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A Decade Later: An Evaluation of the Longer-Term Impacts of a Honduran Conditional Cash Transfer

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I. INTRODUCTION

1. Conditional Cash Transfers

        The simplicity of giving money to the poor for a specific and preapproved purpose appeals to many stakeholders involved in policymaking. Conditional cash transfers (CCTs), particularly those evaluated in randomized controlled trials, have become popular amongst the international development community over the past two decades. Randomized controlled trials are a means of evaluating various development policy interventions intended to alleviate poverty, primarily through improved access to and quality of education and healthcare. CCTs, one form of policy intended to serve this purpose, have become incredibly widespread in the last two decades, reaching nearly every country in Latin America and many countries in Asia and Africa. In some cases, CCTs are the most significant social assistance program in a country; Brazil and Mexico's programs cover many millions of households (Fiszbein & Schady 2009).

        The vast CCT literature has found that CCTs have increased school enrollment and attendance, as well as health care utilization. Many of the outcomes studied are closely linked to the conditions attached to the cash transfer. Most follow-up evaluations have happened while the transfer was still being disbursed or within a year or two after the last disbursement.

        A small section of the literature has ventured to examine longer-term outcomes, including overall schooling attainment, learning and health outcomes. The magnitudes and significance of the impacts in this longer-term literature are relatively smaller than the short-term outcomes.

2. Honduras' CCT Program: PRAF

The Honduran CCT program *Programa de Asignación Familiar* (*PRAF*) was one such CCT program first implemented in the 1990s. In an attempt to address low educational attainment and high levels of poverty in Honduras, the government, in conjunction with the World Bank and the Inter-American Development Bank, implemented a series of CCTs under the name "*PRAF*." *PRAF-II*, the focus of this paper, was implemented between 2000 and 2002. The government ran a randomized experiment in 70 of Honduras' 298 municipalities, giving small transfers to children of certain age groups in exchange for investments in human capital—particularly education and health investments.

A number of short-term evaluations found significant positive impacts of *PRAF* on school enrollment, attendance, and health care utilization, as well as negative impacts on child labor outside the home. These short-term effects observed during the transfer period and within 2 years after the last disbursement provide a potential pathway for longer-term positive educational and labor supply outcomes.

This paper will quantify the long-term impact of Honduras' *PRAF* through 2012, 10 years after the last *PRAF* transfer was disbursed. We use Honduran government household survey data from 2001 to 2012 to estimate the impact of the program on education and labor supply outcomes. We take advantage of the randomized assignment of beneficiaries to treatment and control groups in order to find causal estimates.

We find that the treatment had significant impacts on educational outcomes for recipient children relative to the control group. The results are particularly strong for children in the poorest municipalities (measured as the lowest two quintiles of mean height-for-age). We also find that the treatment impact on overall schooling attainment

increased over time after the treatment, suggesting that the treatment impact was not only sustained post-transfer but that it actually grew.

Some of the outcome variables are especially strong for a particular age group within the overall recipient cohort; for example, the impact on overall grade attainment is strongest for the oldest cohort, while the attendance impact is highest for the youngest cohort. The impacts on labor supply are generally insignificant and of small magnitudes, suggesting that any long-term impacts on labor supply may not yet be visible, or simply are not being captured by our variable definitions.

These results are supported by robustness checks, including an alternative designation of the treatment and control groups and an examination of attrition in the dataset. We find that the regression results are similar between the main and alternative treatment designations. We also find that attrition is minimal and insignificant and likely has no impact on the results of this paper.

These significant results, particularly for *PRAF*'s educational impacts, have implications for Honduran policy, since a new CCT, Bono 10 Mil, is being implemented now. Policymakers can more carefully target future transfers towards these proven high-impact groups, particularly the poor. These results also hold broader importance for the existing literature on CCTs, which lacks significant evidence on their long-term impacts.

II. LITERATURE REVIEW

Conditional cash transfers are a branch of cash transfers that are conditioned on a human capital investment, such as school enrollment or attendance and health monitoring, intended to alleviate short-run and long-run poverty. The transfer is generally disbursed to a household head. Some percentage of the administrative cost often goes towards monitoring of conditions and ensuring that recipients adhere to the stated conditions.

The following chapter will layout the theory behind CCTs, as well as conveying an understanding of the immense variety of CCTs that differ in their targeting, conditions, monitoring and evaluation. For the purpose of this literature review, I will focus on Latin American conditional cash transfers and their impacts, keeping in mind that the CCTs vary significantly in their exact conditions, targeting/eligibility, and level of monitoring, which can make the magnitudes of impacts difficult to compare.

Impact evaluations of varied conditional cash transfers have shown a significant effect of transfers on such outcomes as poverty, school enrollment and attendance, education attainment, test scores and cognition, child labor supply, and visits to health centers. A few papers have ventured to answer questions about longer-term outcomes such as graduation and labor force participation, but the vast majority of the literature evaluates programs within a few years of their implementation. In education policy, varied CCTs have been proven effective in many categories. For transfers conditional on education, commonly studied outcomes include effects on school attendance or enrollment, short-term learning, child labor supply, and overall educational attainment. On the health side, common outcomes include visits to health clinics, measures of

nutrition, vaccination coverage, and prenatal and antenatal checkups. These effects have primarily been studied after a relatively short time period following the transfer, and even sometimes while the transfer was still being disbursed. The method of evaluation also varies across CCTs, since not all CCTs included random assignment in the original design (e.g. Chile's *Solidario*).

1. Targeting

CCTs tend to focus on particular groups of people through eligibility criteria, generally targeting at the community-level or at an individual or household-level. At the community level, eligibility is often based on geography, with cutoffs applied at certain city, county or state boundaries. *PRAF* is one such program in which all households within a certain boundary were automatically eligible. Some CCTs want to ensure that every household who receives the transfer meets some standard of poverty. In this case, they may choose to target individuals or households, often using income tests or approximations to ensure that recipients are poor enough. This test usually takes the form of a proxy means test, but can also be done through direct means testing or community-based assessments.

For education and health CCTs aimed at children, there are eligibility criteria that further narrow the targeting of the CCT to a particular age group, often using grade or age cutoffs. Some programs are aimed at very niche populations, such as Kenya's orphan and vulnerable children population (World Bank, "Kenya"). Some CCTs are directed at at underserved populations in an attempt to equalize educational attainment. Many programs are aimed at girls, who tend to be underrepresented in primary and secondary education, particularly in Asia and Africa; the ratio of girls to boys in primary and secondary education in low and middle-income countries was 95.7 to 100 in 2011 (World

dataBank). Many CCTs, from Cambodia to Indonesia to Pakistan, target education CCTs primarily at girls in an attempt to close this gap. This literature review will focus on CCTs aimed at both genders and which targeted slightly varied age groups and used different techniques to target poor populations in their respective countries, from geographic boundaries to proxy means tests.

2. Conditionality

One arm of the literature considers the importance of conditionality and monitoring, and ultimately suggests that conditions have some effect, but not always a significant one. Fiszbein and Schady (2009) suggest that conditions should only be used in cases in which there is an underinvestment in human capital and where the economy is "anti-poor." In other words, "the use of a CCT is predicated on the assumption that the pure income effect is insufficient and thus conditions are required to generate a further substitution effect in favor of investments" in human capital (Fiszbein & Schady 2009). In the case in which the income effect is sufficient, conditions are essentially redundant, and potentially inefficient if they lead to overinvestment in human capital.

In a system with no market failures, but where marginal private costs are higher than social costs in a human capital market, conditions which help close that gap can lead to more efficient outcomes (Behrman & Skoufias 2012). But, as Ravallion (2003) proposes, there may be market failures that have led to underinvestment in education and health. These market failures may need to be addressed before a CCT is the best solution. For example, if there is a lack of information about the returns to schooling, the income effect won't be sufficient to lead to increased investment; if parents systematically underestimate the returns to education, this may lead to underinvestment. CCTs may be able to correct this market failure if they are very carefully targeted towards the market

problem. But, in this case, simply disbursing information about the returns to schooling may lead to more students enrolling in school than a CCT. Thus, CCTs may be an efficient solution in some contexts, and outside of these contexts, they may create distortions that lead to inefficient outcomes, such as overinvestment.

Baird, Ferreira, Özler and Woolcock (2013) compared 35 studies of unconditional cash transfers (UCTs) and CCTs, and found that both CCTs and UCTs have a significant effect on enrollment. These results indicate that CCTs increase the probability of enrollment by 41% and UCTS by 23%. The effect sizes are always larger for CCT programs compared to UCT programs, but the difference is not significant. Programs that are explicitly conditional, which monitor compliance and penalize non-compliance, have substantively larger effects (60% improvement on odds of enrollment versus 18-25%). A World Bank review of social safety net programs noted that very few papers evaluate the marginal effect of conditionalities. Of those that do, including those that evaluate randomized conditionalities in Mexico and Ecuador, results show that a lack of conditionalities reduces the likelihood of school attendance.

In Morocco, Benhassine et al. (2013) find that a labeled cash transfer, in which families are told what the transfer is intended for but aren't monitored, reduced the primary school dropout rate by 70% and increased reentry by 85% among those who had dropped out before the program. It also cut the share of students who had never attended school by 43%. Relative to the LCT, CCTs had a significantly lower impact on these outcomes. The authors posit that conditionality discourages families with weaker students from participating in the program for fear that their children would not be able to meet the requirements.

Conditions do, however, play an important role in government-sponsored CCTs, since politicians favor the attachment of conditions as an assurance that the money is less fungible and will be used for the explicit purpose supported by the government. As Behrman and Skoufias (2012) note, conditions may also reduce the stigma associated with receiving government welfare. Overall, this means that conditions may improve the political feasibility of cash transfers.

The monitoring of conditions can be very costly due to the magnitude of administrative costs associated with keeping up with recipients. Caldés, Coady and Maluccio (2006) found that enforcing conditionality and targeting accounted for 31% of the administrative costs of Honduras' *PRAF*, 49% of Nicaragua's *Red de Protección Social*, and 60% of Mexico's *PROGRESA/Oportunidades*, demonstrating the very high cost of monitoring and enforcement, especially when the transfer is targeted at the individual or household level.

Within the sphere of conditional cash transfers, the conditions themselves vary significantly, from attendance and enrollment to achievement. Barrera-Osorio et al. (2011) find that transfers that are conditional on graduation and tertiary enrollment tend to have a stronger effect on attendance and enrollment at the secondary and tertiary levels. This may be because a graduation requirement ensures that a student must both enroll in and attend school, or because a retention requirement necessitates that students both regularly attend school and retain adequate grades. The attainment or achievement requirement may provide an additional and more effective incentive.

3. Short-Term Educational Outcomes
Because many conditional cash transfers are conditional on either enrollment, attendance, or some combination of the two, the vast majority of impact evaluations

report some effect on these outcomes. For the most part, the impacts are positive and statistically significant, but the magnitude varies across CCTs and between studies. The variation in the magnitude of the effects can be partially attributed to differences in the sizes or targeting of the transfers, as well as the particular context of education in each country.

Latin American CCT Programs: Enrollment & Attendance Impact

Country	Transfer	Amount[1]	Targeted Group	Baseline Enrollment	Impact on Enrollment[2]	Notes
Mexico	PROGRESA/ Oportunidades	20-22%	Grades 0-5 Grade 6 Grades 7-9	94% 45% 42.5%	1.9 8.7*** 0.6[3]	Largest impact for females
Nicaragua	Red de Protección Social	27-29%	Ages 7-13	72%	12.8***[4]	Largest impacts for children in coffee-cultivating communities (Gitter Barham 2008) in households that work in agriculture (Ford 2007)
	Atención a Crisis	18%	Ages 7-15	90.5%	7.3***[5] (Attendance)	
Colombia	Familias en Acción	17%	Ages 8-13 Ages 14-17	91.7% 63.2%	2.1** 5.6***[6]	
Chile	Chile Solidario	7%	Ages 6-15	60.7%	7-9***[7]	
Ecuador	Bono de Desarrollo Humano	6-10%	Ages 6-17	75.2%	10.3**[8]	Largest impact for the bottom quintile (Oosterbeek 2008)
Honduras	PRAF	5-9%	Ages 6-12	66.4%	8***[9]	

Source: Adapted from Schady & Fiszbein (2009).

[1] As a percentage of pre-transfer consumption among beneficiaries
[2] Percentage points
[3] Schultz (2004)
[4] Maluccio & Flores (2005)
[5] Macours & Vakis (2008)
[6] Attanasio et al. (2005)
[7] Galasso (2011)
[8] Schady & Araujo (2008)
[9] Galiani & McEwan (2013)

3.1 Enrollment & Attendance in Larger Transfers: PROGRESA/Oportunidades & RPS

Mexico's *PROGRESA/Oportunidades* and Nicaragua's *Red de Protección Social*, two of the largest transfers as percentages of beneficiaries' per capita expenditure (20-22% and 27-29% respectively), saw some of the largest effects on enrollment and attendance. Both of the transfers, which began in the mid to late 1990s, were conditional on school enrollment and 85% attendance, and led to enrollment increases of 8.7 percentage points in Mexico (Schultz 2004) and 12.8 percentage points in Nicaragua (Maluccio & Flores 2005) while the transfer was still being disbursed. In Nicaragua, the effect on attendance was even larger, at 23 percentage points. The effects were concentrated differently among the population of beneficiaries. For Mexico, the targeting covered a wide range of students from grades 0 through 9, but the significant effects were concentrated around grade 6, at the point where enrollment rates fall from 96% to 58% in Mexico (Schultz 2004). Both Schultz (2004) and Skoufias et al. (2001) found the highest marginal effects were for girls. In Nicaragua, targeting was specific to children ages 7 to 13 who had not yet completed grade 4, since baseline primary school enrollment was quite low (72%). The program was particularly impactful for attendance, which increased by 20 percentage points. Gitter and Barham (2008) and Ford (2007) discovered that the effects were strongest for poor children in coffee-cultivating communities, and whose households worked exclusively in agriculture. In a divergence from the effects of *PROGRESA/Oportunidades*, Dammert (2009) found that Nicaragua's *RPS* had stronger attendance effects for boys, reaching 18 percentage points in 2001, relative to 12 percentage points for girls.

3.2 Enrollment & Attendance in Smaller Transfers: Solidario, BDH, & PRAF
For CCTs that comprise lower shares of beneficiaries' per capita expenditure,

such as Chile's *Solaridio* program (7%) and Ecuador's *Bono de Desarrollo Humano* (6-

10%), the magnitudes of effects on attendance and enrollment are understandably

smaller. Galasso (2011) found that enrollment increased by 7.5 percentage points, up

from the very low level of 60.7%. In Ecuador, Schady and Araujo (2008) found a 10.3

percentage point increase in enrollment from the baseline level of 75.2%. Oosterbeek et

al. (2008) found that the effect was largest for the bottom quintile, implying that the

transfer was most effective for households for whom the small amount constituted a

larger proportion of their per capita consumption. Given the relatively small amounts of

the transfers, the magnitudes of the effects are quite large.

Honduras's *PRAF*, the focus of this paper, constitutes one of the smaller transfers

as a percentage of median per capita expenditure—only 5 percent for the average

recipient household, according to Galiani and McEwan (2013). As such, the authors'

finding of an 8-percentage point increase in enrollment after just one transfer is quite

impressive. Like for many other CCTs, the largest effect was for children in the two

quintiles of municipalities with the lowest average height-for-age scores, a proxy for

poverty, while the effect was insignificant for the three highest quintiles.

3.3 Enrollment & Attendance Pathways: Decreased Dropout and Increased
Reenrollment
Many evaluations have found that one of the most important pathways for these

increases in enrollment and attendance is decreased dropout and increased reenrollment.

It seems that CCTs encourage students who otherwise would have dropped out of school

or who had already dropped out of school to stay in school or reenroll. Baird, McIntosh

and Özler (2009) find that Malawi's re-enrollment rate among those who had dropped out

of school before the start of their CCT program increased by 2 percentage points, while the dropout rate fell from 11 to 6 percent. Likewise, Behrman et al. (2001) find that for children ages 11 to 14, *PROGRESA/Oportunidades* 's impacts on attainment were due to a decrease in the dropout rate, particularly in the transition from primary to secondary school. Overall, *RPS*'s effect is significant and shows an average improved retention rate of 6.5 percent (Maluccio & Flores 2005). An unanticipated benefit of the program was the large effect on those making the transition from fourth to fifth and sixth grades. The transfer also encouraged reentry among those who had dropped out. De Janvry et al. (2006) find that one reason for reduced dropout may be insulation from shocks. They found that the *PROGRESA/Oportunidades* transfers largely or completely protected children from the effect of these shocks on school enrollment. These results indicate the importance of targeting students at dropout prone ages, typically around the transition from primary to secondary school.

3.4 Attainment Impacts

Latin American CCTs: Attainment Impact

Country	Transfer	Amount[10]	Targeted Group	Baseline Enrollment	Impact on Overall Schooling Attainment	Notes
Mexico	PROGRESA/ Oportunidades	20-22%	Grades 0-5 Grade 6 Grades 7-9	94% 45% 42.5%	0.2 years[11] 0.66 years[12]	Decreases in dropout rate & increase in reenrollment for ages 11-14[13]
Nicaragua	Red de Protección Social Atención a Crisis	27-29% 18%	Ages 7-13 Ages 7-15	72% 90.5%	6.5% increase in retention[14] 0.2 years[15]	
Colombia	Familias en Acción	17%	Ages 8-13 Ages 14-17	91.7% 63.2%	4-8% increase in high school graduation[16]	Largest impacts for girls in rural areas

Source: Adapted from Schady & Fiszbein (2009).

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[10] As a percentage of pre-transfer consumption among beneficiaries
[11] Behrman et al. (2010)
[12] Schultz (2004)
[13] Behrman et al. (2001)
[14] Maluccio & Flores (2005)
[15] Del Carpio & Macours (2009)
[16] Baez & Camacho (2011)

These effects on attendance and enrollment translate into effects on overall grade attainment and schooling. Behrman, Parker and Todd (2005) found that Mexico's *PROGRESA/Oportunidades* led to a 0.2-year increase in overall grade attainment in 2003. This result, based on an analysis utilizing the 18-month period of differential exposure to Mexico's program, creating a randomized control group, was significantly lower than a previous prediction by Behrman, Sengupta and Todd from 2001, which expected, based on the trajectory at that time, that the program would increase schooling by 0.6 years. Importantly though, Behrman, Parker and Todd's (2005) effect is closer to the original estimate for youth around the transition between primary and secondary school, at 0.5 years. The significantly smaller finding in 2003 may indicate that the impacts on grade attainment diminished over time in Mexico, but the results remained strong for students at that crucial point of transition. Del Carpio and Macours (2009) find very similar results in Nicaragua's *Atención a Crisis*, a smaller CCT than *RPS*, in which attainment increased up to 0.2 years.

3.5 Attainment Pathways: Learning & Cognition
One possible pathway for increased grade attainment and other improved schooling outcomes to lead to better long-term outcomes may be improved learning and cognition. The literature on CCTs shows a large variety of impacts on learning outcomes, but mostly with disappointing results. In Latin America, Baez and Camacho's (2011) regression discontinuity design found no long-term effect on children's test scores in Colombia's *FA* program, and high school graduates who received the transfer performed no better than their cohort. Behrman, Sengupta and Todd (2000) found no significant impact of Mexico's *PROGRESA/Oportunidades* on test scores, and a later follow-up

found no impact of 2 additional years of transfers on language or math tests (Behrman, Parker & Todd 2005). Ponce & Bedi (2008) similarly found no impact of Ecuador's *BDH* on test scores. The learning/cognition bright spot in Latin America is Nicaragua; Barham, Macours and Maluccio (2013) found a significant long-term impact (after 7 years) of Nicaragua's *RPS* on math and language achievement scores for young men of a one-quarter standard deviation. Macours, Schady and Vakis (2012) also found a significant impact of Nicaragua's *Atención a Crisis* on cognitive outcomes—particularly in language. The effects were found to be largest for older preschool children due to a reallocation of money within households towards more nutrient rich foods, in addition to increase use of preventative healthcare, rather than a direct effect of enrollment or attendance in school. Outside of Latin America, the results are similarly dismal. In Cambodia, the *Education Sector Support Project* (CESSP) had no impact on children's long-term test scores despite a 21.5 percentage point increase in enrollment for grades 7-9 (Filmer & Schady 2009). Based on the current evidence, it seems unlikely that the primary pathway for improved long-term outcomes would be better learning outcomes.

