An Econometric Analysis of the Mechanisms for Increasing Human Capital through Conditional Cash Transfer Programs in Colombia

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For the voices that are usually not heard

Abstract

Using information from a Conditional Cash Transfer (CCT) program in Colombia, this thesis examines some of the mechanisms through which such initiatives contribute to improving human capital in poor households. These programs offer poor households a monetary transfer contingent on children attending regular visits to the doctor and school attendance.

First, we look at the causal effect of childrens improved physical development due to the program on their cognitive development. We estimate this effect by instrumenting an intent-to-treat effect of treatment on the anthropometric measures. We find that exposure to the program does not explain gains in physical development one year after the intervention but it reduces the incidence of being underweight four and ten years later. These differentials in physical development explain around one quarter of a standard deviation gain in long-run cognitive development. Improvements in height are found only in children younger than 2.

Second, improved child health is one primary aim of CCT programs; we investigate if there are potential spillover effects on the health of other non-targeted adults living in the same household as the child. We use a difference-in-difference model to estimate the intent-to-treat estimate of the spillover effect. We find significant improvements in the health of adults in treatment households, both in terms of incidence of illness (in the short run) and in the severity of illness (in the medium run). The main channel for this effect seems to be the availability of better information and the creation of a health public good within the household as a result of the program.

Finally, while CCT programs have been designed to improve long term human capital among poor families through incentives and conditions, we test if they change behaviour through changes in preferences and aspirations rather than, or in addition to, the effect of the conditions and incentives. Using the poverty score for program allocation we estimate the program effect on participation using a regression discontinuity (RD) design. We find no evidence of the program working through time preferences or parents aspirations for their childrens schooling in participating households.

The positive program impacts on health and nutrition status of children and adults identified in this thesis and the effects on schooling identified in previous studies have important implications in terms of long run human capital development of children in poor households, for example, the receipt of higher wages when adults. However, those impacts appear to be driven by the ongoing receipt of the cash transfers and the associated conditions. Thus if the program was to stop, we would expect investment in childrens human capital to revert to pre-program levels.

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Chapter 1

Introduction

Conditional Cash Transfer programs (CCTs) consist of regular stipends given to poor households which are conditional on investment in the human capital of their children. These programs usually transfer money to the mothers of the household and the conditions usually include children's school attendance, regular medical check-ups, participation in vaccination programs and participation of mothers in health information seminars. The monetary transfer and promotion of children's health, nutrition and education aims to reduce both current poverty faced by those households and the intergenerational transmission of poverty.

In the past, unconditional money or in-kind transfers were the predominant welfare policies in developing countries. The quantification of their effect on poverty reduction, increased wellbeing or development (e.g. school attendance, consumption, child health care and so on) was, however, not always consistent or systematically captured, mainly because of a lack of data. More recently, some work has been done finding positive effects on desired outcomes from unconditional cash transfers, however, those effects are considerably smaller when compared to the outcomes achieved (in education mainly) if a condition is attached to the transfer (Akresh, de Walque, & Kazianga, 2013; Baird, McIntosh, & Ozler, 2011). From the mid-1990s, CCT programs started to be the most popular public policy to fight poverty, which by design were accompanied in many cases by the active measurement of their effects[1]. In 2001 the CCT program *Familias en Acción* was implemented in Colombia

[1] This approach was proposed as an alternative to social assistance policies and is more demand-side

based on the pioneering Mexican version *Progresa*, now called *Oportunidades*. By the end of that decade, almost all countries in Latin America had implemented a version of a CCT to promote human capital development. As the main aim of the CCT programs is to improve the schooling, health and nutrition of children, extensive research and systematic evaluations have been conducted in order to measure program impacts on these variables.

While there is some variation in the implementation, conditions and targeting process of the CCT program in each country, the effectiveness of this approach has been widely documented around the world. The effects in terms of education have been mainly associated with the increased level of use of services. The biggest effect has been found in enrolment rates. In Mexico, there is evidence of the positive effect of the program on school enrolment and progression, reduction of grade failure, repetition and dropout rates (Jere R Behrman, Sengupta, & Todd, 2005; Schultz, 2004; Todd & Wolpin, 2006). In Nicaragua (*Red de Protección Social*) and Brazil (*Bolsa Escola*), a positive effect on attendance and grade progression and a decrease in dropout rates have been found (Maluccio & Flores, 2005; Soares, Ribas, & Osório, 2010). In Ecuador (*Bono de Desarrollo Humano*) and Colombia there is evidence of increases in enrolment of 10 percentage points for children aged 7 to 17 and around 6 percentage points for children aged 14 to 17, respectively (Attanasio et al., 2010; Schady, Araujo, Peña, & López-Calva, 2008). Additionally, in Colombia the probability of finishing high school has improved by around 6 percentage points for girls and children in rural areas. There is however no evidence of increased test scores (Baez & Camacho, 2011).

Positive program effects on children's regular visits to medical check-ups and growth and development monitoring as well as an increase in immunization rates have been reported in Honduras, Nicaragua, Jamaica and Colombia (Attanasio, Battistin, Fitzsimons, & Vera-Hernandez, 2005; Attanasio, Gomez, Heredia, & Vera-Hernandez, 2005; Barham & Maluccio, 2009; Levy & Ohls, 2007; Morris, Flores, Olinto, & Medina, 2004). In Colombia a 22.8 and 33.2 increase in the percentage of children who regularly go to medical check-ups aged 0 to 1 and 2 to 4 respectively; the number of children less than 2 years old with DPT[2]

oriented, its advantages as a public policy approach are discussed by Laura B. Rawlings and Rubio (2005) and Laura B Rawlings (2005).

[2] DPT stands for Diphtheria, Pertussis and Tetanus. This is a vaccine which helps to prevent these

vaccination increased by 8.9 percentage points. There were some cases such as Brazil or Ecuador where there were no effects on health utilization. Some of the explanations for this are related to constraints on the supply side (Paxson & Schady, 2010; Soares et al., 2010). In terms of health and nutrition, the evidence suggests positive impacts due to reductions in the incidence of illness for children and adults, anaemia and prevalence of stunting in children. These results are however, heterogeneous by age groups and urban and rural areas (Attanasio, Battistin, et al., 2005; Attanasio, Gomez, Rojas, & Vera-Hernandez, 2004; J. Behrman & Parker, 2011; Jere R. Behrman & Hoddinott, 2005; P. Gertler, 2004; P. J. Gertler, Boyce, S, 2001; Hoddinott & Bassett, 2008; Maluccio & Flores, 2005). For example, the height of Mexican children aged 1 to 3 increased on average 0.95cm more than children who were not participating; and Nicaraguan children younger than 5 increased their height-for-age Z score by 0.17. In Colombia the increase in height-for-age Z score was 0.16 but only for children aged 0 to 2. Additionally, a 10 percentage point reduction in the prevalence of diarrhoea for children up to 4 years old was found in rural areas but no effect for older children or children in urban areas (Attanasio, Gomez, et al., 2005) and a weight increase for children 0 to 6 of around 2kg in rural areas but not in urban areas (Attanasio, Syed, & Vera-Hernandez, 2004).

It is expected that some of the transfer has an effect on household consumption and that indeed, additional consumption helps to explain gains in nutritional status. For Colombia, where the average amount received by households represents around 24% of total consumption, Attanasio and Mesnard (2006) estimate that total consumption increased by around 15%. There was also an increase in food consumption where the higher increase was found in protein consumption. Similarly in Mexico, Hoddinott and Skoufias (2004) show the increases in total consumption were around 13% and median household caloric acquisition increased by 6.4%. This increase was in the order of 7% and between 12% to 20% in Honduras and Nicaragua (Hoddinott & Wiesmann, 2008; Maluccio & Flores, 2005). In those countries where child labour force participation is high, the effect of the program was not clear ex-ante in terms of consumption as the transfer could have a pure income effect. However, evidence suggests that child labour has also decreased as a result of CCT programs, finding the higher effects usually among older children and boys. In

three common infectious diseases.

Colombia however, there was no evidence of a significant decrease in the time spent in income-generating activities by children (Attanasio et al., 2010; Edmonds & Schady, 2009; Skoufias, Parker, Behrman, & Pessino, 2001).

Some others variables of general interest that have been evaluated are poverty, adults' labour supply, household composition, fertility rates, savings decisions, among others. While in terms of poverty it seems intuitive to find a reduction, which has been supported by the reduction of the headcount index and poverty gap, the effect of the program is not that clear for the other variables and in fact, there are reasons for both decreases and increases in all. A comprehensive revision of outcomes evaluated in different countries can be found in Fiszbein, Schady, and Ferreira (2009).

These results give us a clear overview of the positive effects CCT programs are having on human capital and poverty reduction in developing countries. Nonetheless, there are still some gaps in the literature. For some outcomes such as school achievement, learning rates and nutritional status, the evidence is not homogeneously conclusive. The consequent question is 'Why?' Further, many other variables have received little or no attention, sometimes due to data issues. For example there is no clear understanding of the role that enhanced cognitive ability plays in the effectiveness of the schooling component of the program; or how program design for the youngest children can enable them to perform better once in school; or if the programs affect parents' underlying preferences for human capital development of their children. The main purpose of this research is to contribute to the understanding of the mechanisms through which a public policy such as a CCT can affect the development of human capital in the long-run in developing countries.

1.1 Research Questions

This research seeks to answer three questions:

1. Do the increases in nutrition and improved health of children due to the CCT program translate into improved cognitive development? This is important because

undernourished children and poor health status has been traditionally associated with late school enrolment and poor progression. If CCT programs have an effect on cognitive development this enables long term human capital accumulation among participating children;

2. Can CCT programs have an indirect effect on the health of adults even when they do not receive any direct benefit? If this is the case, healthier adults are more likely to be more productive and potentially increase human capital accumulation of the children in the household. This will have significant effects on the reduction of the intergenerational poverty cycle;

3. Can CCT programs change preferences and school aspirations of parents for their children in the long run? Changes in preferences and hopes are important to drive changes in behaviour even in the absence of incentives, such as a cash transfer or conditions. If they change due to participation in the program it is likely that parents are willing to keep investing in children human capital even if the program was to stop. We will use the CCT implemented in Colombia to look at this question which is not widely explored in the literature.

1.2 *Familias en Acción*

In brief, *Familias en Acción* –Colombian CCT– was implemented in poor small areas[3] and has been active since 2001. The overall aim was the creation of long-run human capital in households experiencing extreme poverty (the first quintile of poor families with young children) through monthly monetary transfers, which were expected to improve the education, health and nutrition of children in rural households.

Those households classified as poor were eligible for two types of transfers. The first was a lump sum of approximately US$25 for households with children 0 to 6 years old. This transfer was conditional upon attendance every two months at growth and development check-ups; adherence to an immunization schedule; and hygiene, diet and contraception

[3] Small areas refer to municipalities with less than 100,000 inhabitants and municipalities that are not the main town of the state.

conferences by participating primary caretakers – also known as 'titulars'[4]. This transfer aimed to improve food consumption as well as the health and nutritional status of the children; we will refer to it as the health and nutrition component of the program.

The second was a transfer of approximately US$8 for each child in primary school, and US$16 for each child in secondary school[5]. This transfer was offered to households with children ages 7 to 17 and was subject to an average school attendance rate of more than 80% per child. The main aim of this transfer was to improve children's schooling and we will refer to it as the education component of the program.

Throughout the country some other versions of the program were implemented later on, for example in bigger urban areas with modified conditions or a changed target population. However, this thesis is based only on the ongoing program implemented from 2001 in small municipalities in Colombia and for the poorest in these municipalities. This is the general program description in terms of eligibility, conditions and benefits. Some more details of their implementation and operation will be provided in each chapter as required.

The thesis is divided into three main chapters that will address each of the above mentioned objectives. At the end of each chapter we will include the references, tables, figures and appendices relevant for that chapter. We describe the generalities of each one here.

1.3 Linking Children's Physical Development and Cognitive development in Rural Areas

Is nutritional status a key input to cognitive ability which in turn allows children to perform better at school? There is evidence in the economic literature that a lack of nutrition in early life translates into stunted growth and a delay in cognitive development, which can only be partially recovered in childhood and adolescence (Alderman, Hoddinott, & Kinsey,

[4] '*Titulars*' of the program is a concept usually describing the mother of the child receiving the benefit, but in some cases included the father or the grandmother if the mother did not live in the household.

[5] The Colombian school system is divided into five years of primary school followed by four years in basic secondary and then two years of middle secondary school, for a total of six years of secondary education. After finishing secondary school, individuals can pursue vocational or tertiary education.

2006; Grantham-McGregor, 1995; Grantham-McGregor et al., 2007; Nores & Barnett, 2010; Outes-Leon, Porter, & Sanchez, 2011). Also, poorly nourished children tend to start school later, progress through school less rapidly, have poorer academic achievement and perform less well on cognitive achievements tests when older, including in adulthood. In addition the productivity level of those adults tends to be lower (Heckman, Stixrud, & Urzua, 2006; Hoddinott, Maluccio, Behrman, Flores, & Martorell, 2008; Maluccio et al., 2009). In the context of the CCT impact analysis, while enrolment and attendance rates have increased, there is not a clear understanding of why test scores or achievement have not improved consistently (Baez & Camacho, 2011; Ponce & Bedi, 2010).

Conditional cash transfer programs have been found to have positive effects on children's nutritional status in Nicaragua (Maluccio & Flores, 2005) and, among younger children in rural areas, in Colombia (Attanasio, Battistin, et al., 2005), but not in Brazil (De Janvry & Sadoulet, 2005) or Honduras (Morris et al., 2004). However, none of these studies include measures of child cognitive development. Studies that have attempted to study the effect of the program on cognitive development are (Fernald, Gertler, & Neufeld, 2008) in Mexico, (Paxson & Schady, 2010) in Ecuador and (Macours, Schady, & Vakis, 2012) in Nicaragua. They have not found robust effects.

