure of poverty at the household or child level, say the household poverty index, that potentially reflects an endogenous response to the treatment.¹⁴

4. Empirical Strategy

As mentioned before, I exploit the initial village-level randomization that was carried out while *Oportunidades* was being phased in starting in 1998. This randomized incorporation, which, in principle, guarantees that the characteristics of those residing in treatment and control villages were equal in expectation, meant that eligible households in the treated villages were exposed to the program for 18 months before eligible households in the original control villages started receiving program benefits. Thus, I compare student achievement outcomes of eligible children who were born in this 18-month window in the treatment villages who were exposed to the program during the prenatal and early postnatal periods with those of eligible children who were born at the

¹³ Galiani and McEwan (2013) also assess the effects of the Honduran CCT on education outcomes using quintiles at the village-level determined by a poverty proxy also used to identify the locations to intervene in the first place.

¹⁴ Disaggregating the villages by the percent of households in extreme poverty produces very similar results, as the great majority of the villages fall in the same quintile.

same time but because they resided in the control villages were only exposed to the program later—see Figure 1. In my analysis, I fit the following intent-to-treat ordinary-least-squares (OLS) model:

$$SCORE_{igv} = \alpha + \beta_1 TREAT_v + \beta_2 BOY_i + \boldsymbol{\pi}_i + \boldsymbol{\gamma} \mathbf{X}_{ihv} + \varepsilon_{igv}, \quad (1)$$

In Equation (1), the dependent variable, $SCORE_{igv}$ is the end-of-year standard score in mathematics or Spanish discussed above for child i in grade g in village v . $TREAT_v$ is a dichotomous indicator variable for the villages that were randomly selected to receive the program in May 1998. In principle, randomization guarantees that a simple post-treatment comparison of means captures the effect of the program. Yet, including BOY_i , a gender indicator, $\boldsymbol{\pi}_i$, a vector of month-of-birth fixed effects, and $\mathbf{X}_{ihv}$, a vector of the previously described—see Table 2—child-specific, household- and village-level controls, respectively, may improve the precision of my estimates and correct for chance deviations in the distribution of characteristics between early and late beneficiaries before the program. As my specification combines scores of children in all grades from third through sixth into a single measure (one for mathematics and one for Spanish) to be able to assess overall effects, I cluster the standard errors, which are also robust¹⁵, at the level of the child. I also conduct a check on whether the initial village-level randomization was successful in achieving balance across treatment status. Column 6 of Table 2 shows p-values associated with the coefficient estimates of $TREAT_v$ on regressions like Equation 1 with each characteristic in the table used as the dependent variable. Given there are 20 characteristics and I reject equality (at the 10% significance level) in only one case, I conclude the randomization was successful at balancing treatment and control groups.

¹⁵ Specifically, by robust I mean that I have heteroskedasticity-consistent standard errors.

In my hypothesized model, β_1 , which characterizes the causal effect on long-term cognitive achievement of prenatal and early-postnatal exposure to the program, is my parameter of interest. If it is positive and significant, then it indicates that the differential exposure to *Oportunidades* during these sensitive periods has lasting effects in cognitive domains up to 15 years after the program began.

As described before, I also assess heterogeneity of effects by poverty. I first interact my question predictor, $TREAT$, with indicators for each quintile of village poverty. Following Deming (2011), I let:

$$SCORE_{igv} = \alpha + \sum_{q=1}^5 \beta_q TREAT_v + \sum_{q=1}^5 \delta_q (1 - TREAT_v) + \gamma X_{ihv} + \varepsilon_{igv} \quad (2)$$

where q indexes the quintiles of village poverty, and the rest of the notation is as before in Equation (1). The flexibility of having separate coefficients for each village poverty quintile for both those in treatment villages, β , and those in control villages, δ , allows me to test various hypotheses. Specifically, I can test whether children in early treatment are equal to those in late treatment overall and within each quintile. I can also test whether the village-poverty quintiles are statistically significantly different overall and within each group. I first estimate Equation (2) for the Spanish and Math achievement outcomes and plot the early or control coefficients for each village-poverty quintile. I then proceed to estimate a simpler model in which I pool the top four (first through fourth) quintiles of village poverty in a single indicator but then let the bottom village-poverty quintile—the poorest—have its own indicator to assess differential effects. I also assess effects by primary school grade—3rd through 6th—by interacting $TREAT$ with indicators for each

grade. When assessing grade-specific effects by village poverty I fully interact my treatment indicator with grade-specific indicators and indicators for being in the top four quintiles and the bottom quintile of village poverty (i.e. triple interactions).

5. Results

Table 4 shows my main results.

5A. Overall Effects

Table 4 presents the effect of early-life exposure to *Oportunidades* for my overall sample and by village-poverty quintiles. It also presents results disaggregated by gender. Column 2 presents the coefficient estimates associated with the treatment village indicator in Equation 1. Column 4 and Column 6 provide the estimates associated with the interaction of a treatment village indicator with an indicator of whether the child belongs to a village in the top 4 quintiles (Column 4) or the bottom quintile of poverty (Column 6). Panel A presents results for all observations, whereas Panel B and C show the results for girls and boys, respectively. The odd-numbered columns present, control means, which document that children in these contexts score on average roughly a half of a standard deviation below the national average in both Spanish and math, with the differences being close to three quarters of a standard deviation below the national average for the bottom quintile (Column 5). Comparing with the national test score distribution, a half standard deviation below puts these children at the 33th percentile for Spanish and 34th percentile for math. The equivalent result for those in the bottom

quintile is 26th percentile for Spanish and 28th for math. Girls, overall and in each quintile, perform at least as well or better than boys.

The estimates in Column 2 show early-life CCT exposure had an effect across all tested primary school grades of approximately 0.07 standard deviations for Spanish and math, which are significant at the 5 percent level. Columns 4 and 6, however, provide evidence that the effects on cognitive achievement are driven by children in the bottom quintile of village poverty. The impacts of the program on these children are close to 0.30 standard deviations for both Spanish and math and are statistically significant at the 1 percent level.

Both girls and boys share these large gains in achievement. Although the effects are slightly larger for boys, these are not statistically significantly different. Remarkably, the magnitudes of these effects and the lack of effects in the other four quintiles imply that children in the poorest villages now perform on a par with children in moderately poor villages. Boys in the treatment villages actually perform better than boys in control villages.¹⁶

Figures 3A (Spanish) and 3B (math) plot the coefficient estimates from Equation 2 that are associated with the treatment village indicator fully interacted with village poverty quintiles. The reference category is the control group in the first poverty (least poor) quintile. In a sense, the plots provide a visual presentation of the effects presented in Table 4. We see that children in the bottom poverty quintile in the control villages score about 0.30 standard deviations lower in both Spanish and math than children in the other four quintiles, where achievement is more uniform. Those children in the bottom

¹⁶ Subtracting the 0.32 effect on math from the control mean of -0.67 is -0.35, which is 0.12 standard deviations better than the control mean for the top 4 quintiles.

quintile in treatment villages, however, score roughly on a par with children in all the other quintiles. The differences in achievement in the bottom poverty quintile are statistically significantly different between those children in the treatment and control villages for both Spanish and math. Yet, there are no statistically significant differences in achievement between children in the bottom poverty quintile in the treatment villages and children in the top four poverty quintiles in the control villages. There is also no statistically significant difference between children in the bottom quintile and those in the top four quintiles in the treatment villages.