4. Short-Term Child Labor Impacts

Some evidence has shown that these increases in enrollment and attendance are mirrored by drops in child labor. Ravallion & Wodon (2000) show that child labor decreases in response to a school subsidy if child leisure and schooling are complements in the family's utility function. As such, the finding that drops in child labor mirror increases in schooling enrollment may indicate that families substitute their child's time between work and school. Skoufias et al. (2001) found that *PROGRESA/Oportunidades* reduced the probability of working among boys ages 8-11 by 21 percent, and 12.4 percent for boys ages 12-17. They found no impact for younger girls, but girls ages 12-17

experienced reductions of up to 17 percent. Schady and Araujo (2008) find a large impact of Ecuador's *BDH* on children work of 17 percentage points. Edmonds and Schady (2009) posit that the Ecuadorian transfer delayed children's entry into paid employment by keeping them in school longer, despite the transfer being less than 20% of the wage paid to child laborers in the labor market. They found that school expenditures rose, but total expenditures fell due to the foregone child labor earnings. In Nicaragua, *Atención a Crisis* reduced child labor primarily for older boys who used to work more and for boys who were further behind in school (Del Carpio & Macours 2009). In Honduras, Galiani and McEwan (2013) determined that *PRAF* decreased work outside the home by 3 percentage points and work inside the home by 4 percentage points after the first disbursement of the transfer. Although the vast majority of the evidence is exclusive to boys, Borraz and González (2009) find that the Uruguayan CCT reduced female child labor in Montevideo, but had no significant impact for males. There is some evidence that short-term reductions in child labor during the transfer disbursement period may carry into the longer-term post-transfer. Behrman, Parker & Todd (2005) found that *PROGRESA/Oportunidades* significantly decreased child labor for rural boys five and a half years after the transfer, but had no impact on rural girls. Rural boys in the treatment group were also less likely to work in agriculture, indicating that the largest effect was for children in households engaged primarily in agriculture, who may have been among the poorest students. It may be then, that one of the significant effects of CCTs is pulling children out of agricultural work and putting them back into school.

5. Short-Term Poverty & Consumption Impacts
        Various studies have shown that CCTs can significantly increase income and consumption for poor families, pulling many of them above the poverty line. Hoddinott

and Skoufias (2003) reported that *PROGRESA/Oportunidades* increased the mean monthly consumption level per household by increasing caloric intake. Ultimately, Mexico's CCT reduced the number of people living below the poverty line by 10 percentage points, and the severity index of poverty was reduced by 45% (Skoufias et al. 2001). Maluccio & Flores (2005) found that Nicaragua's *RPS* significantly increased expenditures of recipient families, and especially increased the shares of expenditures spent on food by poorer households. Confirming these trends, Fiszbein & Schady (2009) found that CCTs often change the composition of consumption in a household by increasing spending on higher-quality sources of nutrients. CCTs may also improve savings and investment, increasing long-term consumption. Gertler et al. (2012) found that households receiving *PROGRESA/Oportunidades* transfers consumed 74 cents of each dollar, and invested the rest, "permanently increasing long-term consumption by about 1.6 cents" per dollar. This increased propensity to save and invest may be one pathway through which longer-term outcomes may change for CCT recipient children.

6. Short-Term Health Impacts

Many CCTs intended to improve health outcomes have had positive and significant impacts on various measures of health, including the probability of child stunting, immunization rates, growth monitoring, use of preventive health care, health center visits, and antenatal care. There are mixed results with regards to the ultimate consequences of these changes on the overall health of children, for example through measuring infant mortality and incidence of disease.

Since many health CCTs condition receipt on regular visits to health centers, there are generally significant effects on check-ups, and in a few cases these translate into longer-term improved health outcomes. Overall, however, the evidence is unclear at best.

For Mexico's *PROGRESA/Oportunidades* program, studies have found conflicting impacts on visits to health facilities. While Adato and Hoddinott (2009) found that health visits increased by 18 percent, Gertler (2000) found no significant effect on the number of visits to health facilities. However, IFPRI (2002) reported a 4.7 percentage point reduction in illness rates for children ages 0-2 and a decreased probability of child stunting for children ages 12-36 months. Adato and Hoddinott (2009) also found a 12 percent reduction in illness. However, other CCTs generally had little to no impact on rates of illness, despite significant increases in their respective conditions. For example, Attanasio et al. (2005) found that Colombia's *Familias en Acción* increased the number of children taken to growth and development monitoring by 22.8 percentage points for children ages 0-1 and by 33.2 percentage points for ages 2-4, but they found no impact on immunization rates, while IFPRI (2002) found a significant impact of *Familias en Acción* on immunization rates.

Honduras' *PRAF* had fairly wide-reaching and significant effects on a number of health outcomes for pregnant mothers and children ages 0-3. *PRAF* was conditional on regular visits to health centers, so it is unsurprising that one of its largest effects was on health center visits; Morris et al. (2004) reported using 2002 data from an IFPRI (2003) intermediate assessment that *PRAF* increased the likelihood of a child being taken to a health center in the past month by 20.2 percentage points. Check-ups increased by 17-22 percentage points, and the number of children with vaccination cards increased 4-7 percentage points (the small number here is likely due to Honduras's high rates of immunization before the transfer). Additionally, there was an 18-20 percentage point increase in the number of pregnant women who received 5 or more prenatal check-ups,

but there was no effect on post-natal check-ups. Surprisingly, though, these numbers did not translate into lower incidence of many diseases. Diarrheal diseases were not impacted, dietary consumption did not change, and children's z-scores did not improve. This lack of impact on disease prevalence is fairly representative of the overall literature; the World Bank's metareview of social safety nets found very few interventions that decreased disease in any significant way.

7. Long-Term Results and Simulations

The overall significant and positive impact of CCTs on educational outcomes begs another question: does this short-term increase in educational outcomes have any significant long-term impact on recipients' wellbeing? The positive impact on educational outcomes provides a plausible pathway for longer-term outcomes such as higher productivity/wages, better employability and higher labor supply.

For a few CCTs, longer-term follow-ups after beneficiaries stopped receiving transfers have indicated some sustainability of these short-term impacts on attendance and enrollment. Adato & Hoddinott (2009) found that two years after households stopped receiving benefits in Nicaragua's *RPS* program, school enrollment dropped by 12.5 percentage points, but was still 8 percentage points higher than before the program, implying some sustainability of impact. Supporting this idea, Barham, Macours & Maluccio (2013) found that one year after the program, the treatment group still completed 0.62 years of additional schooling. Their 7-year follow-up of the program yielded a barely decreased coefficient of 0.5 years of additional schooling. Additionally, the authors found a ¼ standard deviation increase in math and language achievement scores for young men, representing one of the only CCTs that has had a significant and lasting impact on learning outcomes.

Behrman, Parker & Todd's 2005 and 2009 follow-ups for Mexico's

PROGRESA/Oportunidades found a significant increase in enrollment five-and-a-half

years after the last transfer receipt, which translated into a 0.2 year increase in schooling

for rural youth, and a nearly 0.5 year increase for youth close to the transition between

primary & secondary school. Although the authors found no impact on

achievement/learning, this was somewhat expected, given that so few short-term papers

found significant results. They did find a significant reduction in male labor force

participation of approximately 4 percentage points, due to delayed entry into the

workforce associated with increased schooling. Males in the treatment group, ages 15 to

21 in 2003 at the point of evaluation, were also less likely to work in agriculture, and less

likely to migrate out of the household by 6 percentage points. It may be that these

individuals have lower expected gains to migration, since their income by staying in the

household has increased. These 5.5-year results for *PROGRESA/Oportunidades* certainly

indicate the possibility of sustained impacts in Mexico, particularly in terms of overall

schooling, but also hint at other possible impacts on labor supply and migration.

Projecting the short-term results on grade attainment forward into the recipients'

futures, Fiszbein & Schady (2009) suggest that the 0.2-year increase in schooling found

in Behrman, Park & Todd (2005) would cause students exposed to 2 more years of

PROGRESA/Oportunidades to earn 2% higher wages than the rest of their cohort.[17] This

estimate, based on the assumption that the Mincerian return to education is 10%, may be

an accurate estimation of the true effect of education on wages for some Latin American

countries. According to Psacharapoulos & Patrinos (2004), estimations of returns to

schooling in Mexico range from 6.5% to 16.1%.

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[17] Supposing that the Mincerian return to education is 10%.

For Honduras, the subject of this paper, estimates range from 9.3% to 12.5%, outperforming the Latin American/Caribbean average, but all estimates were calculated using data between 1986 and 1991. If these estimates are still accurate 20 years later, the use of the 10% benchmark may be appropriate. The lack of recent data on returns to schooling makes an exact prediction precarious, but the overwhelming consensus amongst economists is that improving the stock of education in a country contributes significantly to income and economic growth. Long-term evaluations of CCTs must determine whether or not the small increases in enrollment and attendance associated with CCT receipt have any long-term effects on income for recipient households and individuals.

An intermediate impact assessment for Honduras's *PRAF* by Glewwe & Olinto (2004) found that students in the treatment group in 2002, after the last transfer payment, were 3.3 percentage points more likely to be enrolled than those in the control group, falling significantly from the finding by Galiani and McEwan (2013) of 8 percentage points in 2001. As in the case of Nicaragua's *RPS*, this finding indicates that the effects of *PRAF* may diminish over time. This paper's 10-year follow-up could also help quantify the degree and timing of dissolution of the effects.

This paper will attempt to fill this void in the literature by examining long-term outcomes such as overall attainment, attendance, literacy, and labor supply up to 10 years after the last *PRAF* transfer was disbursed. Understanding the long-term benefits of Honduras's *PRAF* will help researchers determine which interventions are most effective in alleviating poverty in both the short-term and long-term, and will help prevent policymakers from investing in interventions that have no enduring impact.

III. BACKGROUND ON HONDURAS & PRAF

1. Honduras Background

       Honduras is classified by the World Bank as a lower-middle income country. In 2012, GDP per capita was US$2,323, far below the Latin American & Caribbean average of US$9,192. This averaged number does not capture the high level of inequality within Honduras. In 2011 the Gini coefficient was 57 (World dataBank). The income share of the bottom quintile in 2009 was just 2 percent—the lowest recorded by the World Bank in the entire world in that year. The growth of inequality was especially large in the years leading up to *PRAF-II* in the 1990s (Cohen, Franco & Villatoro 2006, 296).

       The educational stock in Honduras is quite low, with average years of schooling in the EPHPM dataset (2001-2012) at 5.11 years, less than the 6 years it takes to complete primary school, and literacy at 76%. The flow is more optimistic; the World Bank found that net primary school enrollment was high in 2012 at 94%, but low relative to much of the rest of Latin America. It has improved, however, since the establishment of the *PRAF* program in 2000, when it was at 88% (World dataBank). The larger problem lies beyond enrollment in both attendance and completion. School attendance is just 61.3% for those under 18 in the EPHPM dataset, and the World Bank found that progression to secondary school in 2011 was just 68%. This progression between schools is one of the margins where CCTs have been particularly effective.

       Employment opportunities are also fairly dismal. Only 58% of those ages 16-65 in the EPHPM dataset reported having worked (either paid or unpaid) in the week before the survey. This reflects a much harsher reality for Honduran workers than the official unemployment rate of 4.4% would suggest (World dataBank).

2. PRAF Background

       The Honduran CCT *Programa de Asignación Familiar* (*PRAF*) was first

implemented in the late 1990s, following the lead of early adopters of CCTs such as

Mexico and Brazil. The first form of *PRAF*, created in 1990, was supported by the World

Bank and the Inter-American Development Bank to help counteract the impacts of

structural adjustment policies. This transfer was aimed at women and children, and in its

first 10 years of existence averaged over 300,000 beneficiaries (Moore 2010). However,

the program had many serious flaws including constantly changing beneficiaries and poor

targeting; Cohen, Franco & Villatoro (2006, 294) noted that 30-40% of the beneficiaries

belonged to the top two income quintiles.

       In 1998, the IDB provided a $50 million loan to the Honduran government to

create the new and improved pilot project *PRAF-II* (Moore 2008). The International Food

Policy Research Institute (IFPRI) also contributed ideas for the redesign of *PRAF*. This

version contained two treatment arms—a health transfer for young children (ages 0-3 and

pregnant women) conditioned on regular visits to health centers and an education transfer

for an older cohort (ages 6-12 in grades 1-4) conditioned on school enrollment and 85%

attendance. There was also an attempt at a supply-side intervention through investments

in schools, but only 7 percent of the education funds and 17 percent of the health funds

were disbursed (Moore 2008), so I ignore this element of the program, following the

precedent set by Galiani & McEwan (2013). The total budget was $50 million over the 3-

year period (Caldés, Coady, & Maluccio 2006).

       PRAF-II came at a particularly important time for Honduras after Hurricane

Mitch destroyed public infrastructure in October and November 1998. The U.S. State

Department (1999) found that the hurricane lowered GDP in 1999 by 2 percent and caused $3 billion in damages, destroying more than 3,000 schools in its wake.

The program was geographically targeted at poor households using Honduras's municipalities. Honduras's 18 departments are divided into 298 municipalities. The municipalities were ranked by poverty level using a nutritional proxy for poverty—mean height-for-age. The 70 municipalities with lowest mean height-for-age were chosen to participate in the RCT; these municipalities were primarily clustered in the western part of the country, which is poorer on average. Forty municipalities were randomly assigned to receive a version of the treatment and thirty municipalities were assigned to the control group.
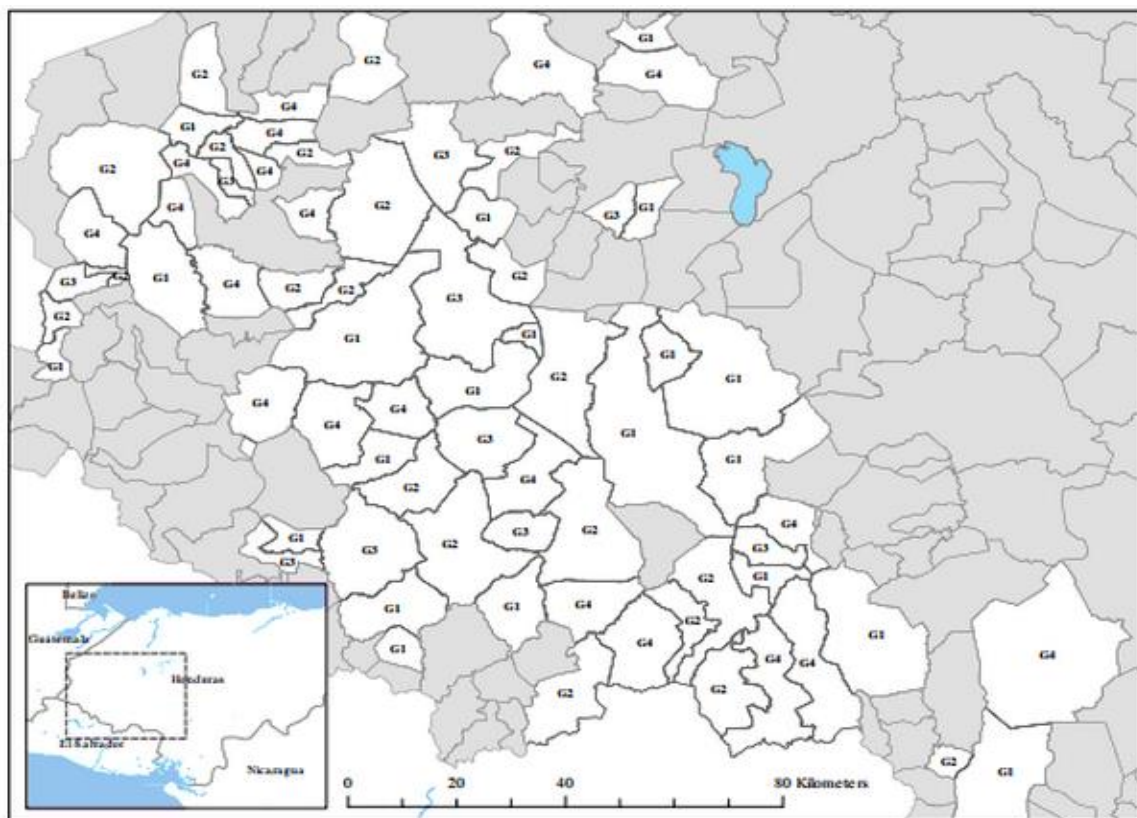
Figure 1: Map of Treatment & Control Municipalities. **Source:** Galiani & McEwan (2013). **Notes:** Participating municipalities are white, while non-participating municipalities are gray. The Treatment Group is G1 & G2 and Control Group is G3 & G4.

Within these municipalities, eligibility was simply determined by age and grade. Children ages 6 to 12 who had not yet completed 4th grade were eligible for the education transfer. Pregnant women and children under 3 were eligible for the health transfer. Households with multiple eligible children could receive up to 3 education transfers and up to 2 health transfers. The initial project proposal included income testing to determine eligibility at the household level, but an evaluation found that 87% of households in the municipalities fell below the US$2-per-day poverty line, and so it was determined that means testing would be a waste of resources (IFPRI 2000).

Households were eligible for up to 3 payments of approximately 828 Lempiras (US$58) per year for education transfer eligible children (Adato & Hoddinott 2010), and up to 2 payments of 644 Lempiras (US$40) per year for children eligible for the health transfer (Galiani & McEwan 2013). The education transfer amount was chosen to cover the annual costs of matriculation fees and other school-related costs (502 Lempiras), as well as the average annual income contributed by students in the eligible age group to their household (326 Lempiras) (Adato & Hoddinott 2010). The average transfer amount was small relative to other CCTs, at just 5% of median per capita expenditure (Galiani & McEwan 2013) or 7% of pre-transfer consumption compared to 29% of pre-transfer consumption in Nicaragua and 22% in Mexico (Fiszbein & Schady 2009).

The monitoring and enforcement of conditions in *PRAF* was minimal. Morris et al. (2004) noted that health transfer beneficiaries had to regularly submit certified, bar-coded attendance slips at health center visits, but no beneficiaries were suspended for non-compliance. The education transfer beneficiaries, on the other hand, were subjected to no such mechanism for monitoring attendance, making the transfer de facto

conditional only on enrollment. Caldés, Coady and Maluccio (2006) note that *PRAF-II*'s low cost relative to *PROGRESA/Oportunidades* and *RPS* is in part due to fewer resources being devoted to the monitoring of conditions—*PRAF-II* devoted 14% of its resources to conditioning, versus 16% in *RPS* and 25% in *PROGRESA/Oportunidades*.

A number of short-term evaluations have identified significant impacts of *PRAF* on educational, child labor and health outcomes. Galiani & McEwan (2013) found that students in the treatment group in 2001, one year into the treatment, were 8 percentage points more likely to enroll in school than students in the control group. The results are stronger for students in poorer municipalities. They also found a 3-percentage point reduction in child labor outside the home.

Morris et al. (2004) reported using 2002 data from an IFPRI (2003) intermediate assessment that the reenrollment rate for the treatment group rose 17 percentage points and school attendance rose 4.3-4.6 percentage points in 2002, 2 years into the treatment. This is equivalent to an attendance increase of almost one additional school day per month and 10 additional school days per year.

In 2004, 2 years after the last transfer was disbursed, Glewwe & Olinto (2004) found that students were 3.3 percentage points more likely to be enrolled in school. There were also modest effects on the dropout rate of 2-3 percentage points.

Moore (2008) found that in 2002, two years into the transfer, children eligible for the health transfer increased their visits to health centers by 15-21 percentage points. Check-ups increased 17-22 percentages points, and the number of children with vaccination cards increased 4-7 percentage points. There was also an 18-20 percentage

point increase in the number of pregnant women who received 5 or more prenatal check-ups.

These positive results set the stage for the possibility of significant long-term impacts. Increased health care usage, school attendance and enrollment are all potential pathways for positive long-term outcomes including overall educational attainment and labor supply.

IV. DATA/METHODOLOGY

1. Household Survey

This paper uses Honduran government data from the Encuesta Permanente de Hogares de Propósitos Múltiples[18] (EPHPM) survey. This government-supported household survey is conducted once or twice per year, usually in May and September. It samples both rural and urban areas and covers each of Honduras's 18 departments every year. The questionnaire asks household members about their living situation, household composition, educational background, employment, income, and health.