The proposed research will thus be one of only a handful of studies to examine impacts of CCTs on cognitive development and the first in the Colombian context. The chapter will examine

1. Is there a causal relationship between physical development early in life with later cognitive development?
2. How long does it take for nutritional improvements to translate into improvements in cognitive development?
3. Are there differences related to the age of exposure to the nutritional and health component?

To examine the impacts on cognitive development, additional field work was carried out. I participated in selecting the cognitive ability module, which was integrated into the data collection in the program evaluation survey done in 2011-2012. I participated in the training of enumerators for the fieldwork in Colombia and the piloting of the questionnaires used.

The main findings are that nutritional gains one year after the program do not lead to improvements in cognitive ability. However, longer term health gains do appear to be associated with improvements in cognitive ability. When comparing children aged 2 to 6 from the poorest rural families in Colombia four years after the start of the intervention with similar children who were not eligible for participation, we find that gains in weight sustained four years after the program are associated with an improvement on the cognitive ability test. Also when comparing the same children 10 years after the intervention, we still find higher cognitive development explained by the sustained gains in weight.

1.4 Health Spillover Effects of a Conditional Cash Transfer Program

As the main aim of the program is to build human capital in children from poor households, the research has been focused on children's outcomes. However, children belong to households and families on which the program could be having an impact as well. One reasonable question is this: Is anything happening to the health status of other members in the household? If there is an improvement, it may be argued then that the creation of human capital can be boosted as a result of adoption of healthier practices in the household. Also, healthier adults can increase their availability in productive activities which increases the income of the household and the wealth of children reinforcing the children's human capital formation.

The analysis of the intra-household impact of the program on adults' health is somewhat new in the literature as i) in Colombia there is evidence of positive effects on health among children, but as far as we know there is no evidence of effects on adults' health and ii) there is very little evidence of intra-household indirect effects (spillovers) in health in developing countries. Indeed, in the Mexican version of the CCT program,[6] one condition in order to receive the transfer was the attendance of all adults in the household at an annual check-up, but in the Colombian version that condition was not included. As a result, it is quite sensible to think that there is at least one straightforward channel through which the health among adults would improve in the Mexican program[7]. In the Colombian program, attendance by the caregiver at health centres and information sessions may still generate health spillovers to other household members.

The indirect effect on adults' health can occur through increases in the health care demand by adults, changes in preferences towards healthy behaviours in the household, decrease of illness transmission within the household or as a result of a positive income effect. As an adult (usually the caregiver-*titular*) has to accompany children to regular check-ups, once in the health facilities they can also demand health care. *Titulars* also can change their preferences as they are exposed to information during medical check-ups and in the regular classes that they have to attend, where they learn about hygiene, nutrition and general health. That information may be shared with other adults in the household, spreading healthier practices and behaviours such as, washing hands before eating, washing food before consumption or selection of more nutritional food[8], which also would reduce the probability of disease contagion. Finally, the cash transfer can relax budget constraints in the household. The specific questions asked in this chapter are:

1. Is the Colombian CCT program creating intra-household indirect effects in health among adults?
2. Are those indirect effects heterogeneous across gender and ages groups?
3. If yes, what are the mechanisms through which these indirect effects are operating?
4. Is there a difference between the impacts in the short and medium term?[9]

[6] As the Mexican program was accompanied by very detailed data collection their CCT program is more widely studied.

[7] Assuming that attendance at medical check-ups will increase the chances of detecting health problems and receiving the respective treatment.

[8] Attanasio, Battistin, and Mesnard (2012) find an improvement in consumption of protein and vegetables among treated households.

[9] The work presented in this chapter was done in collaboration with Professor Pushkar Maitra.

Our results show that there are indeed strong intra-household indirect effects within households. In the short run, the strongest effects are on self-reported illness. Non-targeted individuals (adults) in treatment households were significantly less likely to be ill in the 15-days prior to the survey compared to adults in control households. The effects persist over a longer period of time and indeed over time it leads to better long term health and a reduction in the severity of illness, captured by lower rates of hospitalization. Additionally, we find that the effects are quite heterogeneous and the effects are stronger for men and the elderly. Our results suggest that the mechanism is household level public goods (information sharing) and contagion, happening through changes in behaviour and not a relaxation of the household budget constraint as a result of the cash transfer that is driving the results.

All of this has significant effects on the inter-generational poverty cycle. Healthier adults are more productive and this increase in productivity of adults is likely to positively affect the human capital of the next generation. None of this is captured by examining only the effects on the targeted group. This program has had significant within household spillovers and simply by looking at the direct effects, one would significantly underestimate the effects of the program. Proper cost-benefit analysis of such CCT programs needs to take into account the improved health of the non-targeted individuals and the consequent reduction in both the incidence and severity of illness, resulting in improvements in long term health.

1.5 Can a CCT change parents' underlying behavioural parameters and hopes for their children?

Research has shown that parents' time preferences play an important role in investment decisions like children's schooling (Basu, 2003; J. Das, Do, & Özler, 2005; M. Das, 2007). We use the term (high) time preferences as the preference for immediate utility over delayed utility; this is also referred to as impatience or a high discount rate. If parents are impatient they are more likely to send their children to work than to send them to school. Furthermore, parents' aspirations for their children are good predictors of children's school outcomes (Chiapa, Garrido, & Prina, 2012; Davis Kean, 2005; Halle, Kurtz-Costes, & Mahoney, 1997). This chapter examines whether there is evidence of the program changing these parameters. If the program has an impact on these parameters, parents are likely to keep investing in human capital formation of their children, even if the program was to stop. Further, if time preferences and educational aspirations parameters are transmitted generation to generation, children will learn from their parents to be more patient and increase their educational aspirations. Hence, the program will increase the investments made by parents in their own children, while also positively affecting the investment decisions children will make in the future for their own and their children's human capital. For example, he or she may be more likely to decide to invest in training that will lead to higher labour wages in the future. This benefit will in turn have a positive effect on the human capital of the next generation.

The specific questions of interest in this chapter are:

1. Are parents who are exposed to the program more patient than other similar parents because of their participation in *Familias en Acción*?

2. Is the program increasing parents' aspirations for their children's educational outcomes?

3. Is the sustained cash transfer (positive income shock) the main channel via which the underlying discount rates change? Or does the program cause preferences to change for other reasons?

Only two other studies of which we are aware study time preferences and aspirations in a similar context. The first one comes from an unconditional cash transfer in Kenya targeting households with one deceased or chronically ill parent (Martorano, Handa, Halpern, & Thirumurthy, 2014). They do not find differences in intertemporal choice of the parent or caregiver when comparing between treatment and control locations two years after exposure to the program. The identification of this effect was however not clear as by the time of the evaluation, both treatment and control locations were receiving the treatment. The second paper studies changes in parents' aspirations for their children's years of education in the Mexican CCT (Chiapa et al., 2012). They find in the short-term *Progresa* is associated with an increase in educational aspirations of about a third of a school year. They hypothesize that the positive effect on the parents' aspirations comes from mandated exposure to educated professionals (doctors and nurses). Our study adds to the limited empirically-based literature of changes in time preferences parameters and aspirations, as well as complementing the literature that explores the role of the conditions and the mechanisms for achieving the outcomes found previously in the literature by CCTs.

We find that *Familias en Acción* does not seem to work through parent's time preferences. Time preferences appear unaffected. This is true for families that are currently receiving the monetary payments and those who received payments in previous years. Hence, time preferences do not seem to be affected by income (the transfer) nor exposure to the education-promotion aspects of the program. We also find that the program is not having a positive effect on the number of years they want their children to study. Overall, this suggests that the observed changes in parent's behaviours and decision-making are not being driven by aspirational change or change in underlying preferences but are coming either from the cash, the conditions or both. This suggests that if the cash payments and attached conditions were to disappear, the program is likely to have no lasting effects (beyond the additional schooling and nutritional gains directly resulting from the received payments and conditions).

Chapter 2

Linking Children's Physical Development with Cognitive Development in Rural Areas

2.1 Introduction

It is well documented that a lack of nutrition in early life translates into stunted growth and a delay in cognitive development which can only be partially recovered in childhood and adolescence (Alderman, Hoddinott, & Kinsey, 2006; Grantham-McGregor, 1995; Grantham-McGregor et al., 2007; Nores & Barnett, 2010; Outes-Leon, Porter, & Sanchez, 2011). Also, poorly nourished children tend to start school later, progress through school less rapidly, have poorer academic achievement and perform less well on cognitive achievement tests when older, including in adulthood. In addition, the productivity level of these adults tends to be lower (Heckman, Stixrud, & Urzua, 2006; Hoddinott, Maluccio, Behrman, Flores, & Martorell, 2008; Maluccio et al., 2009). The main goals of Conditional Cash Transfer (CCT) programs are to improve the nutritional status of children and their education attendance. These programs have been found to have positive effects on child nutritional status in Nicaragua (Maluccio & Flores, 2005) and, among children in rural areas, in Colombia (Attanasio, Battistin, Fitzsimons, & Vera-Hernandez, 2005), but not in Brazil (de Janvry, Finan, Sadoulet, & Vakis, 2006) or Honduras (Morris, Flores, Olinto, & Medina, 2004). One might expect in the context of CCT impact analyses that improvements in nutritional status and education enrolment also result in improvements in academic performance, however test scores have not consistently improved (Baez & Camacho, 2011; Garcia & Hill, 2010; Baird, Ferreira, Özler, & Woolcock, 2013; Benson, 2012; Ponce & Bedi, 2010; Saavedra & Garcia, 2013). There is not a clear understanding of why this is the case.

Understanding the impact of a conditioned monetary transfer on nutrition and health is not straightforward as the relationship is less direct than, for example, the mechanism via which programs that provide supplemented food to children, result in extra nutrients and better nutritional status. Supplemented food programs are a policy alternative to improve child nutritional status. Such programs usually provide daily food to children with vitamins, minerals and micronutrients added. While there is evidence of the link between specific components in supplemented food and improved nutrition of children, in the context of CCT programs the relationship is not clearly understood. The challenge in the CCT context is that there is no control over food intake and its quality. Although, programs that provide supplemented food can associate effects on the outcome variables more directly to the amount of vitamins and minerals included in them, the implementation of these programs requires a high level of monitoring[1]. In contrast, the implementation and delivery of the CCT does not require a daily follow up. Traditionally supplemented food has been used as one of the ways to improve children's nutrition in public policy. The introduction of CCT programs raises the question of whether a program with little control over how children are fed can attain similar results as supplemented food programs. Both strategies have advantages and disadvantages. There is a trade-off between the lower delivery/monitoring costs and the expected food inputs on children in CCT programs. We will show that a nutritional CCT in Colombia has positive effects on child nutrition and improves cognitive development in the long term. These impacts are similar to those attained by a more logistic-costly supplemented food program in Guatemala.

The evidence of the effects of CCT programs on cognitive ability is still limited. To our knowledge, only three studies have assessed the impact of cash transfers on cognitive ability

[1] See Sazawal et al. (2013) ;Maluccio et al.(2009).

development. Fernald, Gertler, and Neufeld (2008) find that a doubling of the amount transferred to households in the Mexican CCT resulted in better nutritional status, motor development and cognitive development among children 2 to 6 years old. However, there is a concern about the endogeneity of the variation in the amount of the transfer that is used to identify the impact of the program, as the eligibility for that variation was not random and depends entirely on the individual's previous behaviour in terms of school performance (Attanasio, Meghir, & Schady, 2010). Paxson and Schady (2010) analyse the effect of the Ecuadorian CCT program on health, cognitive and behavioural outcomes for children between 3 and 7. Their results are modest for cognitive development for all children, and they find no effect for children who are better off. Finally, Macours, Schady, and Vakis (2012) find that children in households assigned to receive a transfer had higher levels of development nine months after the program began in Nicaragua. They also find that the results are maintained two years after the transfers were discontinued. However, these results are not robust to changes in economic specification and the magnitude of the associated effects is small.

This paper attempts to verify the hypothesis of the causal relationship between physical developments early in life with later cognitive development. Previous studies of the Colombian program had found a positive impact on health and nutrition in children. Some positive effects on short term (weight) and long term (height) nutritional status in children mainly due to higher consumption levels in the household were reported by Attanasio, Gomez, Rojas, and Vera-Hernandez (2004). Also, the program increased the percentage of children with an up-to-date schedule of preventive health care visits and decreased the level of child morbidity specifically reducing the level of diarrhoea but not respiratory problems in rural areas. However, so far there is no evidence of the relation between the impacts of this improvement in nutrition on cognitive ability. Using 2012 data and taking advantage of the exogenous allocation of the intervention in a quasi-experimental setting, we estimate an IV-regression to determine the effect of the differential physical development on the cognitive development in children of 12 to 16 years who were exposed between ages 2 and 6 to the nutritional intervention, versus those who were not exposed. We also use an intent-to-treat estimate to find the effect on differential physical development of children 12 to 16 years who were exposed between ages 2 and 6 to the nutritional intervention one year, four years and ten years after the intervention. While the intervention does not explain gains in physical development after one year of exposure, it shows important effects four and ten years after, mainly reducing the incidence of underweight children and improving BMI. These differentials in physical development explain around 0.25 standard deviations of the gain in long-term cognitive development.