5B. Effects by Grade

Tables 5, 6 and 7 present results of the effect of early-life CCT exposure by primary school grade for the overall sample, the bottom poverty quintile and the top four quintiles, respectively. In table 5, the grade-specific effects are obtained by fully interacting the treatment village indicator with school-grade indicators. As before, I show control means in odd-numbered columns and provide results breaking the sample by gender. The effects are strongest in grade 3 for both girls and boys. There are no effects in grade 4, where coefficients are very close to zero with large standard errors. Across both girls and boys in grades 5 and 6, the effects are smaller in magnitude than those in third grade, but they are generally statistically significant or close to it.

In Table 6 and Table 7, the grade effects are obtained by generating a three-way interaction made up of the treatment village dummy, an indicator for the bottom quintile (or the top four quintiles) of village poverty, and the school grade indicators. The patterns in Table 6 are similar to those in Table 5, but given that these refer to the bottom poverty

quintile, they are considerably larger in magnitude and there are more statistically significant estimates. The effects are largest in grade 3, where children in treatment villages score almost a half of a standard deviation better than those children in control villages. These gains imply moves in the national distribution of third grade scores from the 20th to the 33th percentile in Spanish and from the 19th to the 34th percentile in math. The smallest effects are in grade 4, which correspond to changes in the national distribution from the 27th to the 33th percentile in Spanish and from the 29th to the 35th in math. In grade 5, the effects are larger, but these become smaller yet again in Grade 6. By the end of the tested primary cycle, children exposed to the program prenatally or in the early postnatal period are in the 35th and 43th percentiles in Spanish and math respectively. On the other hand, those children in the control group who were only later exposed to *Oportunidades* are in the 29th and 36th percentiles in Spanish and math, respectively. Although effects are larger in magnitude in third grade, it may be important to evaluate them with regards to the natural progression or growth that occurs from each grade to the next. Hill et al. 2008 present evidence that at least in the U.S. the natural growth in nationally normed tests tends to be larger in earlier primary grades than in later ones. If this pattern held in Mexico in the populations I study, then this would mean that at least relative to the natural progression or gain from one specific grade to the next, the effects of early CCT exposure in third grade are not necessarily larger than those in fifth grade or even those in sixth grade. Table 7 largely confirms that early CCT exposure did not have an impact on cognitive achievement among children in the top four quintiles of village poverty.

5C. Potential Mechanisms Behind Cognitive Achievement Effects

Generally, recipients of CCTs receive a package of services, cash and in-kind transfers. This design makes it very difficult to cleanly isolate the specific causal effects of specific components of the intervention. In other words, in the absence of experimental variation of program components it is not possible to determine that the estimated effects on outcomes are driven by, for example, the additional cash provided to beneficiary households and not by changes in behavior stemming from the required conditions or the information provided via social marketing or the talks on health and nutrition. Nonetheless, it can still be instructive to assess the strength of one pathway compared to another. I do this by examining the (reduced-form) effects of early-life exposure to *Oportunidades* on behaviors and intermediate outcomes during the prenatal and postnatal periods using data from the household surveys conducted roughly every six months from 1998 to 2000 to evaluate the program. While the regular collection of survey data from households in treatment and control villages facilitates longitudinal analysis, not all relevant questions are asked in every survey round. Moreover, given that those in my sample are born between May 1998 and October 1999, often there is limited or no information about some of these children with regards to a potential mechanism.

Table 8 presents results assessing if a potential mechanism could have been prenatal health care utilization, as measured by whether mothers of the children in my sample received a health check-up during their pregnancies by a nurse or a doctor, and by a midwife. As the outcome is dichotomous, I use a probit regression (similar to equation 2) and the estimates in the table refer to marginal effects. Control means in column 1 document women in the top four quintiles of poverty are much more likely to have a

prenatal health check-up by a doctor or nurse, and less likely to have one conducted by a mid-wife than women in the bottom poverty quintiles. Columns 2 and 4 present reduced-form estimates associated with coefficients of interactions of the treatment indicator with the bottom quintile or top four quintiles of village poverty. In column 2, I show there were no statistically significant differences between treatment and control villages in prenatal health care utilization before *Oportunidades* began, which confirms again that the village-level randomization served to balance characteristics across treatment status. Column 4 shows that mothers of children in the top four poverty quintiles were 13.6 percentage points more likely to receive a check-up during their pregnancies than mothers in the control villages in October 1998, 5 months after the program began. While the coefficient estimate is similar in magnitude for mothers in the bottom quintile, it is not statistically significant and the standard error is quite large. Mothers in either the bottom quintile or top four quintiles in the treatment villages are not statistically significantly less likely to be seen by a midwife than mothers in control villages.

In Table 9, I assess whether growth monitoring visits, the probability of being sick and the probability of breastfeeding could be behaviors or intermediate outcomes during the postnatal period that are related to the effects I find. For the three dichotomous outcomes, I use a probit regression (similar to equation 2) and the estimates in the table refer to marginal effects. As with Table 8, I include a test of whether there were differences in these indicators before the treatment—Column 1. Columns 2 to 6 present results for the post-treatment period. Recall that eligible households in treatment and control villages are only differentially exposed to the program for 18 months starting in May 1998 when those in treatment villages began receiving benefits. After this 18-month

period, control villages were incorporated into the program and eligible households started receiving benefits in November of 1999. Thus, columns 2 to 4 (October 1998, and June and November 1999) assess early CCT exposure effects while the control villages still had not started to receive the program, whereas columns 5 and 6 (March and November 2000) refer to effects of the program after eligible households in control villages had already been incorporated. Importantly, there are no statistically significant differences between those children in the treated and control villages in any of the indicators in Table 9 pre-treatment, which again is evidence that the village-level randomization worked.

Among the indicators in the postnatal period, I find the strongest effects in measures related to growth monitoring. For both the extensive—whether any—and intensive margins—the number—of growth monitoring there are statistically significant effects of early exposure to *Oportunidades* for children in the bottom and the top four quintiles of village poverty. These effects are larger towards the latter parts of the differential exposure period—columns 3 and 4. As could be expected, the effects stop abruptly in March 2000 once control households have been exposed to the program—the coefficient estimates are much closer to zero and not statistically significant.

There is also some evidence that early CCT exposure had an effect on the number of days children were sick in the last four weeks. Although, with one exception, they are not statistically significant, the six estimates in the differential exposure period (columns 2, 3 and 4) are all negative—indicating fewer days being sick. In March 2000, once children in the control villages have begun receiving benefits, the signs of the estimates abruptly flip and become positive—although again they are not significant.

Early CCT exposure appears to have no positive effect on breastfeeding behavior, although the already high incidence likely leaves very little margin for any impact. The probability of breastfeeding is particularly high in villages in the bottom quintile of poverty, where more than 98% of mothers nursed their children before the program began. Moreover, program planners were actually more concerned that given that the program provided daily nutrition supplements for children aged 4 to 24 months—as well as for pregnant and lactating mothers—beneficiary mothers would be less likely to breastfeed. As in earlier results with a wider population of mothers (Hernandez Franco et al. 2000), there is no evidence of a negative effect of the program on whether mothers breastfed. Unfortunately, the follow-up surveys don't include questions on breastfeeding beyond June 1999, which limits my ability to assess effects along the intensive margin of breastfeeding—the total number of months that a mother nursed her child.

