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An Assessment of the Medium Term Impacts of a Honduran Conditional Cash Transfer Experiment on Education and Fertility

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Abstract

Conditional cash transfer (CCT) programs have been widely used in the developing world as a tool to break the poverty cycle. Short-term studies have found positive impacts of CCTs on education and health outcomes. However, little is known about longer-term outcomes of beneficiaries and any unintended consequences, namely on fertility, that there may be. This paper assesses medium-term (5-6 years after program implementation) impacts of a Honduran CCT, PRAF-II, on education and fertility. PRAF-II was a random experiment that provided cash transfers to 40 out of 70 poor municipalities. Conditions included school enrollment and regular health checkups. Using data from USAID's Honduran DHS Survey from 2005-2006, I find that very young children receiving cash transfers conditioned on health checkups were more likely to enroll in primary school on time. Children receiving cash transfers conditioned on school enrollment saw sustained increases in education acquisition, even after cash payments ended. Finally, I find that there was no persistent impact on fertility, as women simply moved forward their birthing decisions.

Table of Contents

I. Introduction **1**

II. Literature Review **3**

1. The Relationship Between Nutrition, Health, and Education 5
2. Short-Run Effects of CCT Programs on Education 7
   - 2.1 Effects on School Enrollment, Dropout, Attendance, and Grade Promotion 7
   - 2.2 Medium and Long-Term Effects on Schooling 12
3. Impacts on Fertility and Birth Choice 14

III. Background **16**

1. Background on Honduras 16
2. Background on PRAF-II 17

IV. Data **20**

1. Construction of Datasets and Variables Used to Measure Impacts on Education 21
2. Construction of Datasets and Variables Used to Measure Impacts on Fertility 23
3. Endogeneity Issues from Migration 23

V. Specification **24**

1. Specification Model for Measuring Impacts on Education 24
2. Specification Model for Measuring Impacts on Fertility 26

VI. Results **26**

1. Effect on Education for Health Cohort 26
2. Effect on Education for Education Cohort 29
3. Effects on Fertility 30

VII. Discussion **33**

1. Health Cohort 33
2. Education Cohort 33
3. Fertility Implications 34

VIII. Conclusion **35**

IX. References **37**

X. Tables **42**

I. Introduction

In the last two decades, conditional cash transfer (CCT) programs have grown in use across the developing world as a tool to break the poverty cycle. The goals of CCT programs are two-fold: alleviate current poverty and encourage investments in human capital accumulation. The most common conditions with which families must comply in order to receive benefits relate to health and education, as education accumulation and healthcare are widely accepted as two vital factors to breaking the poverty cycle. Families must take their children for regular health checkups, growth monitoring, and vaccinations. They must also enroll their children in school, and encourage steady attendance. In this way CCT programs encourage increased demand for good health and schooling.

Much of the current research on CCT programs has focused on impacts on beneficiaries in the short-run, or one to two years after the implementation of these programs (Fiszbein and Schady 2009; de Janvry et al. 2006; Ranganathan and Lagarde 2012). The vast literature surrounding short-run impacts has found a very positive effect on school enrollment and educational attainment. The literature has also found a positive impact of CCT receipt on health center attendance and improved health outcomes such as birth weight and height. However, there has been little research that examines the impact of CCT programs in the longer-run, as most data related to program impacts are collected shortly after program implementation (1-2 years later). The small but growing literature that does exist tends to find that effects are smaller and less significant in the longer-run (Baez and Camacho 2011; Barham, Macours, and Maluccio 2013; Behrman, Parker, and Todd 2011; Filmer and Schady 2014). There is also a wide-ranging literature that finds improved maternal and postnatal health, but little is known about unintended impacts on fertility rates. Stecklov et al. (2007) find that CCT programs in Mexico

(PROGRESA) and Nicaragua (RPS) decreased fertility rates while Honduras's *Programa de Asignación Familiar*, or PRAF-II, increased fertility.

In this paper, I investigate the medium-term impact of a Honduran CCT program, *Programa de Asignación Familiar*, or PRAF-II. PRAF-II was implemented as a randomized controlled experiment in 70 of Honduras's 298 municipalities between 2000 and 2002. It targeted very poor families and gave small cash transfers conditioned on school attendance and health center check-ups. Many short-term impact evaluations have found positive effects on enrollment and educational attainment (Glewwe and Olinto 2004; Galiani and McEwan 2013). I attempt to discern the educational impacts of PRAF-II on two cohorts of children: those who received only the health transfers and those who received the education transfers. I assess whether any short-term benefits persist into the medium-term (5-6 years after implementation). I also examine whether the findings of Stecklov et al. (2007) of increased fertility signified a permanent increase in fertility rates or simply a change in birth timing.

I use data from USAID's 2005-2006 Demographic and Health Surveys from Honduras to estimate the effects of PRAF-II on education and fertility outcomes. Because PRAF-II was implemented as a randomized controlled experiment at the municipality level, any difference in outcomes that I find between the treatment and control municipalities should represent the causal impact of PRAF-II. The DHS data reports primary sampling units, which can then be linked to municipalities, thus allowing identification of whether individuals received PRAF-II payments or not.

Results show that PRAF-II increased primary school enrollment amongst health transfer recipients by decreasing rates of delayed enrollment. PRAF-II also increased educational attainment for both health and education transfer recipients in the medium-term, showing persistence in the positive impacts from the short-run. Like Stecklov et al. (2007), I find that women tended to increase fertility after the implementation of PRAF-II. However, the magnitude of this increase is quite small, and represents only a shift in birth timing rather than a permanent increase in fertility.

II. Literature Review

Conditional cash transfers have been widely adopted, especially in Latin America, in an attempt to encourage investments in human capital accumulation. The majority of programs are implemented by governments, with a smaller subset of CCT programs run by other non-governmental organizations (Baird et al. 2014). Many of these programs have been implemented as pilot studies or experiments to allow for analysis and comparison of certain outcomes related to schooling and health. CCT programs are generally targeted towards the poorest strata of households.

CCT programs encourage increased demand in education and healthcare by imposing conditions that change the cost of these human capital investments. By providing cash to households, CCT programs induce an income effect that should increase investments. Moreover, by reducing the direct and indirect costs of schooling or health checkups, CCT programs encourage increased investments through a substitution effect. CCT programs, by indirectly promoting the use of education and health services, may also prompt households to update their beliefs about the efficacy of each service, thereby increasing the acquisition of better health and education (O. P. Attanasio, Oppedisano, and Vera-Hernandez 2015).

Critics of CCT programs often argue that the programs add unnecessary strain to administrative capacity while the marginal benefit of the conditions are unknown (Baird, McIntosh, and Ozler 2011). The poorest households may not take up benefits because the conditions may be still be too costly for compliance. Thus, the CCTs may not be targeting those most in need (Fiszbein and Schady 2009). However, applying conditions to the receipt of cash transfers often makes cash transfers more politically palatable and, if individuals are myopic or if there is little awareness about the benefits of education and improved healthcare, then the conditions may be necessary to induce take-up of education and healthcare (Fiszbein and Schady 2009).

There is a vast literature that performs impact evaluations of CCT programs. The most widely studied CCT program is Mexico's Education, Health, and Nutrition Program (PROGRESA), later renamed *Oportunidades*, as it was the first large-scale program to incorporate both health and education components (Rawlings and Rubio 2005). Later programs modeled after PROGRESA include Colombia's *Familias en Acción*, Honduras's PRAF, and Nicaragua's *Red de Protección Socia* (RPS). Outside of Latin America, CCT programs include the Zomba Cash Transfer Program in Malawi, the Punjab Education Sector Reforms Programme (PESRP) in Pakistan, the CESSP Scholarship Program in Cambodia, and a CCT experiment in West China (Schultz 2004; Benedetti, Ibarraran, and McEwan 2015; Barham, Macours, and Maluccio 2013; O. P. Attanasio, Oppedisano, and Vera-Hernandez 2015; Alderman, Hoddinott, and Kinsey 2006; Mo et al. 2013).

In the rest of this chapter, I will examine the current literature surrounding the impact of CCTs on child health outcomes, education, and fertility.

1. The Relationship Between Nutrition, Health, and Education

Undernourishment and malnourishment during both neonatal and antenatal stages of child development negatively impact short-term and long-term physical and mental growth. In Zimbabwe, higher height-for-age z-scores amongst preschoolers is associated with higher heights as adults (Alderman, Hoddinott, and Kinsey 2006). In the United States, poverty and food insecurity negatively affect children's educational attainment (Sandstrom and Huerta 2013). Children who experience food insecurity have worse reading skills than food-secure children, particularly if food insecurity occurs earlier in childhood. Victora et al. (2008) confirm these findings for a variety of other countries: undernutrition is highly correlated with shorter adult height, fewer years of schooling, and reduced economic productivity.

Developmental stunting resulting from poor nutrition often delays on-time school enrollment for poor children (Todd and Winters 2011). By delaying primary school enrollment, parents attempt to compensate for reduced cognitive development (Glewwe and Jacoby 1995). Using data from Ghana, Glewwe and Jacoby (1995) find a highly significant negative impact of height-for-age z-scores on delayed school enrollment, and no evidence that borrowing constraints or rationing spots in schools are causing the delayed enrollments. Since this study, many analyses have found similar relationships between delayed school enrollment and early childhood malnourishment. In Zimbabwe, Alderman, Hoddinott, and Kinsey (2006) find that improvements in height-for-age z-scores are associated with a younger school starting age. In Pakistan, Alderman et al. (2001) find that improved nutrition and larger height-for-age z-scores increased primary school enrollment. In Guatemala, children who experienced nutritional deficiencies led to decrease grade attainment, decreased likelihood of completing primary school and secondary school, lower cognitive test scores, and slower grade progression (Maluccio et al. 2009).

It is possible that, by attempting to improve nutrition and health, CCT programs will be able to target children's stunted development and thereby encourage on-time school enrollment. A substantial literature finds that CCT programs improve health outcomes of children, whether it be by increasing food consumption or health checkups (Fiszbein and Schady 2009).

Assessments of increased food consumption from CCT interventions abound. Hoddinott and Skoufias (2003) find that treated localities under Mexico's PROGRESA CCT increased calorie consumption by between 3% and 17.5%. Maluccio and Flores (2005) find that Nicaragua's RPS supplemented total annual per capita household expenditures by 18 percent, the majority of which was spent on food. If CCT programs are truly able to address current and long-term poverty, then the hope is that child height and weight would increase after the implementation of CCT programs. Gertler (2004), Rivera et al. (2004), and Behrman and Hoddinott (2005) all find that young children treated under Mexico's PROGRESA increased height by an average of nearly 1 cm compared to the untreated children. Gertler (2004) also finds that treatment children are 8.6% less likely to be stunted after treatment, although this is not a statistically significant result. Behrman and Hoddinott's (2005) results are subject to heterogeneity. The impact of PROGRESA on children's height was larger for more impoverished children. Rivera et al. (2004) find that, amongst the poorest and younger children, PROGRESA was associated with an increased height of about 1.1cm.