2.2 The Conditional CashTransfer Program and Experimental Design

The Colombian CCT program, *Familias en Acción* (FA), provides cash transfers to poor rural households conditional on their compliance with requirements benefiting children's nutrition, health and education. In families with children aged 0 to 6, a monthly lump sum amount was given to the caregiver, subject to the children's attendance at regular growth and development check-ups and caregiver attendance at health educational workshops where they were advised about nutrition and health care practices. The transfer was provided to complement the food consumption of the household and was expected to result in a higher quantity and quality of food[2]. Moreover, the requirement of attending medical check-ups was expected to improve the medical care of children. Besides growth and development check-ups for children 0-6 and mother's attendance at the workshops, the program did not contain any incentive or additional activities that were likely to improve child cognitive development such as early stimulation or pre-schooling programs. In families with children aged 7-17, a different amount was given per child according to the level of education he or she was attending and was subject to a minimum 80% school attendance rate. These transfers represented between 16% and 25% on average of the household monthly income, depending on the number of children attending school.

If a household was only eligible for the nutritional component of the program because they only had children 0 to 6; once a child reached age 7 the household was eligible for the education component for that child. This means that all children who were 2-6 at the start

[2] For this population, the food consumption represents around 71% of the total consumption (Attanasio & Mesnard, 2006).

of the program, once they turned 7, gradually started to be eligible for the educational incentive. Five years after the program started, it was implemented in the areas that were originally defined as controls, so that all children started to receive the intervention at this time. In that year, the children who were 2-6 at the start of the program were already 7 to 11 which meant that the control group started to receive a transfer upon school attendance. However, none of them were ever eligible for the nutritional component of the program. This thereby leaves us still with a clean control group in terms of the nutritional intervention, allowing us to compare children aged 2 to 6 at the start of the program that were exposed to the nutrition and education intervention after 6 (treatment group) with those who were exposed only to the education component after age 6 (control group). Figure 2.1 shows the participation in each component of the program by children who were eligible at the baseline for the nutritional intervention. Children who were at the start of the program aged 0 to 6 were between 10 and 16 years old by the time cognitive development was measured (2012). Nevertheless, to rule out any kind of contamination between control and treatment children, we will compare only children 2 to 6 that were never exposed to the nutritional incentive in control areas[3].

This program was designed in the context of a post-economic recession to help the poorest and most vulnerable populations in the country. The program targeting strategy was designed at two levels: first at municipality and then at the household level. We carefully describe the allocation of the program as we argue that this allocation is exogenous at the household level and this assumption is a key point for our identification strategy.

At the municipality level three conditions were established for eligibility. The first condition was that the municipality had to be classified as rural. This means it has to have less than 100,000 inhabitants and not be the main town of the state; additionally the municipality could not be located in the area receiving aid for recovery after the 1995 earthquake. The second condition was that the municipality had to have a bank branch. This condition was required for the program's operational purposes. Finally, the third condition was that the municipality had to have infrastructure capacity in order to implement the

[3] Children aged 0 and 1 in 2002 were aged 5 and 6 by 2007 and were still eligible for the nutritional component of the program. We are aware that 0 – 2 years of age is a critical period for nutrition. We address this issue in the results section.

program. To evaluate these eligibility criteria, an independent office called *Fondo de Inversion para la Paz* (FIP)[4] was commissioned to carry out an ex-ante evaluation to assess the selection criteria. The evaluation consisted of determining whether the municipality had enough education and health infrastructure to be able to respond to ordinary baseline demand and the demand to be generated by the program. From the total of 900 potential municipalities, 715 were evaluated and 622 met the requirements. These 715 municipalities were those whose local government showed interest in participating in the program and provided all the necessary information for the evaluation. The evaluation considered health indicators such as number of doctors/inhabitants, number of nurses/inhabitants, projected medical appointments, and projection of check-up appointments; and education indicators such as number of students per teacher, classroom area per student, dropout rate, and enrolment growth rate. Those indicators where reduced to a weighted indicator of health and education capacity and based on a lower bound cut-off, it was established if the municipality had enough infrastructure. All the municipalities that were diagnosed as satisfactory by the evaluation were considered eligible for the program.

At the household level three conditions were established for eligibility. First, the household had to be classified as level 1 in the SISBEN index[5]. This index is a score between 0 and 100 for each family and is constructed as a proxy indicator of the resources of the household according to their life conditions. The variables considered for the index construction are related to dwelling conditions, high economic dependence, income, income generation capacity and number of school unenrolled children. A family is classified as poorer if their score is closer to 0 and richer if closer to 100. The SISBEN office established score cut-offs to divide the families into 6 levels, where level 1 represented the poorest and level 6 the least poor. Second, the household had to have at least one child aged 0 to 17 by the time the program was operational. Third, the household had to live in a municipality where the program was going to operate. The program started between the end of 2002 and early 2003, and by October of 2002 had 407,076 eligible families and 362,406 beneficiaries (Attanasio et al, 2004).

[4] The FIP was created in 1998 as an office to allocate and fund programs towards peace building in the country.

[5] The SISBEN index is the instrument designed by the Colombian government to rank households and target social expenditure.

2.3 Conceptual and Theoretical Framework

In the first part of this section we present a model of nutrition absorption to simplify the channels through which the *Familias en Acción* program is likely to have improved physical development. In the second part, we develop a model of cognitive development including the role of physical development and the role of *Familias en acción* on cognitive development through physical development. Overall, the main aim of the section is to set up a theoretical framework to guide the empirical work.

2.3.1 Physical Development

Anthropometric measures are commonly used as a representation of physical development. In Latin American and commonly in developing countries, height and weight retardation, compared to developed countries standards, starts after the 6^{th} month of life and accelerates rapidly after the second year of life. In fact, it is recognized in anthropometric history that adult stature is a proxy for living conditions during childhood; particularly lessened physical development is related to adverse childhood conditions. Nutritionists and human biologists recognise that growth is affected by the interplay of diet and feeding practices on the one hand, and morbidity –particularly infection– on the other (Chernichovsky & Zangwill, 1988; Cole, 2003; Martorell & Habicht, 1986). In this subsection we explicitly explore ways in which *Familias en Acción* may affect physical development.

Nutrition absorption is the main mechanism that affects physical development besides genetic components. When living in poverty, protein-energy malnutrition occurs with greater frequency among young children due to the higher energy nutritional requirements relative to body mass that usually are barely met, and because of the high incidence of infectious disease. These components are represented in Figure 2.2. Limited food provision is accompanied by protein-energy deficits and intake deficits in vitamins and minerals that are essential for normal physical development. Health is believed to determine the efficiency of the diet in the production of nutritional status. For example, disease can limit the absorption of nutrients. The frequency, duration and severity of infectious illness all play a role. Preventing infections and diseases is as important as control and treatment once sick. Adequate access to medical care and immunization helps to reduce the duration and the severity of incidence of infections. The most common infections in poor environments are diarrhoea and respiratory infections, both of which have an effect of depressing child appetite and also limiting the absorption of nutrients. A systematic review (Martorell, 1980) shows that while in developed countries there is no association between illness and physical growth, in developing countries common childhood ailments, in particular diarrheal disease[6], are clearly associated with poor physical development (Martorell,1980; Scrimshaw & SanGiovanni, 1997). As a result, normal physical development is a good indicator of the levels of nutrition absorption in low provision settings.

We measure physical development through weight and height. We decompose the investment (or environment) factors to show how different health and nutrition components intertwine in function n which defines the nutrients that are absorbed by the body and manifested in child height and weight.

$$PD = n\left(FI, H, E, \mu\right) \tag{2.1}$$

Physical Development (PD) will be the result of a nutrition-health function n. This function is a reinforcing combination of food intake (FI), health care (H), environmental conditions (E) and genetic endowments (μ). Food intake (FI) includes not only the amount of food an individual consumes but also the quality of the diet; however, the absorption will be limited in case of disease. The adverse effect of illness can be diminished if incidence of illness is lower or if illness is shorter, as the body will be able to absorb more nutrients. For example, children with diarrhoea have a lower chance of extracting the nutrients, but if the diarrhoea is treated promptly the severity of the effect would be lessened[7]. The challenge for children with diarrhoea is that they do not hold the food long enough for the acids to extract the nutrients to be absorbed. The role of medical care (H)

[6] Nutrition absorption is also limited by enteric disturbances, as the food does not remain long enough in the gastro-intestinal track to be absorbed.

[7] There are transitory and permanent conditions that affect the health status, but we do not distinguish between them in the model.

is then preventive and curative. Environmental conditions (E) interact with infection rates and food provision. For example, low income is usually related to poor environments that adversely affect exposure to pathogens, such as overcrowded housing or lack of sanitation practices and facilities. Also, low levels of public services provision affects disease patterns like potable water, garbage collection or preventive and curative medical care facilities (Martorell, 1980). Finally, (μ) represents the child genetic endowments. All these factors interact and reinforce each other.

We assume that investment in each of the components is decided in each period following a standard consumption function subject to a budget constraint. If we introduce the effect of a treatment (T), like the nutritional and health incentive of the *Familias en Acción* program, the effect on PD will be determined by

$$\frac{\partial PD}{\partial T} = \frac{\partial n}{\partial FI}\frac{\partial FI}{\partial T} + \frac{\partial n}{\partial H}\frac{\partial H}{\partial T} + \frac{\partial n}{\partial E}\frac{\partial E}{\partial T} \tag{2.2}$$

The program offers access to extra income, compulsory attendance at medical care check-ups and health educational workshops which in turn complement food consumption, better sanitation and health practices and access to medical care (preventive or curative).

We expect $\frac{\partial n}{\partial FI}\frac{\partial FI}{\partial T}$ to be positive as the program is expected to relax income constraints and lead to better decisions on diet selection (through mothers attending educational talks, and receiving information on nutrition). Empirical evidence showed that, the share of food consumption was not affected by the program, but that total consumption and food consumption increased in treatment rural areas by about 15 percentage points when compared with control rural areas. This increase is equivalent to the amount transferred by the program. Also, there is an increase in the share of proteins in food as well as in cereals. Moreover, income did not have an impact on adult-items consumption or food prepared out of the household. (Attanasio & Mesnard, 2006). The second term $\frac{\partial n}{\partial H}\frac{\partial H}{\partial T}$ is also expected to be positive as one of the requirements for the transfer is that children have to attend regular medical check-ups. At these visits, the progression in growth and development of the child is evaluated as well as other health issues and mothers also receive advice from medical personnel. One year after the program was implemented, preventive health visits increased by around 30 percentage points when comparing treatment and control areas (Attanasio et al., 2005). The third term $\frac{\partial n}{\partial E}\frac{\partial E}{\partial T}$ is expected to be positive as sanitation and health practices in the entire household have improved as a result of the information obtained in the educational talks and medical advice. We discuss this in Chapter 3. Overall, better health status is expected as children are receiving better health care at home and are exposed to healthier environments and healthy behaviours as a result of more informed mothers. Attanasio et al. (2005) find a decrease in diarrhoea prevalence in rural areas of 10 percentage points.

We cannot empirically isolate the effect of T on each factor. We will interpret systematic variation in PD with *treatment* as the total effect of T, given that the effects are positive and mutually reinforcing. This is $\frac{\partial PD}{\partial T} > 0$. This variation in PD after treatment will be used in the identification strategy to isolate the effect of physical development on cognitive development from the other factors.

2.3.2 Cognitive Development

The medical literature has recognized the difficulty as well as the need to isolate the effect of nutritional factors from other environmental factors on brain and cognitive development (Anjos et al., 2013; Bryan et al., 2004; Walker et al., 2011). We have seen a strong association between these two variables; however, there is still limited evidence on the causality. There are of course ethical limitations to experimental tests with children. Some studies have shown the positive association of poor diets and retarded infant cognitive development. In contrast to other environment factors (education or early stimulation), nutrition is an environmental factor that can directly mediate the expression of genetic factors. This process occurs by providing the specific molecules that enable genes to bring to bear their potential on brain growth and therefore cognitive development (Rosales, Reznick, & Zeisel, 2009). Some of the most important nutrients responsible for the normal development of the brain are protein, energy, some fats, iron, zinc, copper, iodine, selenium, vitamin A, choline, and folate. In poor households, poor nutritional intake will lead to a lack of almost all of these components. Although it is complicated to isolate the roles these components play in the brain, advances in neuropsychological development assessments have enabled the process. For example, protein-energy malnutrition causes both global deficits, which are testable by general cognitive developmental testing, and area-specific effects (hippocampus and the cortex[8]), testable by problem solving and language developmental testing (Anjos et al., 2013). The connection between limited intake of minerals and vitamins during pregnancy has also been linked with change in neuronal functioning, cognitive development, motor development and recognition memory[9] which can be assessed by psychometric tests. Poor nutrition (lack of most nutrients) will be manifested in poor performance on general cognitive tests. Remedial interventions need to start as early in life as possible.

Brain and cognitive development starts from conception; cognitive investments are subject to cumulative effects and sensitive age periods. Cumulative effects refer to the effect of cognitive development investments in one period building on cognitive development achieved in the previous period. It has been found that infant nutrition interventions have a permanent effect on cognitive function and the strength of the evidence depends on the age window[10] and the period when the follow-up is conducted for evaluation. Cognitive functions related to motor and verbal ability start developing earlier than other cognitive functions and they are much more influenced by other environment factors. In contrast, memory, problem solving and spatial reasoning are cognitive functions that do not stabilize until early adulthood. "Sensitive periods" refer to the age-window of intervention when investments (or lack of investments) have a larger effect on boosting (or limiting) cognitive development. The period between ages 0 and 5 is a time for rapid and dramatic changes in the brain (DeLong, 1993; Rosales et al., 2009); interventions during this window of time have larger effects on cognitive development than later in life. For example, children who suffered from poor intrauterine growth can recover if early nutrition interventions take place. Interventions during breastfeeding periods (0-6 months old) show for children 18

[8] The hippocampus belongs to the limbic system; one of the main roles is to capture information for long and short-term memory and also for spatial orientation. The cerebral cortex plays a key role in memory, attention, problem solving, and language.