Interactions with the health sector for the mother and children may represent a path to a healthier pregnancy and delivery, as well as positive postnatal outcomes for the children. While the limited evidence on prenatal health care utilization does not allow me to confirm this, the richer evidence on postnatal outcomes is suggestive of such direction. Specifically, the positive child growth monitoring outcomes and the morbidity results may be indicative, in turn, of greater attachment of the mothers with their children and a trajectory of appropriate child development including future cognitive achievement. This type of experience and trajectory is consistent with Engle et al. (2011), who suggest that integrating early child development activities into the existing prenatal and postnatal health care systems may be the most successful at reaching children under three years of age.

6. Robustness Checks

The key limitation in my analysis is that my sample of children is partly determined after the treatment. That is, given that the period of differential exposure is 18 months, there are both poor households who conceived before they started receiving program benefits as well as households who conceived after. The latter group of households is potentially problematic, as endogenous fertility decisions could bias my estimates of the program's effect on children's cognitive achievement. A priori, the direction of the bias is not clear. One possible scenario is that the more disadvantaged parents among the eligible in treatment villages opt to have more children to obtain greater program benefits. In this case, the children in the sample would disproportionately come from the more disadvantaged families in the treatment group, which would bias the treatment effect downwards. A second scenario is that the more disadvantaged parents among those receiving benefits choose to have fewer children because of the safety net now provided by *Oportunidades*, which would lead to an upward bias.

The designers of the program were very aware of fertility-related concerns and took several steps to address them. First, although there are cash transfers for each school-age child, households reach benefit caps after a few children. Also, households were specifically told benefits were only guaranteed for three years, and transfers associated with school-age children only begin in third grade, generally only eight years after birth. As mentioned earlier, the program placed a strong emphasis on active participation and ownership, and consequently, a shift in the traditional welfare program approach. More importantly, Schultz (2004), Stecklov, Winters, Todd & Regalia (2007), and Barham (2012) using different estimation strategies and samples have documented

that *Oportunidades* had no effects on fertility. Nonetheless, I perform two additional checks to assess the likelihood of this type of selection bias.

Figures 4A and 4B present plots of nonparametric polynomial regressions of overall student achievement on month of birth. The sample includes all children from eligible households born between January 1998 and December 2000 in the bottom quintile of village poverty. The 0 dash line represents the beginning of program benefits for eligible households in treatment villages in May 1998. The 18th month dash line represents October 1999 when eligible households in the original control villages began receiving benefits. The figures suggest the program starts having an effect on cognitive achievement around the sixth month after its start. By the 10th month, around the time that there would be no or very few children conceived post-treatment, the effect of the program is already about 0.30 standard deviations. From this point the effect is large although fluctuates moderately all the way until about the time that eligible households in the control villages are incorporated into the program. There are little or no effects after this point. Given there are sizeable effects since the sixth month, the overall effects could not simply be the result of endogenous fertility decisions. In fact, that there are no effects in the first few months is consistent with literature suggesting that neonatal and child development outcomes are more sensitive to nutritional shocks during the last trimester of pregnancy (Rush et al. 1980; Almond, Hoynes and Whitmore Schanzenbach 2010 and 2015).

Table 10 applies the intuition of the figures to a regression framework using Equation 2. The overall effects of early CCT exposure for children in the bottom poverty quintile in Spanish and math scores are about 0.30 standard deviations—same as those

presented in column 6 (Panel A) of Table 4. The second row shows that the average effect on those largely conceived before the program started and who were born in the first 10 months of differential exposure is smaller but very consistent with what is in the figures. Excluding those children that were not exposed prenatally during the last trimester of their mothers' pregnancy but within the first 10 months of differential exposure, we see that the estimates are already very close to the overall effect of the program. Limiting the sample to those children born from the fifth to the 18th month of differential exposure shows the effect is roughly 0.40 standard deviations. This suggests that the effect is strongest among those children who are exposed prenatally to the program at least during the last trimester of their mother's pregnancy.

7. Conclusion

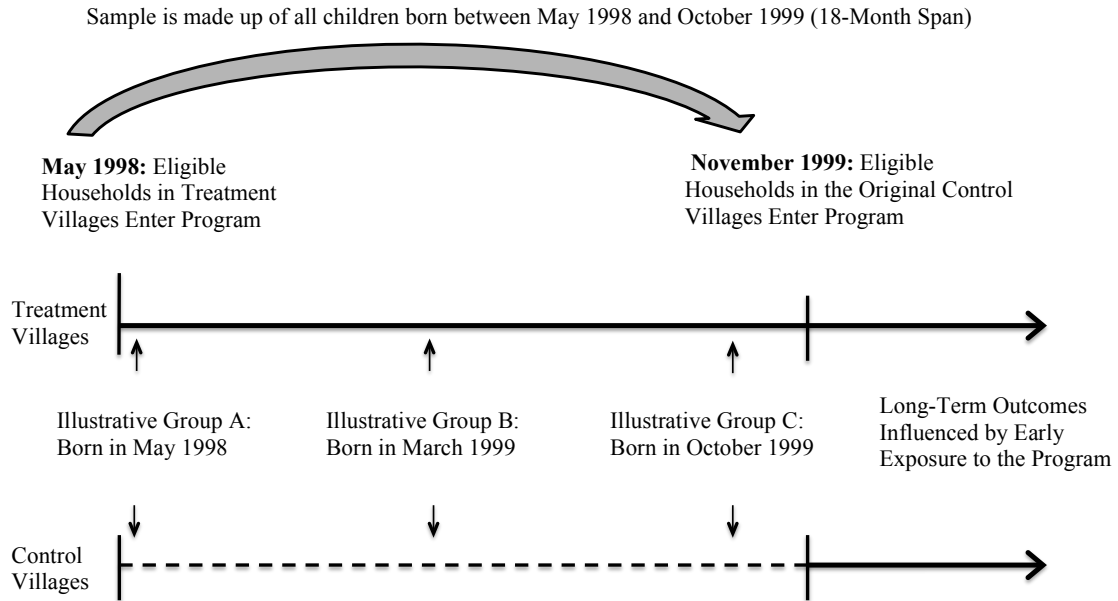
I assess the effects of early CCT exposure on long-term cognitive achievement by exploiting the initial village-level randomization that was carried while *Oportunidades* was being phased in starting in 1998. My paper is the first, as far as I know, to provide evidence of the stability and persistence of effects of a positive shock during both the prenatal and early postnatal period on cognitive development as measured by standardized student achievement outcomes in a developing country. I find there are statistically significant and persistent effects in both math and Spanish at least until the end of the primary education cycle. Although the effects are positive and significant for my full sample, these are strongly concentrated on children in the poorest villages. Notably, the effects are large enough to put their cognitive achievement on a par with children from more moderately poor villages. Importantly, it suggest that properly targeted CCTs even without an early childhood stimulation or parenting component may

be a viable option to support the most disadvantaged and poorest households in similar contexts. This is particularly relevant as other interventions that have had success with modestly poor communities, have failed at improving education outcomes among the very poorest (Gertler, Patrinos, and Rubio-Codina 2012).