The breadth and depth of available variables give this paper the scope to answer many different questions about treatment effects on everything from education to labor supply. The data are managed and interpreted by the Instituto Nacional de Estadística. The questions and sections of the survey vary slightly between the survey years used in this paper (2001-2012), since the questionnaire has evolved over time. Any discrepancies in the availability of certain variables are noted in Appendix Table C.

We also use a municipality-level dataset created by Galiani and McEwan (2013), which identifies which municipalities were part of the treatment and control groups. We merge this dataset with the EPHPM household survey data to determine which children would have been exposed to either the treatment or control.

2. Matching Children to Treatment and Control Groups

In our evaluation, we use the EPHPM survey questions about current municipality and municipality of birth to identify the children who were age-eligible and were born in treatment or control municipalities before the treatment began. We focus on children who were born before the year 2000 to avoid the issue of endogeneity; we do not believe that

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[18] Translation: Household Survey of Multiple Purposes.

there was significant contamination of either the control or treatment group, since municipalities are large and moving would be burdensome for poor families. However, since other evaluations have shown that similar treatment can increase birth rates in treatment municipalities, we avoid that potential issue by excluding children born after the treatment began in the year 2000. This means we exclude some children who would have become eligible for the health transfer post-2000 (birth years 2001 and 2002).

To match children to either control or treatment municipalities, we utilize their reported municipality of birth as a proxy for their municipality at the point of randomization. This variable is collected the same way in nearly every survey year,[19] giving us comparable data across survey years. We acknowledge that some children may have moved between birth and the year 2000, creating some measurement error in this variable by including children who may have moved out of treatment or control municipalities and/or excluding children who moved into treatment or control municipalities before 2000. However, the error is balanced across the treatment and control groups. The measurement error will bias our regression coefficients towards zero, giving us a minimum for the range of impact of the treatment.

After reducing our dataset down to age-eligible children, we use municipality of birth to match these children to either control or treatment municipalities using data from the earlier follow-up evaluation. Each municipality is assigned a unique identifying code, allowing us to merge the two datasets and assign children to a control or treatment municipality. We use Galiani and McEwan's (2013) definition of control and treatment municipalities, assigning the 10 municipalities that received only supply-side support to

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[19] Except survey years 2001, May 2002 and March 2003 for children under 5.

the control group due to the minimal disbursement of these funds and the lack of a transfer to households.

Using all of the survey years for which this data is available, there are 23,026 observations who were age-eligible for the education or health transfer in the treatment group and 12,676 observations in the control group.[20] Breakdowns of observations by survey year and by treatment and control group can be found in Appendix Table A.

Because of data constraints, this matching process means we exclude households who would have been eligible, including those with pregnant women who would have been eligible for the health transfer. It also means that we include other households who would not have been eligible, including those with age-eligible children who didn't meet the grade cut-offs (1st to 4th), or those whose children were not enrolled at the time.

We estimate an intent-to-treat effect based on the assumption that all eligible children would have received the transfer, ignoring the possibility of administrative difficulties or households becoming ineligible by refusing to adhere to the conditions. This is a reasonable assumption based on Glewwe and Olinto's (2004) finding that there is a high take-up rate and that the conditions were weakly enforced.

3. Variable Descriptions & Summary Statistics

To identify treatment effects, we use a number of dependent variables relating to educational outcomes and labor supply, as well as many independent variables to control for child-specific variables. Below are the summary statistics for each of these variables. Full descriptions for all variables used can be found in Appendix Table C.

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[20] There were 40 municipalities in the treatment, and only 30 in the control group, which helps account for the discrepancy in numbers of observations.

Summary Statistics

VARIABLE NAME	RANGE		NUMBER OF OBSERV-ATIONS	MEAN				
	Min	Max		All Observ-ations	Treatment & Education Transfer Eligible	Treatment & Health Transfer Eligible	Control & Education Transfer Eligible	Control & Health Transfer Eligible
Dependent Variables								
Highest Grade Completed	0	25	1,137,372	4.526	4.059	1.196	3.823	1.043
Attendance	0	1	1,139,570	0.3261	0.5652	0.7318	0.5438	0.6902
Literacy	0	1	1,139,608	0.7331	0.833	0.4751	0.8012	0.4324
Worked Last Week	0	1	1,045,307	0.3991	0.2446	0.039	0.235	0.0352
Hours Worked Last Week	0	168	1,043,271	18.22	11.032	1.066	10.56	0.962
Independent Variables								
Treatment	0	1	125,204	0.6311	1	1	0	0
Age	0	116	1,150,357	25.355	13.33	7.463	13.259	7.392
Female	0	1	1,150,604	0.5171	0.4967	0.5017	0.4881	0.4893
Survey Year & Month	20015	20125	1,150,604	20063.51	20061.31	20070.82	20060.17	20070.26
Birth Year	1888	2012	1,150,357	1980.38	1992.184	1998.971	1992.145	1998.99
Block	1	5	125,204	3.172	3.147	3.179	2.904	2.848

4. Empirical Strategy
   4.1 Main Regression Specifications
   Because of the randomized implementation in treatment and control

municipalities in the year 2000, we are able to use a fairly straightforward empirical

strategy to isolate any effects of the treatment. Our most basic model uses the *Treatment*

dummy variable (0 if in control group, 1 if in treatment) for education and health eligible

children to estimate the impact of the treatment on our various Y variables. We estimate

using ordinary least squares and with standard errors clustered by municipality of birth:

(1) $Y_{itc} = \beta_0 + \beta_1 Treatment_i + \varepsilon$

   From there, we add fixed effects to account for changes over time since the

treatment (survey year, *t*), for the different age cohorts within the treatment (birth year,

c), and for the level of poverty in the municipality, measured using mean height-for-age

quintiles (block, *b*). We also include a dummy variable for gender. This is the main

regression equation.

(2) $Y_{itc} = \beta_0 + \beta_1 Treatment_i + \beta_2 female + \gamma_t + \gamma_c + \gamma_b + \varepsilon$

   4.2 Heterogeneity Regression Specifications
   We then examine the result for heterogeneity by level of poverty, age of the child,

the passage of time since the treatment, and gender. Galiani and McEwan (2013) showed

significantly disparate educational outcomes for the lowest two quintiles, Blocks 1 and 2,

when compare to the upper 3 quintiles, Blocks 3-5. I follow the lead of Galiani and

McEwan (2013) by pooling Blocks 1 and 2 together and Blocks 3-5 together.

   First, we interact *Treatment* and the two Block categories. Block is a categorical

variable that captures relative levels of poverty, using mean height-for-age z-score

quintiles as a proxy. Children in Block 1 are the poorest using this measure, and children

in Block 5 are the least poor. We compare the treatment impacts for Blocks 1 and 2 versus Blocks 3-5, following the precedent set by Galiani & McEwan (2013):

(3) $Y_{itc} = \beta_0 + \beta_1 Treatment * Block1 - 2_i + \beta_2 Treatment * Block3 - 5_i + \beta_3 female + \gamma_t + \gamma_c + \gamma_b + \varepsilon$

Our next interaction is with child-specific variables such as age, broken down within the education transfer into birth years cohorts, and gender:

One specification breaks down the education cohort into an older group, 1988-1992, and a younger group, 1993-1996, and breaks down the health cohort into three groups based on birth year: 1998, 1999 and 2000, so the regression for the health cohort has a third interaction category.

(4) $Y_{itc} = \beta_0 + \beta_1 Treatment * OlderCohort_i + \beta_2 Treatment * YoungerCohort_i + \beta_3 female + \gamma_t + \gamma_c + \gamma_b + \varepsilon$

Another specification acknowledges that children born in certain years would have received more intense treatment than other children. Based on the children's ages, we can determine how many transfers children were eligible for, up to three. One would expect that children who received more cash transfers would see larger and more persistent effects. We assign children born in the years 1990-1994 in the education cohort and 2000 in the health cohort to the High Exposure group. We assign children born in 1988-1989 and 1995-1996 in the education cohort and 1998-1999 in the health cohort to the Low Exposure group.

(5) $Y_{itc} = \beta_0 + \beta_1 Treatment * HighExposure_i + \beta_2 Treatment * LowExposure_i + \beta_3 female + \gamma_t + \gamma_c + \gamma_b + \varepsilon$

We then examine the differential impact of the treatment by gender. As mentioned in the literature review, much of the previous literature has shown that CCTs have the largest marginal effect on girls' educational and labor supply outcomes. For example, Galiani and McEwan (2013) found different impacts of *PRAF* on child labor by gender.

(6) $Y_{itc} = \beta_0 + \beta_1 Treatment * Male + \beta_2 Treatment * Female + \gamma_t + \gamma_c + \gamma_b + \varepsilon$

Finally, we allow for variation in treatment effects over time. At first we used dummy variables for *Treatment* interactions with each survey year. Due to the wide variation in sample sizes for the various survey years, however, using all 17 individual surveys creates enormous noise that is hard to interpret and which makes it difficult to see a pattern in the results. As such, we pool the surveys into four 3-year bins (2001-3, 2004-6, 2007-9, 2010-12) to smooth out some of this noise and create more equivalent survey year bins.

(7) $Y_{itc} = \beta_0 + \beta_{1-4} Treatment * SurveyYearBins_i + \beta_5 Female + \gamma_t + \gamma_c + \gamma_b + \varepsilon$

For each of these heterogeneity regressions, we run each regression for the full sample, for Blocks 1-2, and for Blocks 3-5, following the precedent set by Galiani and McEwan (2013). Since we find that the most consistent heterogeneity is by poverty level, breaking the other heterogeneity equations into block groups shows that Blocks 1-2 have consistently higher magnitude outcomes within each of these heterogeneity groups.

5. Robustness Checks
      5.1 Created Variable: Municipality in 2000
      We use a created variable, *Municipality in 2000*, which gives us a potentially

more accurate reading of children's locations in the year 2000. The surveys ask questions

in each year about where respondents previously lived between their current municipality

and their municipality of birth. In some survey years, respondents are asked where they

lived 5 years before the survey year[21], allowing us to determine where they were in the

years 1998, 1999, 2000, 2001, and 2002, respectively. In other survey years, respondents

are asked how long they have lived in their current municipality, allowing us to

extrapolate for those who had moved whether they were still living in their current

municipality in the year 2000. We only utilize the survey years in which we can

definitively determine where the respondent was in the year 2000, which includes all

survey years May 2007 and later.[22]

      Like the *Municipality of Birth* variable, this *Municipality in 2000* variable may

also contain some measurement error, but is likely more precise than *Municipality of*

Birth for those children for whom we can find a value. Because we were only able to

precisely determine *Municipality in 2000* in survey years after 2006, there are fewer

survey years with which to work, and therefore fewer observations. However, for those

children for whom we were able to find a value, this treatment designation is likely much

more precise than *Municipality of Birth*. This is especially true for the children of the

education cohort, who were born as early as 1988, since they are more likely to have

moved in the 12 years before randomization. On the other hand, for the health cohort,

there were only 2 years between the births of the oldest children and the year 2000,

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[21] Survey years 2003, 2004, 2005, 2006 and 2007
[22] Excluding September 2007.

leaving only a couple of years in which they may have moved. For those born in 2000,

Municipality of Birth is the same as *Municipality in 2000*. We use this variable to assign

children to either treatment or control municipalities as a robustness check.

V. RESULTS

A. Main Results

This results section is organized by outcome variable, showing and explaining the results for both the education and health cohorts for each of the education and labor supply outcome variables. The first section is for the main results and the following section investigates the heterogeneity of these effects by level of poverty, age cohort, level of exposure to the treatment, passage of time since the treatment, and gender.

1. Highest Grade Completed
  1.1 Education Cohort

Table 1.1
Main Effects on Highest Grade Completed (Education Cohort)

VARIABLES	(1) Highest grade	(2) Highest grade	(3) Highest grade	(4) Highest grade	**(5) Highest grade**
Treatment	0.236	0.250	0.192	0.205	**0.202**
	(0.164)	(0.151)	(0.153)	(0.147)	**(0.147)**
Block Fixed Effects		YES	YES	YES	**YES**
Survey Year Fixed Effects			YES	YES	**YES**
Birth Year Fixed Effects				YES	**YES**
Female					**0.316*** **
					(0.0477)
Constant	3.823***	3.584***	1.596***	2.845***	**2.697*** **
	(0.127)	(0.240)	(0.177)	(0.178)	**(0.180)**
Observations	27,428	27,428	27,428	27,428	**27,428**
Adj R-squared	0.002	0.006	0.262	0.396	**0.399**

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Specification (5) is our preferred main specification.

Our main regressions show an increase of approximately 0.2 years of schooling (*highest grade*) for the treatment group versus the control group. Our preferred specification (5) shows an increase of 0.202 years of schooling for the treatment group.

However, the results are insignificant, likely due to measurement error in the designation of the treatment and control groups. This means that the main coefficient of 0.2 years of schooling is likely a lower bound on the actual effect.

1.2 Health Cohort

Table 1.2
Main Effects on Highest Grade Completed (Health Cohort)

VARIABLES	(1) Highest grade	(2) Highest grade	(3) Highest grade	(4) Highest grade	**(5) Highest grade**
Treatment	0.152**	0.162***	0.121**	0.112**	**0.111****
	(0.0621)	(0.0583)	(0.0486)	(0.0480)	**(0.0477)**
Block Fixed Effects		YES	YES	YES	**YES**
Survey Year Fixed Effects			YES	YES	**YES**
Birth Year Fixed Effects				YES	**YES**
Female					**0.106****
					(0.0183)
Constant	1.043***	1.046***	-0.0740	0.412***	**0.362****
	(0.0386)	(0.0745)	(0.0558)	(0.0608)	**(0.0622)**
Observations	8,219	8,219	8,219	8,219	**8,219**
Adj R-squared	0.002	0.002	0.663	0.719	**0.720**

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Specification (5) is our preferred main specification.

For the health cohort, the magnitude of the coefficient is somewhat lower. This could be a reflection of the lower amount given for the health transfer (up to 3 transfers of 828 Lempiras for the education cohort versus up to 2 transfers of 644 Lempiras for the health cohort) or of the conditions being focused on health rather than education. Another explanation might be that this younger cohort is mostly still in school, so the full long-term results may not yet be visible.

The main regressions for highest grade completed show that the health cohort saw an impact of .11 years of schooling, approximately half the size of the impact for the

education cohort. However, the results for the health cohort are significant because the standard errors are much lower than for the education cohort. This is likely a reflection of lower attenuation bias, which will be further discussed later.

These results fit well into the larger literature. Impacts found in other evaluations range from 0.2 years in *PROGRESA/Oportunidades* (Behrman et al. 2010) for the younger cohort and in Nicaragua's *Atención a Crisis* (Del Carpio & Macours 2009), up to 0.66 years in *PROGRESA/Oportunidades* for the oldest cohort (Schultz 2004).

As with all of the results reported in this paper, these effects of 0.11 years of schooling for the health cohort and 0.2 years for the education cohort are likely lower bounds on the actual effect, and especially for the younger health cohort since these coefficients may not yet reflect the full impact of the treatment given that the treatment impact on *highest grade* increases over time (this will be discussed later in the heterogeneity section). The health cohort was still largely in school in 2012 (66 percent) versus a minority of the education cohort (21 percent), so the health cohort's full impact may not yet have been realized.

2. Attendance
  2.1 Education Cohort

Table 2.1
Main Effects on Attendance (Education Cohort)

VARIABLES	(1) attend	(2) attend	(3) attend	(4) attend	**(5)** **attend**
Treatment	0.0214	0.0230	0.0305*	0.0270	**0.0269**
	(0.0198)	(0.0192)	(0.0181)	(0.0183)	**(0.0184)**
Block Fixed Effects		YES	YES	YES	**YES**
Survey Year Fixed Effects			YES	YES	**YES**
Birth Year Fixed Effects				YES	**YES**
Female					**0.0119***
					(0.00713)
Constant	0.544***	0.551***	0.777***	0.472***	**0.466***
	(0.0158)	(0.0271)	(0.0222)	(0.0243)	**(0.0244)**
Observations	27,461	27,461	27,461	27,461	**27,461**
Adj R-squared	0.000	0.001	0.106	0.226	**0.226**

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Specification (5) is our preferred main specification.

The main regression also shows that the education cohort treatment group was 2.7 percentage points more likely to be attending school than the control group. The magnitude of the impact on *attendance* is low relative to the literature, but the heterogeneity results show that these outcomes are much higher for certain groups of students and for certain periods of time after the treatment. These results are also insignificant due to the high standard error, which we discuss later.

2.2 Health Cohort

Table 2.2

Main Effects on Attendance (Health Cohort)

VARIABLES	(1) attend	(2) attend	(3) attend	(4) attend	**(5)** **attend**
Treatment	0.0415***	0.0464***	0.0481***	0.0475***	**0.0472***
	(0.0153)	(0.0143)	(0.0143)	(0.0141)	**(0.0141)**
Block Fixed Effects		YES	YES	YES	**YES**
Survey Year Fixed Effects			YES	YES	**YES**
Birth Year Fixed Effects				YES	**YES**
Female					**0.0160***
					(0.00909)
Constant	0.690***	0.689***	-0.000420	0.0354**	**0.0278***
	(0.0126)	(0.0153)	(0.0134)	(0.0145)	**(0.0148)**
Observations	8,236	8,236	8,236	8,236	**8,236**
Adj R-squared	0.002	0.003	0.450	0.455	**0.456**

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Specification (5) is our preferred main specification.

The effect on *attendance* was especially high at 4.7 percentage points and significant at the 1% level. This is likely so much higher than the education cohort because most of the health cohort was still in school through the last of the survey years, since they were 12-14 years old in 2012, the last survey year (versus up to 24 years old in the education cohort). Thus, the impact on *attendance* would likely be higher throughout all of the survey years.

3. Literacy
    3.1 Education Cohort

Table 3.1
Main Effects on Literacy (Education Cohort)

VARIABLES	(1) literate	(2) literate	(3) literate	(4) literate	**(5) literate**
Treatment	0.0315	0.0342*	0.0311	0.0316*	**0.0314**
	(0.0212)	(0.0194)	(0.0195)	(0.0189)	**(0.0189)**
Block Fixed Effects		YES	YES	YES	**YES**
Survey Year Fixed Effects			YES	YES	**YES**
Birth Year Fixed Effects				YES	**YES**
Female					**0.0297***
					(0.00510)
Constant	0.801***	0.767***	0.536***	0.583***	**0.569***
	(0.0181)	(0.0337)	(0.0297)	(0.0282)	**(0.0281)**
Observations	27,462	27,462	27,462	27,462	**27,462**
Adj R-squared	0.002	0.006	0.087	0.138	**0.139**

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Specification (5) is our preferred main specification.

The main regression specification (5) shows that the children in the treatment group were 3.1 percentage points more likely to be literate than their control group counterparts. The result is only significant in a few of the specifications, likely due to the high standard errors associated with the education cohort regressions

3.2 Health Cohort

Table 3.2
Main Effects on Literacy (Health Cohort)

VARIABLES	(1) literate	(2) literate	(3) literate	(4) literate	**(5) literate**
Treatment	0.0427*	0.0458**	0.0370*	0.0344*	**0.0341**
	(0.0225)	(0.0199)	(0.0206)	(0.0205)	**(0.0205)**
Block Fixed Effects		YES	YES	YES	**YES**
Survey Year Fixed Effects			YES	YES	**YES**
Birth Year Fixed Effects				YES	**YES**
Female					**0.0220****
					(0.00927)
Constant	0.432***	0.415***	-0.0344	0.106***	**0.0959*****
	(0.0173)	(0.0300)	(0.0237)	(0.0258)	**(0.0263)**
Observations	8,236	8,236	8,236	8,236	**8,236**
Adj R-squared	0.002	0.005	0.418	0.476	**0.476**

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Specification (5) is our preferred main specification.

The regression results for *literacy* in the health cohort show an impact of 3.4
percentage points in the treatment group, very similar in magnitude to the impact on the
education cohort. The *literacy* results are one of few specifications in which the standard
errors are higher for the health cohort than for the education cohort. This may be related
to the fact that literacy levels are nearly zero for this cohort until the later survey years
(starting in 2005).