CCT programs have also been shown to successfully increase health service utilization amongst children and mothers (Glassman et al. 2013; Lagarde, Haines, and Palmer 2007; Gaarder, Glassman, and Todd 2010; Ranganathan and Lagarde 2012). In Honduras, PRAF-II resulted in increased antenatal care and routine check-ups, but no increase in post-partum check-ups (Morris et al. 2004). A newer Honduran CCT program, Bono 10,000, increased health service center use amongst very young children (Benedetti, Ibarraran, and McEwan 2015). Those in the treatment group were 2.2 percentage points more likely to report that their infant children were weighed in the month preceding the follow-up survey. Parents in the treatment group were also 4 percentage points more likely to state that the reason for their child's last visit was a regular check up. In Chile, child recipients of *Chile Solidario* cash transfers increased their rates of regular checkups (Galasso 2006). In Colombia, there was a marked increase in children taken to growth and development monitoring for those who received cash transfers (O. Attanasio et al. 2005). Similar results are found in Jamaica and Nicaragua (Fiszbein and Schady 2009).

Todd and Winters (2011) expressly examine the impact of Mexico's *Oportunidades* program on on-time enrollment. They find that early health and nutrition interventions in the form of a CCT can increase the probability of on-time school enrollment. I will add to this literature by examining whether PRAF-II led to on-time school enrollment and whether these effects persisted into the medium-run.

2. Short-Run Effects of CCT Programs on Education

2.1 Effects on School Enrollment, Dropout, Attendance, and Grade Promotion

CCT programs have a demonstrated ability to raise school enrollment rates and decrease dropout rates. Amongst the various programs already mentioned, several have implemented a randomized controlled trial. In Mexico, PROGRESA increased enrollment rates at the primary school level by a little over 1 percentage point, and at the secondary school level by about 6-7 percentage points (Schultz 2004). Using a difference-in-differences estimator, Skoufias et al.

(2001) find similar results. Attendance amongst boys increased by between 1.3 and 1.8 percentage points at the primary school level for boys and by about 5 percentage points at the secondary school level. For girls, the impact is negligible in primary school, but even larger than boys' in secondary, with recipient girls seeing an increase in attendance by between 7 and 10 percentage points, though the increase markedly drops off by age 16 (Skoufias et al. 2001).

In Nicaragua, Barham et al. (2013) exploit random variation in early versus late-treatment of RPS to analyze school enrollment. They find that, while the early-treatment group continues to receive benefits, it is more likely to be enrolled in school and miss fewer days of school compared to the late-treatment group. However, two years later, when the early-treatment group no longer receives benefits, it is less likely to be enrolled in school and miss more days of school, suggesting that the investments in human capital last only as long as the treatment. Maluccio and Flores (2005) find that RPS produced a net increase in school enrollment of 13 percentage points. In Nicaragua, Barham et al. (2013) and Maluccio and Flores (2005) also find positive effects of the CCT on grade progression. The early treatment group received about a half-grade more of education by 2002, a quantity that was sustained through 2004 even after cash receipt ended (Barham, Macours, and Maluccio 2013). Maluccio and Flores (2005) report that, between 2000 and 2002, the number of children in grades one through four advancing two grades increased by 7.4 percentage points under RPS.

In Brazil, the effect of *Bolsa Escola* on reducing dropout rates was highly statistically significant. Using a differences-in-differences method, de Janvry et al. (2012) find that the program reduced dropout rates by around 8 percentage points, or around 66%. Glewwe and Kassouf (2012) find that *Bolsa Escola* increased enrollment by between 5.5 and 6.5% and lowered dropout rates by about 0.5 percentage points. *Bolsa Escola* improved grade promotion rates by 0.9 percentage points for younger grades (1-4) and 0.3 percentage points for older grades (5-8) (Glewwe and Kassouf 2012).

In Honduras, PRAF-II, the focus of this study, has had similarly positive impacts. Glewwe and Olinto (2004) examine the differences in impact in 2001 and 2002. They find that in both 2001 and 2002, the impact of the program is positive and statistically significant at the 1% level on school enrollment, although the impact is smaller in 2002 than in 2001. Galiani and McEwan (2013) find similar results. Children in the treatment groups are between 7 and 8.3 percentage points more likely to attend school. The effects of PRAF-II on daily attendance, were also very positive. The program was able to reduce the number of days absent by a large and statistically significant (1% level) amount (Glewwe and Olinto 2004). The Bono 10,000 program showed similar increases in school enrollment. The overall effect was to increase likelihood of enrollment by 3.8 percentage points for the treatment group. Glewwe and Olinto (2004) find that PRAF-II had a positive and statistically significant impact on grade promotion for the treatment group in 2002 but not in 2001. Since Glewwe and Olinto find no change in dropout rates between 2001 and 2002, they hypothesize that the difference in grade promotion is due to increased learning.

Outside of Latin America, randomized controlled trials have been conducted in Malawi and China. In a study conducted between 2008 and 2009 in Malawi, households were randomly selected to receive a conditional cash transfer, an unconditional cash transfer (UCT), or nothing. Baird et al. (2011) find that dropout rates in the control group are 5-6 percentage points higher than in both the conditional and unconditional groups. The CCT branch was also more effective at lowering dropout rates compared to the UCT branch, significant at the 5% level. What's more, the impact seems to have persisted through 2010, a year after the payments ended in 2009.

Attendance in the CCT arm is also about 8 percentage points higher than the control group's rate. This works out to be about 10 school days per year, a substantial amount. In China, Mo et al. (2013) examine the impact of a CCT program on students in 7th grade. They find that the groups receiving the CCT consistently had dropout rates 8 percentage points lower than treatment group. After controlling for school-level effects, the effect dropped a small amount, to 7 percentage points.

Other evaluations have used quasi-experimental methods and have yielded similar results. In Colombia, *Familias en Acción* led to significant increases in the probability of school enrollment (O. Attanasio et al. 2006). In Pakistan, PESRP gave girls a stipend conditional on being enrolled in grades 6-8 (Chaudhury and Parajuli 2010). Chaudhury and Parajuli (2010) find that the program was able to increase enrollment by around 6 students, or an 8% increase amongst girls.

There is substantial heterogeneity in CCT impact on school enrollment. The effect depends on a variety of factors including baseline enrollment characteristics, school grade, age, socioeconomic status, and design of the CCT (the size of the transfer and timing of payments).

In Honduras, PRAF-II increased overall enrollment by about 7 to 8 percentage points, but much of this effect was due to increases in enrollment amongst the poorest in the sample (Galiani and McEwan 2013). Specifically, those in the two poorest blocks saw a 17.8 percentage point and 10.4 percentage point increase in likelihood of enrollment, but in the three wealthier blocks, the positive impact was smaller and not statistically significant. Glewwe and Olinto (2004) find that in 2002, households with higher per capita expenditures were not affected by the program in a significant way, but those with lower per capita expenditures experienced an impact on school enrollment about two times the average. In addition, children from poorer households were less likely, compared to children from wealthier households, to drop out of school. They also find heterogeneity in grade progression results: the impact of the program is stronger amongst poorer households (Glewwe and Olinto 2004). Bono 10,000 also showed differential impacts by poverty level, number of children, and grade level at time of enrollment. The 3.8 percentage point greater probability of being enrolled in school cited above was largely due to the 6.2 percentage point greater probability of students in grades 4-6 being enrolled in school (Benedetti, Ibarraran, and McEwan 2015). The effect of the program was less positive and insignificant for other grade levels. The program also impacted children from poorer households more than children from wealthier households.

In Colombia, the effect the CCT on school enrollment for children ages 8-13 in rural areas was greater than for children in urban areas: 7 percentage points versus 5 percentage points (O. Attanasio et al. 2006). The effect of the CCT on younger children is smaller, although the pattern between rural and urban areas was still prevalent. Children in rural areas saw an effect just over 2 percentage points, while children in urban areas saw an effect of just over 1 percentage point (O. Attanasio et al. 2006). In China, the conditional cash transfer was less effective at preventing dropouts for students who performed better on baseline tests—it reduced dropout rates by only 3 percentage points compared to the 7 percentage point average (Mo et al. 2013). The CCT was also more effective with wealthier students and girls, although the difference was insignificant.

Given the heterogeneity in effects found in this literature, I will assess the educational impact of PRAF-II by income level and by gender.

2.2 Medium and Long-Term Effects on Schooling

Despite the vast amount of knowledge on the short-run effects of CCTs, there is little evidence on medium-term or long-term effects on schooling. What studies do exist generally find that the positive impacts of school enrollment were sustained through the long-term.

In Mexico, Behrman, Parker and Todd (2011) examine schooling outcomes after five and half years of an 18-month exposure to the PROGRESA/*Oportunidades*. They find that both boys and girls increased grade attainment by an average of one-fifth of a grade. The largest effects are found for those who had completed 5 years of schooling before the first cash transfer in 1997. They also find that the positive increases in enrollment and attainment found in the short-run do not diminish over time. With five years of exposure, estimated impacts are even larger. Boys aged 9-10 prior to the program start date accumulate 1.0 more grades of schooling than comparable boys. Boys 11-12 accumulated an additional 0.9 grades of schooling, and boys 13-15 accumulate about 0.5 years of additional schooling. Girls between 9-12, on the other hand, accumulate 0.7 to 0.8 years of additional schooling, with no significant impact for older girls (Behrman, Parker, and Todd 2011). This may be because girls show higher enrollment rates at baseline.

Fernald, Gertler, and Neufeld (2009) examine PROGRESA/*Oportunidades*'s impact on schooling outcomes 10 years after the program began. They find that *Oportunidades*'s had no significant long-term impacts on verbal or cognitive test scores, although the authors note several reasons why they may have found no impact. These reasons include measurement tests that were not sensitive enough to detect differences and that the additional exposure time of the early-exposure group may not have been long enough to effect change.

In a 10-year follow up study, Barham, Macours, and Maluccio (2013) examine only boys in Nicaragua's RPS program. They find persistent gains in educational attainment of nearly half a grade for boys who had longer exposure to the program compared to boys with shorter exposure. This result suggests that boys in the late treatment group either dropped out or were too old to receive transfers, and did not re-enroll once transfers became available to their regions. Early-treatment boys also showed greater achievement on Spanish language and math tests.

In a nine-year follow-up study of Colombia's *Familias en Acción*, participant children were found to be between 3.3 and 4.5 percentage points more likely to finish high school compared to non-participant children (Baez and Camacho 2011). Baez and Camacho find heterogeneous results by gender. OLS and matching estimates yield positive and statistically significant impacts on high school graduation rates for girls, while for boys the effect is unclear. OLS estimates indicate no effect while using a matching model yields positive results significant at a 10% level (Baez and Camacho 2011). Children in rural areas also benefit more from the CCT than children in urban areas.