While the literature of CCTs and early childhood development outcomes is mixed, one way to reconcile these results is to realize that programs have been targeted at children of different intensities of poverty. Children who were exposed later in life than those in my sample but still in early childhood and belonging to *Oportunidades* households showed effects on behavioral outcomes. Yet, there were no effects on their cognitive outcomes. Ecuador's BDH only had effects on child development outcomes on those belonging to the poorest households or those living in rural areas. Nicaragua, is the only country in which two different CCTs have shown effects on cognitive development on the overall sample of children. Yet, the Nicaraguan sample of children is poorer and more delayed than the one from Ecuador (Macours, Schady and Vakis 2012) and likely than the one from Mexico as well. The analysis in my study, which shows effects on the overall sample, although largely driven by children in the poorest villages, is consistent with this approach to understanding the mixed results in the previous literature. Finally, I also document that conditions in these poorest villages in the *Oportunidades* program appear to be not just poorer but starkly different in terms of the intensity of poverty and the level of education among would-be mothers.

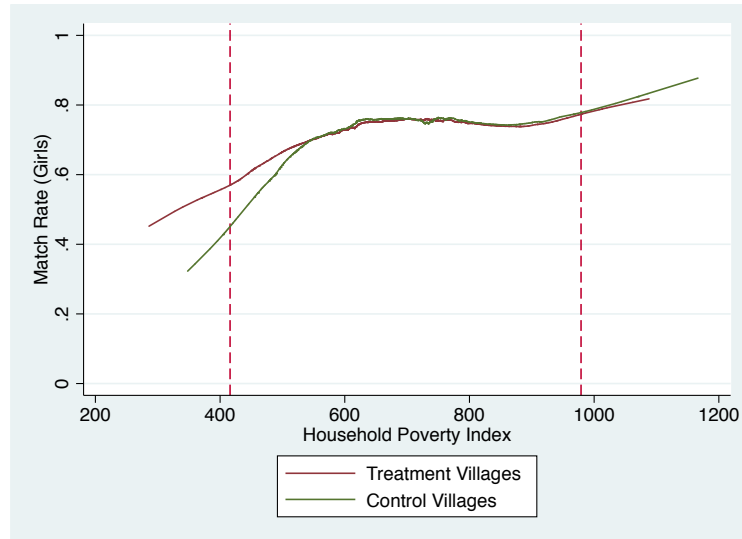
8. Figures and Tables

FIGURE 1. Timeline of Exposure to *Oportunidades*.



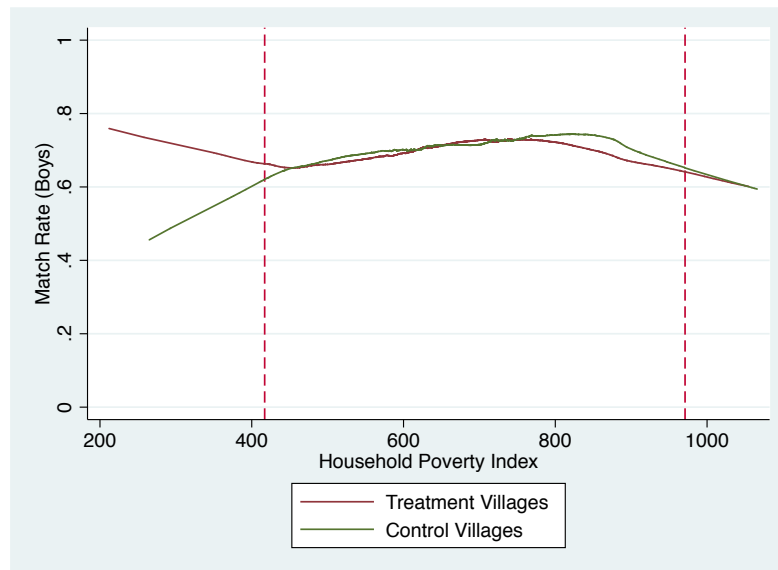
Notes: The solid line, in contrast to the dotted line, represents receipt of benefits. The analytic sample is made up of all children born between May 1998 and October 1999. This 18-month birth span corresponds to the delay in incorporation to the program between the treatment and original control villages. The oldest children in the sample are born in May 1998 (Group A), and are differentially exposed while in utero for a few weeks at most—at the end of their mothers’ pregnancy, and then for almost 18 months early in life before eligible households in the original control villages are enrolled in the program in late 1999. At the other end of the exposure spectrum are children born in October 1999 (Group C). These children are differentially exposed prenatally and for a few weeks after birth before eligible households in the original control villages started receiving benefits. In between Groups A and C, there are children with various levels of differential prenatal and early postnatal exposure, and also children whose parents were differentially exposed for some time before conception. For instance, those born in March 1999 (Group B), are children coming from households that were differentially exposed to *Oportunidades* for about two months before conception. After this pre-conception differential exposure of the households, then the children were differentially exposed prenatally (for the entire pregnancy) and also for the first seven months of life before eligible households in the original control villages began receiving program benefits.

FIGURE 2A. Match Rate by Poverty Index (Girls)



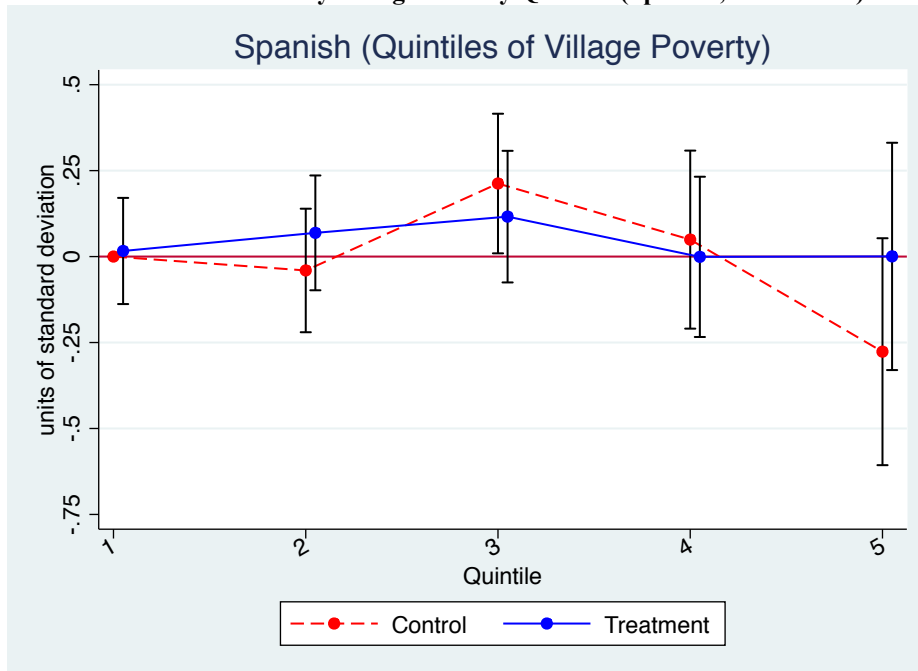
Notes: Plot of nonparametric locally-weighted regressions of the match rate on the household poverty index using a bandwidth of 0.5. Includes all girls born between January 1998 and December 2000. Dashed lines represent the 1 and 99 percentiles of the distribution.