[9] See for a review (Bryan et al., 2004; DeLong, 1993; Georgieff, 2007; Rosales et al., 2009)

[10] Age window is referred to in the literature as the age cohort at the time of the intervention (e.g. all children between ages 2 to 6 in 2002).

months old, a positive but weak positive association but if the effect is evaluated by age 7, it shows a strong positive association with cognitive function (Lucas, 1998). Cumulative effects and sensitive periods define cognitive development as a multistage technology[11].

In the Early Childhood Development literature, nutritional outputs and cognitive skills have traditionally been studied separately as two different outcomes in human capital formation; however we are interested in the causal effect of nutrition on cognitive development. To illustrate the role of nutrition and health, represented by physical development[12] (PD) on cognitive development (CD), we use a dynamic model of human capabilities based on Cunha et al. (2007) and Todd et al. (2003) incorporating the role of PD. Both types of outcomes have cumulative features and we incorporate this in our model. The model shows that early-life investments affect later outcomes and that later investments build on previous ones. Thus, investments in PD at some point will have a permanent effect on all the following periods of life.

Assuming CD is a multistage technology, we define CD at a certain age t as the result of previous CD, family characteristics (F) which are assumed to be time invariant (e.g. parental education or number of siblings), school inputs (S) and present investments. We separate the role of PD as an input from other investment factors (I) such as clothing or recreation time, given that the amount of (I) is decided at the beginning of each period and is independent of the previous periods while PD has a cumulative nature.

CD when a child is $t+1$ years old is

$$CD_{t+1} = f\left(CD_t, PD_{t+1}, I_t, S_t, F\right) \tag{2.3}$$

Similarly,

$$PD_{t+1} = n\left(PD_t, I_t, F\right) \tag{2.4}$$

[11] A more comprehensive description of these concepts can be found in the human capability formation model developed in Cunha and Heckman (2007) and Heckman (2007).

[12] Detailed explanation of this representation will be presented in the following subsection.

The intuition behind equations (2.3) and (2.4) is that parents make investments in their children and these have an effect on CD and PD that is cumulative over time. Their investments can be material (e.g. food, books or medicines) or non material (e.g. intellectual stimulation or attendance at a nursery program) and these influence either CD, PD or both. For example, one might expect that time spent reading books at home or with access to educational games is an investment in equation (2.3) but not in (2.4). Those investments act together with other factors that do not specifically target CD or PD but influence children's general development like family and environmental influences. Some of these investments might include for example the number of siblings, parents' occupation or recreation facilities in the neighbourhood. Finally, schooling decisions and school inputs have an effect on CD, but note that we assume that PD is not a function of schooling. Decisions concerning investments are taken each period.[13]

We are interested in finding the effect of PD on CD holding other factors constant. To simplify the problem we assume only two periods (early-childhood and late-childhood), and later on we introduce the effect of the *Familias en Acción* program (treatment) during late-childhood. Figure 2.3 shows a representation of the periods. The late childhood period (without the treatment), represented by CD_2 and PD_2 is defined by

$$CD_2 = f\left(CD_1, PD_2, I_1, S_1, F\right) \tag{2.5}$$

$$PD_2 = n\left(PD_1, I_1, F\right) \tag{2.6}$$

CD_1 captures cognitive development accumulated during early-childhood and measured at the end of period 0; PD_2 captures the accumulated late-childhood physical development measured at the end of period 1; and I_1 and S_1 parents' investments and school decisions

[13] In our empirical work we also incorporate in the model the role of the age of intervention based on the evidence of sensitive periods (Cunha, 2007; Lucas, 1998; Rosales et al, 2009) and the delayed effect (k) of PD on CD, represented by the k^{th} lag of the PD as input. Equation (2.3) and (2.4) are defined as $CD_{t+1} = f\left(CD_t, PD_{t+1-k}, I_t, S_t, F\right)$ and $PD_{t+1-k} = n\left(PD_{t-k}, I_{t-k}, F\right)$. If $k = 0$ we are considering the contemporaneous effect of PD on CD, but if $k = 1$ we are considering the lagged effect of PD (at age t) on CD (at age $t + 1$).

during period 1. Note that PD_1 is not included in the CD_2 function as it is accounted for in PD_2. Similarly PD_1 captures the effect of early-childhood physical development on late-childhood physical development.

$$CD_1 = f\left(CD_0, PD_1, I_0, F\right) \tag{2.7}$$

$$PD_1 = n\left(PD_0, I_0, F\right) \tag{2.8}$$

CD_0 and PD_0 represents the initial cognitive and physical endowments and we assume them to be independent of the treatment. Also, there are no schooling decisions made in early-childhood.

As physical development and cognitive development are cumulative processes, we can find the total effect of PD over the life course on CD through the effect of PD_1 on CD_2. This is given by

$$\frac{\partial CD_2}{\partial PD_1} = \frac{\partial CD_2}{\partial PD_2}\frac{\partial PD_2}{\partial PD_1} + \frac{\partial CD_2}{\partial CD_1}\frac{\partial CD_1}{\partial PD_1} \tag{2.9}$$

The first term captures the "direct" physical development effect and the second term shows the effect of PD in early childhood through the cumulative nature of cognitive development. We expect both terms to be positive; however we cannot separate them.

Next, we are interested in identifying the effect of *Familias en Acción* on both PD and CD. In our 2 period model all children follow a certain level of cognitive and physical development through early-childhood. The *Familias en Acción* program is a treatment (T) starting and continuing during late-childhood that affects physical development accumulation of one group. PD at the start of period 1 (PD_1) accounts for all the investments made in PD in previous periods (e.g. early-childhood). This feature will be important in the empirical section as accounting for PD before the treatment will account for previous investment decisions. This treatment does not affect all children but only those living in treatment municipalities (T). For those children we have Equation (2.6) defined as $PD_2^T = n(PD_1, I_1, F, T)$. This treatment shock will have a permanent effect on CD_2 given by $\frac{\partial CD_2}{\partial T} = \frac{\partial CD_2}{\partial PD_2^T}\frac{\partial PD_2^T}{\partial T}$. If $\frac{\partial CD_2}{\partial PD_2^T} > 0$ then the shock will have a positive effect on CD_2. We assume that $CD_1 \perp T$ and $PD_1 \perp T$. We are aware that there are other potential channels (e.g. labour supply, intellectual stimulation) through which T could improve CD, we will explore these alternative channels in the robustness section.

2.4 Estimation Strategy, Data and Results

2.4.1 Identification and Empirical Specification

Low socioeconomic status environments and poor nutrition are associated with poor cognitive ability (Grantham-McGregor, 1995; Hagger-Johnson, Batty, Deary, & von Stumm, 2011; Rosales et al., 2009; Walker et al., 2011). Generally factors such as income, parents' endowments and living conditions determine both nutritional status and cognitive development. Because the same factors explain PD and CD,the identification of a causal link between improved nutrition and cognitive status is a challenge, For example, a positive effect of better nutrition on cognitive development can be confounded with better living conditions. To determine the causal effect of nutritional status on cognitive development, we exploit the exogenous variation in physical development given by the *Familias en Acción* intervention. We estimate the effect of physical development on cognitive development instrumenting for anthropometric measures of nutritional status. We thus estimate an intent-to-treat effect. This approach rules out the potential endogeneity between physical and cognitive development. We calculate the short, medium and long run intent-to-treat effect to look at the persistence of the program effect. The anthropometric measures at the baseline are used as an additional instrument to account for the cumulative effect of physical development before the program. Following from equations (2.5) and (2.6), the estimation equations are:

$$CD_i = \alpha_0 + \alpha_1 PD_{ki}^t + \sum_{j=1}^{k} \beta_i X_{ijm} + \varepsilon_m \tag{2.10}$$

and,

$$PD_{ki}^t = \gamma_0 + \gamma_1 T_m + \gamma_2 PD_{ki}^{t_0} + \sum_{j=1}^{k} \delta_i X_{ijm} + \vartheta_m \tag{2.11}$$

where CD_i is the cognitive ability indicator for the child i. PD_{ki}^t is the k^{th} anthropometric measure[14] in period t for child i; and X are control variables at child, household (j) and municipality (m) level. In equation (2.11) we estimate the intent-to-treat effect on the anthropometric measure, where T_m takes the value of 1 in the municipalities the program was operational and 0 otherwise, and $PD_{ki}^{t_0}$ represents the anthropometric measure at the baseline. As the treatment variable is at the municipality level we cluster the standard errors at this level in both equations. Note the specification of PD_{ki}^t is assuming a cumulative technology as described in the theoretical framework i.e. it is a function of initial PD, and the treatment is included as described in equation (2.6).

We first estimate the ITT effect of the program on nutrition 1, 4 and 10 years after the intervention started in equation (2.11). The coefficient γ_1 captures the impact on nutrition coming from the exposure to the nutritional intervention $t - t_0$ years after the implementation of the program. As children in the sample have low height and weight, γ_1 is expected to be positive. We also expect γ_2 to be positive as it is capturing the cumulative nature of physical growth during childhood. If improvements in physical development are related to improvements in cognitive development, we expect α_1 to be positive. When $\gamma_1 = 0$, we interpret the result as no program effect on nutrition. Finally we interpret having simultaneously $\gamma_1 > 0$ and $\alpha_1 = 0$ as a positive effect from the program on nutrition but an insignificant effect from nutritional status on cognitive development. In the theoretical model we assumed that $CD_1 \perp T$. Unfortunately, we cannot test this as there is not information on cognitive ability at the baseline or in any other wave. We

[14] $k \in \{weight, height, BMI\}$

assume that cognitive development at the baseline is uncorrelated with the implementation of the program *Familias en Acción*. We consider this assumption plausible as the testable variables, including the nutritional status of the children, are similar in treatment and control areas ($PD_1 \perp T$ and $F \perp T$). Those results are shown in the following data section.

For instrumental variables estimation to yield unbiased estimates, the exclusion restrictions must hold. The main assumption made here is that for the children in our sample, the program has an impact on cognitive ability only through improvements in nutritional status. This is a strong assumption. For example, it is possible that participation in the nutrition component of the program might lead parents to prefer to enrol their children in school earlier in order to become eligible for the education component of the program. Also in some households there will be older school-aged siblings who are already participating in the educational component of the program and this might change parental preferences for investments in items that improve cognitive development for all the children in the household (e.g. books or toys); and/or might change parental preferences over childrens time allocation towards tasks such as homework.

We explore these potential violations of the exclusion restrictions below and do not find evidence in support of such violations. We show in the robustness section that there are no differences in age of enrolment of children in treatment and control municipalities. We also rely on Attanasio et al.,(2010) who do not find differences in school attendance and time allocated to income generating activities by young children in treatment and control areas. Attanasio & Mesnard, (2006) also find no evidence of differences in education expenditure. Finally we re-estimate our main model to test the impact of exposure to the educational component amongst some subsamples finding no impact. The combination of these results suggest that children in treatment communities were not systematically more exposed to cognitive stimulation, sent earlier to school or had more time to play or do homework.

2.4.2 Data

We use the data from the *Familias en Acción* program evaluation, which consists of four waves of surveys over a 10 year period. The first one in 2002 was used to establish the baseline before the program started. One year later the first follow-up was performed in order to establish the initial impact of the program. By 2006 the second follow-up was collected to assess the medium term impact of the program. At the end of 2011 and the beginning of 2012 a third follow-up was conducted to assess the long term impacts of the program.

The evaluation sample was designed in a way that allowed a valid comparison of outcome variables from areas where the program was going to operate with areas where the program was not going to operate. The implementation of the program was not random; instead all the eligible municipalities were offered the opportunity to participate and eventually the program became operational in all of them. The evaluation of the program was then designed based on a quasi-experimental methodology. From the treatment municipalities, 50 Primary Sample Units (PSU)[15] were selected for the evaluation using a stratified random sample, controlling for regional, socioeconomic and infrastructure variables. The control PSUs were drawn from the remaining municipalities classified as rural that were not eligible for the program, mainly because they did not have a bank branch or did not send the documentation for the diagnostic evaluation done by the FIP[16]. For each treatment PSU a similar control PSU was matched within the same stratum. The selection of control municipalities considered the number of inhabitants, urban-rural composition, a quality of life index, and an index of school and health care availability in each of the strata identified in the treatment PSUs. At final count, the sample was made up of 122 municipalities, 57 were treatment and 65 were control; a total of 11462 households, 6773 from treatment and 4689 from control municipalities; and a total of 68608 people.

The sample selected for our analysis was made up of children aged 2 to 6 at the start of the program (2002) in rural areas. There are three main reasons for the selection of this area and age range. First, previous evidence found that the program had a positive effect on health and anthropometrics of children in rural areas (Attanasio et al., 2005). Second, the expansion of the program to control municipalities five years after its start

[15] A PSU was usually one municipality, but in cases where the number of eligible families was small, one or more municipalities were added to the PSU following geographical restrictions and population characteristics.

[16] In a small number of cases, the controls included some municipalities just above 100,000 inhabitants.

limits the comparability of the original control and treatment municipalities. We avoid cross contamination of the nutritional component of the program in the control sample by excluding children 0 and 1 at the start of the program. And third, the data used for this analysis comes from the long term impact evaluation of the program. The government had included a language development test for children who were 0 to 1 at the base line; and for ethical reasons it was decided to not impose more than one test on the children. As a result the cognitive test we are interested in (The Raven test) was only implemented on children who were 2 or older at the base line.