When looking at the impact of the CESSP Scholarship Program (CSP) in Cambodia, Filmer and Schady (2014) find that children receiving the scholarship were 19 percentage points more likely to be enrolled in school and spent 5.6 more hours in school as a result of the scholarship almost five years after the baseline survey was taken. This estimate is very large relative to the estimates of program impact in other countries. Filmer and Schady hypothesize that the large impact may be because weaker students continue to remain in school, as the CSP scholarship primarily helped retain school children with lower test scores (Filmer and Schady 2014).

3. Impacts on Fertility and Birth Choice

Much debate surrounds unintended consequences of financial support to poor parents on fertility outcomes. However, the majority of the research done in this field has focused on more highly developed countries. Recent literature suggests that policies providing financial support do not result in large changes in fertility, supporting the idea that parents choose instead to invest in quality rather than quantity of children (Gauthier 2007). It is also possible that in the face of cash transfers, women simply change the timing of their fertility decisions, rather than changing the number of children they choose to have.

While the impact of CCTs, at least in the short-term, on health and education is well-established, there is little evidence regarding the impact of CCTs on marriage and fertility decisions. It is possible that, as mentioned above, cash transfers could induce women to increase fertility. However, with the health and education conditions, it is also possible that the increased knowledge could decrease fertility rates.

In a gender-targeted Pakistani CCT, Alam et al. (2011) find that recipient girls delay marriage by 1.4 years and have 0.3 fewer children, although these results are only marginally statistically significant. Amarante et al. (2011) find that, in Uruguay, CCTs led to a decline in out-of-wedlock births but an increase in total fertility. In Malawi, Baird, McIntosh, and Ozler (2011) find that CCTs had no effect on marriage or pregnancy rates. In another study of the same Malawian CCT program, girls enrolled in school at baseline are found to desire fewer children and want to delay birth timing (Baird et al. 2013). Baird et al. (2013) also find that marriage rates declined only amongst girls not enrolled in school at baseline. The difference in outcomes is largely due to increased enrollment amongst drop-out girls who then decreased marriage and pregnancy rates. Amongst baseline schoolgirls, the marginal effect was very small.

Stecklov et al. (2007) review three Latin American CCTs, PRAF-II in Honduras, RPS in Nicaragua, and PROGRESA in Mexico for impact on fertility and marriage rates. Their results suggest that the impact on fertility rates is dependent on CCT design. They measure fertility in two ways: the probability that a woman had a child in the 12 months preceding the follow-up survey (p(B)), and the probability that a woman had a child in the 12 months preceding the follow-up survey *or* the probability that a woman is pregnant at time of the survey (p(BCP)). Using a difference-in-differences estimation, they find that RPS and PROGRESA had essentially no effect on fertility. In Honduras, however, they find that PRAF-II increased p(B) and p(BCP) by 1.7 and 3.9 percentage points, respectively, but only the effect on p(BCP) is statistically significant. RPS and PROGRESA distributed lump-sum cash transfers to households, so more children meant that more people had to meet conditions without receiving extra cash. PRAF-II payments were dependent on the number of eligible recipients, so an increase in the number of children meeting the requirements also meant an increase in the number of payments households received. Moreover, PRAF-II maintained an open roster so that, as households bore more children, cash transfer size would increase. For these reasons, it is possible that PRAF-II would increase fertility while RPS and PROGRESA would not.

In this paper, I will add to the existing literature on CCTs in three ways. First, I examine the impact of health CCT receipt (health cohort) on education outcomes. By 2005-2006, when the data I use was collected, children in the health cohort were quite young, at between 6-9 years old, so gains in education are likely due to improvements in early childhood nutrition. Next, I assess the impact of education CCT receipt (education cohort) on education outcomes 5-6 years after the program's implementation. I assess whether the positive short-run effects on enrollment and grade progression continue into the medium-term. Lastly, I examine the impact of PRAF-II on fertility rates, and attempt to determine whether PRAF-II resulted in an overall increase in children, or simply a change in birth timing.

III. Background

1. Background on Honduras

Honduras, once home to several Mesoamerican cultures until Spanish colonization, became an independent nation in 1821. After decades of military rule, it now operates as a republic. The country is 90% mestizo (mixed Amerindian and European), 7% Amerindian, 2% black, and 1% white ("The World Factbook 2013-2014"). Honduras is designated as a lower middle income country by the World Bank, and as of 2014, has a GDP per capita of $5,000 (2015 US$) ("The World Factbook 2013-2014"). Its population size is 7.962 million ("World DataBank"). Poverty rates tend to be higher amongst rural and indigenous people ("The World Factbook 2013-2014"). Its Gini coefficient, as of 2013, is 53.7 compared to Brazil's 52.9, Mexico's 48.1 (in 2012), and Colombia's 53.5 ("GINI Index (World Bank Estimate)").

Educational attainment remains a concern in Honduras, despite the fact that primary school enrollment is near 100% ("The World Factbook 2013-2014"). Secondary school enrollment is much lower, at only about 50% ("UNICEF Honduras Statistics").

Honduras's population growth rate has slowed since the 1990s, but is still at around 2%, due to the fact that women have an average of 2.78 children each ("The World Factbook 2013-2014"). An average of 7.1% of children are reported as underweight ("The World Factbook 2013-2014").

2. Background on PRAF-II

PRAF-II was the second iteration of the Honduran PRAF program, which began in 1990. PRAF, or *Programa de Asignación Familiar*, was originally intended to act as compensation for the poor after a series of structural adjustment policies, provided for by the International Monetary Fund and World Bank, that adversely affected the extremely poor. The goal of the first iteration of PRAF, henceforth referred to as PRAF-I, was to prevent poor families from falling below a critical consumption level. The program attempted to improve nutrition, health, and education. Funding for PRAF-I originally came from the Inter-American Development Bank (IADB) and the World Bank.

PRAF-I was unable to fulfill its original goal on several fronts. The targeting mechanism to find the appropriate households that would receive the cash transfers was flawed. Teachers and health center employees determined which children in their jurisdiction were from the poorest households and were thus eligible for the cash transfers. This subjectivity resulted in politically motivated household choices. This poor targeting mechanism meant that there was significant leakage of funds towards higher-income households. Moore (2008) reports that the average income of urban Hondurans who benefited from the program was nearly 60 lempiras higher than the income of urban non-beneficiaries.

Because of these weaknesses of the original PRAF-I program, the IADB launched PRAF-II in 1998 with input from the International Food Policy Research Institute (IFPRI). PRAF-II was meant to serve as a prototype for a remodeled PRAF-I. PRAF-II, unlike PRAF-I, which was originally intended to compensate the poor for structural adjustment policies, was designed to increase human capital accumulation, particularly by improving education and health, to decrease chronic poverty. Through demand-side incentives, namely CCTs, PRAF-II attempted to increase demand for education and health. PRAF-II also included supply-side interventions that attempted to improve health and education infrastructure. These supply-side interventions, however, were largely ineffective as they were poorly implemented.

The demand-side interventions consisted of two CCTs: an education transfer and a health transfer. Table 1 presents an overview of both transfers.

The education transfer was open to children between the ages of 6 and 12 years old who had not yet finished the fourth grade. They were required to be enrolled in school within the first month of the academic year (March), miss no more than 20 days per school year, and not repeat a grade more than once. The enrollment requirement was strictly enforced, but attendance was poorly monitored, so for most families, only the enrollment requirement was enforced. Each child could receive 644 lempiras, or US$ 43 per person, per year. A maximum of three children per family were eligible to receive the transfer (Caldés, Coady, and Maluccio 2006).

The health transfer was open to pregnant or nursing mothers and children three or younger. In order to receive the transfer, eligible mothers had to make five pre-natal visits during the pregnancy and attend a post-partum check-up. Eligible children had to attend nutritional and health check-ups. The transfers summed to an approximate 812 lempiras, or US$54 per person, per year. The transfers were valid for up to two children per family (Caldés, Coady, and Maluccio 2006).

Cash payments were distributed twice a year. On average, yearly transfer amounts per household comprised a mere 5% of total preprogram household expenditure of experimental municipalities (Galiani and McEwan 2013).

IFPRI assisted in designing a targeting mechanism that would better restrict the conditional cash transfers to the poorest families. Targeting in PRAF-II was designed at the municipality level. In order to find the municipalities with the highest malnutrition levels, each of the 298 municipalities was ordered from lowest to highest values of mean height-for-age z-scores of 1st graders based on data collected from the 1997 school census. The 70 municipalities with the lowest z-scores were then divided into 5 quintiles of 14 municipalities. From each quintile, 4 municipalities were randomly chosen to receive only the CCTs; 4 were chosen to receive the CCTs and supply-side investments; 2 were selected to receive only the supply-side investment, and 4 were selected to receive nothing at all. Because of the poor implementation of the supply-side investments, the randomization effectively divided the municipalities into 2 groups: the treatment group received CCTs and the control group did not. In total, there were 40 municipalities in the treatment group and 30 in the control. In October 1999, there was a public randomization ceremony that announced the 40 municipalities in the treatment group and the 30 municipalities in the control. The baseline survey occurred between August and December 2000 and the follow-up survey occurred between May and August 2002. The first payments were made in November 2000 (Galiani and McEwan 2013).

PRAF-II had two qualities that subjected it to a possible unintentional increase in fertility. Unlike other CCT programs, PRAF-II kept its eligibility roster open (Stecklov et al. 2007). In other words, as women became pregnant, or as children turned 6 years old, they were added to the roster to receive the transfers. In the Nicaraguan RPS program, eligibility was closed after the baseline census was conducted, with only minimal changes to the roster. In the Mexcian PROGRESA program, the roster was closed after initial targeting, but was updated after three years. Moreover, PRAF-II was a per-person transfer, rather than the per-household transfer structure of RPS and PROGRESA (Stecklov et al. 2007). Stecklov et al. (2007) find that this combination of design characteristics of PRAF-II resulted in an increase in fertility. Because of this finding and mixed results from an intermediate analysis of the program, PRAF-II was redesigned between 2002 and 2003 so that payments were awarded at the household level and not on a per-child basis. I thus use the 2002 follow-up survey dates (May-August 2002) as the end date of this PRAF-II design.

PRAF-II fully ended in 2006. The new iteration of PRAF, or PRAF-III, was implemented in late 2007 and early 2008 (Moore 2008). Thus, there is no reason to believe that PRAF-III's new design would contaminate estimates of medium-term impacts of PRAF-II since the data I use is from 2005-2006. It is possible that any PRAF-II payments after 2002 will change the interpretation of impacts from a brief, 2-year exposure of PRAF-II. However, the majority of the children in my sample would have aged out of receiving payments by 2006, when PRAF-II fully ended, so any impact can still be interpreted as the results of a 2-year exposure to a CCT. Many of the impacted women, too, would no longer have been eligible for payments.

IV. Data

This paper uses data from the 2005-2006 Women's Honduran Demographic and Health Surveys (DHS) conducted by ICF International and USAID. The survey data was collected between October 2005 and May 2006, approximately 6 years after the baseline survey was conducted and the first transfer was completed. DHS surveys collect information on fertility, maternal health, child health, mortality and morbidity, and basic information about education.