FIGURE 2A. Match Rate by Poverty Index (Boys)



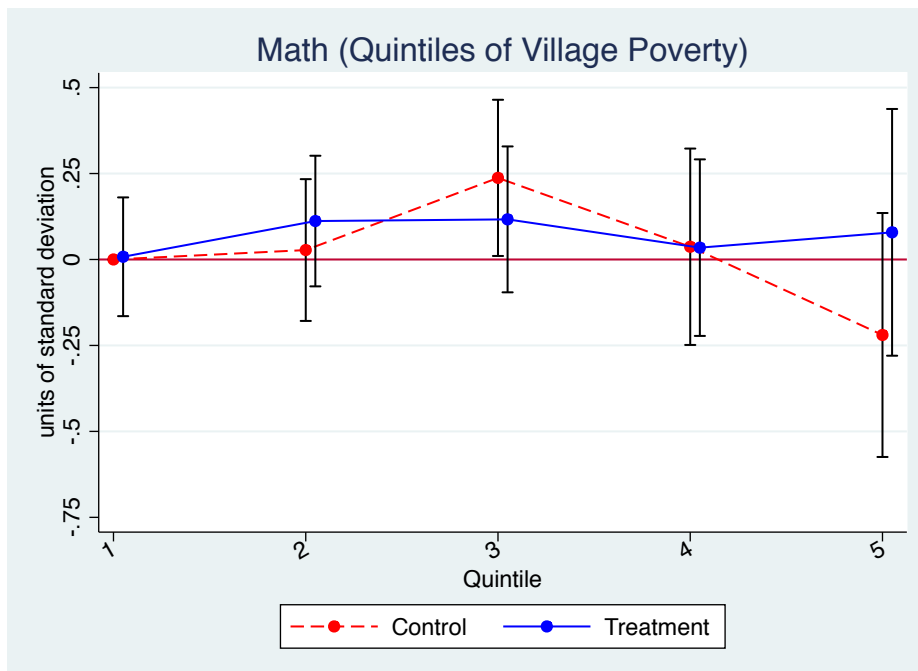
Notes: Plot of nonparametric locally-weighted regressions of the match rate on the household poverty index using a bandwidth of 0.5. Includes all boys born between January 1998 and December 2000. Dashed lines represent the 1 and 99 percentiles of the distribution.

FIGURE 3B. Impact of CCT during Prenatal and Early Postnatal Period, by Village Poverty Quintile (Spanish, All Grades)



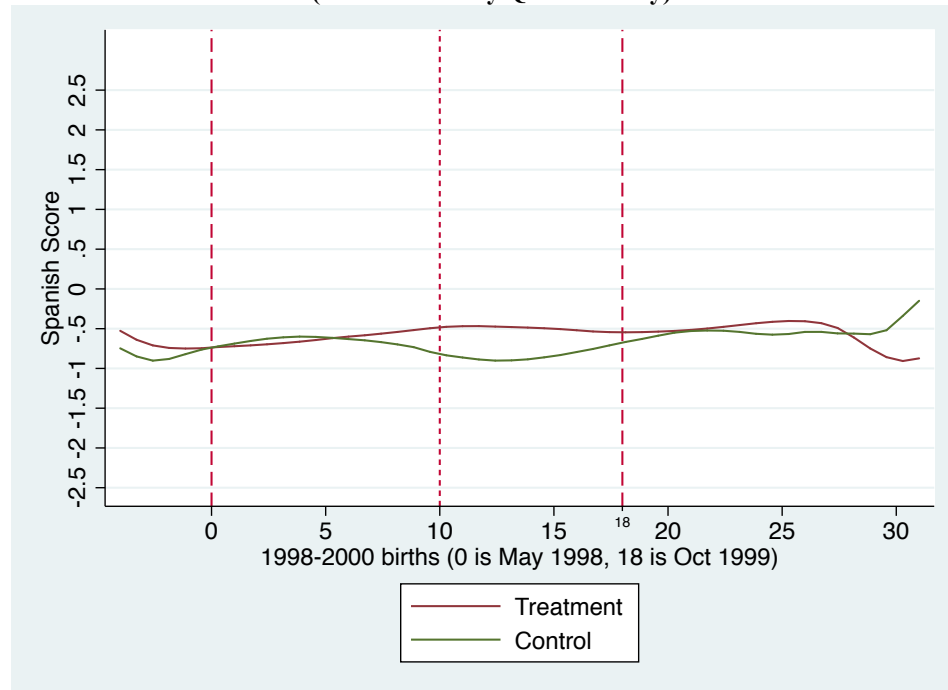
Notes: The figure plots coefficient estimates (and 90% confidence intervals) from Equation 2 that are associated with the treatment village indicator fully interacted with village poverty quintiles. The reference category is the control group in the first poverty (least poor) quintile.

FIGURE 3A. Impact of CCT during Prenatal and Early Postnatal Period, by Village Poverty Quintile (Math, All Grades)



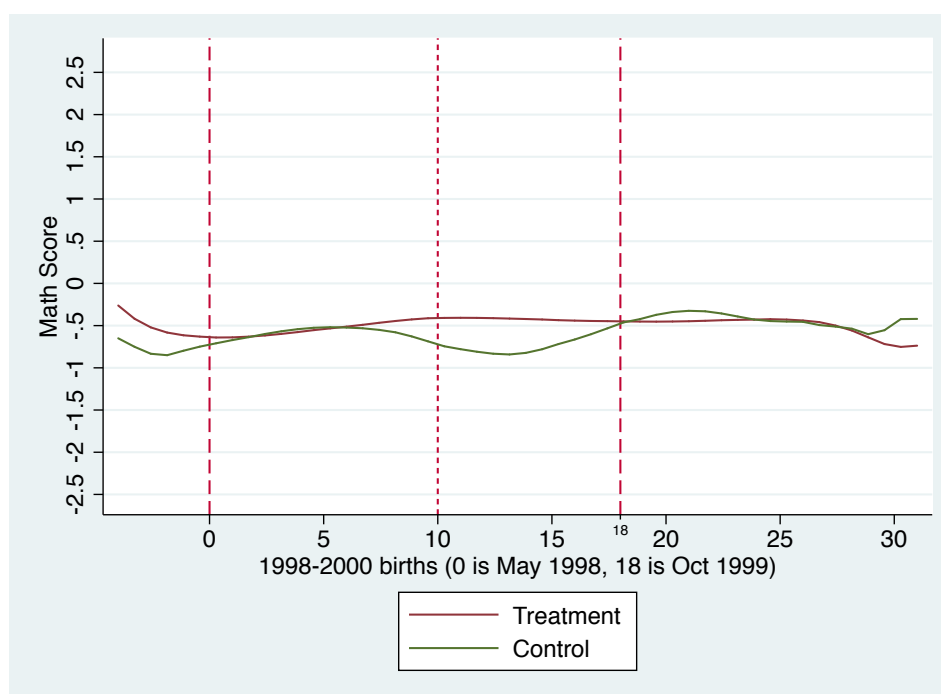
Notes: The figure plots coefficient estimates (and 90% confidence intervals) from Equation 2 that are associated with the treatment village indicator fully interacted with village poverty quintiles. The reference category is the control group in the first poverty (least poor) quintile.

**FIGURE 4A. Overall Effect of the Program on Spanish Achievement by Birth Month
(Bottom Poverty Quintile Only)**



Notes: Plot of nonparametric polynomial regressions of student achievement (Spanish) on month of birth using a rule-of-thumb bandwidth estimator, an Epanechnikov kernel, and a third degree polynomial. The sample includes all poor children born between January 1998 and December 2000 in the bottom quintile of village poverty. The 0 dash line represents the beginning of program benefits for eligible households in treatment villages in May 1998. The 18th month dash line represents October 1999 when eligible households in the original control villages began receiving benefits.