The variables of interest for the analysis are physical and cognitive development. Physical development is represented by anthropometric measures taken in all the survey rounds for children aged 2 to 6 at the baseline. Those children were weighed and measured in the four survey rounds and in the last round they were also tested using a version of the Raven's Progressive Matrices Test in order to measure their cognitive development. We were interested in this particular test as the Raven's Progressive Matrices Test is a non-verbal multiple choice test which is often referred to as a general intelligence test. The Raven's test has the advantage of measuring cognitive ability independently of educational factors; as a result the test minimizes the effects of language and culture on performance. The Raven test has a raw score between 0 and 12. Scores between 0 and 5 reflect a low cognitive performance, scores between 6 and 10 reflects medium and 11 or above outstanding performance. Figure 2.4 shows the distribution of the Raven test score of the children in *Familias en Acción* at the end line. This sample has low cognitive development, the average score is 5.19 and the standard deviation is 2.86. Comparing the performance with other children of similar ages in Chile or Germany, we find that our sample performs on average 4.5 points lower. A more comprehensive description of the cognitive ability assessment done in the fourth wave is presented in Appendix A2 - 1.

There are, however, four underlying issues to be considered in order to use the resulting dataset. The first three are related to the timing of program implementation and operational changes and the last one relates to the challenges of the panel-surveying process itself.

Firstly, despite the baseline being designed and planned to contain information about the households and people before the program started, for political reasons the program was implemented in some municipalities before the baseline survey date. Of the 57 treatment municipalities selected, 26 were already running the program. This means that some households were already receiving the payments by the time of the survey. We exclude these municipalities from the analysis to rule out any anticipation effect and keep a pure baseline comparison. Including those municipalities could lead to an underestimation of the true effect of the nutrition intervention. Effects of nutritional intervention can have a rapid effect on outcomes and also households could modify their decisions according to the expected benefit.

Secondly, by 2005, the program was expanded by the government, just as the second follow-up was performed at the end of 2005 and at the beginning of 2006. Consequently 13 of the previous control municipalities were interviewed as treatments. We keep these municipalities in the estimations as control municipalities, but we discuss the sensitivity of our estimation results to the exclusion of these municipalities in the robustness section. Overall levels of migration during the treatment time (2002-2006) were low (lower than 1.5%), reducing the probability of contamination.

Thirdly, given the evidence of success of the program, the government decided to expand the program to all eligible areas in 2007 which meant that all the municipalities previously taken as control municipalities started to participate in the program. This expansion of the program can generate confounding effects when directly comparing treatment and control municipalities. For variables like education and wealth where exposure to the program and its intensity is still ongoing, differences between treatment and control municipalities after 2007 cannot be related to program effects. For other variables like nutritional status, comparisons between the original control and treatment municipalities can still be done. As explained in the experimental design section, our sample selection takes advantage of this expansion of the program in the municipalities. The implication of the program implementation in control areas means that all children started to receive the intervention in 2007. By that year, the children aged 2 to 6 eligible for the nutrition incentive at baseline were already 7 to 11, which means that the original control group started to receive a transfer upon school attendance. However because of their age, none of them were eligible for the nutritional component of the program. In terms of the nutritional intervention we still have a clean control group, we will be comparing children 2 to 6 in 2002 that were exposed to the nutrition and to the education incentive after age 6 (treatment group) versus those who were exposed only to the education incentive after age 6 (control group).

Finally, in spite of the large number of households that could be tracked in all the follow-ups, there was a proportion that could not be found. Additionally, due to budget restrictions only a sample of 70% of the original sample was randomly tracked in the third follow-up. The level of attrition was 6.2%, 17.1% and 28.8%[17] in the first, second and third follow-ups respectively. This level of attrition in the second and especially in the third follow-up is high and may generate biases in estimation, especially if the attrition is not random and is related to the variables of interest or to the program operation[18]. We conduct tests to verify the randomness of the attrition. We find that the attrition is on average not related to the treatment or to the initial nutritional status of children. From those results we conclude that there is low risk of attrition bias. The performed tests and results are presented in Appendix A2 - 2.

Our sample consists of data from children who at the start of the program were aged 2 to 6, that have anthropometric measures during all survey rounds, and that did the Raven test. The final sample includes 506 children in the treatment group and 467 children in the control group.

We find that this sample is balanced at the baseline and consists of children with low levels of physical development. Table 2.1 panel A shows the difference in physical development indicators at the baseline between children in treatment and control municipalities; we find none of the differences are statistically different from zero, suggesting that we have a balanced sample in terms of physical development at the baseline. When comparing

[17] These figures represent the percentage of households who were included in the sample to be tracked but were not found.

[18] The raw attrition rates in the first follow up were 5.6% and 6.7% for control and treatment municipalities. In the second follow up attrition was 17% in both treatment and control communities. And finally, in the third follow up attrition was 31.2% and 27.0% for control and treatment municipalities respectively

the nutritional status of the sample analysed with the population of reference[19] in panel B, we find that they are underweight and shorter in all age groups. We also present the differences in observable characteristics between treatment and control household at the baseline in Table 2.2. There are no differences except for the possession of a motorcycle, quality of walls in house, other assets and chores as the main activity in the household heads. However, these differences do not show a systematic pattern, and so do not suggest that treatment municipalities are systematically richer or poorer. We will include these variables as controls in the estimations.As mentioned before treatment municipalities were matched with control municipalities. Table 2.3 presents tests of differences of the main indicators at municipality level. We find that the only statistically significant difference is the number of bank branches as specified in the experimental design. We conclude there is fair evidence of balance at the baseline between control and treatment households in terms of the children physical development and most other characteristics. Hence we expect fair comparability of child outcomes between treatment and control municipalities[20].

The indicator we are using for cognitive development is assumed to be independent of schooling and cultural context and is a measure of general intelligence. However, to rule out systematic differences across municipalities we include municipality education indicators such as education attendance rate, adults' average years of education and education supply (for example, number of public schools in rural and urban areas and the teacher/student ratio). Moreover, we control for the time spent by the child in completing the test, to account for differences in the level of understanding of the task. Second, we include controls for variables that differ between treatment and control at baseline. As shown before, only a few variables are statistically different (shown in table 2.2). Third, if the assumption that cognitive and physical development are cumulative technologies holds, including explanatory household, child and parent characteristics at baseline accounts for the investment decisions at treatment time and physical development at baseline accounts

[19] The reference standards come from the WHO – Multicentre Growth Reference Study which collected growth data from around 8500 children from a diverse ethnic background and cultural settings. The countries included were Brazil, Ghana, India, Norway, Oman and the USA.

[20] In chapter 3 we find that when comparing observable characteristics of all households with children 0 to 6 at the baseline not only in rural areas but in urban this balanceness does not hold. It is still not clear why this is the case but it seems that households living in urban areas of the control municipalities as somehow better off.

for all previous investments. Additionally, most of the household and parent characteristics are assumed to be time invariant. All investments after the program started should be accounted for by the physical development measure at the follow-up. Finally, we include age fixed effects in all estimations.

2.4.3 Results

First Stage

Table 2.4 presents the ITT estimates (first stage) of the difference in each anthropometric measure between children in the treatment and in the control group. This treatment effect is the parameter of interest that if significant will help us to account for the potential endogeneity between cognitive and physical development. Standard errors clustered by municipality are presented in parenthesis.[21] When comparing children in treatment and control municipalities one year after the intervention started (Panel A: short run effects), those in treatment municipalities weighed on average 0.20 kg more (column 1), were 0.21 cm taller (column 2) and had 0.21 s.d. higher BMI (column 3). However, none of those differences are statistically different from zero at the 5% significance level. Other explanatory variables (household and family characteristics) included in the model are mostly not significant (see appendix A2 - 3), which is consistent with the theoretical model of the cumulative nature of physical development. In essence the physical development variable at the base line captures previous investments[22]. When looking at the program effect on medium and long-run physical development we find more promising results.

Panel B presents the effect of the program four years after the start of the implementation (medium run effects) comparing the baseline with the second follow-up. We find that

[21] Full set of results are presented on Appendix A2 - 3.

[22] When looking at the F stat for weak instruments, the equations for weight and height (columns 1 and 2) have a higher F than the rule of thumb of 10 but for BMI (column 3). The combination of these results implies that even when the anthropometric measure at the baseline accounts for the cumulative nature of physical development (F stat higher than 10), the program is not having an effect on physical development in the short-run ($\gamma_1 = 0$). We present Shea's partial R^2 measure expected to be large enough (as suggested by Bound, Jaeger, and Baker (1995)) to test the relevance of the instruments. We also include robust Kleibergen-Paap Wald F statistic as we are clustering errors by municipality. They are compared to Stock-Yogo critical values.

on average children in treatment municipalities weigh half a kilogram more than their counterparts in control municipalities controlling for age and their BMI was 0.25 s.d. higher. We do not find a treatment difference in terms of height. These program impacts are statistically significant at the 5% level.

Finally, panel C shows the effect of the program 10 years after the start of its implementation (long run effects). Children at age 12 to 16 are on average 1.4 kilos heavier than children who were not exposed to the nutritional intervention. It is of particular note that we are not finding differences in terms of height between treatment and control children. The literature suggests that height is a more accurate indicator of long term nutritional status. We find a correct sign on the height measure in both the medium and long term but it is in all cases statistically insignificant. Explicitly, kids who were already 2 years old at the baseline are unlikely to become taller. We find a significant positive program effect on BMI in the medium and long run that is statistically significant at the 5% and 10% level, respectively. However, we treat these results cautiously as the instruments in the BMI equations are weak.

We conduct F-tests of the validity of instruments. The F-statistic is higher than 10 in the short, medium and long run for weight and height. We will focus on the results where the F stat is greater than 10 and the treatment effect is statistically different from zero as we are interested on the effect of the exogenous variation associated with treatment[23].

Second Stage

Table 2.5 presents the effect of physical development at first to third follow-ups on cognitive development at the third follow-up (equation 10)[24]. The first trio of columns -1 to 3- shows the effect of physical development at first follow-up (which accounts for the short-run effect of the program) on cognitive development. The second and third trio show the

[23] Note that the F-stat > 10 for height is driven by the height at the baseline. This is expected for all physical development measures. For height, it is expected as height in a certain period is determined by previous height levels (accounting for differences in investments prior the program). The opposite example is BMI where the treatment effect is statistically significant but BMI at the baseline is not. For weight these results indicate a valid first stage only in the medium and long run.

[24] Full set of results are presented on Appendix A2 - 4.

effect of physical development at the second and third follow-up on cognitive development, respectively. The first column of each trio shows the OLS estimates. While the second column shows the IV estimates on the raw test score, the third column of each trio shows the IV on the standardized test score[25].

In panel A, we find that in the one year comparison, an increase in children's weight by one kg will on average increase cognitive development by 0.09 units in the raw score, equivalent to 0.3 standard deviations. Similarly the effect of a one kg increase by the second and third follow-up respectively increases the cognitive development score by 0.10 and 0.05 units, representing 0.3 and 0.2 standard deviations. We interpret the results as a positive causal relationship between weight and cognitive development. This effect suggests increases of 0.07, 0.20 and 0.25 s.d. in the cognitive development of children in the short, medium and long run, respectively as a result of the nutritional intervention at age 2 to 6.

When looking at the effect of weight on cognitive development in Panel A, we find that in most cases the OLS estimator is insignificant even when the standard errors are very similar. A comparison of the OLS and IV point estimates shows that OLS underestimates the effect. This suggests that the endogeneity correction reflects a negative relationship between physical development and cognitive development. This is unexpected and it is not clear why this would be the case. When performing a Durbin-Wu-Hausman test for endogeneity, we find that in the short and long run we reject that the OLS estimates are consistent and fully efficient.

Panel B and Panel C show the effect of height and BMI. Changes in height are positively associated with cognitive development in the first and third follow-up. However, we need to be cautious about concluding that this is causal as the exogenous variation associated with the treatment in the first stage was not significant. BMI in the third follow-up has a significant effect on cognitive development even though the instruments are weak.

To summarize, we find that a one kilogram increase in weight 1 year after the program results in a 0.07 s.d. increase in cognitive development of children. If this weight gain is

[25] We include this column to make the interpretation of the results easier. The standardized score is calculated within the sample.

sustained at 4 years after the program, we find a 0.2 s.d. increase in cognitive development and if sustained 10 years after then a larger cognitive development gain of 0.25 s.d.. We put these results in perspective by comparing them with the point estimates obtained by Maluccio et al. (2009) when analysing the effect of an early childhood nutritional supplementation intervention in Guatemala on adult's cognitive development (measured with the same test). They find that 3 years of exposure to the intervention is associated with a 0.25 standard deviation increase in cognitive development. Hence our findings suggest that CCT programs can have an impact on cognitive development of similar magnitude to a food supplementation program.

Effect of the program on Height

One of the most recognized variables in the literature as being a good representation of long term nutritional status is height. The previous results suggest that the program is not having an effect on height in the short, medium and long run. A possible explanation for this is that the children in our sample are over 2 at baseline. If height is largely determined by age 2 as it is well established in the literature (Martorell, 1995; Shrimpton et al., 2001), it is unlikely that we can find a systematic positive effect.