Data gathered from the DHS surveys are organized into several datasets. I use the Household dataset and the Individual (women) dataset. Since the survey covers all Honduran municipalities, it was necessary to identify the individuals residing in the 70 municipalities in this experiment. The datasets constructed from the Women's DHS Survey does not include municipality information, only information about the primary sampling unit. In order to identify the municipality in which a woman resides, it was necessary to use data from the men's 2005-2006 DHS survey, which includes both municipality and primary sampling unit information. I was thus able to identify the individuals in the treatment and control group within the women's datasets.

1. Construction of Datasets and Variables Used to Measure Impacts on Education

To measure PRAF-II's impact on education, I separately assess the impact of the health cash transfers and the education cash transfers. To evaluate the impact of the health cash transfers, I identify the children who would have received health cash transfers. I call these children the health cohort. To evaluate the impact of the education cash transfers, I identify the children who would have received education cash transfers, or the education cohort.

To construct my final health cohort dataset, I use the household-level dataset and identify those individuals who were between 0 and 3 years of age in 2000, or between 6 and 9 at the time of the DHS survey. I chose not to include children who would have aged into the program, i.e. born after 2000 as those children would not have been old enough to be enrolled in school and therefore would have no available education data.

Table 2 presents summary statistics of independent control variables that should be exogenous to PRAF-II. The reported p-values indicate that all independent variables are statistically the same.

To construct my final education cohort dataset, I use the household-level dataset from the women's survey and identify those children who were between 6 and 12 years old between 2000 and 2002. The end dataset included children between the ages of 9 and 17 if interviewed in 2005, and between ages 10 and 18 if interviewed in 2006.

Table 3 presents summary statistics of independent control variables that should be exogenous to PRAF-II. All reported p-values suggest that all means are statistically the same except for mother's years of education. It is unclear why there is this statistical difference, but the difference is quite small, only about one-tenth of the average education level for mothers in this sample.

Outcome variables of interest measure the impact of PRAF-II on quantity of schooling. Quantity is measured by dummy variables indicating whether or not a child has attended primary school or secondary school. Quantity is also measured by a dummy variable measuring whether or not a child has completed or is enrolled in a certain grade level. Table 4 includes a list of dependent variables and definitions used to measure education outcomes.

In Table 5, I present summary statistics of education outcomes for the health cohort as of the 2005-2006 survey. It appears that the treated children are more likely to be enrolled and attending primary school, have acquired more grade levels, and have completed grades 1 and 2. Treated children, however, appear less likely to have completed grade 3. The raw differences can be interpreted as the causal impact of PRAF-II, due to the randomized nature of PRAF-II's cash payments.

In Table 6, I present summary statistics of education outcomes for the education cohort. Treated children appear more likely to have attended primary school and grades 1-6. PRAF-II does not seem to have had any impact on secondary school attendance or completion of grade 7.

2. Construction of Datasets and Variables Used to Measure Impacts on Fertility

To construct the dataset used to measure fertility, I use a woman-level dataset from the DHS survey results. To identify women who could have been impacted by the PRAF-II, I identify women who were at least 14 years of age between 2000 and 2002. While age 14 may seem young, many women in Honduras, particularly those in poor areas, already have children at that age. Stecklov et al. (2007) use age 14 as their lower bound age for measuring fertility as well.

In Table 7, I present means of independent variables used in my analysis that should be exogenous to PRAF-II. It is concerning that the average woman's years of education is statistically different between treatment and control groups. The difference between the two is about half a grade, or about one-tenth of the average years of women's education, which is quite small.

The primary outcome variables of interest to measure fertility quantity are conception flow variables. They measure the probability of a woman giving birth in each year of the program, as well as each year after the program's end for 3 years. Conception date was calculated as 40 weeks or 280 days, the average duration of a pregnancy, before the birth date. Table 8 includes a list of dependent variables and definitions used to measure fertility outcomes. In Table 9, conceptions seem to have increased in the first year of the program for those treated, but seem to fall afterwards.

3. Endogeneity Issues from Migration

An issue with reported impacts may result from individuals migrating into treatment areas after the start of the experiment. It is possible that individuals who move into treatment areas are fundamentally different from those who do not, which would bias estimated results. In order to prevent such bias, I remove individuals who moved into treatment areas after December 2000, or the end date of the baseline survey.

V. Specification

Because Honduran municipalities were randomly assigned to be in treatment and control groups, I use a simple OLS model to assess the impact of PRAF-II on various education and fertility measures. My main assumption is that prior to PRAF-II's implementation, treatment and control municipalities are largely the same. Unfortunately, I am unable to test this assumption because I do not have data prior to 2005/2006. However, prior literature has tested this assumption and has found that treatment and control groups are largely the same (Galiani and McEwan 2013; Glewwe and Olinto 2004).

1. Specification Model for Measuring Impacts on Education

To assess program impact on education for both health and education cohorts, I run the following regression (Equation 1):

$$Outcome_{ijk} = \beta_0 + \beta_1 Treatment_{jk} + X_{ijk}\mu + \delta_k + \varepsilon_{ijk}$$

where $Outcome_{ijk}$ measures various education outcomes, including the probability that an individual attended primary school or secondary school for individual *i* in municipality *j* in strata *k*. $Outcome_{ijk}$ also measures whether an individual has attended a certain grade level.

$Treatment_{jk}$ is a dummy variable indicating whether an individual resided in a municipality that received cash transfers. The coefficient of interest, β_1, measures the impact of the program on education outcomes, in other words, the estimated difference in educational outcomes between individuals receiving cash transfers and individuals who did not receive any cash transfers. It is possible to interpret this coefficient as a causal effect because of the random assignment of municipalities to receive or not receive cash transfers. As such, any difference in outcome will not be influenced by systematic differences between treatment and control groups.

I add X_{ijk}, a vector of control variables, to increase precision of estimates of β_1. Control variables include the child's age, gender, mother's years of education, and father's years of education. I also add in strata-fixed effects, or δ_k, to control for the impact of place of residency on β_1, as unobserved variation across municipalities may impact education results.

I also assess any heterogeneity in program impact by strata and gender. The equation I use to assess heterogeneity by stratum is (equation 2):

$$Outcome_{ijk} = \beta_0 + \beta_1 Strata_k{}^H + \beta_2(Treatment_{jk} * Strata_k{}^L) + \beta_3(Treatment_{jk} * Strata_k{}^H) + X_{ijk}\mu + \delta_k + \varepsilon_{ijk}$$

where $(Treatment_{jk} * Strata_k{}^L)$ and $(Treatment_{jk} * Strata_k{}^H)$ are interaction terms between an individual's assignment type and strata with *H* indicating relatively higher-income strata and *L* indicating relatively lower-income strata. The coefficient on the interaction terms indicate the impact of the CCT on treated kids in higher-income strata relative to untreated kids in higher-income strata and treated kids in lower-income strata relative to untreated kids in lower-income strata. Division of the 5 strata into high and low income groups is modeled after Galiani and McEwan (2013).

The equation I use to assess heterogeneity by gender is (equation 3):

$$Outcome_{ijk} = \beta_0 + \beta_1 Boy_i + \beta_2(Treatment_{jk} * Boy_i) + \beta_3(Treatment_{jk} * Girl_i) + X_{ijk}\mu + \delta_k + \varepsilon_{ijk}$$

where $(Treatment_{jk} * Boy_i)$ and $(Treatment_{jk} * Girl_i)$ are interaction terms between an individual's assignment type and gender. The coefficient on the interaction terms indicate the impact of the CCT on treated boys relative to untreated boys and treated girls relative to untreated girls.

2. Specification Model for Measuring Impact on Fertility

I again use a simple OLS regression model (equation 1) to estimate program impact on various fertility measures. To assess program impact on conceptions, my dependent variables of interest are the year-to-year flow of conceptions during the program's duration. I control for the woman's age and education level. I again assess any heterogeneity of PRAF-II by strata using Equation 2.

VI. Results

1. Effect on Education for Health Cohort

I examine how receipt of conditional cash transfers affected education outcomes five to six years after PRAF-II was implemented. I begin my analysis with children who were between 0 and 3 years old at the beginning of the program and received only the health cash transfer.

Because PRAF-II was implemented as a random experiment, the difference in the mean outcomes should approximate program impact. The odd columns of Table 10 and 11 provide differences in means from summary statistics, with samples clustered by municipality. The results of these regressions suggest that PRAF-II was able to improve primary school enrollment and early grade level attainment.

Table 10 reports the impact of PRAF-II receipt on primary school enrollment and total grade level attainment. Panel A reports the effect of PRAF-II on the entire sample of eligible children. Children in the treatment group were 4.1 percentage points more likely to be enrolled in primary school, although this difference is not statistically significant (p-value of 0.104). After controlling for a number of different variables, children in the treatment group were 5.9 percentage points more likely to be enrolled in primary school, a result that is statistically significant at the 5% level. Columns 3 and 4 of Table 10 report the impact of PRAF-II receipt on total grade attainment. Recipient children acquire an average 0.059 more grade levels than their counterparts. Though this is not statistically significant, this increase in grade level acquisition is reflected in the improved likelihood of students having completed grades 1 and 2, as seen in Panel A of Table 11. The impact of PRAF-II on grade level completion is largest in grade 2, with recipient children 4.5 percentage points more likely to have completed 2nd grade. This result is significant at the 5% level. By 3rd grade, the positive effects of PRAF-II have diminished, and receiving PRAF-II actually results in a decreased likelihood of having completed 3rd grade, although this impact is small and insignificant (p-values of 0.708 for column 5 and 0.612 for column 6).

Previous literature has found that CCT program effects differ substantially by income levels. In Honduras, PRAF-II is a small cash transfer, meant only to offset the costs of seeking health check-ups or school. In poorer families, the cash transfer may be more necessary for offsetting the costs of check-ups or school attendance. Thus, the impact of PRAF-II may be larger for poorer families. However, it is also possible that the cash transfer is not large enough to offset the cost of check-ups or school attendance, in which case the impact of PRAF-II may be larger for wealthier families.

In Panel B of Table 10, I report primary school enrollment impacts by strata. For children in lower-income strata, cash transfer receipt increases the likelihood of primary school enrollment by 5.7 percentage points, statistically significant at the 10% level. For children in the higher-income strata, cash transfer receipt increases the likelihood of primary school enrollment by 6.2 percentage points, also statistically significant at the 10% level. I fail to reject the null hypothesis that these coefficients are equal. Interestingly, despite the similar impact of PRAF-II on primary school enrollment for children in both strata, children in higher income strata see a decrease in total grade level acquisition, although the impact is not statistically different from 0.

In Panel B of Table 11, I examine the probability of individual grade completion by strata. The discrepancy between improvements in school enrollment and total grade completion between children in higher and lower-income strata seen in Panel B of Table 10 seems to be due mostly to PRAF-II's much larger impact in grade 1 for lower-income strata and the decrease in probability of grade 3 completions for children in higher-income strata.