FIGURE 4B. Overall Effect of the Program on Math Achievement by Birth Month (Bottom Poverty Quintile Only)



Notes: Plot of nonparametric polynomial regressions of student achievement (math) on month of birth using a rule-of-thumb bandwidth estimator, an Epanechnikov kernel, and a third degree polynomial. The sample includes all poor children born between January 1998 and December 2000 in the bottom quintile of village poverty. The 0 dash line represents the beginning of program benefits for eligible households in treatment villages in May 1998. The 18th month dash line represents October 1999 when eligible households in the original control villages began receiving benefits.

TABLE 1: Overall Match Rate, and also by Gender and Year of Birth

	All (1)	Treatment (2)	Control (3)	Difference (p-value) (4)
<u>Panel A: All Births from January 1998 to December 2000</u>				
Girls and Boys	5,722	3,474	2,248	
% Matched	0.717	0.716	0.718	0.92
Girls	2,782	1,657	1,125	
% Matched	0.731	0.731	0.73	0.95
Boys	2,940	1,817	1,123	
% Matched	0.703	0.701	0.706	0.85
<u>Panel B: All "Poor" Births from May 1998 to October 1999 (Analytic Sample)</u>				
Girls and Boys	2,593	1,593	1,000	
% Matched	0.720	0.725	0.712	0.66
Girls	1,275	764	511	
% Matched	0.74	0.749	0.729	0.55
Boys	1,318	829	489	
% Matched	0.7	0.703	0.695	0.82
<u>Panel C: All Births in 1998 from Analytic Sample</u>				
Girls and Boys	1,330	826	504	
% Matched	0.708	0.715	0.696	0.61
Girls	659	394	265	
% Matched	0.742	0.748	0.732	0.72
Boys	671	432	239	
% Matched	0.675	0.685	0.657	0.52
<u>Panel D: All Births in 1999 from Analytic Sample</u>				
Girls and Boys	1,263	767	496	
% Matched	0.732	0.735	0.728	0.83
Girls	616	370	246	
% Matched	0.739	0.75	0.724	0.57
Boys	647	397	250	
% Matched	0.726	0.723	0.732	0.83

Notes: Panel A shows the number of children born in the *Oportunidades* evaluation sample from January 1998 to December 2000, as well as the percent that is matched with student achievement records from 2007 to 2012. P-values in column 4, which are adjusted for correlation at the village-level, test whether the match rate (% matched) between children born in treatment villages (column 2) and those born in control villages (column 3) is equal. Panel B restricts the number of children to those in my main analytic sample--those born into poor households from May 1998 to October 1999. Panels C, and D are analogous to B but disaggregate my main analytic sample by the year of birth of the children. *** denotes statistical significance at 1%, ** at 5%, and * at 10%.

TABLE 2. Summary Statistics and Randomization Check

	All villages with children born during May 1998 and Oct. 1999			Poor and matched only		Random. Check
	All children	Poor only	Poor and matched only	Top 4	Bottom	
				Quintiles	Quintile	
	(1)	(2)	(3)	(4)	(5)	(6)
<u>Individual Characteristics</u>						
Female	0.50	0.49	0.51	0.50	0.52	0.35
Age (in June 2012)	13.43	13.43	13.42	13.43	13.41	0.56
Mother finished primary education	0.38	0.33	0.36	0.41	0.24	0.92
Mother speaks indigenous language	0.39	0.43	0.46	0.33	0.75	0.66
<u>Household Characteristics</u>						
Head of household's age	37.41	35.79	35.11	35.34	34.58	0.92
Head speaks indigenous language	0.42	0.45	0.48	0.34	0.78	0.60
Head only speaks indigenous language	0.04	0.04	0.05	0.03	0.09	0.99
<u>Village Characteristics</u>						
Population	383	388	399	385	431	0.81
% speaks indigenous language	0.39	0.42	0.44	0.31	0.74	0.31
% households with dirt floor	0.64	0.68	0.68	0.60	0.86	0.52
% households with piped water service	0.33	0.30	0.30	0.38	0.09	0.083*
% households with electricity	0.70	0.66	0.67	0.78	0.41	0.36
% households receive in-kind food aid	0.14	0.14	0.15	0.15	0.14	0.96
Prenatal care services	0.45	0.45	0.47	0.48	0.45	0.40
Delivery care services	0.38	0.39	0.42	0.40	0.47	0.37
Infant care services	0.48	0.49	0.51	0.48	0.56	0.24
% of pregnancies with any prenatal care	0.90	0.89	0.90	0.92	0.83	0.11
Average month of first prenatal visit	3.82	3.88	3.89	3.73	4.25	0.26
Average # of growth monitoring visits	2.32	2.29	2.34	2.43	2.15	0.80
<i>N</i> (children)	3,404	2,594	1,867	1,297	570	
<i>N</i> (households)	3,230	2,468	1,831	1,273	558	
<i>N</i> (villages)	476	448	407	320	87	

Notes: Columns (1) to (5) present means of pre-treatment covariates. Columns (3) refers to my analytic sample--those who are poor and matched to student achievement records. Column (6) presents p-values associated with the indicator of treatment villages from Equation (1) with each characteristic as the dependent variable--standard errors clustered at the village level. ***, **, * denote statistical significance at the 1, 5, and 10 percent level, respectively.

TABLE 3. Village Characteristics by Quintile of Marginalization (Poverty)

	All	1st quintile	2nd quintile	3rd quintile	4th quintile	Top 4 quintiles (1-4)	5th quintile (bottom)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Marginalization index (standardized)	0	1.23	0.67	0.13	-0.5	0.38	-1.53
Poverty index (standardized)	0	0.6	0.34	0.23	-0.19	0.24	-0.96
Poor households	0.53	0.36	0.41	0.48	0.59	0.46	0.82
Extremely poor households	0.28	0.18	0.19	0.2	0.29	0.21	0.54
Years of schooling for women aged 14 to 45	4.55	5.52	4.98	4.79	4.42	4.93	3.00
Women aged 14 to 45 with primary schooling or more	0.49	0.63	0.54	0.52	0.47	0.54	0.28
Female	0.49	0.5	0.49	0.49	0.49	0.49	0.49
Age (1997)	24.4	25.7	24.4	25.2	24.1	24.9	22.4
Population	248	264	263	245	239	253	231
Speaks indigenous language	0.27	0.058	0.11	0.16	0.4	0.18	0.63
Number of prenatal visits	4.23	4.88	4.75	4.4	3.89	4.48	3.23
kAny growth monitoring visits in last 12 months	0.82	0.82	0.84	0.83	0.84	0.83	0.78
<i>N</i>	506	102	101	101	101	405	101

Notes: The table presents village-level means. The bottom quintile refers to the most marginalized or poorest villages as measured by an index capturing education, housing and labor market characteristics. The number of prenatal visits refers to mothers whose children were born in the last 2 years. Any growth monitoring visits in the last 12 months refers to children born from 1993 to 1997.