To test if γ_1^{height} is equal to zero because the sample of children is outside the critical age range 0-2 rather than there being no program effect; we estimate the IV regression using the sample of children 0 to 1 at the beginning of treatment. We could not include this sample in our initial estimation because the Raven cognitive ability test was not conducted on those children[26]. However an alternative test was administrated to these children –Peabody Picture Vocabulary Test (PPVT). The PPVT is a test of receptive vocabulary and is an indicator of language/verbal ability or scholastic aptitude. We are aware this is not an ideal test because it is highly affected by schooling and language stimulation. However, we examine whether height affects Peabody test scores controlling for parents' education, education supply (number of rural schools, number of urban schools and number of students per teacher), average years of schooling in the municipality and

[26] The government did not allow us to do the Peabody and Raven test for this sample as this could be too onerous for children and also could affect the accuracy of the test.

household schooling preferences including average age of enrolment into school in the household. We reestimate equations 2.10 and 2.11 using the sample of children 0 to 1 at baseline with the receptive language test as our measure of cognitive development.

Table 2.6 presents the first stage of the estimation. We find that for this sample (0 to 1) one year after the intervention started, children in treatment municipalities are on average 0.67 cm taller than children the same age in control municipalities and they are 0.28 kilos heavier. Looking at the long term effects we find that almost 10 years after the start of the intervention children in the treatment municipalities are 1.67 cm taller and 2.12 heavier than children in control areas, which reflects a long term positive impact on long term nutritional status. We highlight the positive and significant effect obtained in height for this sample.

When we look at the second stage in Table 2.7 Panel B we find that height has a positive causal effect on language cognitive development. An increase of 1 cm in the second follow-up increases the language cognitive development by 0.25 standard deviations; similarly an improvement of the same size in the third follow-up increases language cognitive development by 0.22 standard deviations. In Panel A we also find that an increase of 1 kilogram in weight in the second and third follow-up, respectively cause an increase of 0.99 and 0.54 in the score of language cognitive development (0.6 and 0.3 standard deviation). This result in weight is associated with an increase in cognitive development of 0.27 and 0.69 standard deviations[27] in the medium and long run due to the nutritional intervention.

Overall, we argue that this result is coherent with the literature's findings that height is usually determined by age 2 years and this age range is recognized as a sensitive period where external factors have a high impact[28]. This means that the program is having impact on weight and height for rural children aged below 2 and on weight for those over 2. The causal relationship between physical growth and language cognitive development seems to be positive.

[27] The effect is calculated by multiplying the treatment effect coefficient on weight in the first stage by the coefficient of weight on cognitive development in the second stage.

[28] The nutrition and medical literature shows that critical periods and re-programmig for height is under 2 years old (Martorell, 1995; Shrimpton et al., 2001).

A challenge arising from these results is that we cannot distinguish in the estimations the isolated effect of the nutrition intervention from the school intervention. All these children were eligible for the schooling intervention and as has been mentioned before, language development is a highly flexible cognitive skill and affected by the environment, such as school attendance[29]. Note however, that we control for age of enrolment and schooling inputs at the municipality level. We address this education effect in the following section, finding no major concerns in the interpretation of the results presented.

2.5 Robustness Checks

2.5.1 Education and school inputs

The human capital literature recognizes the relevance of school inputs to cognitive achievement and skills formation (Cunha, Heckman, Lochner, & Masterov, 2006; Todd & Wolpin, 2003, 2007). The results presented above assume that municipality average schooling, children's attendance at school and school quality and provision are similar in treatment and control municipalities. If some municipalities are systematically more educated than others, this could affect the ability of the instrument to capture the nutritional impact on cognitive development. If treatment affects household schooling decisions which in turn affect cognitive development, this would invalidate treatment as an instrument. Remember that after the expansion of the program all children (treatment and control) were eligible for the education component of the program after age 7. However, before the expansion, school decisions could potentially have been different between treatment and control municipalities. For example, if children's attendance or age of enrolment was different across treatment and control municipalities due to children enrolling early to participate in the program in treatment municipalities, the result for cognitive development would be capturing the combined effect of the nutrition and education intervention. We argue and present evidence to show that this is not the case.

First, the Raven test is designed to be independent of education factors. Hence the above

[29] All children in the sample were enrolled in school at the time of the third follow-up interview.

concern is less valid in the results that use the Raven test. The Peabody test however is more sensitive to education factors. To address this concern, in the Peabody equations we additionally controlled for differences in school quality and infrastructure, including municipality variables of school supply and schooling and literacy rates in the municipality.

Second, rural attendance of children from primary school is relatively high, 90%, limiting the room for improvement and enrolment decision changes because of the program. For the youngest children, there was no difference in attendance between control and treatment municipalities after the program was implemented, only for children in late primary school there was a difference of around 2 percentage points in rural areas. Most of the effects on school enrolment were found in secondary school attendance (Attanasio et al.2010). This means that the educational incentive did not affect attendance for children in municipalities where the program was implemented.

Third, to further explore the unequal exposure of children to the educational component we estimate equations 2.10 and 2.11 using only those children aged 2 at the baseline. For children in this subsample, both control and treatment communities had exactly the same exposure to the educational component (see figure 2.1). Table 2.8 present the results. They are comparable to the ones presented in the main results section, although they are less precise due to the smaller sample. This is encouraging as this is a cleaner sample in terms of the exposure to the educational component of the program.

Fourth, in all estimations we included the average age of enrolment in the household and child age of enrolment as control variables. These variables are expected to capture the household effect of schooling enrolment preferences and the specific enrolment age effect. In the main results, while the actual age of enrolment seems to not have an effect on cognitive development, higher average age of enrolment in the household reduces the cognitive test score. The fact that age of enrolment is not significant is important as this suggests that the individual age of enrolment of the child does not drive differences in cognitive development. The household effect seems more important. Average school enrolment in the household is, as expected, negatively related to cognitive development. This finding is important as our empirical strategy assumes that the program has an impact on cognitive development only through improvements in physical development. This assumption would not hold if household participation in the educational component of the program changes the behaviour and educational decisions of parents towards their children. For example, if older children are already participating in the educational component of the program, they may decide to enrol younger children in school earlier so as to be eligible for that component once they meet the age requirement. We test if there are differences by treatment by regressing average age of school enrolment in the household on treatment. The results are presented in table 2.9. They suggest no differential school decisions within the household between treatment and control municipalities.

Alternatively, we test if household exposure to the education intervention affects our results. We estimate equations 2.10 and 2.11 controlling for the number of older siblings eligible for the educational component of the program at the baseline. Results are presented in Table 2.10. The results are unaffected and the coefficient on the number of older siblings is insignificant.

Finally, if school provision and quality are different between treatment and control municipalities, this could also bias the estimates of cognitive development. For example, if schools in treatment areas have more and better educational resources, higher cognitive development is expected; as a consequence the program effect would be overestimated. However, school quality and school resources have relatively small effects on ability deficits and have little effect on test scores by age across children from different socioeconomic groups (Cunha, Heckman, Lochner, & Masterov, 2006). By the time children were 7 years old, they were eligible to participate in the education stimulus of the program upon school enrolment. We do not find evidence that supports a differential school input between children in treatment or control municipalities, as mentioned before, in rural areas the program has no effect on school attendance in primary school as a result of already high rates of enrolment at the primary level.[30] Note also that a minimum level of school infrastructure was a criteria used to select treatment and control municipalities and also that the program did not provide any additional support to improve schools. We expect that

[30] The program can affect the age of enrolment in initial treatment municipalities. We include a control for this in the estimation. However, in the impact evaluation report (IFS-Econometria-SEI, 2006) it was found that the program had no effect on year of enrolment in rural areas.

the effect of school inputs on children's cognitive development is similar in treatment and control municipalities.

2.5.2 Parents' Labour Market Participation

A change in parental labour supply is another avenue via which treatment could affect cognitive development. If so, this would invalidate treatment as an instrument. In the models presented in Tables 2.4 and 2.5 we included variables reflecting parents' labour market participation to control for income provision in the household. We are aware that the transfer of the program could generate perverse incentives that decrease labour market participation in the household; as there could be a substitution income effect in the household. Decreased labour participation could affect cognitive development if spending on child development is reduced, or if that decrease leads to an increase of parental time allocated to child care and cognitive stimulation. We find both situations unlikely but examine this empirically below.

If labour income is replaced by the cash transfer we expect no change in consumption. However, (Attanasio & Mesnard, 2006) shows evidence of increased food consumption in the household and no increase of adult items consumption when analysing the households expenditure. Increased consumption is contrary to the hypothesis of an income substitution effect. The number of hours worked weekly and labour force participation of the head of the households are not affected by the program. The evaluation report of the program (IFS-Econometria-SEI, 2006) shows that the program, on the contrary, slightly increased labour supply in treatment rural areas by 2.7 percentage points when compared to control areas. They also reported no program effect on the number of hours worked by adults in rural areas.

We examine labour participation looking at the numbers of hours worked by the head of the household. Table 2.11 presents a differences-in-differences tobit estimation[31] of

[31] We use an OLS as well as a tobit estimation to account the censored at zero feature of the dependent variable for some household heads that reported that they worked 0 hours. In rural areas 13.9%, 18.5%, 17.5% and 19.0% of the household heads reported to have worked 0 hours in the previous week of the survey by the baseline, first, second and third follow-up, respectively.

the program effect on number of hours worked by the household head. We estimate the program effect in the first, second and third follow up finding no program effect in all of them when comparing treatment and control municipalities[32].

2.6 Conclusions

We estimate an IV model to overcome the endogeneity between physical and cognitive development. Physical development was represented by anthropometric measures in our model. We develop a simple model of nutrition absorption to show how the different inputs of a conditional cash transfer program are expected to influence physical development as captured by weight, height and BMI. This change in nutritional absorption will help us to account for the effect of physical development on general cognitive development.

Our main findings are that short term impacts on nutrition do not lead to improvements in cognitive ability but longer term nutritional gains improve cognitive ability. When comparing children aged 2 to 6 from the poorest rural families in Colombia four years after the starting of the intervention we find that gains in weight lead to an increase in cognitive development. Also when comparing weight improvement 10 years after the intervention between children that received the intervention and those who did not, we find that gains in weight and reductions in underweight lead to even larger gains in cognitive development. We do not find evidence of the program increasing height in this sample. This is consistent with only interventions before age 2 having an impact on long term height gain. When limiting the sample to children aged 0 to 1 at the start of the intervention we find a positive effect of the program on height (and weight). Additionally, we find a positive causal relationship between physical and cognitive development measured by receptive language. We highlight the fact that the sample under analysis is made up of children who are at risk of being underweight[33] and stunted; and have low weight and height even after the intervention. This is important because lack of nutrients prevent cognitive

[32] It is also possible that the program could affect the age at which the child started to participate in income-generating work and that this might have a negative relationship with cognitive development. Attanasio et al. (2010) however finds no effect on child labour participation, and hours spent working (in income-generating activities or domestic work) in rural areas.

[33] Between 0 and 1 standard deviation bellow the norm.

development, but extra nutrients given to children with normal nutritional status do not necessarily have a positive effect on cognitive development. That is, these results should not be extrapolated to a well-nourished population.

In both exercises we find an increasing total effect of the nutritional intervention on cognitive development over time. When using the Raven cognitive test and comparing the effect of the weight improvement using different lags we find that for children aged 2 to 6 the total effect of the treatment on cognitive development is 0.06 (short-run), 0.20 (medium-run) and 0.25 (long-run) standard deviations. This increase in cognitive development in the long run as a result of improvement in weight is equivalent to an increase in mother's years of education by 20% for this sample. When using the Peabody cognitive test we find that for children 0 to 1 the nutritional program is associated with an increase in cognitive development due to sustained height gains (over the same periods) of 0.12, 0.18 and 0.38 standard deviations. The increasing total effect over time of the nutritional intervention is consistent with the theoretical framework that suggests cognitive development is a cumulative process.

The findings have important implications in terms of human capital accumulation, not only because this is one of the main aims of conditional cash transfer programs, but also because increased cognitive development has been found to have important effect on future wages (Hoddinott et al., 2008; Maluccio et al., 2009). Early childhood investment improves readiness for school and school attainment. This is translated into higher wages in adulthood. Furthermore this increases the likelihood of better provision and investment in nutrition, education, stimulation, health care and education for their own children.

There are two main policy implications. First, given we find that medium and long run physical development predominantly explains variations in cognitive development, evaluations that consider only short run nutritional impacts of programs can be underestimating the effect of the program in the long run and may mislead decision-making in terms of continuation or discontinuation of programs. Second, since 1970 the identification of cost-effective programs to reduce malnutrition has been at the heart of public policy discussion (Martorell, 1980)[34]. When determining which kind of program to implement in order

[34]Although a cost effectiveness comparison of both programs is relevant from the policy point of view,

to boost human capital among poor families, it is important to understand the trade-off between supplemented food distribution that implies heavily monitored programs, versus less administratively intensive conditional cash transfer programs to achieve sustained long-term effects on nutrition and cognitive development. This paper finds that nutritional impacts of conditional cash transfer programs are similar in magnitude to those of supplemented food intervention programs (Maluccio et al, 2009).

it is beyond the scope of this thesis.