4. Labor Supply: Probability of Working & Hours Worked
4.1 Education Cohort

Table 4.1a

Main Effects on Probability of Working (Education Cohort)

VARIABLES	(1) worked	(2) worked	(3) worked	(4) worked	(5) worked
Treatment	0.00978	0.00815	0.00321	0.00559	**0.00787**
	(0.00764)	(0.00721)	(0.00721)	(0.00731)	**(0.00701)**
Block Fixed Effects		YES	YES	YES	**YES**
Survey Year Fixed Effects			YES	YES	**YES**
Birth Year Fixed Effects				YES	**YES**
Female					**-0.267***
					(0.00899)
Constant	0.235***	0.230***	0.0800***	0.321***	**0.446***
	(0.00532)	(0.00834)	(0.0121)	(0.0148)	**(0.0131)**
Observations	27,461	27,461	27,461	27,461	**27,461**
Adj R-squared	0.000	0.000	0.092	0.191	**0.289**

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Specification (5) is our preferred main specification.

Table 4.1b

Main Effects on Hours Worked (Education Cohort)

VARIABLES	(1) hours	(2) hours	(3) hours	(4) hours	(5) hours
Treatment	0.471	0.391	0.109	0.216	**0.330**
	(0.356)	(0.336)	(0.326)	(0.346)	**(0.333)**
Block Fixed Effects		YES	YES	YES	**YES**
Survey Year Fixed Effects			YES	YES	**YES**
Birth Year Fixed Effects				YES	**YES**
Female					**-13.00***
					(0.406)
Constant	10.56***	10.45***	2.452***	13.32***	**19.40***
	(0.244)	(0.403)	(0.441)	(0.715)	**(0.697)**
Observations	27,421	27,421	27,421	27,421	**27,421**
Adj R-squared	0.000	0.000	0.088	0.181	**0.285**

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Specification (5) is our preferred main specification.

The labor supply results are not significant or particularly large, with the treatment group being 0.78 percentage points more likely to have worked in the week before the survey. The treatment group also worked 0.33 hours more than the control group in the week before the survey.

As mentioned before, these magnitudes should be interpreted as lower bounds on the true effect, since attenuation bias has likely biased our coefficients towards zero.

The small impact seems to make sense based on the findings of McEwan & Galiani (2013), since treated children were less likely to work because they were more likely to enroll in and regularly attend school. This ambiguous impact found in the main results begins to make more sense as a narrative when we allow for changes in the effect over time and by age group, as we do in the heterogeneity section.

4.2 Health Cohort

Table 4.2a

Main Effects on Probability of Working (Health Cohort)

VARIABLES	(1) worked	(2) worked	(3) worked	(4) worked	**(5) worked**
Treatment	0.00379	0.00348	0.000524	0.000306	**0.00154**
	(0.00563)	(0.00516)	(0.00493)	(0.00494)	**(0.00482)**
Block Fixed Effects		YES	YES	YES	**YES**
Survey Year Fixed Effects			YES	YES	**YES**
Birth Year Fixed Effects				YES	**YES**
Female					**-0.0573***
					(0.00470)
Constant	0.0352***	0.0441***	0.0114*	0.0114*	**0.0406***
	(0.00441)	(0.00574)	(0.00638)	(0.00639)	**(0.00703)**
Observations	7,041	7,041	7,041	7,041	**7,041**
Adj R-squared	-0.000	0.001	0.120	0.124	**0.146**

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Specification (5) is our preferred main specification.

Table 4.2b

Main Effects on Hours Worked (Health Cohort)

VARIABLES	(1) hours	(2) hours	(3) hours	(4) hours	**(5) hours**
Treatment	0.104	0.0704	-0.00213	-0.0101	**0.0254**
	(0.187)	(0.170)	(0.161)	(0.163)	**(0.161)**
Block Fixed Effects		YES	YES	YES	**YES**
Survey Year Fixed Effects			YES	YES	**YES**
Birth Year Fixed Effects				YES	**YES**
Female					**-1.647***
					(0.186)
Constant	0.962***	1.204***	0.375	0.375	**1.214***
	(0.141)	(0.192)	(0.241)	(0.244)	**(0.251)**
Observations	7,041	7,041	7,041	7,041	**7,041**
Adj R-squared	-0.000	0.000	0.093	0.098	**0.115**

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$
Specification (5) is our preferred main specification.

Since they are a much younger cohort than the education group and therefore less likely to be labor force participants, the effect on labor supply is even smaller and more insignificant for the health cohort than for the older education cohort, who are much more likely to be labor force participants. Treatment recipients were 0.15 percentage points more likely to have worked in the week before the survey and they worked 0.025 more hours. Once again, the allowance of change in the impact over time and between age groups in the heterogeneity sections provides a clearer narrative of this seemingly ambiguous effect.

B. Heterogeneity of Results

This heterogeneity section breaks down the main preferred specification (5) into treatment effects by block group/level of poverty, survey year bins/passage of time, age group, level of exposure to the treatment, and gender. For variable descriptions, see Appendix Table C.

1. Highest Grade Completed
  1.1 Education Cohort

Table 5.1a

Highest Grade Heterogeneity by Block and Passage of Time (Education Cohort)

VARIABLES	Preferred Main Specification Highest Grade	(1) Highest Grade	(2) Highest Grade	(3) Highest Grade	(4) Highest Grade
Treatment	0.202				
	(0.147)				
Treatment*Blocks 1-2		0.566**			
		(0.247)			
Treatment*Blocks 3-5		-0.0318			
		(0.150)			
Treatment*Survey Years 2001-3			0.0412	0.155	-0.0374
			(0.102)	(0.171)	(0.119)
Treatment*Survey Years 2004-6			0.127	0.538*	-0.104
			(0.156)	(0.290)	(0.143)
Treatment*Survey Years 2007-9			0.488**	1.053***	0.141
			(0.221)	(0.366)	(0.240)
Treatment*Survey Years 2010-12			0.0338	0.826*	-0.461
			(0.317)	(0.471)	(0.388)
P-Value for Joint Hypothesis Test		0.0432**	0.0484**	0.0114**	0.3082
Sample Included	Full	Full	Full	Blocks 1-2	Blocks 3-5
Observations	27,428	27,428	27,428	9,978	17,450
Adj R-squared	0.399	0.399	0.399	0.409	0.398

Robust standard errors in parentheses, clustered by Municipality of Birth
*** $p<0.01$, ** $p<0.05$, * $p<0.1$
All specifications include fixed effects for block, survey year and birth year, as well as a dummy variable for female.

Further specifications including interaction terms show that this result is primarily driven by poorer students (those in the two lowest mean height-for-age quintiles). As shown in specifications (1), the treatment effect in Blocks 1 and 2 is 0.57 years, reaching beyond even the highest magnitude results in other long-term CCT follow-ups. The treatment effect in Blocks 3-5, on the other hand, is negative and insignificant, showing that the overall result of 0.2 years of schooling is driven entirely by the lower blocks.

As seen in specification (2) the effect on *highest grade* attained actually increased over time, but at a decreasing rate. The margin of difference between the treatment and control groups got larger as time passed since the treatment, but the gap increased most rapidly in the earlier years closest to the time of treatment.

In specifications (3) and (4) we break down this treatment effect over time by block groups and find that the treatment effect grew most rapidly for Blocks 1 and 2 to over 1 year of schooling in survey years 2007-9. This coefficient is likely significant because of the larger sample size of the surveys in this survey year grouping.

Table 5.1b
Highest Grade Heterogeneity by Age, Level of Exposure, and Gender (Education Cohort)

VARIABLES	Preferred Main Specification Highest grade	(1) highest grade All Blocks	(2) highest grade Blocks 1&2	(3) highest grade Blocks 3-5	(4) highest grade All Blocks	(5) highest grade Blocks 1&2	(6) highest grade Blocks 3-5	(7) highest grade All Blocks	(8) highest grade Blocks 1&2	(9) highest grade Blocks 3-5
Treatment	0.202									
	(0.147)									
Age Group										
Treatment* Older Cohort		0.210	0.661*	-0.0731						
		(0.188)	(0.327)	(0.198)						
Treatment* Younger Cohort		0.193*	0.493***	0.0182						
		(0.110)	(0.174)	(0.110)						
Level of Exposure										
Treatment* High Exposure					0.182	0.586**	-0.0817			
					(0.152)	(0.262)	(0.150)			
Treatment* Low Exposure					0.229	0.571**	0.0341			
					(0.147)	(0.242)	(0.161)			

Gender										
Treatment*Male								0.172	0.604**	-0.0826
								(0.161)	(0.260)	(0.171)
Treatment*Female								0.234	0.553**	0.0201
								(0,143)	(0.251)	(0.144)
P-Value for Joint Significance Test		0.8768			0.4638			0.4671		
Sample Included	Full	Full	Blocks 1-2	Blocks 3-5	Full	Blocks 1-2	Blocks 3-5	Full	Blocks 1-2	Blocks 3-5
Observations	27,428	27,428	9,978	17,450	27,428	9,978	17,450	27,428	9,978	17,450
Adj R-squared	0.399	0.399	0.409	0.398	0.399	0.409	0.398	0.399	0.409	0.398

Robust standard errors in parentheses, clustered by Municipality of Birth
*** p<0.01, ** p<0.05, * p<0.1
Treatment*Older Cohort includes birth years 1988-1992. Treatment*Younger includes birth years 1993-1996.
Treatment*High Exposure includes those who were eligible for each all three years of the treatment throughout 2000-2 (birth years 1990-4). Treatment*Low Exposure includes those who were eligible for less than the full three years (birth years 1988-9, 1995-6).
All specifications include fixed effects for block, survey year and birth year, as well as a dummy variable for female.

The magnitude of the treatment effect on *highest grade* is highest for the older half of the education group (birth years 1988-1992) at 0.21 years of schooling, versus 0.19 years of schooling for the younger half. However, a joint significance test could not determine that these two magnitudes were not equal at the 5% level.

This difference in magnitude became starker and more significant when broken down by Block. Children in the older half of the education cohort in Blocks 1-2 saw a treatment effect of 0.66 years of schooling versus the younger half in Blocks 1-2 who saw a treatment effect of 0.49. Both of these treatment effects are much higher than for their age-equivalent cohort in Blocks 3-5, whose treatment effects were nearly 0 and insignificant.

This difference in treatment effect between the older and younger halves of the group is noticeably larger than the gap between those who were fully exposed to all 3 years of the treatment versus those who were partly exposed. In specification (4), which includes all blocks, the difference between the two is insignificant and the magnitude of the treatment effect for those partially exposed is actually higher than the effect for those fully exposed. Even, when broken down by block, the margin of difference remains

small. In Blocks 1-2, the fully exposed cohort saw a treatment effect of 0.59 years of schooling and the partially exposed cohort saw a treatment effect of 0.57 years.

In our basic heterogeneity by gender specification (7), the treatment effect for boys and girls is not significantly different, but the magnitudes suggest that girls saw a higher treatment effect. However, when broken down by block, it becomes clear that boys in Blocks 1-2 benefitted more from the treatment than girls in Blocks 1-2. The treatment effect for boys was 0.6 years of schooling versus 0.55 for girls.

1.2 Health Cohort

Table 5.2a
Highest Grade Heterogeneity by Block and Passage of Time (Health Cohort)

VARIABLES	Preferred Main Specification Highest grade	(1) Highest grade	(2) Highest grade	(3) Highest grade	(4) Highest grade
Treatment	0.111**				
	(0.0477)				
Treatment*Blocks 1-2		0.196***			
		(0.0577)			
Treatment*Blocks 3-5		0.0519			
		(0.0656)			
Treatment*Survey Years 2001-3			0.00372	-0.00568	-0.00359
			(0.0268)	(0.0307)	(0.0360)
Treatment*Survey Years 2004-6			0.0416	0.0558	0.0234
			(0.0308)	(0.0391)	(0.0426)
Treatment*Survey Years 2007-9			0.207***	0.328***	0.119
			(0.0765)	(0.0992)	(0.104)
Treatment*Survey Years 2010-12			0.132	0.400*	-0.0343
			(0.166)	(0.211)	(0.222)
P-Value for Joint Hypothesis Test		0.1041	0.0644*	0.0107**	0.5167
Sample Included	Full	Full	Full	Blocks 1-2	Blocks 3-5
Observations	8,219	8,219	8,219	3,021	5,198
Adj R-squared	0.720	0.720	0.720	0.741	0.711

Robust standard errors in parentheses, clustered by Municipality of Birth
*** p<0.01, ** p<0.05, * p<0.1
All specifications include fixed effects for block, survey year and birth year, as well as a dummy variable for female.

For the health cohort, the treatment increased grade attainment by 0.11 years on average. This result was significant at the 1% level. The attainment result was larger in poorer municipalities, much like the results for the education cohort. Treated children in Blocks 1 and 2 had a significant (at the 1% level) impact of 0.196 years of schooling while children in Blocks 3-5 saw an increase of just 0.052 years of schooling, which was insignificant. This theme of higher educational impacts for poorer students will continue throughout the educational outcome variables in this section.

The attainment impact increased over time, as seen in specification (2). When broken down by Blocks 1-2 in specification (3) and 3-5 in specification (4), the largest increases in magnitude are limited to Blocks 1-2, and the treatment impact is as large as 0.33 years of schooling in survey years 2007-9 and significant at the 1% level.

Table 5.2b
Highest Grade Heterogeneity by Age, Level of Exposure, and Gender (Health Cohort)

VARIABLES	Preferred Main Specification highest grade	(1) highest grade	(2) highest grade	(3) highest grade	(4) highest grade	(5) highest grade	(6) highest grade	(7) highest grade	(8) highest grade	(9) highest grade
	All Blocks	All Blocks	Blocks 1&2	Blocks 3-5	All Blocks	Blocks 1&2	Blocks 3-5	All Blocks	Blocks 1&2	Blocks 3-5
Treatment	0.111**									
	(0.0477)									
Age Group										
Treatment*Birth Year 1998		0.176**	0.281***	0.0972						
		(0.0678)	(0.0852)	(0.0927)						
Treatment*Birth Year 1999		0.112**	0.232***	0.0362						
		(0.0545)	(0.0748)	(0.0707)						
Treatment*Birth Year 2000		0.0390	0.0486	0.0160						
		(0.0446)	(0.0470)	(0.0676)						
Level of Exposure										
Treatment*High Exposure					0.0390	0.0486	0.0160			
					(0.0446)	(0.0470)	(0.0676)			
Treatment*Low Exposure					0.145**	0.257***	0.0674			
					(0.0553)	(0.0728)	(0.0722)			
Gender										
Treatment*Male								0.0811	0.188**	0.00507
								(0.0533)	(0.0729)	(0.0681)
Treatment* Female								0.141***	0.190***	0.100
								(0.0502)	(0.0557)	(0.0746)

			Blocks 1-2	Blocks 3-5		Blocks 1-2	Blocks 3-5		Blocks 1-2	Blocks 3-5
P-Value for Joint Significance Test		.0613*			0.0200**			0.1359		
Sample Included	Full	Full	Blocks 1-2	Blocks 3-5	Full	Blocks 1-2	Blocks 3-5	Full	Blocks 1-2	Blocks 3-5
Observations	8,219	8,219	3,021	5,198	8,219	3,021	5,198	8,219	3,021	5,198
Adj R-squared	0.720	0.720	0.740	0.711	0.720	0.740	0.711	0.720	0.739	0.711

Robust standard errors in parentheses, clustered by Municipality of Birth
*** p<0.01, ** p<0.05, * p<0.1
Treatment*High Exposure includes those who were eligible for each all three years of the treatment throughout 2000-2 (birth year 2000). Treatment*Low Exposure includes those who were eligible for less than the full three years (birth years 1998-9).
All specifications include fixed effects for block, survey year and birth year, as well as a dummy variable for female.

This attainment result varied significantly by birth year, with the oldest group (birth year 1998) experiencing a treatment effect of 0.176 additional years of schooling, the middle group (birth year 1999) with a treatment effect of 0.112 years, and the youngest group (birth year 2000) with a treatment effect of just 0.039 years. A test for joint significance found that these treatment effects are significantly different from one another at the 10% level. This age group difference is stronger within Blocks 1 and 2, reaching as high as 0.28 years of schooling for the oldest group (birth year 1998).

For the health cohort, breaking apart the high exposure (birth year 2000) group, who were plausibly eligible for all the transfers, from the low exposure (birth years 1998-9) group, who were only eligible for some of the transfers, does not seem to make a significant difference in the size of the treatment effect. In fact, the magnitude is higher for those in the lowest exposure group.

For the health cohort, the treatment impact on grade attainment is largest for females. Females saw a treatment effect of 0.14 years of schooling while males saw an impact of 0.08 years of schooling. This difference narrows within Blocks 1 and 2, where females and males saw very similar treatment effects.

2. Attendance
2.1 Education Cohort

Table 6.1a

Attendance Heterogeneity by Block and Passage of Time (Education Cohort)

VARIABLES	Preferred Main Specification	(1)	(2)	(3)	(4)
	attend	attend	attend	attend	attend
Treatment	0.0269				
	(0.0184)				
Treatment*Blocks 1-2		0.0679**			
		(0.0283)			
Treatment*Blocks 3-5		0.000476			
		(0.0212)			
Treatment*Survey Years 2001-3			0.0374	0.0955**	-0.00276
			(0.0257)	(0.0392)	(0.0278)
Treatment*Survey Years 2004-6			0.0196	0.0375	0.00802
			(0.0240)	(0.0397)	(0.0307)
Treatment*Survey Years 2007-9			0.0260	0.0629*	0.00362
			(0.0231)	(0.0344)	(0.0311)
Treatment*Survey Years 2010-12			0.00646	0.0568	-0.0242
			(0.0269)	(0.0427)	(0.0341)
P-Value for Joint Significance Test		0.0610*	0.7840	0.2789	0.7181
Sample Included	Full	Full	Full	Blocks 1-2	Blocks 3-5
Observations	27,461	27,461	27,461	9,985	17,476
Adj R-squared	0.226	0.227	0.226	0.237	0.223

Robust standard errors in parentheses, clustered by Municipality of Birth
*** p<0.01, ** p<0.05, * p<0.1
All specifications include fixed effects for block, survey year and birth year, as well as a dummy variable for female.

The *attendance* effect is also largest in the poorer municipalities, much like with overall grade attainment. In Blocks 1 and 2, specification (1) shows that treated students were 6.8 percentage points more likely to be attending school than their control group counterparts in Blocks 1 and 2. The treatment effect for Blocks 3-5 was practically zero and insignificant. This treatment effect for Blocks 1-2 decreased over time after treatment, starting at 9.55 percentage points in survey years 2001-3, decreasing to 5.7

percentage points in survey years 2010-12. This makes intuitive sense since *attendance* steadily decreased in this cohort over time, which would likely reduce the treatment impact on *attendance*. The pattern for Blocks 3-5 over time is less clear-cut, likely because the effect was so much smaller and less significant.

Table 6.1b
Attendance Heterogeneity by Age, Level of Exposure, and Gender (Education Cohort)

VARIABLES	Preferred Main Specification attend	(1) attend	(2) attend	(3) attend	(4) attend	(5) attend	(6) attend	(7) attend	(8) attend	(9) attend
Treatment	0.0269									
	(0.0184)									
Age Groups										
Treatment*Older Cohort		0.0161	0.0608*	-0.0116						
		(0.0216)	(0.0313)	(0.0267)						
Treatment*Younger Cohort		0.0392**	0.0756**	0.0151						
		(0.0187)	(0.0311)	(0.0217)						
Level of Exposure										
Treatment*High Exposure					0.0180	0.0623*	-0.00932			
					(0.0205)	(0.0326)	(0.0239)			
Treatment*Low Exposure					0.0384**	0.0753**	0.0135			
					(0.0184)	(0.0278)	(0.0221)			
Gender										
Treatment*Male								0.0220	0.0676**	-0.00677
								(0.0203)	(0.0313)	(0.0239)
Treatment*Female								0.0319*	0.0684**	0.00803
								(0.0190)	(0.0288)	(0.0228)
P-Value for Joint Significance Test		0.1893			0.1481			0.4848		
Sample Included	Full	Full	Blocks 1-2	Blocks 3-5	Full	Blocks 1-2	Blocks 3-5	Full	Blocks 1-2	Blocks 3-5
Observations	27,461	27,461	9,985	17,476	27,461	9,985	17,476	27,461	9,985	17,476
Adj R-squared	0.226	0.227	0.236	0.223	0.226	0.236	0.223	0.226	0.236	0.223

Robust standard errors in parentheses, clustered by Municipality of Birth
*** p<0.01, ** p<0.05, * p<0.1
Treatment*Older Cohort includes birth years 1988-1992. Treatment*Younger includes birth years 1993-1996.
Treatment*High Exposure includes those who were eligible for each all three years of the treatment throughout 2000-2 (birth years 1990-4). Treatment*Low Exposure includes those who were eligible for less than the full three years (birth years 1988-9, 1995-6).
All specifications include fixed effects for block, survey year and birth year, as well as a dummy variable for female.