**TABLE 4. Impact of CCT during Prenatal and Early Postnatal Period,
Overall and by Village-Poverty Quintiles**

	Overall		Top 4 Quintiles		Bottom Quintile	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A. All Children</i>						
Spanish	-0.53	0.070** (0.035)	-0.41	-0.035 (0.040)	-0.75	0.285*** (0.065)
Math	-0.49	0.074** (0.037)	-0.39	-0.032 (0.044)	-0.67	0.306*** (0.070)
<i>N</i>	6,084					
<i>Panel B. Girls</i>						
Spanish	-0.45	0.108** (0.049)	-0.30	0.013 (0.059)	-0.72	0.255*** (0.088)
Math	-0.46	0.105** (0.053)	-0.35	0.016 (0.063)	-0.67	0.277*** (0.097)
<i>N</i>	3,084					
<i>Panel C. Boys</i>						
Spanish	-0.63	0.052 (0.049)	-0.54	-0.056 (0.055)	-0.79	0.307*** (0.095)
Math	-0.53	0.057 (0.053)	-0.45	-0.057 (0.063)	-0.67	0.322*** (0.099)
<i>N</i>	3,000					
Controls	No	Yes	No	Yes	No	Yes

Notes: Odd-numbered columns present control means, while even-numbered columns present treatment effect estimates. Column 2 presents coefficients from Equation 1, which refer to overall effects. Columns 4 and 6 present coefficients from a specification similar to Equation 2, in which the treatment village indicator is fully interacted with indicators for being in the top 4 quintiles and the bottom quintile of poverty. All regressions include the individual, household and village-level controls in Table 2. Standard errors, shown in parentheses, allow for clustering at the individual level. ***, **, * denote statistical significance at the 1, 5, and 10 percent level, respectively.

TABLE 5. Impact of CCT during Prenatal and Early Postnatal Period by School Grade

	Grade 3		Grade 4		Grade 5		Grade 6		All Grades	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
<i>Panel A. All Children</i>										
Spanish	-0.672	0.139*** (0.048)	-0.532	0.007 (0.044)	-0.463	0.087* (0.045)	-0.464	0.057 (0.045)	-0.531	0.070** (0.035)
Math	-0.694	0.147*** (0.053)	-0.452	-0.035 (0.048)	-0.416	0.096** (0.047)	-0.409	0.103** (0.047)	-0.489	0.074** (0.037)
<i>Panel B. Girls</i>										
Spanish	-0.642	0.155** (0.068)	-0.429	0.040 (0.062)	-0.362	0.130** (0.063)	-0.374	0.126** (0.061)	-0.445	0.108** (0.049)
Math	-0.735	0.222*** (0.073)	-0.4	0.015 (0.068)	-0.349	0.090 (0.065)	-0.38	0.123* (0.065)	-0.457	0.105** (0.053)
<i>Panel C. Boys</i>										
Spanish	-0.703	0.138** (0.068)	-0.64	-0.003 (0.062)	-0.579	0.064 (0.065)	-0.574	0.011 (0.067)	-0.625	0.052 (0.049)
Math	-0.653	0.087 (0.076)	-0.508	-0.072 (0.068)	-0.492	0.120* (0.068)	-0.444	0.103 (0.069)	-0.525	0.057 (0.053)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Odd-numbered columns present control means, while even-numbered columns present treatment effect estimates. Columns 2, 4, 6, and 8 present coefficients from an interaction of treatment village indicator with a full set of school-grade indicators. Column 10 presents coefficients from Equation 1. All regressions include the individual, household and village-level controls in Table 2. Sample sizes are the same within each panel and refer to the same number of observations as the panels in Table 4. Standard errors, shown in parentheses, allow for clustering at the individual level. ***, **, * denote statistical significance at the 1, 5, and 10 percent level, respectively.

**TABLE 6. Impact of CCT during Prenatal and Early Postnatal Period
by School Grade Among Bottom Village-Poverty Quintile**

	Grade 3		Grade 4		Grade 5		Grade 6		Pooled	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
<i>Panel A. All Children</i>										
Spanish	-0.915	0.436*** (0.086)	-0.69	0.174** (0.084)	-0.707	0.316*** (0.086)	-0.699	0.220*** (0.084)	-0.754	0.285*** (0.065)
Math	-0.978	0.498*** (0.092)	-0.595	0.179** (0.091)	-0.604	0.345*** (0.091)	-0.479	0.218** (0.088)	-0.668	0.306*** (0.070)
<i>Panel B. Girls</i>										
Spanish	-0.917	0.401*** (0.114)	-0.62	0.129 (0.117)	-0.663	0.293** (0.116)	-0.687	0.208** (0.102)	-0.72	0.255*** (0.088)
Math	-1.01	0.469*** (0.121)	-0.579	0.173 (0.129)	-0.569	0.314** (0.125)	-0.508	0.176 (0.117)	-0.665	0.277*** (0.097)
<i>Panel C. Boys</i>										
Spanish	-0.914	0.460*** (0.124)	-0.762	0.218* (0.119)	-0.757	0.326** (0.128)	-0.715	0.219 (0.136)	-0.791	0.307*** (0.095)
Math	-0.944	0.508*** (0.136)	-0.611	0.178 (0.130)	-0.642	0.355*** (0.133)	-0.441	0.259** (0.130)	-0.671	0.322*** (0.099)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Odd-numbered columns present control means, while even-numbered columns present treatment effect estimates. Columns 2, 4, 6, and 8 present coefficients from the triple interaction of a treatment village indicator, school grade indicators and an indicator for the bottom quintile of village poverty. Column 10 presents coefficients from an interaction of a treatment village indicator with an indicator for the bottom quintile. All regressions include the individual, household and village-level controls in Table 2. While I only present results for the bottom village-poverty quintile, sample sizes are the same within each panel and refer to the same number of observations as the panels in Table 4. Standard errors, shown in parentheses, allow for clustering at the individual level. ***, **, * denote statistical significance at the 1, 5, and 10 percent level, respectively.

**TABLE 7. Impact of CCT during Prenatal and Early Postnatal Period
by School Grade Among Top 4 Village-Poverty Quintiles**

	Grade 3		Grade 4		Grade 5		Grade 6		Pooled	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
<i>Panel A. All Children</i>										
Spanish	-0.526	-0.015 (0.058)	-0.444	-0.076 (0.051)	-0.34	-0.022 (0.053)	-0.349	-0.025 (0.054)	-0.411	-0.035 (0.040)
Math	-0.522	-0.039 (0.064)	-0.374	-0.136** (0.056)	-0.321	-0.013 (0.055)	-0.375	0.065 (0.057)	-0.394	-0.032 (0.044)
<i>Panel B. Girls</i>										
Spanish	-0.473	-0.002 (0.086)	-0.327	-0.025 (0.075)	-0.214	0.034 (0.074)	-0.216	0.056 (0.075)	-0.299	0.013 (0.059)
Math	-0.566	0.076 (0.092)	-0.304	-0.068 (0.080)	-0.241	-0.016 (0.077)	-0.316	0.095 (0.079)	-0.346	0.016 (0.063)
<i>Panel C. Boys</i>										
Spanish	-0.578	-0.016 (0.080)	-0.571	-0.102 (0.071)	-0.487	-0.044 (0.075)	-0.508	-0.063 (0.077)	-0.536	-0.056 (0.055)
Math	-0.479	-0.128 (0.090)	-0.449	-0.185** (0.080)	-0.414	0.018 (0.080)	-0.445	0.066 (0.083)	-0.446	-0.057 (0.063)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Odd-numbered columns present control means, while even-numbered columns present treatment effect estimates. Columns 2, 4, 6, and 8 present coefficients from the triple interaction of a treatment village indicator, school grade indicators and an indicator for the top four quintiles of village poverty. Column 10 presents coefficients from an interaction of a treatment village indicator with an indicator for the top four quintiles. All regressions include the individual, household and village-level controls in Table 2. While I only present results for the top four quintiles, sample sizes are the same within each panel and refer to the same number of observations as the panels in Table 4. Standard errors, shown in parentheses, allow for clustering at the individual level. ***, **, * denote statistical significance at the 1, 5, and 10 percent level, respectively.