2.7 Figures

Figure 2.1: Cognitive Ability Sample Composition

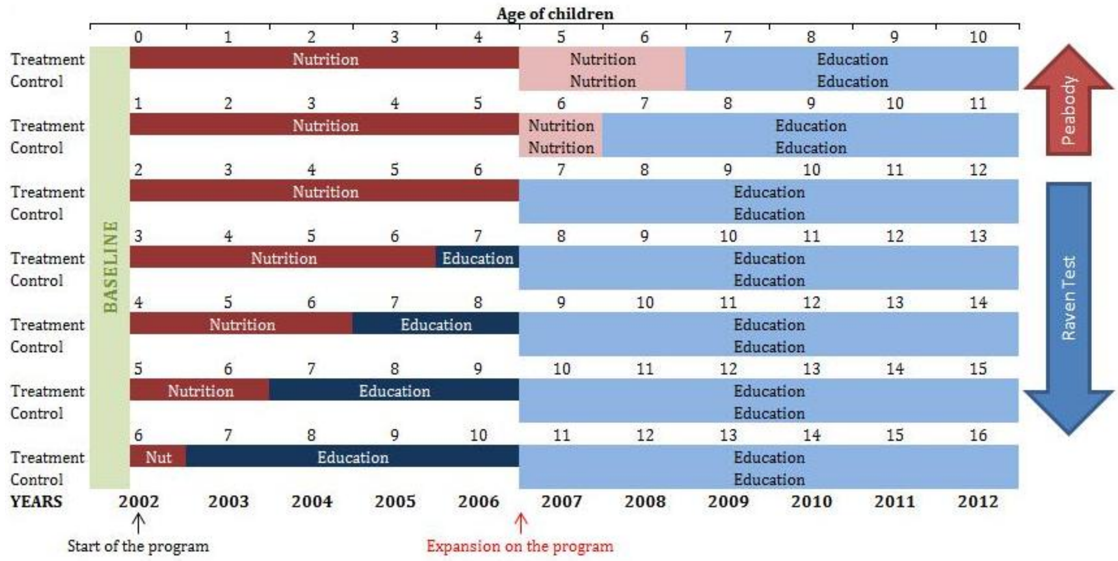
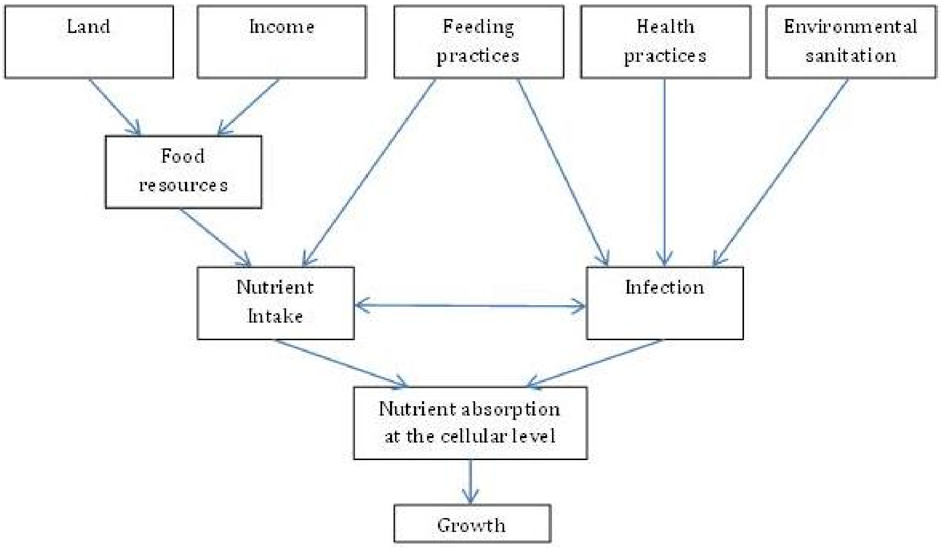


Figure 2.2: Possible mechanisms of child growth



(Martorell & Habicht, 1986).

Figure 2.3: Cognitive development investment accumulation

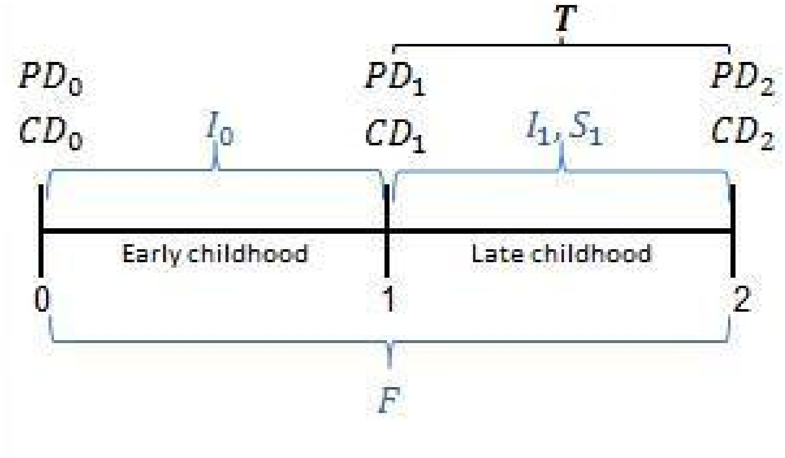
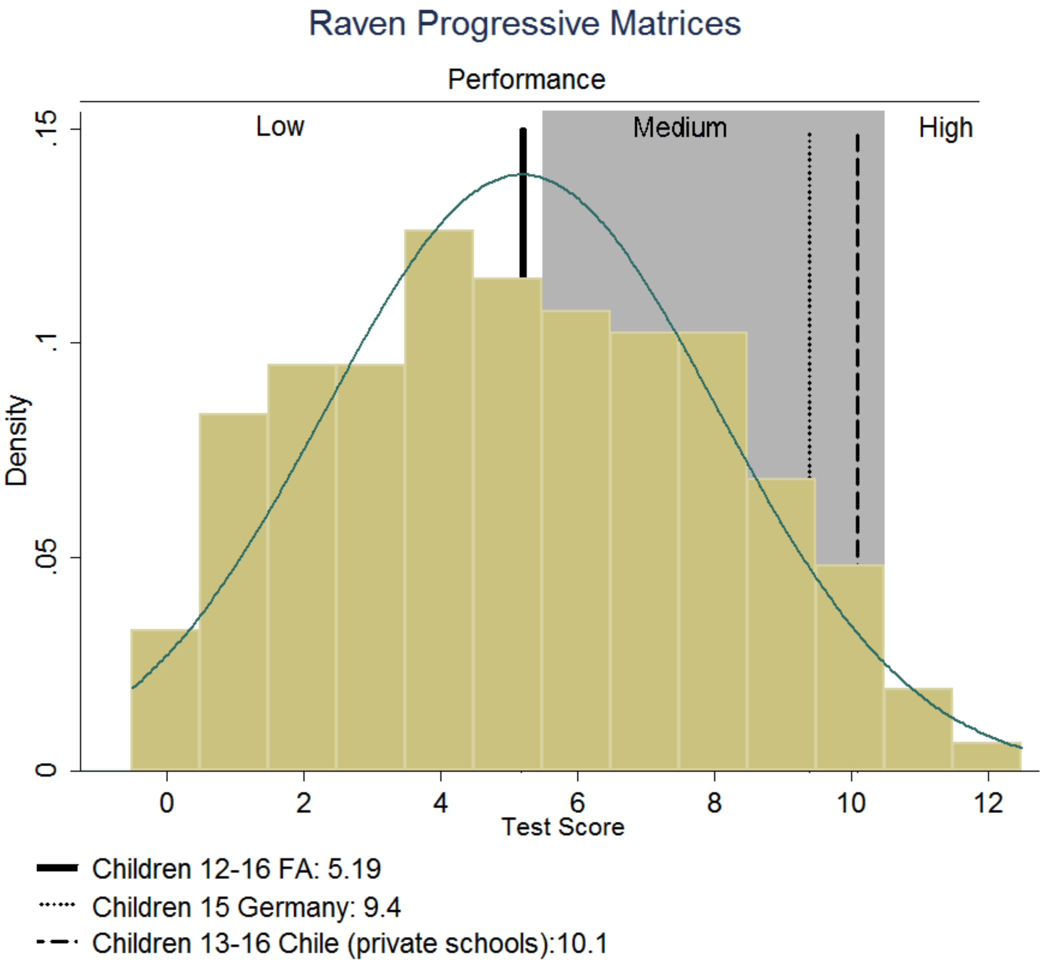


Figure 2.4: Cognitive development Children 12 to 16 – Raw score



2.8 Tables

Table 2.1: Sample means of physical development indicators by age 2 to 6 and other characteristics at baseline

PANEL A

	Treatment (T)	Control (C)	Difference (T-C)
Weight-for-age	12.67	12.851	-0.181
	(0.320)	(0.251)	(0.230)
			[-0.79]
Height-for-age	87.921	88.612	-0.691
	(1.099)	(0.848)	(0.777)
			[-0.89]
BMI	16.437	16.322	0.116
	(0.297)	(0.241)	(0.188)
			[0.62]
Z score-Weight-for-age	-0.834	-0.773	-0.061
	(0.084)	(0.052)	(0.099)
			[-0.62]
Z score-Height-for-age	-1.538	-1.397	-0.142
	(0.146)	(0.077)	(0.165)
			[-0.86]
Z score-BMI	0.28	0.187	0.093
	(0.103)	(0.066)	(0.123)
			[0.76]

Notes: Municipality clustered errors in parentheses. T-stat in squared brackets.
Significance: *** p< 0.01, ** p< 0.05, * p< 0.1.

PANEL B

	Age	Sample mean	Reference population	Difference
Weight (kg)	2	12.73	12.97871	-0.249
	3	13.98	15.15003	-1.17
	4	15.68	17.23881	-1.559
	5	17.25	19.26429	-2.014
	6	19.06	21.36936	-2.309
Heigth (cm)	2	88.09	91.17314	-3.083
	3	93.04	99.36943	-6.329
	4	99.79	106.3717	-6.582
	5	105.5	112.5221	-7.022
	6	111.0	118.1786	-7.179
BMI	2	16.40	15.6662	0.734
	3	16.27	15.38429	0.886
	4	15.71	15.25624	0.454
	5	15.44	15.26018	0.18
	6	15.43	15.34964	0.08

Notes: Reference population WHO - Multicentre Growth Reference Study.

Table 2.2: Baseline Descriptive Statistics

	Treatment (T)	Control (C)	Difference (T-C)
Household Characteristics			
Household has electricity service	0.812	0.860	-0.048
	(0.045)	(0.031)	(0.055)
Household has piped gas service	0.008	0.008	-0.000
	(0.004)	(0.005)	(0.007)
Household has piped water	0.551	0.413	0.138
	(0.066)	(0.069)	(0.095)
Household has sewage system	0.064	0.076	-0.012
	(0.015)	(0.025)	(0.029)
Household has waste collection service	0.043	0.093	-0.050
	(0.012)	(0.029)	(0.032)
Household has electricity ilumination	0.109	0.087	0.022
	(0.035)	(0.019)	(0.040)
Household has access to a landline phone	0.042	0.056	-0.014
	(0.014)	(0.016)	(0.022)
Water facility is inside the house	0.493	0.396	0.097
	(0.069)	(0.069)	(0.098)
Households has WC connected to sewer or septic tank	0.366	0.388	-0.022
	(0.047)	(0.044)	(0.065)
The house has a separate kitchen	0.759	0.702	0.056
	(0.072)	(0.062)	(0.095)
Fuel for cooking: gas in cylinder	0.202	0.230	-0.029
	(0.046)	(0.039)	(0.060)
Fuel for cooking: wood	0.910	0.879	0.031
	(0.018)	(0.024)	(0.030)
Water receives treatment before drinking	0.621	0.638	-0.017
	(0.077)	(0.045)	(0.090)
Households has health access	0.180	0.110	0.071
	(0.039)	(0.024)	(0.046)
Proportion of people per room	3.950	4.206	-0.256
	(0.142)	(0.130)	(0.193)
Household has fridge	0.231	0.202	0.029
	(0.034)	(0.032)	(0.047)
Household has sewing machine	0.066	0.051	0.016
	(0.019)	(0.016)	(0.024)
Household has a black-white TV	0.263	0.258	0.004
	(0.035)	(0.034)	(0.049)
Household has a radio	0.403	0.469	-0.066
	(0.042)	(0.032)	(0.053)

Continued ...

Table 2.2 (continued)

	Treatment (T)	Control (C)	Difference (T-C)
Household has a bicycle	0.334	0.289	0.045
	(0.059)	(0.047)	(0.075)
Household has a motorcycle	0.072	0.022	0.049*
	(0.025)	(0.007)	(0.026)
Household has fan	0.170	0.202	-0.032
	(0.070)	(0.057)	(0.090)
Household has blender	0.398	0.334	0.064
	(0.060)	(0.032)	(0.068)
Household has color TV	0.295	0.261	0.034
	(0.036)	(0.029)	(0.046)
Household has kerosene lamp	0.080	0.096	-0.016
	(0.028)	(0.025)	(0.038)
Household has a boat	0.034	0.028	0.006
	(0.014)	(0.015)	(0.021)
Household has energy plant	0.011	0.006	0.005
	(0.006)	(0.004)	(0.007)
Ownership of this house	1.589	1.736	-0.147
	(0.082)	(0.070)	(0.109)
Household has other assets	0.225	0.121	0.105**
	(0.046)	(0.024)	(0.051)
Household has money saved	0.032	0.022	0.009
	(0.010)	(0.008)	(0.013)
The household has small livestock (chikens, etc.)	0.854	0.848	0.006
	(0.027)	(0.033)	(0.043)
The household has big livestock (cows, etc.)	0.515	0.551	-0.036
	(0.068)	(0.048)	(0.083)
Good quality walls	0.321	0.312	0.009
	(0.030)	(0.043)	(0.052)
Medium quality walls	0.647	0.640	0.007
	(0.037)	(0.043)	(0.057)
Poor quality walls	0.029	0.048	-0.019
	(0.015)	(0.013)	(0.020)
Good quality floor	0.037	0.112	-0.075**
	(0.008)	(0.033)	(0.033)
Medium quality floor	0.472	0.357	0.115
	(0.063)	(0.032)	(0.071)
Poor quality floor	0.491	0.531	-0.040

Continued ...