I also assess heterogeneity in impact between boys and girls in Panel C of Tables 10 and 11. Girls who receive the cash transfer increase their likelihood of primary school attendance by 5 percentage points compared to girls who did not receive cash transfers. Boys who receive the cash transfer increase their likelihood of on-time enrollment in primary school by 6.8 percentage points. The impact on total grade level acquisition is about the same, although boys see a statistically significant impact on grade 2 completion while girls do not (Column 4 of Panel C of Table 11).

2. Effect on Education for Education Cohort

I next measure the effect of cash transfer receipt on education outcomes for children who would have received the education cash transfer. Children in this cohort must have been between the ages of 6 and 12 during the program years to receive the cash transfer.

The odd columns of Tables 12-14 provide raw differences in means between treatment and control group, with clustering by municipality. These differences suggest that the education cash transfer had a positive, although statistically indistinguishable from 0, effect on primary school enrollment and early grade level completion.

I find no significant impact of being in a treatment group on the probability of attending primary school or secondary school or grade level attainment. However, all coefficients are positive except for probability of having attended secondary school (Panel A of Table 12). In Tables 13 and 14, I examine the probability of having completed a specific grade. Recipients are more likely to have completed all grades except for grade 7 and 8, which matches the decreased likelihood of attending secondary school.

In Panel B of Table 12, I assess heterogeneity in the impact of PRAF-II by strata. The effect of PRAF-II seems to be much more positive in the lower income than in the higher income strata. In fact, it seems that the decline in attendance of secondary school can be attributed to a 4.5 percentage point decrease in probability of having attended secondary school amongst recipients in the higher income strata. The most compelling impact of PRAF-II seems to be on grade level for recipients in lower-income strata. Those individuals acquire an average of 0.465 more grade levels than non-recipients. Recipients in higher-income strata acquire only 0.07 more grade levels than non-recipients. Likewise, recipients in lower-income strata see larger improvements in grades 1-8 completion (Panel B of Tables 13 and 14) compared to non-recipient counterparts.

In Panel C of Table 12, I assess heterogeneity in impact of PRAF-II by gender. Boys see a greater positive impact on primary school attendance, although girls see a larger impact on grade level acquisition. Both effects are statistically insignificant. This is also reflected in the probabilities of having completed individual grades. The impact on recipient boys for grade 1 and 2 completion is larger than the impact for recipient girls. However, after grade 3, the impact on recipient girls is larger through 8th grade (Panel C of Tables 13 and 14).

3. Effects on Fertility

I first look for an impact on fertility by attempting to replicate the results from Stecklov et al. (2007). They measure fertility using two variables. The first, p(B), is the probability that a woman had a child in the 12 months preceding the follow-up survey. The second, p(BCP) is the probability that a woman had a child in the 12 months preceding the follow-up survey *or* that a woman is pregnant at time of the survey. They find that PRAF-II increased p(B) and p(BCP) by 1.7 and 3.9 percentage points, respectively, but only the effect on p(BCP) is statistically significant.

I was unable to replicate their results. My estimates suggest that PRAF-II decreased p(B) by 0.4 percentage points and increased p(BCP) by 0.9 percentage points (Table 15). 90% confidence intervals on my estimated impact of PRAF-II also do not overlap with 90% confidence intervals attached to the results from Stecklov et al. (2007).

I continue to assess the impact of PRAF-II on fertility by looking at the impact on variables that measure the flow of fertility. Doing so should allow me to assess whether women were having more children overall or if they were simply pushing forward their decision to have children. Because Stecklov et al. (2007) did not use data that included the years following PRAF-II's conclusion, they are unable to measure such a flow change.

In Panel A of Table 16, I first measure the flow of conceptions for all women aged 14 and older during the duration of the program. The results suggest that women in the treated group conceive 0.012 more children during the program and 0.007 more children after the program. These results, although statistically insignificant, seem to suggest that treated women were not changing the timing of their fertility decisions, but were more likely to have children overall.

However, when I limit my sample to only women who would have been affected by PRAF-II, I obtain different results. Because the health transfer was only given to women who had fewer than 2 children, there should be no fertility effect on women with 2 or more children during the program's duration. To check that this is true, I limit my sample of women to those who had 2 or more children at the start of the program. I find that women in the treatment group conceive 0.008 fewer children during the program duration and conceive 0.015 more children following the program's end (Panel B of Table 16). Thus, women who had 2 or more children at the start of the program were not induced to have children and instead seemed to delay childbearing.

In Panel C of Table 16, I examine the effect on women who had fewer than 2 children at the program's introduction, i.e. 1 or fewer children. Treated women conceived 0.04 more children during the program and after the program's end, treated women conceived 0.006 fewer children, although neither of these effects are statistically significant. In Panel C of Table 17, we see that most of the increase in conceptions during the program occurred in the first year, when treated women conceived 0.035 more children than women in the control group. In the second year, treated women were only increasing conception by 0.005 more children. After the program ends, however, treated women with 1 or fewer children prior to the start of the program were less likely to have children compared to their untreated counterparts, as seen in columns 2 and 6 of Panel C of Table 18.

When I further limit the sample to women who had no children at the program's start, it becomes even clearer that women were only changing the timing of their fertility decisions, and that PRAF-II did not induce sustained increased fertility rates. In Panel D of Table 16, we see that, although treated women with no children at the program's start conceived 0.036 more children during the program than untreated women with no children, after the program's end, treated women conceived 0.058 fewer children. This result is further corroborated by looking at the yearly flow of conceptions. In Table 17, we see that in the first year of the program, treated women with 0 children beforehand conceived 0.038 more children, a value that is statistically significant at the 5% level. But, by the second year, they in fact conceive 0.002 fewer children. The magnitude of this decline grows larger following the end of the program, to 0.041 fewer children 1 year after, 0.015 more children 2 years after, and 0.032 fewer children 3 years after (Panel D of Table 18). Despite the increase in year 2, overall, these results suggest that women who had no children at the program's beginning were most likely to change their fertility timing decisions.

VII. Discussion

1. Health Cohort

It is noteworthy that PRAF-II was able to increase primary school enrollment amongst the health cohort. There is a substantial literature that examines the role of childhood nutrition on educational outcomes. Glewwe and Jacoby (1995) find that children in low-income countries tend to delay primary school enrollment due to early childhood malnutrition. The size of the cash transfer was small, a mere 5% of average household expenditure (Galiani and McEwan 2013). Thus, the improvement in school enrollment should not be due to an income effect. Moreover, by 2005 and 2006, when the DHS survey was conducted, the children in this cohort would have been between 5 and 9 years old. At this age, children are unlikely to have dropped out of school, so the positive impact of PRAF-II is not from preventing drop-outs. The improvement in primary school enrollment is thus likely due to the improved health and nutrition of recipient children, whether that be from increased use of health services or an improved understanding of health and nutrition.

2. Education Cohort

The impact of PRAF-II on education outcomes is largely positive, but insignificant. I attempt to determine whether these positive outcomes reflect a positive, sustained impact of PRAF-II on education, or if the results simply point to an initial increase in educational attainment that drops off after the program's end.

To do so, I estimate the expected average grade level increase if children were to leave school once CCT receipt ended, and compare that to my coefficient estimate of grade level increase for recipients. Galiani and McEwan (2013) find that after one year of PRAF-II receipt, PRAF-II was able to increase enrollment by an average of 8 percentage points. If this impact is sustained for only two years, I estimate that this would yield an average grade level increase of 0.16 grades. In Column 6 of Panel A of Table 12, I find that beneficiaries of PRAF-II completed an average of 0.234 more grade levels than non-beneficiaries. This suggests that the positive schooling enrollment effects of PRAF-II found in the short-term persist past the two years of program implementation. For recipient children in strata 1 and 2, Galiani and McEwan (2013) find that enrollment increased by 14.9 percentage points, which would yield an average of 0.298 more grade levels for recipients, if there was no sustained impact past the experiment's end date. I estimate an average increase in grade level attainment of 0.465 grades (Column 6 of Panel B of Table 12), which again suggests the positive enrollment effects in the short-term persisted into the medium-term.

From this back-of-the-envelope calculation, it is clear that PRAF-II did result in sustained improvements in education. This suggests that, even after the transfer of cash ended, recipient children chose to stay in school or progressed through grades at a higher rate than non-recipient children.

3. Fertility Implications

As mentioned in the previous section, there seemed to be an increase in fertility during the program's duration, but this increase dropped off after cash transfers ended. This suggests that women were simply changing the timing of their childbearing, and not choosing to have more children, as Stecklov et al. (2007) imply. It is possible that, if cash transfers had continued, then women would have continued to have more children. However, I think this is unlikely since women with 0 or 1 or fewer children prior to the program's start increased their fertility during the first year of the program. By year 2, treated women were not having children at a much higher rate than untreated women, supporting the idea that women were only moving forward their decisions to have children.

VIII. Conclusion

This paper analyzes the impact of a Honduran CCT program, PRAF-II, using DHS survey data from 2005-2006. PRAF-II was designed as a randomized controlled experiment, giving 40 out of 70 municipalities cash transfers. PRAF-II awarded two types of cash transfers: health and education. Health cash transfers were available to very young children and pregnant women who went to routine health check-ups. Education cash transfers were available to children between 6-12 of age who were enrolled in school and maintained good attendance.

My paper adds to the existing literature by examining medium-term impacts of cash transfers on education and possible unintended consequences on fertility rates. I find that PRAF-II was able to improve on-time primary school enrollment amongst young children who received the cash transfers. The impact appears greater for lower-income strata and girls, although all impacts are statistically insignificant. PRAF-II also saw sustained short-term increases in school enrollment and grade acquisition into the medium term. Fertility rates did not experience a sustained increased. Rather, fertility rates spiked during the first year of the program and leveled off after the program's end for women in treated municipalities.

My results are largely indistinguishable from 0, due to the lack of power in the USAID DHS dataset. It would be useful to redo this analysis with a more powerful dataset, and examine if short-term benefits of PRAF-II truly were sustained into the medium term, and to see if PRAF-II resulted only in a change in birth timing, rather than a change in the total stock of births. To truly test whether PRAF-II and other conditional cash transfer programs are able to break the poverty cycle, future research should follow the outcomes of beneficiaries into adulthood.

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X. Tables

Table 1: Transfer types and conditions

	Education Transfer	Health Transfer
Requirements	• Enrolled in grades 1-4 • <20 absences • No repetition of a grade more than once	• Women: 5 pre-natal visits, 1 post-partum • Children: Regularly attend check-ups
Beneficiaries	• Children ages 6-12	• Pregnant women • Children under age 3
Transfer Size	• US $43 per child, per year	• US $54 per person, per year
Quantity	• Max 3 transfers per household for a total of 6 cash payments per household per year	• Max 2 transfers per household for a total of 4 cash payments per household per year

Table 2: Independent variable means for health cohort

	Treatment	Control	P-value
Age	7.504	7.511	1
Female	0.525	0.464	1
Mother's years of education	3.374	3.316	0.734
Mother's education missing	0.080	0.068	1
Father's years of education	3.153	3.026	0.393
Father's education missing	0.288	0.282	1

Notes: Sample includes children ages 0-3 in 2000.