TABLE 8. Potential Mechanisms - Prenatal Period

	Pre Treatment		Post Treatment	
	Mar. 98		Oct. 98	
	(1)	(2)	(3)	(4)
<u>Any prenatal check-up by nurse/doctor</u>				
Bottom Poverty Quintile	0.306	0.051 (0.056)	0.524	0.129 (0.124)
Top Four Quintiles	0.685	-0.029 (0.03)	0.663	0.136** (0.063)
<i>N</i>		1,661		300
<u>Any prenatal check-up by midwife</u>				
Bottom Poverty Quintile	0.7	-0.0945 (0.0579)	0.333	-0.03 (0.121)
Top Four Quintiles	0.438	-0.019 (0.037)	0.363	-0.0945 (0.0579)
<i>N</i>		1,661		300

Notes: Odd columns present control means. Columns 2 and 4 present marginal effect estimates from a probit regression similar to Equation 2, in which the treatment village indicator is fully interacted with indicators for being in the top 4 quintiles and the bottom quintile of poverty. Column (2) refers to prenatal checks in the last 12-month period by pregnant women in the villages in the sample. Consequently, these pregnant women did not necessarily refer to mothers of those children in my sample. All regressions include the individual, household and village-level controls in Table 2. Standard errors, shown in parentheses, allow for clustering at the village level. ***, **, * denote statistical significance at the 1, 5, and 10 percent level, respectively.

TABLE 9. Potential Mechanisms - Early Postnatal Period

	Pre Treatment	Post Treatment				
	Mar. 98	October 98	June 99	Nov. 99	Mar. 00	Nov. 00
	(1)	(2)	(3)	(4)	(5)	(6)
<u>Any growth monitoring</u>						
Bottom Poverty Quintile	0.013	0.061	0.099**	0.046*	0.005	0.015
	(0.028)	(0.048)	(0.046)	(0.027)	(0.02)	(0.017)
Control mean	[0.795]	[0.761]	[0.842]	[0.928]	[0.93]	[0.961]
Top Four Quintiles	-0.019	0.029	0.073***	0.013	0.019	-0.000
	(0.168)	(0.038)	(0.025)	(0.015)	(0.017)	(0.012)
Control mean	[0.854]	[0.854]	[0.881]	[0.948]	[0.95]	[0.97]
N	3,553	608	1,079	1,475	1,613	1,596
<u># of Growth monitoring visits</u>						
Bottom Poverty Quintile	-0.085	0.455*	0.311	0.93***	0.226	0.602
	(0.079)	(0.254)	(0.327)	(0.250)	(0.205)	(0.486)
Control mean	[2.158]	[1.537]	[2.585]	[2.732]	[3.185]	[4.865]
Top Four Quintiles	0.0819	0.188	0.456**	0.411**	0.141	0.414*
	(0.063)	(0.156)	(0.176)	(0.165)	(0.174)	(0.23)
Control mean	[2.367]	[1.741]	[2.640]	[3.016]	[3.5]	[4.776]
N	3,519	613	1,070	1,465	1,604	1,532
<u>Sick in the last four weeks</u>						
Bottom Poverty Quintile	0.023	0.007	0.018	-0.056	-0.031	0.065*
	(0.048)	(0.066)	(0.061)	(0.0393)	(0.0491)	(0.035)
Control mean	[0.336]	[0.164]	[0.149]	[0.254]	[0.229]	[0.083]
Top Four Quintiles	0.004	0.020	-0.034	-0.040	0.052*	0.018
	(0.025)	(0.045)	(0.032)	(0.028)	(0.028)	(0.065)
Control mean	[0.381]	[0.245]	[0.239]	[0.308]	[0.26]	[0.162]
N	3,502	611	1,083	1,490	1,616	1,609
<u># of Days sick in last four weeks</u>						
Bottom Poverty Quintile	0.267	-0.097	-0.171	-0.21	0.222	0.174
	(0.418)	(0.347)	(0.377)	(0.307)	(0.210)	(0.170)
Control mean	[2.294]	[0.731]	[0.959]	[1.188]	[.976]	[.369]
Top Four Quintiles	-0.001	-0.335	-0.232	-0.463**	0.132	0.234
	(.255)	(.451)	(.262)	(.208)	(.329)	(.179)
Control mean	[2.968]	[1.848]	[1.326]	[1.775]	[1.194]	[.865]
N	3,490	612	1,054	1,442	1,543	1,605
<u>Any breastfeeding</u>						
Bottom Poverty Quintile	-0.02	-0.008	-0.003			
	(0.017)	(0.014)	(0.038)			
Control mean	[.983]	[.961]	[.983]			
Top Four Quintiles	0.005	0.025	0.009			
	(.008)	(.019)	(.011)			
Control mean	[.941]	[.913]	[.951]			
N	3,482	613	1,030			

Notes: Control means shown between brackets. Columns 1 to 6 present coefficient estimates from a specification similar to Equation 2, in which the treatment village indicator is fully interacted with indicators for being in the top 4 quintiles and the bottom quintile of poverty. For the three dichotomous outcomes (any growth monitoring, sick in the last four weeks, any breastfeeding), the estimates refer to marginal effects from a probit regression. Column (2) refers to prenatal checks in the last 12-month period by pregnant women in the villages in the sample. Consequently, these pregnant women did not necessarily refer to mothers of those children in my sample. All regressions include the individual, household and village-level controls in Table 2. Standard errors, shown in parentheses, allow for clustering at the village level. ***, **, * denote statistical significance at the 1, 5, and 10 percent level, respectively.

TABLE 10. Effects on the Bottom Poverty Quintile by Period of Birth

		Spanish		Math
	(1)	(2)	(3)	(4)
All 18 months	-0.531	0.285*** (0.065)	-0.489	0.306*** (0.070)
From 1st to 10th month	-0.501	0.119 (0.079)	-0.47	0.17** (0.085)
From 5th to 10th month	-0.493	0.214** (0.105)	-0.462	0.234** (0.12)
From 5th to 18th month	-0.54	0.396*** (0.078)	-0.494	0.403*** (0.083)

Notes: Odd-numbered columns present control means, while even-numbered columns present treatment effect estimates. Columns 2 and 4 present coefficients from a specification similar to Equation 2, in which the treatment village indicator is fully interacted with indicators for being in the bottom quintile of poverty. The sample is restricted in each case to the number of months between May 1998 and October 1999 listed in the table. All regressions include the individual, household and village-level controls in Table 2. Standard errors, shown in parentheses, allow for clustering at the individual level. ***, **, * denote statistical significance at the 1, 5, and 10 percent level, respectively.

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