The effect on *attendance* is primarily driven by the younger cohort, who saw an insignificant increase of nearly 4 percentage points versus the older cohort who saw an

increase of 1.6 percentage points. The treatment effect on *attendance* is most likely larger

in the younger cohort (birth years 1993-1996) because they were more likely to be

attending school throughout the entire period for which we have surveys (2001-2012).

Differences in exposure to the treatment did not have a significant impact on

attendance. In fact, the low exposure group exhibited a larger treatment effect than the

high exposure group.

Females also experienced a nearly 1-percentage point higher effect on *attendance*

relative to males. This gap narrows within Blocks 1-2, where males and females

experienced a gap of just 0.1 percentage points.

2.2 Health Cohort

Table 6.2a
Attendance Heterogeneity by Block and Passage of Time (Health Cohort)

VARIABLES	Preferred Main Specification attend	(1) attend	(2) attend	(3) attend	(4) attend
Treatment	0.0472***				
	(0.0141)				
Treatment*Blocks 1-2		0.0618***			
		(0.0165)			
Treatment*Blocks 3-5		0.0373*			
		(0.0205)			
Treatment*Survey Years 2001-3			0.0621***	0.0654**	0.0528*
			(0.0188)	(0.0259)	(0.0262)
Treatment*Survey Years 2004-6			0.0585**	0.105***	0.0310
			(0.0222)	(0.0330)	(0.0285)
Treatment*Survey Years 2007-9			0.0422*	0.0353	0.0527
			(0.0239)	(0.0240)	(0.0368)
Treatment*Survey Years 2010-12			0.00443	0.0447	-0.0301
			(0.0429)	(0.0646)	(0.0518)
P-Value for Joint Significance Test		0.3521	0.6734	0.2961	0.5663
Sample Included	Full	Full	Full	Blocks 1-2	Blocks 3-5

Observations	8,236	8,236	8,236	3,023	5,213				
R-squared	0.456	0.456	0.456	0.511	0.426				

Robust standard errors in parentheses, clustered by Municipality of Birth
*** p<0.01, ** p<0.05, * p<0.1
All specifications include fixed effects for block, survey year and birth year, as well as a dummy variable for female.

The *attendance* effect for the health cohort was largest in Blocks 1 and 2 just like the education cohort at 6.2 percentage points. Like for the education cohort, the effects on overall *attendance* decreased after survey years 2004-6 as the cohort aged and fewer students attended school. The treatment impact was over 10 percentage points in survey years 2004-6 for Blocks 1-2 but fell to 4.5 percentage points in survey years 2010-12. Because this group is a younger cohort than those who received the education transfer, the magnitude of the impact on *attendance* remained higher through the final survey years, since the health cohort was more likely to still be in school.

Table 6.2b
Attendance Heterogeneity by Age, Level of Exposure, and Gender (Health Cohort)

VARIABLES	Preferred Main Specification attend	(1) attend	(2) attend	(3) attend	(4) attend	(5) attend	(6) attend	(7) attend	(8) attend	(9) attend
Treatment	0.0472***									
	(0.0141)									
Age Group										
Treatment* Birth Year 1998		0.0371**	0.0740***	0.00881						
		(0.0184)	(0.0210)	(0.0249)						
Treatment* Birth Year 1999		0.0444**	0.0633***	0.0310						
		(0.0175)	(0.0225)	(0.0257)						
Treatment* Birth Year 2000		0.0609***	0.0512**	0.0747***						
		(0.0181)	(0.0232)	(0.0257)						
Level of Exposure										
Treatment*High Exposure					0.0609***	0.0512**	0.0747***			
					(0.0181)	(0.0232)	(0.0257)			
Treatment*Low Exposure					0.0407***	0.0687***	0.0196			
					(0.0151)	(0.0174)	(0.0208)			
Gender										
Treatment*Male								0.0377**	0.0690***	0.0186
								(0.0181)	(0.0240)	(0.0239)
Treatment* Female								0.0571***	0.0572***	0.0573**
								(0.0153)	(0.0194)	(0.0220)

P-Value for Joint Significance Test	0.4391				0.2257			0.2847	
			Blocks 1-2	Blocks 3-5		Blocks 1-2	Blocks 3-5		Blocks 1-2	Blocks 3-5
Sample Included	Full	Full	Blocks 1-2	Blocks 3-5	Full	Blocks 1-2	Blocks 3-5	Full	Blocks 1-2	Blocks 3-5
Observations	8,236	8,236	3,023	5,213	8,236	3,023	5,213	8,236	3,023	5,213
Adj R-squared	0.456	0.456	0.510	0.426	0.456	0.510	0.426	0.456	0.510	0.426

Robust standard errors in parentheses, clustered by Municipality of Birth
*** p<0.01, ** p<0.05, * p<0.1
Treatment*High Exposure includes those who were eligible for each all three years of the treatment throughout 2000-2 (birth year 2000). Treatment*Low Exposure includes those who were eligible for less than the full three years (birth years 1998-9).
All specifications include fixed effects for block, survey year and birth year, as well as a dummy variable for female.

The effect was largest for the youngest of the health cohort (birth year 2000), who saw an increase of 6.1 percentage points relative to their control group equivalent versus just 4.44 percentage points for those born in 1999 and 3.7 percentage points for those born in 1998.

The treatment effect for high exposure treatment recipients was 6.1 percentage points relative to 4 percentage points for low exposure recipients. However, this different was insignificant. This gap does not hold up when broken down by block. Within Blocks 1-2, low exposure recipients had a higher impact on *attendance* than high exposure recipients.

Females in the treatment group got a boost in *attendance* impact relative to males of nearly 2 percentage points. This difference in treatment effects by gender varied significantly by block. Within Blocks 1 and 2, males saw a larger treatment impact than females by 1 percentage point, while in Blocks 3-5 females had a nearly 4 percentage point advantage.

3. Literacy

   3.1 Education Cohort

Table 7.1a
Literacy Heterogeneity by Block and Passage of Time (Education Cohort)

	Preferred Main Specification	(1)	(2)	(3)	(4)
VARIABLES	literate	literate	literate	literate	literate
Treatment	0.0314				
	(0.0189)				
Treatment*Blocks 1-2		0.0761**			
		(0.0369)			
Treatment*Blocks 3-5		0.00259			
		(0.0153)			
Treatment*Survey Years 2001-3			0.0289	0.0618	0.00523
			(0.0280)	(0.0544)	(0.0273)
Treatment*Survey Years 2004-6			0.0255	0.0917*	-0.0157
			(0.0223)	(0.0448)	(0.0165)
Treatment*Survey Years 2007-9			0.0457**	0.0929**	0.0195
			(0.0187)	(0.0341)	(0.0180)
Treatment*Survey Years 2010-12			0.00319	0.0214	-0.00756
			(0.0144)	(0.0207)	(0.0184)
P-Value for Joint Significance Test		0.0702*	0.1795	0.1119	0.2729
Sample Included	Full	Full	Full	Blocks 1-2	Blocks 3-5
Observations	27,462	27,462	27,462	9,986	17,476
Adj R-squared	0.139	0.141	0.140	0.157	0.132

Robust standard errors in parentheses, clustered by Municipality of Birth
*** p<0.01, ** p<0.05, * p<0.1
All specifications include fixed effects for block, survey year and birth year, as well as a dummy variable for female.

Like for the other education outcome variables, the *literacy* impacts were highest in the poorer municipalities, with Blocks 1 and 2 seeing an increase of 7.6 percentage points (significant at the 5% level), far higher than the overall average of 3.1 percentage points and the impact for Blocks 3-5 of just .26 percentage points.

The overall *literacy* impact increased over time through survey years 2007-9 and then decreased slightly in survey years 2010-12. This pattern was driven primarily by the

large increases in the treatment effect for Blocks 1-2. There was a much less clear pattern in the change in treatment effect for Blocks 3-5.

Table 7.1b
Literacy Heterogeneity by Age, Level of Exposure, and Gender (Education Cohort)

VARIABLES	Preferred Main Specification literate	(1) literate	(2) literate	(3) literate	(4) literate	(5) literate	(6) literate	(7) literate	(8) literate	(9) literate
Treatment	0.0314									
	(0.0189)									
Age Groups										
Treatment*Older Cohort		0.0194	0.0699*	-0.0109						
		(0.0186)	(0.0352)	(0.0172)						
Treatment* Younger Cohort		0.0450**	0.0832*	0.0196						
		(0.0214)	(0.0424)	(0.0171)						
Level of Exposure										
Treatment*High Exposure					0.0305	0.0757*	0.00111			
					(0.0193)	(0.0389)	(0.0153)			
Treatment*Low Exposure					0.0326	0.0772**	0.00548			
					(0.0199)	(0.0375)	(0.0180)			
Gender										
Treatment*Male								0.0345	0.0751*	0.00927
								(0.0209)	(0.0399)	(0.0193)
Treatment* Female								0.0282	0.0777**	-0.00331
								(0.0182)	(0.0363)	(0.0136)
P-Value for Joint Significance Test		0.0490**			0.8303			0.5439		
Sample Included	Full	Full	Blocks 1-2	Blocks 3-5	Full	Blocks 1-2	Blocks 3-5	Full	Blocks 1-2	Blocks 3-5
Observations	27,462	27,462	9,986	17,476	27,462	9,986	17,476	27,462	9,986	17,476
Adj R-squared	0.139	0.140	0.157	0.132	0.139	0.157	0.131	0.139	0.157	0.131

Robust standard errors in parentheses, clustered by Municipality of Birth
*** p<0.01, ** p<0.05, * p<0.1
Treatment*Older Cohort includes birth years 1988-1992. Treatment*Younger includes birth years 1993-1996.
Treatment*High Exposure includes those who were eligible for each all three years of the treatment throughout 2000-2 (birth years 1990-4). Treatment*Low Exposure includes those who were eligible for less than the full three years (birth years 1988-9, 1995-6).
All specifications include fixed effects for block, survey year and birth year, as well as a dummy variable for female.

The *literacy* impact is actually driven by the younger cohort, who saw more than twice as large an impact (4.5 percentage points) as the older cohort (1.9 percentage points). The difference was significant at the 5% level. Within Blocks 1 and 2 this 2-percentage point gap remained.

There was no significant difference in *literacy* impact between the high and low exposure groups.

There was a slight but insignificant different in the treatment impacts by gender. Males saw a 3.4 percentage point increase while females saw a 2.8 percentage points increase.

3.2 Health Cohort

Table 7.2a
Literacy Heterogeneity by Block and Passage of Time (Health Cohort)

VARIABLES	Preferred Main Specification literate	(1) literate	(2) literate	(3) literate	(4) literate
Treatment	0.0341				
	(0.0205)				
Treatment*Blocks 1-2		0.0737**			
		(0.0279)			
Treatment*Blocks 3-5		0.00697			
		(0.0262)			
Treatment*Survey Years 2001-3			-0.00297	0.00170	-0.00695
			(0.0103)	(0.0102)	(0.0147)
Treatment*Survey Years 2004-6			0.0413	0.0792**	0.0123
			(0.0256)	(0.0335)	(0.0339)
Treatment*Survey Years 2007-9			0.0500	0.118**	0.00364
			(0.0331)	(0.0457)	(0.0439)
Treatment*Survey Years 2010-12			0.0120	-0.00755	0.0251
			(0.0210)	(0.0367)	(0.0269)
P-Value for Joint Significance Test		0.0848*	0.3164	0.0259**	0.7050
Sample Included	Full	Full	Full	Blocks 1-2	Blocks 3-5
Observations		8,236	8,236	3,023	5,213
Adj R-squared	0.476	0.477	0.476	0.495	0.469

Robust standard errors in parentheses, clustered by Municipality of Birth
*** p<0.01, ** p<0.05, * p<0.1
All specifications include fixed effects for block, survey year and birth year, as well as a dummy variable for female.

For the health cohort, the *literacy* impact was highest for children in Blocks 1-2 at 7.4 percentage percentage points, which is significant at the 1% level. For Blocks 3-5, on the other hand, the *literacy* impact was less than 1 percentage point and insignificant.

These results show that the overall treatment impact on *literacy* for the health cohort of

3.4 percentage points was driven entirely by those in the poorest municipalities.

The effect on *literacy* increased over time through survey years 2007-9, much like

for the education cohort. Like for the education cohort, this was driven by large increases

in the treatment effect for Blocks 1-2.

Table 7.2b
Literacy Heterogeneity by Age, Level of Exposure, and Gender (Health Cohort)

VARIABLES	Preferred Main Specification literate	(1) literate	(2) literate	(3) literate	(4) literate	(5) literate	(6) literate	(7) literate	(8) literate	(9) literate
Treatment	0.0341									
	(0.0205)									
Age Group										
Treatment*Birth Year 1998		0.0277	0.0858***	-0.0112						
		(0.0255)	(0.0296)	(0.0342)						
Treatment*Birth Year 1999		0.0500**	0.0938**	0.0150						
		(0.0242)	(0.0387)	(0.0258)						
Treatment*Birth Year 2000		0.0247	0.0371	0.0178						
		(0.0211)	(0.0272)	(0.0300)						
Level of Exposure										
Treatment*High Exposure					0.0246	0.0371	0.0178			
					(0.0211)	(0.0272)	(0.0300)			
Treatment*Low Exposure					0.0386*	0.0897***	0.00163			
					(0.0223)	(0.0304)	(0.0270)			
Gender										
Treatment*Male								0.0319	0.0730**	0.00297
								(0.0228)	(0.0345)	(0.0274)
Treatment* Female								0.0364	0.0722**	0.0109
								(0.0222)	(0.0272)	(0.0310)
P-Value for Joint Significant Test		0.4209			0.4041			0.8088		
Sample Included	Full	Full	Blocks 1-2	Blocks 3-5	Full	Blocks 1-2	Blocks 3-5	Full	Blocks 1-2	Blocks 3-5
Observations	8,236	8,236	3,023	5,213	8,236	3,023	5,213	8,236	3,023	5,213
Adj R-squared	0.476	0.476	0.493	0.469	0.476	0.493	0.469	0.476	0.492	0.469

Robust standard errors in parentheses, clustered by Municipality of Birth
*** p<0.01, ** p<0.05, * p<0.1
Treatment*High Exposure includes those who were eligible for each all three years of the treatment throughout 2000-2 (birth year 2000). Treatment*Low Exposure includes those who were eligible for less than the full three years (birth years 1998-9).
All specifications include fixed effects for block, survey year and birth year, as well as a dummy variable for female.

The treatment effect on *literacy* for the health cohort was highest for the oldest cohort. For birth year 1999, the treatment effect was 5 percentage points versus 2.5 for birth year 2000. This gap is starker within Blocks 1 and 2, where birth year 1998 saw a treatment effect of 8.6 percentage points, birth year 1999 of 9.4 percentage points, and birth year 2000 of just 3.7 percentage points.

This difference in impacts by age group is reiterated in the differential treatment impacts for high versus low exposure groups. The high exposure group, birth year 2000, saw a lower magnitude treatment effect.

The treatment impacts for *literacy* did not vary significantly by gender. Both males and females saw a similar treatment of effect of just over 3 percentage points. The lack of a gap between the genders was consistent when broken down by block.

4. Labor Supply: Probability of Working
4.1 Education Cohort

Table 8.1a

Probability of Working Heterogeneity by Block and Passage of Time (Education Cohort)

VARIABLES	Preferred Main Specification worked	(1) worked	(2) worked	(3) worked	(4) worked
Treatment	0.00787 (0.00701)				
Treatment*Blocks 1-2		0.0142 (0.00954)			
Treatment*Blocks 3-5		0.00382 (0.00989)			
Treatment*Survey Years 2001-3			0.00717 (0.0108)	0.0195 (0.0119)	-0.00181 (0.0167)
Treatment*Survey Years 2004-6			0.00694 (0.0128)	-0.00544 (0.0152)	0.0159 (0.0186)
Treatment*Survey Years 2007-9			0.0158 (0.0111)	0.0301 (0.0186)	0.00507 (0.0136)
Treatment*Survey Years 2010-12			-0.0196 (0.0201)	-0.0182 (0.0346)	-0.0190 (0.0259)
P-Value for Joint		0.4609	0.4388	0.1699	0.6502

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Hypothesis Test

Sample Included	Full	Full	Full	Blocks 1-2	Blocks 3-5
Observations	27,461	27,461	27,461	9,987	17,474
Adj R-squared	0.289	0.289	0.289	0.300	0.282

Robust standard errors in parentheses, clustered by Municipality of Birth
*** p<0.01, ** p<0.05, * p<0.1
All specifications include fixed effects for block, survey year and birth year, as well as a dummy variable for female.

The treatment effect on the probability of having worked in the past week is small and insignificant, including when broken down by block or by survey year group. The magnitude of the effect is slightly higher within Blocks 1 and 2 but the difference in the treatment effect between Blocks 1-2 and Blocks 3-5 is insignificant. The already small treatment effect diminishes over time since the treatment, becoming negative, though insignificant, in the final survey year group (2010-12).

Table 8.1b
Probability of Working Heterogeneity by Age, Level of Exposure, and Gender (Education Cohort)

VARIABLES	Preferred Main Specification worked	(1) worked	(2) worked	(3) worked	(4) worked	(5) worked	(6) worked	(7) worked	(8) worked	(9) worked
Treatment	0.00787									
	(0.00701)									
Age Groups										
Treatment*Older Cohort		0.0109	0.0103	0.0113						
		(0.0092)	(0.0135)	(0.0125)						
Treatment* Younger Cohort		0.00438	0.0179	-0.00491						
		(0.008)	(0.0137)	(0.0101)						
Level of Exposure										
Treatment*High Exposure					0.00464	0.0163	-0.00192			
					(0.0088)	(0.0116)	(0.0122)			
Treatment*Low Exposure					0.0121	0.0110	0.0115			
					(0.0083)	(0.0124)	(0.0111)			
Gender										
Treatment*Male								0.00816	-0.000345	0.0139
								(0.0135)	(0.0210)	(0.0186)
Treatment* Female								0.00756	0.0291**	-0.00624
								(0.009)	(0.0136)	(0.0110)
P-Value for Joint Significance Test		0.5156			0.4568			0.9737		
Sample Included	Full	Full	Blocks 1-2	Blocks 3-5	Full	Blocks 1-2	Blocks 3-5	Full	Blocks 1-2	Blocks 3-5
Observations	27,461	27,461	9,987	17,474	27,461	9,987	17,474	27,461	9,987	17,474

Adj R-squared	0.289	0.289	0.300	0.282	0.289	0.300	0.282	0.289	0.300	0.282

Robust standard errors in parentheses, clustered by Municipality of Birth
*** p<0.01, ** p<0.05, * p<0.1
Treatment*Older Cohort includes birth years 1988-1992. Treatment*Younger includes birth years 1993-1996.
Treatment*High Exposure includes those who were eligible for each all three years of the treatment throughout 2000-2 (birth years 1990-4). Treatment*Low Exposure includes those who were eligible for less than the full three years (birth years 1988-9, 1995-6).
All specifications include fixed effects for block, survey year and birth year, as well as a dummy variable for female.

The treatment effects for the older and younger cohorts were not significantly different for the education transfer. For Blocks 3-5, the older cohort had a slightly higher magnitude than the younger cohort, but this was insignificant.

Likewise, the high exposure group within the treatment did not see a consistently higher effect on the probability of working. The high exposure group within Blocks 1-2 had a slightly higher coefficient in magnitude but it was still insignificant.

The treatment effect was significantly higher for females in Blocks 1-2 at 2.9 percentage points relative to males' nearly zero effect.