Table 3: Independent variable means for education cohort

	Treatment	Control	P-value
Age	13.463	13.518	0.616
Female	0.474	0.458	1
Mother's years of education	3.004	2.716	0.007
Mother's education missing	0.202	0.217	1
Father's years of education	2.948	2.834	0.201
Father's education missing	0.369	0.396	1

Notes: Sample includes children ages 6-12 between 2000 and 2002.

Table 4: Education outcome variables and definitions

Variable	Definition
Primary	Indicator variable for attending primary school
Secondary	Indicator variable for attending secondary school
Grade level	Total number of grade levels a child has completed
Grade 1	Indicator variable for having completed 1st grade
Grade 2	Indicator variable for having completed 2nd grade
Grade 3	Indicator variable for having completed 3rd grade
Grade 4	Indicator variable for having completed 4th grade
Grade 5	Indicator variable for having completed 5th grade
Grade 6	Indicator variable for having completed 6th grade
Grade 7	Indicator variable for having completed 7th grade
Grade 8	Indicator variable for having completed 8th grade

Table 5: Education summary statistics for PRAF-II effect on health cohort

	Treatment	Control
Attended primary school	0.707	0.666
Grade level	1.228	1.176
Completed grade level		
Grade 1	0.680	0.642
Grade 2	0.384	0.348
Grade 3	0.139	0.151

Notes: Sample includes children who were ages 0-3 in 2000.

Table 6: Education summary statistics for PRAF-II effects on education cohort

	Treatment	Control
Attended primary school	0.975	0.946
Attended secondary school	0.136	0.135
Grade level	4.833	4.600
Completed grade level		
Grade 1	0.975	0.945
Grade 2	0.942	0.892
Grade 3	0.843	0.814
Grade 4	0.716	0.680
Grade 5	0.574	0.543
Grade 6	0.444	0.425
Grade 7	0.137	0.138
Grade 8	0.097	0.083

Notes: Sample includes children who were 6-12 between 2000 and 2002.

Table 7: Independent variable means for sample measuring fertility outcomes

	Treatment	Control	P-value
Woman's Age	30.961	30.748	0.555
Woman's Years of Education	4.209	3.695	0

Notes: Sample includes women who were 14 years or older between 2000 and 2002.

Table 8: Fertility outcome variables and definitions

Variable	Definition
pB	Indicator variable for having given birth in the year prior to the follow-up survey
pBCP	Indicator variable for having given birth in the year prior to the follow-up survey, or pregnant at the time of the follow-up survey
Conceptions during	Number of children a woman conceived during PRAF-II
Conceptions After	Number of children a woman conceived in the three years post PRAF-II
Year 1	Number of children a woman conceived in the first year of PRAF-II
Year 2	Number of children a woman conceived in the second year of PRAF-II
Post 1	Number of children a woman conceived in the first year after PRAF-II
Post 2	Number of children a woman conceived in the second year after PRAF-II
Post 3	Number of children a woman conceived in the third year after PRAF-II

Table 9: Fertility summary statistics for PRAF-II effects on fertility

	Treatment	Control
Number of conceptions		
During	0.321	0.315
During year 1	0.163	0.156
During year 2	0.158	0.158
After	0.395	0.401
1 year after	0.140	0.146
2 years after	0.155	0.150
3 years after	0.100	0.105

Notes: Sample includes women who were 14 years or older between 2000 and 2002.

Table 10: Impact on primary school attendance and total grade level attainment for health cohort

VARIABLES	(1) Primary	(2) Primary	(3) Grade level	(4) Grade level
Panel A				
Treatment	0.041	0.059**	0.052	0.059
	(0.024)	(0.023)	(0.100)	(0.073)
Observations	1,124	1,124	1,037	1,037
R-squared	0.002	0.370	0.001	0.562
Panel B				
Treatment*Strata^{L}	0.036	0.057*	0.091	0.147
	(0.042)	(0.027)	(0.125)	(0.112)
Treatment*Strata^{H}	0.048	0.062*	0.021	-0.012
	(0.040)	(0.035)	(0.089)	(0.060)
Observations	1,124	1,124	1,037	1,037
R-squared	0.002	0.370	0.007	0.563
Panel C				
Treatment*Girl	0.035	0.050	0.047	0.058
	(0.040)	(0.032)	(0.126)	(0.087)
Treatment*Boy	0.040*	0.068**	0.026	0.059
	(0.022)	(0.027)	(0.091)	(0.076)
Observations	1,124	1,124	1,037	1,037
R-squared	0.005	0.370	0.009	0.562
Strata fixed effects and Controls	No	Yes	No	Yes

Notes: *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors are presented in parentheses. Sample is clustered by municipality and includes children who were ages 0-3 in 2000. Controls include age, gender, mother's years of education, father's years of education, and indicator variables for missing data on mother and father's educations.

Table 11: Impact on individual grade level attainment in health cohort

VARIABLES	(1) Grade 1	(2) Grade 1	(3) Grade 2	(4) Grade 2	(5) Grade 3	(6) Grade 3
Panel A						
Treatment	0.037	0.033	0.037	0.045**	-0.012	-0.016
	(0.028)	(0.027)	(0.037)	(0.021)	(0.032)	(0.032)
Observations	1,037	1,037	1,037	1,037	1,037	1,037
R-squared	0.001	0.441	0.001	0.449	0.000	0.216
Panel B						
Treatment*Strata^L	0.055	0.064*	0.034	0.060	0.018	0.024
	(0.045)	(0.035)	(0.043)	(0.039)	(0.039)	(0.042)
Treatment*Strata^H	0.021	0.008	0.044	0.033	-0.042	-0.049
	(0.038)	(0.029)	(0.034)	(0.023)	(0.033)	(0.031)
Observations	1,037	1,037	1,037	1,037	1,037	1,037
R-squared	0.003	0.441	0.008	0.450	0.004	0.218
Panel C						
Treatment*Girl	0.040	0.034	0.022	0.032	-0.000	-0.002
	(0.042)	(0.034)	(0.041)	(0.026)	(0.037)	(0.033)
Treatment*Boy	0.026	0.032	0.041	0.058**	-0.033	-0.030
	(0.025)	(0.030)	(0.040)	(0.021)	(0.029)	(0.034)
Observations	1,037	1,037	1,037	1,037	1,037	1,037
R-squared	0.006	0.441	0.006	0.450	0.010	0.216
Strata fixed effects	No	Yes	No	Yes	No	Yes

Notes: *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors are presented in parentheses. Sample is clustered by municipality and includes children who were ages 0-3 in 2000. Controls include age, gender, mother's years of education, father's years of education, and indicator variables for missing data on mother and father's educations.

Table 12: Impact on primary and secondary school attendance and total grade level attainment in education cohort

VARIABLES	(1) Primary	(2) Primary	(3) Secondary	(4) Secondary	(5) Grade level	(6) Grade level
Panel A						
Treatment	0.029	0.041	0.000	-0.012	0.233	0.234
	(0.021)	(0.024)	(0.032)	(0.026)	(0.284)	(0.273)
Observations	2,324	2,324	2,324	2,324	2,291	2,291
R-squared	0.006	0.025	0.000	0.168	0.003	0.288
Panel B						
Treatment*Strata^L	0.033	0.054	0.054	0.035	0.504	0.465
	(0.030)	(0.043)	(0.046)	(0.038)	(0.357)	(0.434)
Treatment*Strata^H	0.027	0.031	-0.051*	-0.045*	0.014	0.070
	(0.021)	(0.020)	(0.028)	(0.026)	(0.268)	(0.259)
Observations	2,324	2,324	2,324	2,324	2,291	2,291
R-squared	0.008	0.025	0.006	0.171	0.013	0.290
Panel C						
Treatment*Girl	0.019	0.031	-0.000	-0.018	0.365	0.330
	(0.018)	(0.020)	(0.034)	(0.023)	(0.247)	(0.242)
Treatment*Boy	0.037	0.049	-0.000	-0.007	0.112	0.151
	(0.027)	(0.030)	(0.034)	(0.034)	(0.332)	(0.324)
Observations	2,324	2,324	2,324	2,324	2,291	2,291
R-squared	0.007	0.025	0.002	0.169	0.005	0.289
Strata fixed effects and Controls	No	Yes	No	Yes	No	Yes

Notes: *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors are presented in parentheses. Sample is clustered by municipality and includes children who were ages 6-12 between 2000 and 2002. Controls include age, gender, mother's years of education, father's years of education, and indicator variables for missing data on mother and father's educations.

Table 13: Impact on individual grade 1-4 attainment in education cohort

VARIABLES	(1) Grade 1	(2) Grade 1	(3) Grade 2	(4) Grade 2	(5) Grade 3	(6) Grade 3	(7) Grade 4	(8) Grade 4
Panel A								
Treatment	0.030	0.042	0.050*	0.063**	0.029	0.039	0.036	0.038
	(0.022)	(0.025)	(0.029)	(0.029)	(0.036)	(0.031)	(0.051)	(0.049)
Observations	2,291	2,291	2,291	2,291	2,291	2,291	2,291	2,291
R-squared	0.006	0.025	0.008	0.040	0.001	0.087	0.001	0.153
Panel B								
Treatment*Strata^{L}	0.034	0.056	0.052	0.072	0.047	0.055	0.082	0.075
	(0.031)	(0.045)	(0.036)	(0.054)	(0.036)	(0.046)	(0.060)	(0.077)
Treatment*Strata^{H}	0.028	0.032	0.051*	0.056*	0.021	0.028	0.002	0.011
	(0.021)	(0.021)	(0.028)	(0.028)	(0.041)	(0.038)	(0.049)	(0.050)
Observations	2,291	2,291	2,291	2,291	2,291	2,291	2,291	2,291
R-squared	0.008	0.026	0.014	0.040	0.011	0.087	0.014	0.154
Panel C								
Treatment*Girl	0.020	0.031	0.043	0.055*	0.052	0.060**	0.092*	0.091
	(0.019)	(0.021)	(0.029)	(0.029)	(0.030)	(0.028)	(0.048)	(0.053)
Treatment*Boy	0.038	0.051	0.055	0.070*	0.008	0.022	-0.015	-0.009
	(0.028)	(0.031)	(0.034)	(0.035)	(0.044)	(0.038)	(0.061)	(0.055)
Observations	2,291	2,291	2,291	2,291	2,291	2,291	2,291	2,291
R-squared	0.007	0.026	0.009	0.040	0.006	0.087	0.007	0.155
Strata fixed effects and Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors are presented in parentheses. Sample is clustered by municipality and includes children who were ages 6-12 between 2000 and 2002. Controls include age, gender, mother's years of education, father's years of education, and indicator variables for missing data on mother and father's educations.

Table 14: Impact on individual grade 5-8 attainment in education cohort

VARIABLES	(1) Grade 5	(2) Grade 5	(3) Grade 6	(4) Grade 6	(5) Grade 7	(6) Grade 7	(7) Grade 8	(8) Grade 8
Panel A								
Treatment	0.032	0.032	0.019	0.018	-0.001	-0.015	0.014	0.000
	(0.052)	(0.055)	(0.045)	(0.047)	(0.033)	(0.026)	(0.024)	(0.017)
Observations	2,291	2,291	2,291	2,291	2,291	2,291	2,291	2,291
R-squared	0.001	0.234	0.000	0.290	0.000	0.170	0.001	0.151
Panel B								
Treatment*Strata^L	0.087	0.081	0.060	0.056	0.053	0.031	0.050	0.024
	(0.063)	(0.082)	(0.067)	(0.072)	(0.046)	(0.039)	(0.032)	(0.024)
Treatment*Strata^H	-0.013	-0.003	-0.016	-0.009	-0.052*	-0.047*	-0.020	-0.016
	(0.054)	(0.056)	(0.046)	(0.051)	(0.029)	(0.026)	(0.023)	(0.018)
Observations	2,291	2,291	2,291	2,291	2,291	2,291	2,291	2,291
R-squared	0.010	0.235	0.004	0.291	0.006	0.173	0.004	0.152
Panel C								
Treatment*Girl	0.050	0.043	0.027	0.019	-0.001	-0.020	0.027	0.009
	(0.053)	(0.055)	(0.042)	(0.042)	(0.035)	(0.024)	(0.025)	(0.015)
Treatment*Boy	0.015	0.022	0.011	0.017	-0.001	-0.010	0.002	-0.007
	(0.060)	(0.061)	(0.051)	(0.055)	(0.035)	(0.035)	(0.025)	(0.024)
Observations	2,291	2,291	2,291	2,291	2,291	2,291	2,291	2,291
R-squared	0.002	0.234	0.001	0.290	0.002	0.170	0.002	0.151
Strata fixed effects and Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors are presented in parentheses. Sample is clustered by municipality and includes children who were ages 6-12 between 2000 and 2002. Controls include age, gender, mother's years of education, father's years of education, and indicator variables for missing data on mother and father's educations.

Table 15: Replication of Stecklov et al. (2007) results

VARIABLES	(1) pB	(2) pB	(3) pBCP	(4) pBCP
Treatment	-0.010	-0.004	0.003	0.009
	(0.018)	(0.014)	(0.021)	(0.016)
Age		-0.001		-0.002***
		(0.001)		(0.001)
Yrs of Education		-0.014***		-0.019***
		(0.002)		(0.002)
Constant	0.214***	0.316***	0.298***	0.477***
	(0.017)	(0.033)	(0.020)	(0.027)
Observations	2,728	2,728	2,728	2,728
R-squared	0.000	0.016	0.000	0.021
Strata fixed effects	No	Yes	No	Yes

Notes: *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors are presented in parentheses. Sample is clustered by municipality and includes women who were ages 12-47 in 2000. Controls include age and years of education.

Table 16: PRAF-II impact on conception during and after program

VARIABLES	(1) Conceptions during	(2) Conceptions during	(3) Conceptions after	(4) Conceptions after
Panel A				
Treatment	0.006	0.012	-0.006	0.007
	(0.022)	(0.017)	(0.024)	(0.019)
Observations	2,692	2,692	2,692	2,692
R-squared	0.000	0.020	0.000	0.069
Panel B				
Treatment	-0.016	-0.008	0.004	0.015
	(0.025)	(0.019)	(0.036)	(0.027)
Observations	1,536	1,536	1,536	1,536
R-squared	0.000	0.084	0.000	0.140
Panel C				
Treatment	0.036	0.040	-0.020	-0.006
	(0.031)	(0.031)	(0.034)	(0.032)
Observations	1,156	1,156	1,156	1,156
R-squared	0.001	0.008	0.000	0.030
Panel D				
Treatment	0.037	0.036	-0.056	-0.058
	(0.028)	(0.026)	(0.038)	(0.038)
Observations	825	825	825	825
R-squared	0.002	0.005	0.002	0.024
Strata fixed effects and Controls	No	Yes	No	Yes

Notes: *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors are presented in parentheses. Panel A sample includes women who were ages 14 or older between 2000 and 2002. Panel B sample is restricted to women who had 2 or more children prior to PRAF-II. Panel C sample is restricted to women who had 1 or fewer children prior to PRAF-II. Panel D sample is restricted to women who had no children prior to PRAF-II. Samples cluster by municipality. Controls include age and years of education.

Table 17: PRAF-II impact on flow of conceptions during program

VARIABLES	(1) Year 1	(2) Year 1	(3) Year 2	(4) Year 2
Panel A				
assignment2	0.006	0.013	-0.000	-0.001
	(0.016)	(0.015)	(0.013)	(0.011)
Observations	2,692	2,692	2,692	2,692
R-squared	0.000	0.013	0.000	0.011
Panel B				
assignment2	-0.009	-0.004	-0.007	-0.004
	(0.017)	(0.017)	(0.021)	(0.019)
Observations	1,536	1,536	1,536	1,536
R-squared	0.000	0.026	0.000	0.060
Panel C				
assignment2	0.027	0.035	0.009	0.005
	(0.024)	(0.021)	(0.020)	(0.020)
Observations	1,156	1,156	1,156	1,156
R-squared	0.001	0.014	0.000	0.003
Panel D				
assignment2	0.034	0.038**	0.003	-0.002
	(0.022)	(0.015)	(0.022)	(0.021)
Observations	825	825	825	825
R-squared	0.003	0.007	0.000	0.005
Strata FE and Controls	No	Yes	No	Yes

Notes: *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors are presented in parentheses. Panel A sample includes women who were ages 14 or older between 2000 and 2002. Panel B sample is restricted to women who had 2 or more children prior to PRAF-II. Panel C sample is restricted to women who had 1 or fewer children prior to PRAF-II. Panel D sample is restricted to women who had no children prior to PRAF-II. Samples cluster by municipality. Controls include age and years of education.

Table 18: PRAF-II impact flow of conceptions after program end

VARIABLES	(1) Post 1	(2) Post 1	(3) Post 1	(4) Post 2	(5) Post 3	(6) Post 3
Panel A						
assignment2	-0.006	-0.000	0.004	0.008	-0.005	-0.001
	(0.015)	(0.012)	(0.016)	(0.014)	(0.014)	(0.015)
Observations	2,692	2,692	2,692	2,692	2,692	2,692
R-squared	0.000	0.020	0.000	0.024	0.000	0.023
Panel B						
assignment2	0.002	0.005	-0.007	-0.002	0.009	0.012
	(0.022)	(0.018)	(0.024)	(0.022)	(0.017)	(0.016)
Observations	1,536	1,536	1,536	1,536	1,536	1,536
R-squared	0.000	0.032	0.000	0.061	0.000	0.037
Panel C						
assignment2	-0.017	-0.008	0.020	0.021	-0.023	-0.018
	(0.019)	(0.019)	(0.017)	(0.019)	(0.022)	(0.022)
Observations	1,156	1,156	1,156	1,156	1,156	1,156
R-squared	0.001	0.015	0.001	0.010	0.001	0.012
Panel D						
assignment2	-0.046***	-0.041**	0.022	0.015	-0.032	-0.032
	(0.016)	(0.016)	(0.018)	(0.019)	(0.026)	(0.026)
Observations	825	825	825	825	825	825
R-squared	0.004	0.018	0.001	0.007	0.003	0.011
Strata FE	No	Yes	No	Yes	No	Yes

Notes: *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors are presented in parentheses. Panel A sample includes women who were ages 14 or older between 2000 and 2002. Panel B sample is restricted to women who had 2 or more children prior to PRAF-II. Panel C sample is restricted to women who had 1 or fewer children prior to PRAF-II. Panel D sample is restricted to women who had no children prior to PRAF-II. Samples cluster by municipality. Controls include age and years of education.

Table 11: Impact on individual grade level attainment in health cohort

VARIABLES	(1) Grade 1	(2) Grade 1	(3) Grade 2	(4) Grade 2	(5) Grade 3	(6) Grade 3
Panel A						
Treatment	0.037	0.033	0.037	0.045**	-0.012	-0.016
	(0.028)	(0.027)	(0.037)	(0.021)	(0.032)	(0.032)
Observations	1,037	1,037	1,037	1,037	1,037	1,037
R-squared	0.001	0.441	0.001	0.449	0.000	0.216
Panel B						
Treatment*Strata^L	0.055	0.064*	0.034	0.060	0.018	0.024
	(0.045)	(0.035)	(0.043)	(0.039)	(0.039)	(0.042)
Treatment*Strata^H	0.021	0.008	0.044	0.033	-0.042	-0.049
	(0.038)	(0.029)	(0.034)	(0.023)	(0.033)	(0.031)
Observations	1,037	1,037	1,037	1,037	1,037	1,037
R-squared	0.003	0.441	0.008	0.450	0.004	0.218
Panel C						
Treatment*Girl	0.040	0.034	0.022	0.032	-0.000	-0.002
	(0.042)	(0.034)	(0.041)	(0.026)	(0.037)	(0.033)
Treatment*Boy	0.026	0.032	0.041	0.058**	-0.033	-0.030
	(0.025)	(0.030)	(0.040)	(0.021)	(0.029)	(0.034)
Observations	1,037	1,037	1,037	1,037	1,037	1,037
R-squared	0.006	0.441	0.006	0.450	0.010	0.216
Strata fixed effects	No	Yes	No	Yes	No	Yes

Notes: *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors are presented in parentheses. Sample is clustered by municipality and includes children who were ages 0-3 in 2000. Controls include age, gender, mother's years of education, father's years of education, and indicator variables for missing data on mother and father's educations.

Table 12: Impact on primary and secondary school attendance and total grade level attainment in education cohort

VARIABLES	(1) Primary	(2) Primary	(3) Secondary	(4) Secondary	(5) Grade level	(6) Grade level
Panel A						
Treatment	0.029	0.041	0.000	-0.012	0.233	0.234
	(0.021)	(0.024)	(0.032)	(0.026)	(0.284)	(0.273)
Observations	2,324	2,324	2,324	2,324	2,291	2,291
R-squared	0.006	0.025	0.000	0.168	0.003	0.288
Panel B						
Treatment*Strata^{L}	0.033	0.054	0.054	0.035	0.504	0.465
	(0.030)	(0.043)	(0.046)	(0.038)	(0.357)	(0.434)
Treatment*Strata^{H}	0.027	0.031	-0.051*	-0.045*	0.014	0.070
	(0.021)	(0.020)	(0.028)	(0.026)	(0.268)	(0.259)
Observations	2,324	2,324	2,324	2,324	2,291	2,291
R-squared	0.008	0.025	0.006	0.171	0.013	0.290
Panel C						
Treatment*Girl	0.019	0.031	-0.000	-0.018	0.365	0.330
	(0.018)	(0.020)	(0.034)	(0.023)	(0.247)	(0.242)
Treatment*Boy	0.037	0.049	-0.000	-0.007	0.112	0.151
	(0.027)	(0.030)	(0.034)	(0.034)	(0.332)	(0.324)
Observations	2,324	2,324	2,324	2,324	2,291	2,291
R-squared	0.007	0.025	0.002	0.169	0.005	0.289
Strata fixed effects and Controls	No	Yes	No	Yes	No	Yes

Notes: *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors are presented in parentheses. Sample is clustered by municipality and includes children who were ages 6-12 between 2000 and 2002. Controls include age, gender, mother's years of education, father's years of education, and indicator variables for missing data on mother and father's educations.