4.2 Health Cohort

Table 8.2a
Probability of Working Heterogeneity by Block and Passage of Time (Health Cohort)

VARIABLES	Preferred Main Specification worked	(1) worked	(2) worked	(3) worked	(4) worked
Treatment					
Treatment*Blocks 1-2	0.00651 (0.00686)				
Treatment*Blocks 3-5	-0.00178 (0.00649)				
Treatment*Survey Years 2001-3			-0.00435 (0.00795)	-0.00556 (0.00662)	-0.00587 (0.0122)
Treatment*Survey Years 2004-6			-0.00108 (0.00505)	0.000673 (0.00409)	-0.00497 (0.00722)
Treatment*Survey Years 2007-9			0.00536 (0.00791)	0.0145 (0.0126)	0.000859 (0.0103)
Treatment*Survey Years 2010-12			-0.00324 (0.0318)	-0.00624 (0.0476)	0.00263 (0.0416)
P-Value for Joint Hypothesis Test		0.3945	0.8296	0.5782	0.9595

Sample Included	Full	Full	Full	Blocks 1-2	Blocks 3-5
Observations	7,041	7,041	7,041	2,561	4,480
Adj R-squared	0.146	0.146	0.146	0.166	0.134

Robust standard errors in parentheses, clustered by Municipality of Birth
*** p<0.01, ** p<0.05, * p<0.1
All specifications include fixed effects for block, survey year and birth year, as well as a dummy variable for female.

The treatment effect on probability of working was noticeably stronger for those in Blocks 1-2 (0.6 percentage points) than for those in Blocks 3-5 (-0.2 percentage points), but the difference between them was insignificant. There was no clear pattern for probability over working over time since the treatment.

Table 8.2b
Probability of Working Heterogeneity by Age, Level of Exposure, and Gender (Health Cohort)

VARIABLES	Preferred Main Specification worked	(1) worked	(2) worked	(3) worked	(4) worked	(5) worked	(6) worked	(7) worked	(8) worked	(9) worked
Treatment	0.00154									
	(0.00482)									
Age Group										
Treatment* Birth Year 1998		-0.00408	0.00787	-0.0103						
		(0.00897)	(0.0161)	(0.0106)						
Treatment* Birth Year 1999		-0.00609	-0.00761	-0.00684						
		(0.00836)	(0.0110)	(0.0121)						
Treatment* Birth Year 2000		0.0163***	0.0195*	0.0152**						
		(0.00595)	(0.0106)	(0.0067)						
Level of Exposure										
Treatment* High Exposure					0.0163***	0.0195*	0.0152**			
					(0.00594)	(0.0105)	(0.00672)			
Treatment* Low Exposure					-0.00504	0.000412	-0.00865			
					(0.00646)	(0.00922)	(0.00889)			
Gender										
Treatment* Male								0.00332	0.00841	0.00120
								(0.00898)	(0.0122)	(0.0125)
Treatment* Female								-0.000263	0.00448	-0.00425
								(0.00339)	(0.0062)	(0.00386)
P-Value for Joint Hypothesis Test	0.0437**				0.0174**			0.7104		

Sample Included	Full	Full	Blocks 1-2	Blocks 3-5	Full	Blocks 1-2	Blocks 3-5	Full	Blocks 1-2	Blocks 3-5
Observations	7,041	7,041	2,561	4,480	7,041	2,561	4,480	7,041	2,561	4,480
Adj R-squared	0.146	0.147	0.167	0.135	0.147	0.167	0.135	0.146	0.167	0.135

Robust standard errors in parentheses, clustered by Municipality of Birth
*** p<0.01, ** p<0.05, * p<0.1
Treatment*High Exposure includes those who were eligible for each all three years of the treatment throughout 2000-2 (birth year 2000). Treatment*Low Exposure includes those who were eligible for less than the full three years (birth years 1998-9).
All specifications include fixed effects for block, survey year and birth year, as well as a dummy variable for female.

The youngest cohort (birth year 2000) saw positive and significant impacts on the likelihood of working while the oldest cohorts (birth years 1998-9) saw negative and insignificant impacts.

The result for increased likelihood for working was driven entirely by the youngest of the health cohort, those born in 2000, who were 1.6 percentage points more likely to have worked in the last week than those born in 2000 in the control group. Those born in 1998 and 1999 saw decreases in the likelihood of working relative to their equivalent cohort in the control group. This result held up within each block group, suggesting that poverty level did not matter within those age groups.

The particularly high result for children born in 2000 is reflected in the high treatment effects of the high exposure group, which is comprised entirely of those born in 2000. They were eligible to receive more transfers, which may have positively affected their likelihood of working in the long-term.

Males had a higher magnitude treatment effect than females, which was consistent when broken down by poverty level. However, the difference between the male and female treatment effects is insignificant.

5. Labor Supply: Hours Worked
    5.1 Education Cohort

Table 9.1a
Hours Worked Heterogeneity by Block and Passage of Time (Education Cohort)

VARIABLES	Preferred Main Specification hours	(1) hours	(2) hours	(3) hours	(4) hours
Treatment	0.330				
	(0.333)				
Treatment*Blocks 1-2		-0.0889			
		(0.428)			
Treatment*Blocks 3-5		0.600			
		(0.461)			
Treatment*Survey Years 2001-3			0.748*	0.507	0.880
			(0.408)	(0.451)	(0.613)
Treatment*Survey Years 2004-6			0.200	-0.746	0.705
			(0.592)	(0.710)	(0.846)
Treatment*Survey Years 2007-9			0.267	0.155	0.434
			(0.634)	(1.146)	(0.709)
Treatment*Survey Years 2010-12			-0.958	-2.231	-0.104
			(1.199)	(2.206)	(1.382)
P-Value for Joint Significance Test		0.2809	0.4856	0.4140	0.9172
Sample Included	Full	Full	Full	Blocks 1-2	Blocks 3-5
Observations	27,421	27,421	27,421	9,972	17,449
Adj R-squared	0.285	0.285	0.285	0.304	0.275

Robust standard errors in parentheses, clustered by Municipality of Birth
*** $p<0.01$, ** $p<0.05$, * $p<0.1$
All specifications include fixed effects for block, survey year and birth year, as well as a dummy variable for female.

Interestingly, children in poorer municipalities (Blocks 1-2) saw larger treatment effects for the likelihood of working (1.4 percentage points for Blocks 1-2 versus 0.38 percentage points for Blocks 3-5), but lower impact on hours worked relative to those in Blocks 3-5. However, none of these treatment effects were significant. The effects were strongest in the earlier survey years (2001-3) and diminished over time.

Table 9.1b

Hours Worked Heterogeneity by Age, Level of Exposure, and Gender (Education Cohort)

VARIABLES	Preferred Main Specification hours	(1) hours	(2) hours	(3) hours	(4) hours	(5) hours	(6) hours	(7) hours	(8) hours	(9) hours
Treatment	0.330									
	(0.333)									
Age Groups										
Treatment*Older Cohort		0.588	-0.347	1.151**						
		(0.417)	(0.561)	(0.557)						
Treatment*Younger Cohort		0.0375	0.146	-0.0113						
		(0.350)	(0.573)	(0.471)						
Level of Exposure										
Treatment*High Exposure					0.319	0.00660	0.590			
					(0.425)	(0.609)	(0.559)			
Treatment*Low Exposure					0.345	-0.256	0.661			
					(0.401)	(0.530)	(0.547)			
Gender										
Treatment*Male								0.453	-0.939	1.381*
								(0.654)	(1.037)	(0.797)
Treatment*Female								0.204	0.770	-0.147
								(0.409)	(0.654)	(0.507)
P-Value of Joint Significance Test		0.1701			0.9583		0.7760			
Sample Included	Full	Full	Blocks 1-2	Blocks 3-5	Full	Blocks 1-2	Blocks 3-5	Full	Blocks 1-2	Blocks 3-5
Observations	27,421	27,421	9,972	17,449	27,421	9,972	17,449	27,421	9,972	17,449
Adj R-squared	0.285	0.285	0.304	0.275	0.285	0.304	0.275	0.285	0.305	0.276

Robust standard errors in parentheses, clustered by Municipality of Birth
*** p<0.01, ** p<0.05, * p<0.1
Treatment*Older Cohort includes birth years 1988-1992. Treatment*Younger includes birth years 1993-1996.
Treatment*High Exposure includes those who were eligible for each all three years of the treatment throughout 2000-2 (birth years 1990-4). Treatment*Low Exposure includes those who were eligible for less than the full three years (birth years 1988-9, 1995-6).
All specifications include fixed effects for block, survey year and birth year, as well as a dummy variable for female.

The treatment effect for hours worked in the last week was clearly highest in the older half of the education cohort, with a treatment effect of 0.59 additional hours relative to 0.04 hours for the younger half of the cohort. This effect is primarily driven by the older students in the wealthier municipalities (Blocks 3-5), who saw a statistically significant treatment effect of 1.15 hours.

The higher treatment effect for males was especially noteworthy, particularly within Blocks 3-5, who saw the largest overall treatment effect. In the full sample, males

had twice as large a treatment effect as females. Within the Blocks 3-5 sample, this difference was enlarged to 1.38 hours for males and -0.147 hours for females.

5.2 Health Cohort

Table 9.2a
Hours Worked Heterogeneity by Block and Passage of Time (Health Cohort)

VARIABLES	Preferred Main Specification hours	(1) hours	(2) hours	(3) hours	(4) hours
Treatment	0.0254 (0.161)				
Treatment*Blocks 1-2		-0.107 (0.216)			
Treatment*Blocks 3-5		0.114 (0.227)			
Treatment*Survey Years 2001-3			-0.255 (0.334)	-0.149 (0.199)	-0.374 (0.532)
Treatment*Survey Years 2004-6			-0.0490 (0.121)	-0.000208 (0.113)	-0.118 (0.178)
Treatment*Survey Years 2007-9			-0.0209 (0.200)	-0.0375 (0.216)	-0.0418 (0.303)
Treatment*Survey Years 2010-12			0.590 (1.332)	-0.969 (2.153)	1.731 (1.584)
P-Value for Joint Significance Test		0.4893	0.9044	0.8332	0.6862
Sample Included	Full	Full	Full	Blocks 1-2	Blocks 3-5
Observations	7,041	7,041	7,041	2,561	4,480
Adj R-squared	0.115	0.115	0.115	0.132	0.106

Robust standard errors in parentheses, clustered by Municipality of Birth
*** p<0.01, ** p<0.05, * p<0.1
All specifications include fixed effects for block, survey year and birth year, as well as a dummy variable for female.

The impact on hours worked differed by level of poverty. Lower quintiles saw negative impacts on hours worked while the three upper quintiles saw positive impacts. This effect became stronger over time, likely reflecting the fact that more students in the cohort were entering the labor force as they became young adults.

Overall, the insignificant results for labor supply suggest that the health cohort recipients had an increased probability of working but worked fewer hours.

Table 9.2b
Hours Worked Heterogeneity by Age, Level of Exposure, and Gender (Health Cohort)

VARIABLES	Preferred Main Specification hours	(2) hours	(3) hours	(4) hours	(5) hours	(6) hours	(7) hours	(8) hours	(9) hours	(10) hours
Treatment	0.0254									
	(0.161)									
Age Group										
Treatment* Birth Year 1998		-0.311	-0.404	-0.201						
		(0.349)	(0.661)	(0.345)						
Treatment* Birth Year 1999		0.0355	-0.130	0.107						
		(0.248)	(0.363)	(0.347)						
Treatment* Birth Year 2000		0.411***	0.172	0.519***						
		(0.145)	(0.205)	(0.187)						
Level of Exposure										
Treatment* High Exposure					0.410***	0.172	0.518***			
					(0.145)	(0.205)	(0.187)			
Treatment* Low Exposure					-0.146	-0.272	-0.0559			
					(0.226)	(0.329)	(0.305)			
Gender										
Treatment* Male								-0.000222	-0.284	0.202
								(0.323)	(0.436)	(0.440)
Treatment* Female								0.0514	0.0100	0.0288
								(0.0916)	(0.162)	(0.118)
P-Value for Joint Significance Test		0.1245			0.0425**			0.8838		
Sample Included	Full	Full	Blocks 1-2	Blocks 3-5	Full	Blocks 1-2	Blocks 3-5	Full	Blocks 1-2	Blocks 3-5
Observations	7,041	7,041	2,561	4,480	7,041	2,561	4,480	7,041	2,561	4,480
Adj R-squared	0.115	0.115	0.132	0.104	0.115	0.132	0.104	0.115	0.132	0.104

Robust standard errors in parentheses, clustered by Municipality of Birth
*** p<0.01, ** p<0.05, * p<0.1
Treatment*High Exposure includes those who were eligible for each all three years of the treatment throughout 2000-2 (birth year 2000). Treatment*Low Exposure includes those who were eligible for less than the full three years (birth years 1998-9).
All specifications include fixed effects for block, survey year and birth year, as well as a dummy variable for female.

The oldest cohort (birth year 1998) saw negative impacts on hours worked while the youngest cohort (birth year 2000) saw positive and significant impacts (an increase of over 0.4 hours)—the opposite pattern from the effect on probability of working. This

effect within birth year 2000 was particularly strong within Blocks 3-5, where children in this age group had a treatment effect of over 0.5 hours.

This high treatment impact for birth year 2000 was reflected in the high and significant treatment impact for the high exposure group, which is comprised entirely of those born in 2000. The higher number of transfers for which these children were eligible could have been one pathway for these longer-term labor supply results.

The treatment effect on hours worked was higher for females in the health cohort, but not significantly. In Blocks 3-5, where most of the treatment effect is isolated, there is virtually no difference between the treatment effects for males and females.

VI. ROBUSTNESS

1. Municipality in 2000

This section uses the created variable *Municipality in 2000* as an alternative method of assigning children to the treatment and control groups. This variable is only available in survey years September 2002 and 2007-2012 with the exception of September 2007. As a result, the specifications in this section have a small fraction of the observations of those in the Main Results section and only one earlier survey year.

Although the treatment designation using *Municipality in 2000* is available for fewer years and fewer observations within those years, these later surveys contain a series of questions that allow me to more accurately determine where the respondents were in the year 2000. For the children in the control and treatment groups for whom we do have this data, this is a more accurate measurement of their municipality in the year 2000, especially in the later survey years and for the older cohort, where there is likely more measurement error in the treatment designation using *Municipality of Birth*.

Table 10.1a
Preferred Main Specification Outcomes: Main and Alternative Treatment Definitions (Education Cohort)

VARIABLES	(1) highest grade	(2) attend	(3) literate	(4) worked	(5) hours
Treatment using	0.202	0.0269	0.0314	0.00787	0.330
Municipality at Birth	(0.147)	(0.0184)	(0.0189)	(0.00701)	(0.333)
Observations	27,428	27,461	27,462	27,461	27,421
Adjusted R-squared	0.399	0.226	0.139	0.289	0.285
Treatment using	0.314	0.00886	0.0345*	0.00533	0.132
Municipality in 2000	(0.203)	(0.0219)	(0.0188)	(0.00942)	(0.479)
Observations	11,033	11,042	11,043	11,043	11,030
Adjusted R-squared	0.341	0.197	0.111	0.320	0.322

Robust standard errors in parentheses, clustered by Municipality in 2000, *** p<0.01, ** p<0.05, * p<0.1
All specifications include fixed effects for birth year, survey year, block, and a dummy for gender.

The results using the alternative treatment designation hold up to those using the main treatment variable. The magnitude of each of the coefficients is similar to the coefficients using the main treatment variable. The results for *literacy* and *probability of working* were especially similar in magnitude, giving some consistency to the robustness results.

A few coefficients are lower than with the main treatment variable, such as *attendance, probability of working* and *hours worked*, the last of which actually became negative. For *attendance,* the lower result is unsurprising given that the *attendance* effects in the main specifications were concentrated in the earlier survey years. Since *Municipality in 2000* is heavily weighted towards later survey years, there is likely little effect to be found, especially for the education cohort. For the labor supply variables, this is consistent with the main results, which found very few significant or high magnitude impacts on labor supply outcomes, even when broken down in the heterogeneity section.

The coefficient on *highest grade* was notably larger using *Municipality in 2000*, though still insignificant. This is likely due to the higher standard error. Though still insignificant, this high coefficient corroborates the positive coefficient on *highest grade* and shows that the true treatment effect is likely higher than the treatment variable using *Municipality of Birth* shows, likely due to the associated measurement error.

Although we believe that this alternative designation of the treatment and control groups creates far less measurement error than using *Municipality of Birth*, the standard errors are higher using the alternative treatment variable, likely because these specifications contain so few observations. The designation of children to treatment and control groups using *Municipality in 2000* used far fewer survey years and contained

fewer observations, increasing the standard error. The magnitudes of the coefficients may be more accurate, but the small number of observations likely prevents us from seeing a significant outcome.

However, much of the differences between the magnitudes of the coefficients between the two designations can be explained by the difference in time periods covered by the survey. As such, we run the same main specification using *Municipality of Birth* for the same set of surveys as we have available for *Municipality in 2000*. This reveals that the main and alternative treatment designations are almost identical when examined over the same time period.

Table 10.1b
Preferred Main Specification Outcomes: Main and Alternative Treatment Definitions
Using An Identical Set of Survey Years
(Education Cohort)

VARIABLES	(1) highest grade	(2) attend	(3) literate	(4) worked	(5) hours
Treatment using	0.298	0.0120	0.0371**	0.00958	0.162
Municipality at Birth	(0.190)	(0.0199)	(0.0180)	(0.00898)	(0.467)
Observations	10,428	10,977	10,978	10,978	10,966
Adjusted R-squared	0.330	0.201	0.107	0.323	0.329
Treatment using	0.314	0.00886	0.0345*	0.00533	0.132
Municipality in 2000	(0.203)	(0.0219)	(0.0188)	(0.00942)	(0.479)
Observations	11,033	11,042	11,043	11,043	11,030
Adjusted R-squared	0.341	0.197	0.111	0.320	0.322

Robust standard errors in parentheses, clustered by Municipality in 2000
*** p<0.01, ** p<0.05, * p<0.1
All specifications include fixed effects for birth year, survey year, block, and a dummy for gender.

Thus, the alternative treatment designation finds nearly identical treatment effects as the main treatment designation when examined over the same set of survey years. This shows that the main results hold up well to robustness checks.

Table 10.2a

Preferred Main Specification Outcomes: Main and Alternative Treatment Definitions (Health Cohort)

VARIABLES	(1) highest grade	(2) attend	(3) literate	(4) worked	(5) hours
Treatment using	0.111**	0.0472***	0.0341	0.00154	0.0254
Municipality at Birth	(0.0477)	(0.0141)	(0.0205)	(0.00482)	(0.161)
Observations	8,219	8,236	8,236	7,041	7,041
Adjusted R-squared	0.720	0.456	0.476	0.146	0.115
Treatment using	0.120	0.0226	0.0259	0.00704	0.167
Municipality in 2000	(0.0830)	(0.0137)	(0.0207)	(0.00898)	(0.297)
Observations	4,558	4,559	4,559	3,607	3,607
Adjusted R-squared	0.692	0.636	0.537	0.149	0.119

Robust standard errors in parentheses, clustered by Municipality in 2000

*** p<0.01, ** p<0.05, * p<0.1

All specifications include fixed effects for block, survey year and birth year, as well as a dummy variable for female.

For the health cohort, the results using the alternative treatment designation were also quite similar to those using the main treatment designation. The parity of the results held up and most of the magnitudes are very similar, with most coefficients increasing in magnitude.

One slight change in magnitude that mirrored the effects for the education cohort is the coefficient for *highest grade*, which increased from 0.11 in the main treatment designation to 0.12 in the alternative treatment designation, though not remaining significant due to the higher standard error. This somewhat larger coefficient is expected given the very high treatment effects found in survey years 2007-9 using the main treatment variable (as high as 0.33 years of schooling), since the treatment effect on *highest grade* increases over time. This higher magnitude coefficient using the alternative treatment designation was reflected in both the education cohort and the health cohort.

Another significant though opposite signed change in magnitude from the main treatment variable to the alternative is the treatment effect on *attendance*, the probability of attending school, which decreased from 4.7 percentage points using municipality of birth to 2.3 percentage points using municipality in 2000. This, like in the education cohort results, is unsurprising given the survey years included in the *Municipality in 2000* treatment variable, which were shown in the heterogeneity section to have lower *attendance* impacts using the *Municipality of Birth* treatment variable.

Overall, the usage of *Municipality in 2000* likely corrected for some of the measurement error in the use of *Municipality of Birth*, given the increased magnitudes of some of the coefficients. However, the use of mostly the later survey years makes it difficult to find significant results for *attendance*. Otherwise, the coefficients of *Municipality in 2000* treatment effects may give a more accurate picture of the true treatment effect due to the removal of measurement error.

Table 10.2b
Preferred Main Specification Outcomes: Main and Alternative Treatment Definitions Using An Identical Set of Survey Years
(Health Cohort)

VARIABLES	(1) highest grade	(2) attend	(3) literate	(4) worked	(5) hours
Treatment using	0.173**	0.0308**	0.0326	0.00521	0.0778
Municipality at Birth	(0.0785)	(0.0143)	(0.0220)	(0.00932)	(0.316)
Observations	4,213	4,214	4,214	3,305	3,305
Adjusted R-squared	0.686	0.641	0.533	0.149	0.120
Treatment using	0.120	0.0226	0.0259	0.00704	0.167
Municipality in 2000	(0.0830)	(0.0137)	(0.0207)	(0.00898)	(0.297)
Observations	4,558	4,559	4,559	3,607	3,607
Adjusted R-squared	0.692	0.636	0.537	0.149	0.119

Robust standard errors in parentheses, clustered by Municipality in 2000
*** p<0.01, ** p<0.05, * p<0.1
All specifications include fixed effects for block, survey year and birth year, as well as a dummy variable for female.

Like for the education cohort, much of the differences between the main and alternative treatment designations seen in Table 10.2a can be explained by the differences in survey years used to calculate the coefficients. Once those differences have been accounted for, the differences between the two designations are much smaller. This also supports the robustness of the main results.

2. Attrition
2.1 International Attrition

We utilize the migration modules in EPHPM survey years September 2006 and May 2010 to identify individuals who may plausibly have been part of the treatment and control groups who left Honduras. We use the survey questions asked of the household head about members of their household who have left Honduras, including the age of the migrant, to determine their eligibility for the transfer. We then use the household mode of *Municipality of Birth* to plausibly identify the *Municipality of Birth* of the migrant.

It is important to note that this definition only allows us to capture individuals who moved out of the country alone, leaving their family behind. This means we cannot capture the treatment effects for full households that may have left Honduras. This also means that we do not have any observations for the health cohort, since they were too young to leave the country alone (in 2006, they were 6-8 years old and in 2010 they were 10-12 years old).

We find that there are only 17 individuals who were treatment-eligible who were abroad in September 2006 or May 2010 in our dataset. As a result, we conclude that attrition by individuals who were plausibly involved in the treatment or control groups was negligible.

2.2 Domestic and International Migration

We also looked at the probability of moving either domestically or internationally, allowing for the use the full sample of EPHPM survey years.

Table 11
Treatment Effect on Likelihood of Moving: Full Sample (Education Cohort)

VARIABLES	Preferred Main Specification moved	(1) moved	(2) moved	(3) moved	(4) moved
Treatment	-0.0113 (0.0144)				
Treatment*Older Cohort		-0.0113 (0.0144)			
Treatment*Younger Cohort		-0.0113 (0.0144)			
Treatment*Blocks 1-2			-0.0143 (0.0219)		
Treatment*Blocks 3-5			-0.00946 (0.0189)		
Treatment*Survey Years 2001-3					-0.0709*** (0.0221)
Treatment*Survey Years 2004-6					-0.00299 (0.0183)
Treatment*Survey Years 2007-9					0.000298 (0.0187)
Treatment*Survey Years 2010-12					0.0131 (0.0312)
Observations	21,383	21,383	21,383	21,383	21,383
Adjusted R-squared	0.035	0.035	0.035	0.035	0.036

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
All specifications include fixed effects for survey year, birth year, block and a dummy variable for gender.
Treatment*Older Cohort includes birth years 1988-1992. Treatment*Younger includes birth years 1993-1996.

We find that any small treatment effect on the likelihood of moving, which is negative, is isolated to the earliest survey bin, which was primarily during the treatment disbursement period. It makes intuitive sense that an individual might be less likely to move if, simply by remaining in their municipality, they are eligible for a cash transfer. Since they're essentially being paid to stay in that municipality, it follows that individuals in treatment municipalities would be somewhat less likely to move than those in control

municipalities. But, in the long run, it does not seem to be that more children in the control group are moving away, and thus attrition is not a serious issue in the data.

VII. DISCUSSION

1. Heterogeneity of Treatment Effects by Level of Poverty

The main results show that receipt of the transfer seems to have had a significant impact on educational outcomes such as grade attainment, attendance and literacy, while the results for labor supply are less clear-cut.

The educational impacts are particularly large for children in the poorest municipalities (the lowest two mean height-for-age quintiles), with treatment effects reaching up to 0.57 years of additional schooling in the education cohort and 0.2 years of schooling for the health cohort. Within these lower blocks, the oldest cohorts did especially well at 0.66 years of schooling in the education cohort and 0.28 in the health cohort.

These seemingly small increases represent a huge impact relative to the control group: a 0.57-year increase for this students represents a 16.5 percent increase in overall schooling attainment relative to the control group mean in Blocks 1-2 of 3.46 years of schooling for the education cohort. For the health cohort, a 0.2-year increase represents a 20.5 percent increase over the control group mean in Blocks 1-2 of 0.974 years of schooling.

These large impacts can be seen in all of the educational impact variables, including a 6.8 percentage point increase in *attendance* for Blocks 1-2 in the education cohort and an even higher increase of 7.6 percentage points for the younger cohort, who were more likely to still be in school throughout the survey period (2001-2012). Similar impacts on *attendance* were found in the health cohort of 6.2 percentage points, though the largest impacts were for the oldest cohort at 7.4 percentage points. An increase in the

education cohort of 6.8 percentage points is a 13 percent increase over the control group mean in Blocks 1-2 of 52.2 percent attendance. Likewise, in the health cohort, the 6.2 percentage point increase represents a 9.1 percent increase over the control group mean of 68 percent attendance.

Similarly, the 7.6 percentage point increase in *literacy* for the education cohort in Blocks 1-2 represents a 10 percent increase over the control group mean in Blocks 1-2 of 76.3 percent literacy. The 7.4 percentage point increase in *literacy* for the health cohort in Blocks 1-2 is an 18.7 percent increase over the control group mean in Blocks 1-2 of 39.6 percent literacy.

2. Treatment Effects Over Time

While the educational impact differentials by level of poverty were clear and consistent, this was less true of heterogeneity by age, gender, or level of exposure. These varied largely between outcome variables and even between the education and health cohorts.

One major finding is the increasing nature over time of the treatment effect on educational attainment, *highest grade*. This indicates that the treatment effect was not only sustained over the long-term but that it actually continued to build even after the treatment ended.

This may be indicative of a number of phenomena. For example, it could be that administrative issues led to continued disbursement of the treatment even after the treatment period, but this explanation seems highly unlikely and contrary to the IDB follow-up reports on *PRAF*.

A more likely rationale is the nature of pathways in which treated children were more likely to stay in school. Much of the literature suggests that the primary pathway,

particularly for older children, is increased reenrollment and reduced dropout. If this is

the case for *PRAF* as well, it may be that the treatment simply keeps children in school

during the high dropout point at the end of primary school and during the transition to

secondary school; once students make it past this point, they may remain in school for a

while, since there are no other significant dropout points. If this is what is happening for

PRAF, we would see exactly what we see in these results: an increasing gap over time

between the control and treatment groups. In fact, Glewwe and Olinto (2004) found that

in 2001 *PRAF* significantly reduced dropout rates, especially for poorer students, which

supports this possible pathway for the long-term results.

Some alternative regressions using the long-term data support this premise.

Education transfer eligible children in the treatment group were 2.5 percentage points

more likely to complete primary school and 1.2 percentage points more likely to attend at

least one year of secondary school. For Blocks 1-2, these treatment effects jump to a 7.3

percentage point increase in primary school completion and a 4.8 percentage point

increase in the likelihood of completing at least one year of secondary school.

Table 12
Treatment Effect on Primary School Completion and Progression to Secondary School
(Education Cohort)

	Preferred Main Specification	Preferred Main Specification	Preferred Main Specification	Preferred Main Specification	Preferred Main Specification	Preferred Main Specification
VARIABLES	primary	primary	primary	secondary	secondary	secondary
Treatment	0.0248			0.0123		
	(0.0211)			(0.0143)		
Treatment in Blocks 1-2		0.0732**			0.04761**	
		(.0368)			(0.0229)	
Treatment in Blocks 3-5			-0.0051			-0.0092
			(0.0217)			(0.0161)
Observations	27,463	9,987	17,476	27,463	9,987	17,476
R-squared	0.3999	0.3958	0.4011	0.3186	0.3030	0.1482

Robust standard errors in parentheses, clustered by Municipality of Birth.
*** p<0.01, ** p<0.05, * p<0.1
All specifications include fixed effects for survey year, birth year, block and a dummy variable for gender.

Based on these results, it does seem that one of the margins through which the treatment affected students was by smoothing the transition to secondary school, at least for the education transfer. The health cohort is mostly too young to see the full effects at the transition point between primary and secondary school. Future researchers should continue to conduct long-term follow-ups for *PRAF*, particularly for the health transfer cohort for whom some of the effects are not yet visible.

3. Treatment Effects by Age

The inconsistent nature of the educational outcomes' heterogeneity by age makes sense given the two competing narratives about which cohort would be most helped by a CCT. One narrative suggests that older children might be more affected by smoothing the transition from primary to secondary school, which is consistent with the CCT literature's findings of increased reenrollment and lowered dropout around this transition point.

However, there is another compelling narrative that claims that it is most effective to invest in younger children's health and education, since it yields a higher return on the investment. Early intervention via income shocks and improved health (Hoynes, Whitmore Schanzenbach, Almond 2012), for example through social safety nets, has been shown to be particularly effective for very young children. This may help explain why some of the educational impacts were particularly large, especially relative to the low control group mean, for the health cohort, who were under 3 upon receipt of the transfer, even though they received fewer transfers that were smaller in size and not conditioned on educational outcomes whatsoever.

Aside from improved health via the increase in health center visits and check-ups found in Moore (2008), the cash transfer may have altered consumption behavior in the household, allowing the household to spend more on "nutritious foods, early stimulation

or health care" (Macours, Vakis & Schady 2012). At such a young age, the literature on early child development suggests that these investments reap particularly large returns later in these children's lives, for example through improved educational outcomes.

4. Magnitude of Effects
In all, the main and heterogeneity coefficients suggest significant impacts on educational outcomes, particularly for the poorest students. Although the average educational effects seem low relative to the literature, the robustness section coefficients may be better reflections of the true treatment effect due to the reduced measurement error in this alternative treatment designation. Additionally, the heterogeneity of the impact by poverty level shows that the impacts for the poorest two quintiles are much higher than the average across the various educational outcome variables.

The magnitude of the results for overall attainment, *highest grade*, seems consistent with the literature, although slightly low. This could be due to measurement error in the matching of treatment & control to municipality of birth, which would bias the coefficient towards zero.

Attenuation bias can be seen in the high standard errors in the specifications that include the cohort eligible for the education transfers. Since there were many more years between birth and the point of randomization in 2000 for this older group, it makes sense that there would be some inclusion and exclusion error from moving that creates noise in the data. This attenuation bias likely keeps us from finding a significant coefficient for the education transfer group. However, due to the smaller attrition in the health transfer group, the regressions yield more accurate estimates with lower standard errors, which leads to more statistically significant results.

The magnitudes are corroborated by the alternative designation of the treatment and control groups in the Robustness section of the paper, and some magnitudes, particularly for *highest grade*, increase using the alternative designation, which may be a more accurate representation of the actual treatment effect.

5. Possible Pathways for Results

Unfortunately, these specifications don't give us a definitive narrative about the pathway for these results. It could be that the transfer was particularly effective in increasing educational outcomes for poorer students by encouraging school as a financially viable alternative to work (this was, of course, the explicit intention of *PRAF*, whose transfer amounts were set to offset the opportunity cost of sending your child to school from missed labor income).

One of the margins through which *PRAF* likely increased attainment was through reenrollment or reduced dropout, like in much of the existing literature and as discussed earlier. This effect could have come from keeping children in school for more days per year (*attendance*), which meant that treated children did not fall as far behind in school as their control counterparts, making it easier to transition into the next year of school and making dropout less likely. In fact, Glewwe and Olinto (2004) found that *PRAF* recipient students had fewer absences, supporting the possibility of this pathway.

The results could stem from a pure income effect, in which the income transfer allows households to simply increase their consumption of schooling. On the other hand, it also could reflect altered household perceptions about the returns to schooling, which may have led to a change in intra-household allocation of resources. For example, *PRAF* may have increased investment by parents in their children's education, for example if the transfer money was used to buy schoolbooks. Unfortunately, the scope of this paper

and data constraints did not allow for exploration into parents' perceptions or

consumption behavior. Future researchers, if data constraints allow, can spend some time

looking into the evidence for these different pathways.

6. Income Impacts and Cost-Effectiveness
   To get a sense of how these educational treatment impacts translate into the

workplace and long-term wellbeing of recipients, we construct a simple simulation using

a Mincer earnings regression. We use the EMPHPM dataset's construction of actual

monthly income from reported pay and actual time worked to estimate individuals'

income. We run the following Mincer regression, which includes the natural log of the

wage variable on the left-hand side and years of schooling, age and age-squared to

account for experience, and year dummy variables to control for wage differences across

time:

$$\ln(Wages)_{it} = \beta_0 + \beta_1 HighestGrade_i + \beta_2 Age_i + \beta_3 Age_i{}^2 + \gamma_t + \varepsilon$$

   This regression yields a coefficient of 0.13, indicating that each additional year of

schooling translates into a 13 percent increase in income—even higher than the 9.3%-

12.5% that Psacharapoulos and Patrinos (2004) suggest. If this is true, then our main

treatment effect of a 0.2-year increase in schooling would yield a 2.76 percent increase in

income. For students in Blocks 1-2, the main treatment effect of 0.57 additional years of

schooling would yield a 7.9 percent increase in income.

   We construct a cost-benefit analysis to take into account the costs of the

government investment through *PRAF* as well as predicted long-term gains in income for

treated individuals during their working years (defined as ages 13-65, since nearly a

quarter of children in treatment and control municipalities were working by age 13).

In order to calculate the costs of each education transfer per education-transfer-eligible child, we consider administrative costs, government expenditure to cover the cost of additional schooling, and the deadweight loss associated with taxation. We use Caldés et al. (2006)'s definitions of administrative costs for the years 2001-2 and divide by the total number of children eligible for education transfer—77,500 according to Galiani & McEwan (2013). We add in the deadweight loss associated with taxation, using 1.2 as the multiplier, following the precedent of Auriol and Warlters (2012).

Finally, we include the Honduran government's annual expenditure per pupil[23] and multiply by the additional years of schooling obtained by the treatment group. We use two different treatment effects to estimate the government expenditures on the additional years of schooling gained from the treatment—one of 0.2 years of schooling, which is more reflective of the small treatment effects in earlier years, and one of 0.49 years of schooling, which is the significant treatment effect for survey years 2007-9, which better represents the long-term effects of the transfer. Using 0.2 years of schooling, we estimate the total costs per eligible child, including administrative costs and deadweight loss, of 1,073 Lempiras (US$71.56) for 2000, 952 Lempiras (US$63.5) for 2001, and 991 Lempiras (US$66) for 2002. Using 0.49 years of schooling, we estimate the total costs to be 1,842 Lempiras (US$123) for 2000, 1,721 Lempiras (US$115) for 2001, and 1,760 Lempiras (US$117)[24] per education transfer eligible child. We use the cost estimates associated with 0.49 additional years of schooling going forward, since these are more reflective of the long-term impacts of the transfer and consequently are likely a more accurate reflection of the true cost.

[23] Only available for 2010 from the World dataBank.
[24] All converted to 2000 dollars and lempiras using CPI.

We then compare these costs to the long-term income gains associated with additional years of schooling. We use our Mincerian regression to predict the additional wages that will accrue to recipient children relative to the control group for each working year between ages 13 and 65. In total, the government invested 5,323 Lempiras (US$355) in 2001-2002 and we project individual returns of 44,167 Lempiras (US$2,944) in lifetime wages through 2057. In present value terms, this is the equivalent of investing 4,861 Lempiras today and earning 4,266 Lempiras.[25] In other words, the benefit-cost ratio is nearly 1. The internal rate of return of the investment, or the point at which net present value is zero, is quite high at 9 percent, especially considering that we have taken into account many sources of costs, including deadweight loss, while considering only one possible stream of benefits. This is very close to the internal rate of return criteria used by development banks to make education investment decisions. If we relax the 10% discount rate assumption just slightly, we find that the net present value of the investment is positive.

Other methods of quantifying *PRAF*'s cost-effectiveness yield similarly positive results. Given the 828-lempira (US$58) transfer per eligible child in the education transfer cohort, *PRAF* yields an average cost-effectiveness ratio of $11.15 for a 1% increase in schooling attainment.[26] However, for Blocks 1-2 this cost drops dramatically to just $3.51 for a 1% increase in schooling attainment.[27]

Yet another way of quantifying *PRAF*'s cost-effectiveness is examining the transfer amount it would require to increase schooling attainment by a full year. For the average child eligible for the education transfer, it would require five times the current

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[25] Using a 10% discount rate.
[26] Given that the .2-year schooling increase is a 5.2 percent increase over the control group mean of 3.82 years of schooling.
[27] Given that the .57-year schooling increase is a 16.5 percent increase over the control group mean of 3.46 years of schooling.

transfer amount per eligible child to obtain a full year of additional schooling—4,140 Lempiras or US$290. For Blocks 1-2, it would take just 1.75 times the current transfer amount to yield a full year—just 1,447 Lempiras or US$96. This puts *PRAF* on the low cost end of the spectrum relative to other school interventions, similar to adding more teachers to the classroom (Evans & Ghosh 2008).

VIII. CONCLUSION

This thesis found significant impacts of Honduras' *PRAF* transfer on the educational outcomes of its recipients. For the older education transfer cohort, this includes a 0.2-year increase in total schooling,[28] a 2.7-percentage point increase in school attendance,[29] and a 3.1-percentage point increase in literacy rates.[30]

The educational treatment effects were particularly large in the poorest two municipality quintiles, with a schooling increase of 0.57 years,[31] an attendance increase of 6.8 percentage points,[32] and a literacy increase of 7.6 percentage points.[33]
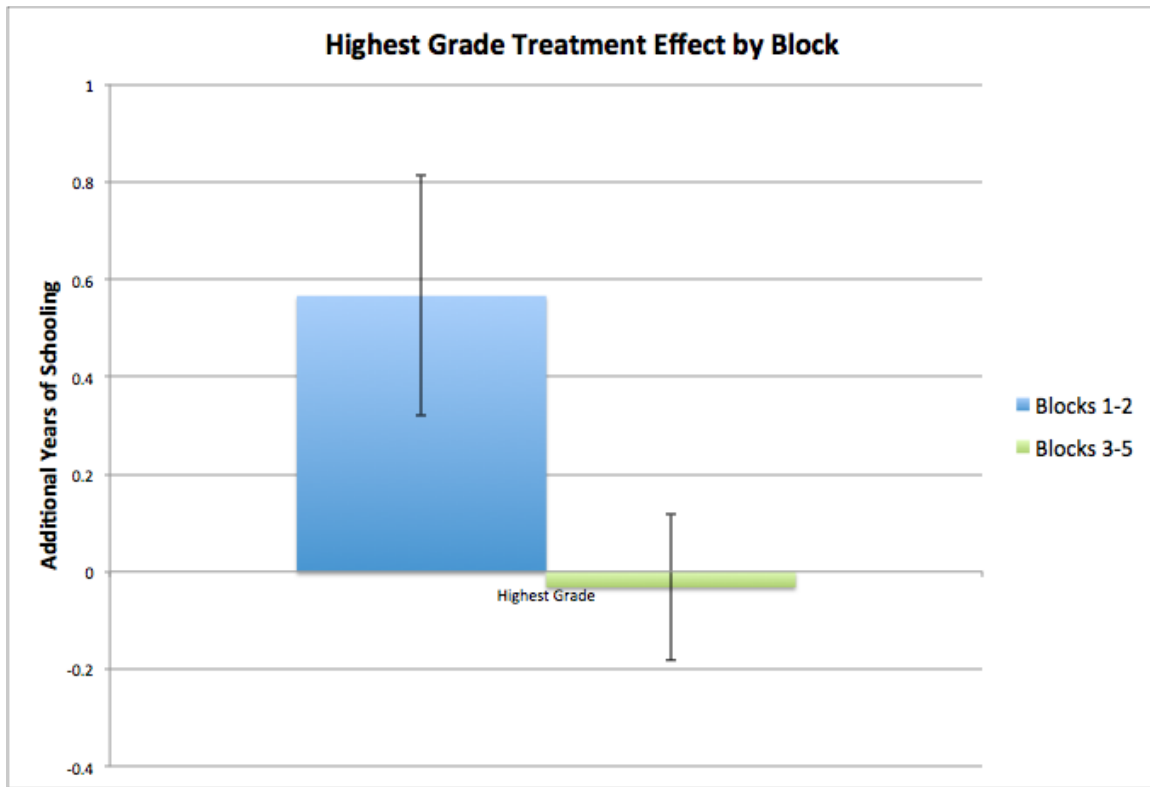
Figure 2: Highest Grade Heterogeneity by Block Group. **Notes:** Standard error bars included.