Table 13: Impact on individual grade 1-4 attainment in education cohort

VARIABLES	(1) Grade 1	(2) Grade 1	(3) Grade 2	(4) Grade 2	(5) Grade 3	(6) Grade 3	(7) Grade 4	(8) Grade 4
<u>Panel A</u>								
Treatment	0.030	0.042	0.050*	0.063**	0.029	0.039	0.036	0.038
	(0.022)	(0.025)	(0.029)	(0.029)	(0.036)	(0.031)	(0.051)	(0.049)
Observations	2,291	2,291	2,291	2,291	2,291	2,291	2,291	2,291
R-squared	0.006	0.025	0.008	0.040	0.001	0.087	0.001	0.153
<u>Panel B</u>								
Treatment*$Strata^L$	0.034	0.056	0.052	0.072	0.047	0.055	0.082	0.075
	(0.031)	(0.045)	(0.036)	(0.054)	(0.036)	(0.046)	(0.060)	(0.077)
Treatment*$Strata^H$	0.028	0.032	0.051*	0.056*	0.021	0.028	0.002	0.011
	(0.021)	(0.021)	(0.028)	(0.028)	(0.041)	(0.038)	(0.049)	(0.050)
Observations	2,291	2,291	2,291	2,291	2,291	2,291	2,291	2,291
R-squared	0.008	0.026	0.014	0.040	0.011	0.087	0.014	0.154
<u>Panel C</u>								
Treatment*Girl	0.020	0.031	0.043	0.055*	0.052	0.060**	0.092*	0.091
	(0.019)	(0.021)	(0.029)	(0.029)	(0.030)	(0.028)	(0.048)	(0.053)
Treatment*Boy	0.038	0.051	0.055	0.070*	0.008	0.022	-0.015	-0.009
	(0.028)	(0.031)	(0.034)	(0.035)	(0.044)	(0.038)	(0.061)	(0.055)
Observations	2,291	2,291	2,291	2,291	2,291	2,291	2,291	2,291
R-squared	0.007	0.026	0.009	0.040	0.006	0.087	0.007	0.155
Strata fixed effects and Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors are presented in parentheses. Sample is clustered by municipality and includes children who were ages 6-12 between 2000 and 2002. Controls include age, gender, mother's years of education, father's years of education, and indicator variables for missing data on mother and father's educations.

Table 14: Impact on individual grade 5-8 attainment in education cohort

VARIABLES	(1) Grade 5	(2) Grade 5	(3) Grade 6	(4) Grade 6	(5) Grade 7	(6) Grade 7	(7) Grade 8	(8) Grade 8
Panel A								
Treatment	0.032	0.032	0.019	0.018	-0.001	-0.015	0.014	0.000
	(0.052)	(0.055)	(0.045)	(0.047)	(0.033)	(0.026)	(0.024)	(0.017)
Observations	2,291	2,291	2,291	2,291	2,291	2,291	2,291	2,291
R-squared	0.001	0.234	0.000	0.290	0.000	0.170	0.001	0.151
Panel B								
Treatment*Strata^L	0.087	0.081	0.060	0.056	0.053	0.031	0.050	0.024
	(0.063)	(0.082)	(0.067)	(0.072)	(0.046)	(0.039)	(0.032)	(0.024)
Treatment*Strata^H	-0.013	-0.003	-0.016	-0.009	-0.052*	-0.047*	-0.020	-0.016
	(0.054)	(0.056)	(0.046)	(0.051)	(0.029)	(0.026)	(0.023)	(0.018)
Observations	2,291	2,291	2,291	2,291	2,291	2,291	2,291	2,291
R-squared	0.010	0.235	0.004	0.291	0.006	0.173	0.004	0.152
Panel C								
Treatment*Girl	0.050	0.043	0.027	0.019	-0.001	-0.020	0.027	0.009
	(0.053)	(0.055)	(0.042)	(0.042)	(0.035)	(0.024)	(0.025)	(0.015)
Treatment*Boy	0.015	0.022	0.011	0.017	-0.001	-0.010	0.002	-0.007
	(0.060)	(0.061)	(0.051)	(0.055)	(0.035)	(0.035)	(0.025)	(0.024)
Observations	2,291	2,291	2,291	2,291	2,291	2,291	2,291	2,291
R-squared	0.002	0.234	0.001	0.290	0.002	0.170	0.002	0.151
Strata fixed effects and Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors are presented in parentheses. Sample is clustered by municipality and includes children who were ages 6-12 between 2000 and 2002. Controls include age, gender, mother's years of education, father's years of education, and indicator variables for missing data on mother and father's educations.

Table 15: Replication of Stecklov et al. (2007) results

VARIABLES	(1) pB	(2) pB	(3) pBCP	(4) pBCP
Treatment	-0.010	-0.004	0.003	0.009
	(0.018)	(0.014)	(0.021)	(0.016)
Age		-0.001		-0.002***
		(0.001)		(0.001)
Yrs of Education		-0.014***		-0.019***
		(0.002)		(0.002)
Constant	0.214***	0.316***	0.298***	0.477***
	(0.017)	(0.033)	(0.020)	(0.027)
Observations	2,728	2,728	2,728	2,728
R-squared	0.000	0.016	0.000	0.021
Strata fixed effects	No	Yes	No	Yes

Notes: *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors are presented in parentheses. Sample is clustered by municipality and includes women who were ages 12-47 in 2000. Controls include age and years of education.

Table 16: PRAF-II impact on conception during and after program

VARIABLES	(1) Conceptions during	(2) Conceptions during	(3) Conceptions after	(4) Conceptions after
Panel A				
Treatment	0.006	0.012	-0.006	0.007
	(0.022)	(0.017)	(0.024)	(0.019)
Observations	2,692	2,692	2,692	2,692
R-squared	0.000	0.020	0.000	0.069
Panel B				
Treatment	-0.016	-0.008	0.004	0.015
	(0.025)	(0.019)	(0.036)	(0.027)
Observations	1,536	1,536	1,536	1,536
R-squared	0.000	0.084	0.000	0.140
Panel C				
Treatment	0.036	0.040	-0.020	-0.006
	(0.031)	(0.031)	(0.034)	(0.032)
Observations	1,156	1,156	1,156	1,156
R-squared	0.001	0.008	0.000	0.030
Panel D				
Treatment	0.037	0.036	-0.056	-0.058
	(0.028)	(0.026)	(0.038)	(0.038)
Observations	825	825	825	825
R-squared	0.002	0.005	0.002	0.024
Strata fixed effects and Controls	No	Yes	No	Yes

Notes: *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors are presented in parentheses. Panel A sample includes women who were ages 14 or older between 2000 and 2002. Panel B sample is restricted to women who had 2 or more children prior to PRAF-II. Panel C sample is restricted to women who had 1 or fewer children prior to PRAF-II. Panel D sample is restricted to women who had no children prior to PRAF-II. Samples cluster by municipality. Controls include age and years of education.

Table 17: PRAF-II impact on flow of conceptions during program

VARIABLES	(1) Year 1	(2) Year 1	(3) Year 2	(4) Year 2
Panel A				
Treatment	0.006	0.013	-0.000	-0.001
	(0.016)	(0.015)	(0.013)	(0.011)
Observations	2,692	2,692	2,692	2,692
R-squared	0.000	0.013	0.000	0.011
Panel B				
Treatment	-0.009	-0.004	-0.007	-0.004
	(0.017)	(0.017)	(0.021)	(0.019)
Observations	1,536	1,536	1,536	1,536
R-squared	0.000	0.026	0.000	0.060
Panel C				
Treatment	0.027	0.035	0.009	0.005
	(0.024)	(0.021)	(0.020)	(0.020)
Observations	1,156	1,156	1,156	1,156
R-squared	0.001	0.014	0.000	0.003
Panel D				
Treatment	0.034	0.038**	0.003	-0.002
	(0.022)	(0.015)	(0.022)	(0.021)
Observations	825	825	825	825
R-squared	0.003	0.007	0.000	0.005
Strata FE and Controls	No	Yes	No	Yes

Notes: *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors are presented in parentheses. Panel A sample includes women who were ages 14 or older between 2000 and 2002. Panel B sample is restricted to women who had 2 or more children prior to PRAF-II. Panel C sample is restricted to women who had 1 or fewer children prior to PRAF-II. Panel D sample is restricted to women who had no children prior to PRAF-II. Samples cluster by municipality. Controls include age and years of education.

Table 18: PRAF-II impact flow of conceptions after program end

VARIABLES	(1) Post 1	(2) Post 1	(3) Post 1	(4) Post 2	(5) Post 3	(6) Post 3
Panel A						
Treatment	-0.006	-0.000	0.004	0.008	-0.005	-0.001
	(0.015)	(0.012)	(0.016)	(0.014)	(0.014)	(0.015)
Observations	2,692	2,692	2,692	2,692	2,692	2,692
R-squared	0.000	0.020	0.000	0.024	0.000	0.023
Panel B						
Treatment	0.002	0.005	-0.007	-0.002	0.009	0.012
	(0.022)	(0.018)	(0.024)	(0.022)	(0.017)	(0.016)
Observations	1,536	1,536	1,536	1,536	1,536	1,536
R-squared	0.000	0.032	0.000	0.061	0.000	0.037
Panel C						
Treatment	-0.017	-0.008	0.020	0.021	-0.023	-0.018
	(0.019)	(0.019)	(0.017)	(0.019)	(0.022)	(0.022)
Observations	1,156	1,156	1,156	1,156	1,156	1,156
R-squared	0.001	0.015	0.001	0.010	0.001	0.012
Panel D						
Treatment	-0.046***	-0.041**	0.022	0.015	-0.032	-0.032
	(0.016)	(0.016)	(0.018)	(0.019)	(0.026)	(0.026)
Observations	825	825	825	825	825	825
R-squared	0.004	0.018	0.001	0.007	0.003	0.011
Strata FE	No	Yes	No	Yes	No	Yes

Notes: *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors are presented in parentheses. Panel A sample includes women who were ages 14 or older between 2000 and 2002. Panel B sample is restricted to women who had 2 or more children prior to PRAF-II. Panel C sample is restricted to women who had 1 or fewer children prior to PRAF-II. Panel D sample is restricted to women who had no children prior to PRAF-II. Samples cluster by municipality. Controls include age and years of education.