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[28] .11-year increase for the health cohort.
[29] 4.7-percentage point increase for the health cohort.
[30] 3.4-percentage point increase for the health cohort.
[31] .2 years for the health cohort.
[32] 6.2 percentage points for the health cohort.
[33] 7.4 percentage points for the health cohort.
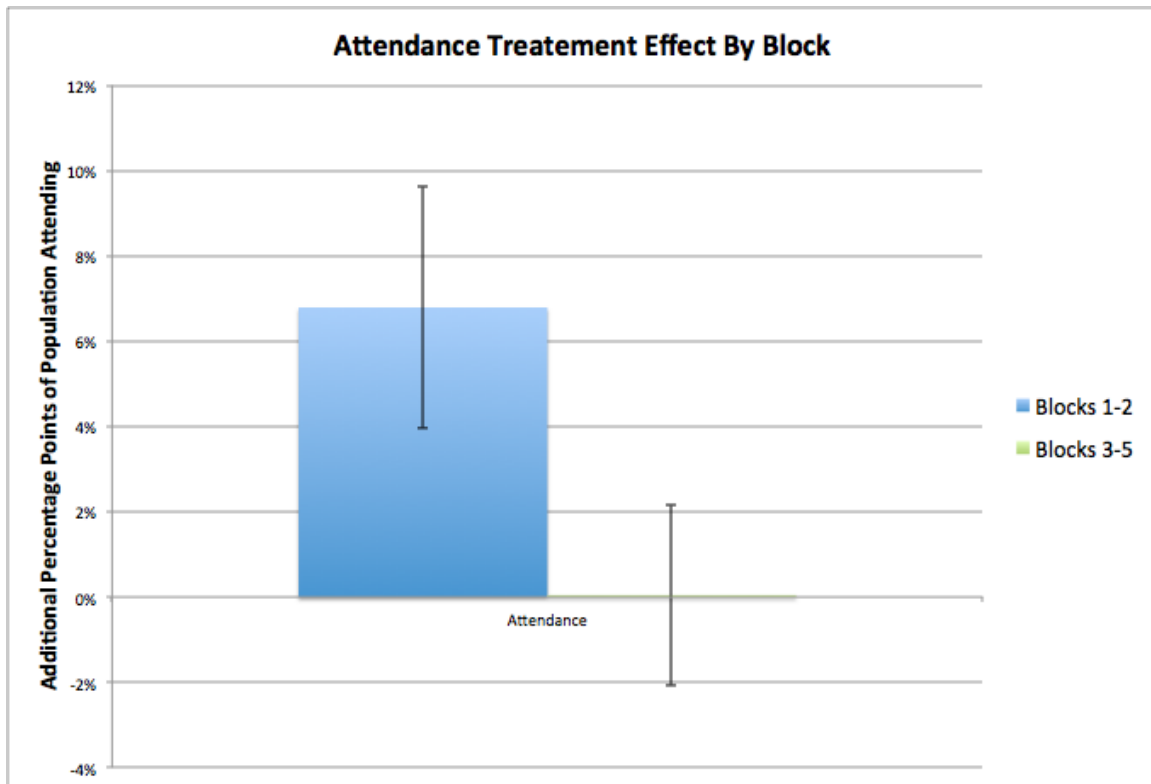
Figure 3: Attendance Heterogeneity by Block Group. **Notes:** Standard error bars included.
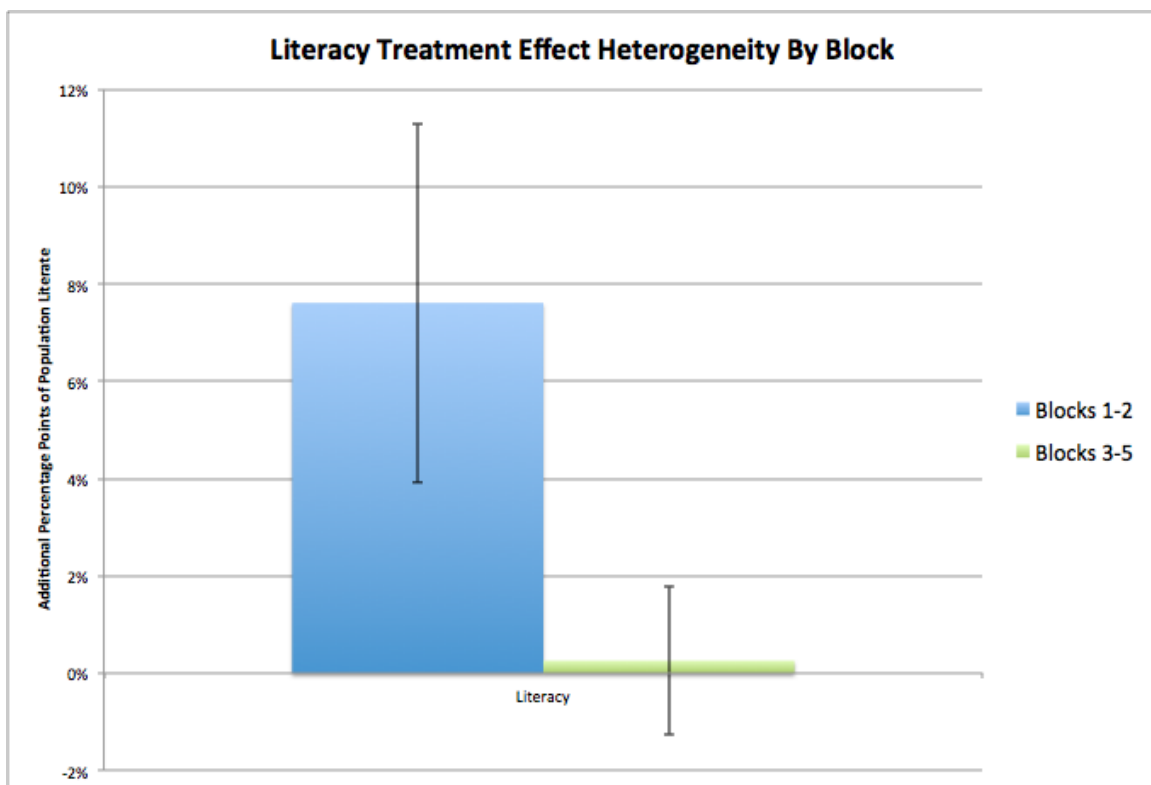
Figure 4: Literacy Heterogeneity by Block Group. **Notes:** Standard error bars included.

Especially for the overall schooling attainment treatment impact, the treatment effect increased over time, reaching a maximum of over 1 year of schooling in Blocks 1-2 in survey years 2007-9.

Highest Grade Heterogeneity By Block & Survey Year
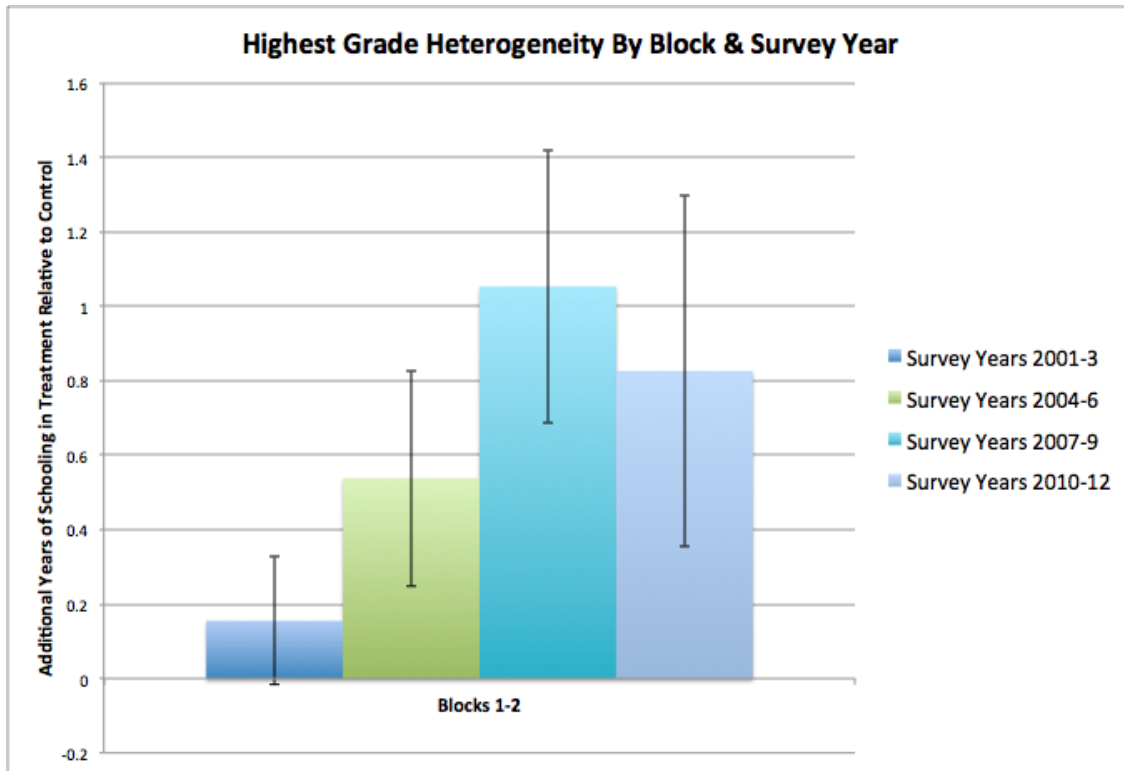
Figure 5: Block 1-2 Highest Grade Heterogeneity by Survey Year Bins. **Notes:** Standard error bars included.

This demonstrates that *PRAF* not only increased schooling during the treatment period but through the years after the last disbursement in 2002, and this effect continued to grow over time. This demonstrates that this CCT's long-term impact was not just sustainable, but mounting.

The robustness results corroborate these main results, suggesting in some cases that the true effect may be higher than the main results show. For overall educational attainment, the slightly increased coefficients using the alternative treatment designation suggest that even the large and significant results found in the main and heterogeneity sections do not fully capture the treatment effect. Additionally, the robustness results

confirm that the sample was not significantly biased by attrition, allowing the main results to hold up.

These results both add to the overall CCT literature, which is currently lacking in long-term follow-ups, but also provide guidance to policymakers inside and outside Honduras. Given Honduras' continued use of CCTs through later versions of *PRAF* and Bono 10 Mil, the results are still extremely relevant for policymakers. Future versions of Honduran CCTs should focus on targeting the poorest of the poor, possibly through means testing, rather than using geographic eligibility. At least with *PRAF-II*'s design and amount, the large effects seen in the poorest two municipality quintiles were virtually nonexistent in the upper three quintiles. The government and its partner organizations clearly got the "biggest bang for their buck" in the poorest households.

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X. APPENDIX

Table A

Survey Year Breakdowns by Treatment Designations Using Municipality of Birth

Survey Year	Sample Size			Number of Municipalities Represented in Municipality of Birth			Annotations of Key Variables
	All	Treatment	Control	All	Treatment	Control	
May 2001	30,842	570	398	290	36	30	-Missing municipality of survey -Missing municipality of birth for ages<5
May 2002	89,931	1,657	868	297	40	30	-Missing municipality of survey -Missing municipality of birth for ages<5
Sept 2002	109,465	2,274	1,239	298	40	30	
March 2003	90,653	1,837	1,055	297	40	30	-Missing municipality of survey -Missing municipality of birth for ages<5
Sept 2003	37,248	706	432	295	37	30	
May 2004	35,947	703	417	295	40	30	
May 2005	34,724	661	417	293	40	30	
Sept 2005	34,542	714	441	294	40	30	
May 2006	98,970	2,064	1,151	298	40	30	
Sep 2006	96,968	2,199	1,135	296	40	30	
May 2007	99,026	1,973	1,095	296	40	30	
Sept 2007	99,323	1,988	1,099	297	40	30	
May 2008	98,963	1,977	1,044	296	40	30	
May 2009	96,850	1,825	929	298	40	30	
May 2010	32,227	637	326	295	40	30	
May 2011	32,146	592	320	292	40	30	
May 2012	32,621	649	310	297	40	30	
Total	1,150,446	23,026	12,676				

Table B

Survey Year Breakdowns by Treatment Designations Using Municipality in 2000

Survey Year	Sample Size			Number of Municipalities Represented in Municipality in 2000		
	All	Treatment	Control	All	Treatment	Control
May 2007	84,133	1,964	1,118	295	40	30
May 2008	82,568	1,982	1,122	294	40	30
May 2009	78,817	1,885	997	296	40	30
May 2010	25,675	642	376	283	37	28
May 2011	25,099	611	337	283	37	29
May 2012	25,067	648	328	286	38	29
Total	321,359	7,732	4,278			

Table C

Variable Descriptions & Survey Year Availability

VARIABLE NAME	DESCRIPTION	DATA SOURCE	SURVEY YEARS AVAILABLE (2001-2012)
Dependent Variables			
Highest Grade Completed	Highest grade in school completed by respondent, not including the current year of schooling if currently attending	EPHPM	All
Attendance	Dummy variable indicating whether the respondent is currently attending school	EPHPM	All
Literacy	Dummy variable indicating whether the respondent knows how to read and write	EPHPM	All
Worked Last Week	Dummy variable indicating whether the respondent worked one hour or more last week, either for pay or for no pay	EPHPM	All
Hours Worked Last Week	Reported hours worked in the week before the survey for primary and secondary occupations, either for pay or for no pay	EPHPM	All
Moved	Dummy variable =0 if respondent has always lived in the same municipality; =1 if survey municipality does not match municipality of birth or if the observation was identified as living abroad	Creating using EPHPM location questions	Sep 2002, Sep 2003, 2004, May 2005, Sep 2005, May 2006, Sep 2006, May 2007, Sep 2007, 2008, 2009, 2010, 2011, 2012
Abroad	Dummy variable =0 if the respondent was in Honduras during the survey year; =1 if a household member identified the observation has having left Honduras	Creating using EPHPM migration modules	Sept 2006, 2010
Independent Variables			
Treatment (Municipality of birth)	Dummy variable =0 if municipality of birth is in control group; =1 if municipality of birth is in treatment group	Municipality-level Evaluation Data	All
Treatment (Municipality in 2000) *Used in robustness section*	Dummy variable =0 if municipality in 2000 is in control group; =1 if municipality in 2000 is in treatment group	Municipality-level Evaluation Data	May 2005, Sep 2005, May 2007, 2008, 2009, 2010, 2011, 2012
Eligible_Education	Dummy variable indicating whether the respondent	Created using birth year	All

	is age eligible for the education transfer (ages 6-12 in 2000-2002)	from EPHPM	
Eligible_Health	Dummy variable indicating whether the respondent is age eligible for the health transfer (ages 0-3 in 2000-2002)	Created using birth year from EPHPM	All
Age	Age of the respondent based on birth year	EPHPM	All
Female	Dummy variable indicating whether the respondent is male (=0) or female (=1)	EPHPM	All
Survey Year	Year and month of the survey	EPHPM	All
Birth Year	Birth year of respondent	EPHPM	All
Block	Mean height-to-age quintile of municipality	Municipality-level Evaluation Data	All
Municipality of Birth	Municipality where respondent was born	EPHPM	2001(Ages>=5), May 2002(Ages>=5), Sep 2002, Mar 2003(Ages>=5), Sep 2003, 2004, May 2005, Sep 2005, May 2006, Sep 2006, May 2007, Sep 2007, 2008, 2009, 2010, 2011, 2012
Municipality in 2000	Municipality where respondent was in the year 2000 at the point of randomization	Created from EPHPM location questions	Sep 2002, May 2007, 2008, 2009, 2010, 2011, 2012
Most recent municipality	Municipality where respondent indicates they lived before the survey municipality	EPHPM	All
Survey municipality	Municipality where respondent indicates they currently live	EPHPM	Sep 2002, Mar 2003 (inferred for non-movers from most recent municipality), Sep 2003, 2004, May 2005, Sep 2005, May 2006, Sep 2006, May 2007, Sep 2007, 2008, 2009, 2010, 2011, 2012
Years Lived Here	The number of years respondent has lived in the survey municipality	EPHPM	2001, May 2002, Sep 2002, May 2007, 2008, 2009, 2010, 2011, 2012
Treatment*Block Group	Dummy variable interactions between treatment and dummy variables indicating whether the respondent is in a municipality in Blocks 1-2 or Blocks 3-5.	Created from Municipality-level Evaluation Data & EPHPM	2001(Ages>=5), May 2002(Ages>=5), Sep 2002, Mar 2003(Ages>=5), Sep 2003, 2004, May 2005, Sep 2005, May 2006, Sep 2006, May 2007, Sep 2007, 2008, 2009, 2010, 2011, 2012
Treatment* High Exposure	Dummy variable interaction between treatment and a dummy variable indicating that the respondent was eligible for the transfer for the full transfer	Created from Municipality-level Evaluation Data & EPHPM	2001(Ages>=5), May 2002(Ages>=5), Sep 2002, Mar 2003(Ages>=5), Sep 2003, 2004, May 2005, Sep 2005, May

	period from 2000-2.		2006, Sep 2006, May 2007, Sep 2007, 2008, 2009, 2010, 2011, 2012
Treatment*Low Exposure	Dummy variable interaction between treatment and a dummy variable indicating that the respondent was eligible for only part of the transfer period from 2000-2.	Created from Municipality-level Evaluation Data & EPHPM	2001(Ages>=5), May 2002(Ages>=5), Sep 2002, Mar 2003(Ages>=5), Sep 2003, 2004, May 2005, Sep 2005, May 2006, Sep 2006, May 2007, Sep 2007, 2008, 2009, 2010, 2011, 2012
Treatment*Older Cohort	Dummy variable interaction between treatment and a dummy variable indicating whether an education cohort respondent was born in 1998-1992.	Created from Municipality-level Evaluation Data & EPHPM	2001(Ages>=5), May 2002(Ages>=5), Sep 2002, Mar 2003(Ages>=5), Sep 2003, 2004, May 2005, Sep 2005, May 2006, Sep 2006, May 2007, Sep 2007, 2008, 2009, 2010, 2011, 2012
Treatment*Younger Cohort	Dummy variable interaction between treatment and a dummy variable indicating whether an education cohort respondent was born in 1993-1996.	Created from Municipality-level Evaluation Data & EPHPM	2001(Ages>=5), May 2002(Ages>=5), Sep 2002, Mar 2003(Ages>=5), Sep 2003, 2004, May 2005, Sep 2005, May 2006, Sep 2006, May 2007, Sep 2007, 2008, 2009, 2010, 2011, 2012
Treatment*Birth Year	Dummy variable interaction between treatment and a dummy variable indicating whether a health cohort respondent was born in 1998, 1999 or 2000.	Created from Municipality-level Evaluation Data & EPHPM	2001(Ages>=5), May 2002(Ages>=5), Sep 2002, Mar 2003(Ages>=5), Sep 2003, 2004, May 2005, Sep 2005, May 2006, Sep 2006, May 2007, Sep 2007, 2008, 2009, 2010, 2011, 2012
Treatment*Survey Year	Series of dummy variable interaction terms between the treatment and survey year bins (2001-3, 2004-6, 2007-9, 2010-12)	Created from Municipality-level Evaluation Data & EPHPM	2001(Ages>=5), May 2002(Ages>=5), Sep 2002, Mar 2003(Ages>=5), Sep 2003, 2004, May 2005, Sep 2005, May 2006, Sep 2006, May 2007, Sep 2007, 2008, 2009, 2010, 2011, 2012
Treatment*Gender	Pair of dummy variable interactions between treatment and male and between treatment and female.	Created from Municipality-level Evaluation Data & EPHPM	2001(Ages>=5), May 2002(Ages>=5), Sep 2002, Mar 2003(Ages>=5), Sep 2003, 2004, May 2005, Sep 2005, May 2006, Sep 2006, May 2007, Sep 2007, 2008, 2009, 2010, 2011, 2012