Table 2.2 (continued)

	Treatment (T)	Control (C)	Difference (T-C)
	(0.065)	(0.044)	(0.078)
Household Composition			
Number of people in the household	6.692	6.483	0.209
	(0.205)	(0.165)	(0.263)
Number of children under 7	2.027	1.944	0.083
	(0.058)	(0.081)	(0.099)
Number of children 7 to 11	1.204	1.135	0.069
	(0.092)	(0.044)	(0.102)
Number of children 12 to 17	0.820	0.815	0.005
	(0.039)	(0.053)	(0.066)
Number of females in the household	1.276	1.343	-0.067
	(0.031)	(0.039)	(0.049)
Household member born in the last 12 months	0.172	0.161	0.012
	(0.020)	(0.029)	(0.035)
Household member died in the last 12 months	0.032	0.022	0.010
	(0.009)	(0.007)	(0.011)
Household member is pregnant	0.075	0.087	-0.013
	(0.013)	(0.017)	(0.022)
Household member migrated in the last 12 months	0.101	0.079	0.022
	(0.015)	(0.015)	(0.021)
Number of households	1.058	1.051	0.008
	(0.021)	(0.016)	(0.026)
Head Characteristics			
Age	41.20	42.16	-0.961
	(0.778)	(0.614)	(0.991)
Single	0.088	0.124	-0.036
	(0.015)	(0.016)	(0.022)
Female	0.080	0.115	-0.036
	(0.014)	(0.017)	(0.022)
No education	0.210	0.213	-0.004
	(0.034)	(0.030)	(0.046)
Incomplete primary School	0.448	0.492	-0.043
	(0.038)	(0.038)	(0.053)
Complete primary School	0.196	0.152	0.045
	(0.020)	(0.022)	(0.030)
Incomplete secondary School	0.058	0.067	-0.009
	(0.013)	(0.014)	(0.019)
Years of education	2.727	2.719	0.008

Continued ...

Table 2.2 (continued)

	Treatment (T)	Control (C)	Difference (T-C)
	(0.186)	(0.130)	(0.227)
Main activity: Work	0.873	0.879	-0.007
	(0.021)	(0.015)	(0.026)
Main activity: Chores	0.019	0.039	-0.021*
	(0.006)	(0.010)	(0.011)
Weekly hours worked	47.097	45.749	1.348
	(1.163)	(1.089)	(1.593)
Monthly Income	209,827	168,203	41,624
	(23,296)	(10,542)	(25,570)
Spouse Characteristics			
Age	35.39	36.19	-0.806
	(0.705)	(0.698)	(0.992)
No education	0.174	0.167	0.008
	(0.033)	(0.034)	(0.047)
Incomplete primary School	0.480	0.484	-0.004
	(0.035)	(0.051)	(0.062)
Complete primary School	0.212	0.199	0.013
	(0.032)	(0.031)	(0.045)
Incomplete secondary School	0.055	0.083	-0.028
	(0.015)	(0.018)	(0.024)
Years of education	3.058	3.099	-0.041
	(0.244)	(0.248)	(0.348)
Main activity: Work	0.138	0.171	-0.033
	(0.034)	(0.035)	(0.049)
Main activity: Chores	0.464	0.407	0.057
	(0.047)	(0.034)	(0.058)
Municipality and geographical characteristics			
Small municipality center	0.424	0.551	-0.126
	(0.126)	(0.091)	(0.156)
Medium municipality center	0.393	0.281	0.112
	(0.128)	(0.081)	(0.151)
Atlantic Region	0.281	0.317	-0.036
	(0.112)	(0.095)	(0.147)
Central region	0.324	0.225	0.099
	(0.120)	(0.067)	(0.138)
Pacific region	0.199	0.126	0.073
	(0.111)	(0.067)	(0.130)
The household lives in grouped populated rural area	0.676	0.691	-0.015

Continued ...

Table 2.2 (continued)

	Treatment (T)	Control (C)	Difference (T-C)
	(0.088)	(0.051)	(0.102)
The household lives in sparsely populated rural area	0.252	0.166	0.086
	(0.084)	(0.043)	(0.094)
Number observations	973		

Notes: Municipality clustered errors in parentheses.
Significance: *** $p< 0.01$, ** $p< 0.05$, * $p< 0.1$

Table 2.3: Municipality descriptive statistics at baseline

	Treatment (T)	Control (C)	Difference (T-C)
Main town with less than 5000 inhabitants	0.323	0.400	-0.077
	(0.088)	(0.061)	(0.106)
Main town with 5000 to 14000 inhabitants	0.323	0.308	0.015
	(0.084)	(0.058)	(0.102)
Main town with more than 14000 inhabitants	0.355	0.292	0.063
	(0.084)	(0.058)	(0.102)
Number of bank branches	1	1.508	-0.508***
	(0.083)	(0.058)	(0.102)
Distance to the main market (Kilometers)	356.774	350.797	5.977
	(22.414)	(15.600)	(27.309)
Distance to a main national road (Kilometers)	0.129	0.108	0.021
	(0.058)	(0.040)	(0.070)
Altitude (meters)	788.032	1048.016	-259.983
	(166.509)	(115.885)	(202.866)
Extension of the town (sq. meters)	908.161	599.615	308.546
	(267.916)	(185.022)	(325.595)
School Attendance rate (Children between 6 and 17)	67.122	68.235	-1.113
	(1.686)	(1.165)	(2.049)
Average years of schooling (Population older than 25)	4.300	4.644	-0.344
	(0.230)	(0.159)	(0.280)
Quality of life index	63.045	64.654	-1.609
	(1.098)	(0.758)	(1.334)
Human Development Index	0.622	0.628	-0.005
	(0.005)	(0.003)	(0.005)
Dissatisfy basic needs Index (NBI in spanish)	46.889	41.938	4.951
	(3.320)	(2.293)	(4.034)
Teacher to student ratio	23.568	23.183	0.385
	(0.882)	(0.609)	(1.072)
Number of schools	12.083	11.018	1.065
	(1.745)	(1.224)	(2.132)
Number of health facilities	0.323	0.619	-0.296
	(0.237)	(0.166)	(0.289)
Number observations	96		

Notes: Standard errors in parentheses.
Significance: *** p< 0.01, ** p< 0.05, * p< 0.1.

Table 2.4: First Stage Estimation Cognitive development children 2 to 6

	(1) Weight	(2) Height	(3) BMI
Panel A: Short Run (First Follow-up)			
Treatment	0.2073	-0.2178	0.2144
	(0.1520)	(0.2800)	(0.1404)
Weight at base line	0.9038***		
	(0.0451)		
Height at base line		0.7433***	
		(0.0796)	
BMI at base line			0.1988
			(0.1338)
Control Variables	Base Line	Base Line	Base Line
Observations	822	822	821
R-squared	0.8489	0.8912	0.3088
F-First stage (Shea)	201.2	47.12	1.989
F-First stage (Kl-Paap)	210.1	46.51	2.189
Panel B: Medium Run (Second Follow-up)			
Treatment	0.5846**	0.2199	0.2579**
	(0.2310)	(0.4335)	(0.1164)
Weight at base line	1.2528***		
	(0.0785)		
Height at base line		0.7210***	
		(0.0952)	
BMI at base line			0.1882
			(0.1281)
Control Variables	Base Line	Base Line	Base Line
Observations	692	645	644
R-squared	0.7728	0.8197	0.2520
F-First stage (Shea)	130.9	28.86	2.985
F-First stage (Kl-Paap)	138.9	29.90	3.461
Panel C: Long Run (Third Follow-up)			
Treatment	1.3638**	0.5516	0.3609*
	(0.6745)	(0.6916)	(0.2021)
Weight at base line	2.1150***		
	(0.1469)		
Height at base line		0.7888***	
		(0.0963)	
BMI at base line			0.3069
			(0.1867)
Control Variables	Base Line	Base Line	Base Line
Observations	712	710	709
R-squared	0.5572	0.6221	0.1979
F-First stage (Shea)	106.9	35.64	2.944
F-First stage (Kl-Paap)	115.4	34.35	3.170

Notes: Robust standard errors in parentheses. Clustered by municipalities. The estimations include age and region fixed effects.
Significance: *** p< 0.01, ** p< 0.05, * p< 0.1.

Table 2.5: Second Stage Estimation Cognitive development children 2 to 6

	t: First Follow-up			t: Second Follow-up			t: Third Follow-up		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Raven test	Raven test	Raven standard	Raven test	Raven test	Raven standard	Raven test	Raven test	Raven standard
Method of estimation	OLS	IV	IV	OLS	IV	IV	OLS	IV	IV
Panel A: Weight									
Weight in t follow-up	0.0526	0.0959**	0.3288**	0.0496	0.1012***	0.3470***	0.0228*	0.0542***	0.1857***
	(0.0422)	(0.0478)	(0.1638)	(0.0333)	(0.0359)	(0.1230)	(0.0128)	(0.0190)	(0.0650)
Age of enrolment	-0.0777	-0.1722	-0.5903	-0.0537	-0.1181	-0.4050	0.0017	-0.0685	-0.2349
	(0.1378)	(0.1316)	(0.4512)	(0.1344)	(0.1325)	(0.4541)	(0.1272)	(0.1231)	(0.4221)
Years of education of spouse	0.1072**	0.1169**	0.4009**	0.1241**	0.1323***	0.4536***	0.1119**	0.1292**	0.4428**
at baseline	(0.0471)	(0.0461)	(0.1582)	(0.0501)	(0.0488)	(0.1675)	(0.0536)	(0.0508)	(0.1741)
Observations	853	822	822	715	692	692	737	712	712
R-squared	0.1925	0.1970	0.1970	0.2097	0.2180	0.2180	0.1886	0.1967	0.1967
Durbin-Wu-Hausman p-value		0.254	0.254		0.0390	0.0390		0.109	0.109
F-First stage (Shea)		201.2	201.2		130.9	130.9		106.9	106.9
F-First stage (Kl-Paap)		210.1	210.1		138.9	138.9		115.4	115.4
Panel B: Height									
Height in t follow-up	0.0299	0.0412*	0.1411*	0.0313	0.0457	0.1566	0.0345**	0.0516**	0.1771**
	(0.0213)	(0.0233)	(0.0799)	(0.0219)	(0.0296)	(0.1014)	(0.0145)	(0.0234)	(0.0802)
Age of enrolment	-0.0667	-0.1686	-0.5781	-0.1172	-0.1758	-0.6026	-0.0006	-0.0727	-0.2493

Continued ...

Table 2.5 (continued)

	t: First Follow-up			t: Second Follow-up			t: Third Follow-up		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Raven test	Raven test	Raven standard	Raven test	Raven test	Raven standard	Raven test	Raven test	Raven standard
Method of estimation	OLS	IV	IV	OLS	IV	IV	OLS	IV	IV
	(0.1359)	(0.1304)	(0.4472)	(0.1377)	(0.1335)	(0.4577)	(0.1280)	(0.1234)	(0.4232)
Years of education of spouse	0.1033**	0.1108**	0.3798**	0.1321**	0.1418***	0.4862***	0.1118**	0.1272**	0.4362**
at baseline	(0.0476)	(0.0473)	(0.1623)	(0.0561)	(0.0547)	(0.1876)	(0.0529)	(0.0502)	(0.1722)
Observations	854	822	822	664	645	645	734	710	710
R-squared	0.1929	0.1987	0.1987	0.2046	0.2168	0.2168	0.1936	0.2032	0.2032
Durbin-Wu-Hausman p-value		0.935	0.935		0.625	0.625		0.473	0.473
F-First stage (Shea)		47.12	47.12		28.86	28.86		35.64	35.64
F-First stage (Kl-Paap)		46.51	46.51		29.90	29.90		34.35	34.35
Panel C: BMI									
BMI in t follow-up	0.0020	0.2656	0.9105	0.0354	0.3741*	1.2825*	0.0240	0.1962*	0.6726*
	(0.0654)	(0.2019)	(0.6923)	(0.0853)	(0.2132)	(0.7308)	(0.0396)	(0.1116)	(0.3827)
Age of enrolment	-0.0694	-0.1817	-0.6230	-0.1081	-0.1720	-0.5896	0.0098	-0.0647	-0.2217
	(0.1386)	(0.1353)	(0.4639)	(0.1379)	(0.1412)	(0.4841)	(0.1280)	(0.1247)	(0.4276)
Years of education of spouse	0.1130**	0.1250***	0.4284***	0.1392**	0.1480***	0.5073***	0.1161**	0.1342**	0.4602**
at baseline	(0.0479)	(0.0476)	(0.1632)	(0.0567)	(0.0557)	(0.1909)	(0.0547)	(0.0535)	(0.1833)

Continued . . .

Table 2.5 (continued)

	t: First Follow-up			t: Second Follow-up			t: Third Follow-up		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Raven test	Raven test	Raven standard	Raven test	Raven test	Raven standard	Raven test	Raven test	Raven standard
Method of estimation	OLS	IV	IV	OLS	IV	IV	OLS	IV	IV
Observations	853	821	821	664	644	644	734	709	709
R-squared	0.1907	0.1814	0.1814	0.2018	0.1882	0.1882	0.1872	0.1776	0.1776
Durbin-Wu-Hausman p-value		0.0237	0.0237		0.0382	0.0382		0.0416	0.0416
F-First stage (Shea)		1.989	1.989		2.985	2.985		2.944	2.944
F-First stage (Kl-Paap)		2.189	2.189		3.461	3.461		3.170	3.170

Notes: Robust standard errors in parentheses. Clustered by municipalities. The estimations include age and region fixed effects.

Significance: *** $p< 0.01$, ** $p< 0.05$, * $p< 0.1$.

Table 2.6: First Stage Estimation Cognitive development children 0 to 1

	(1) Weight	(2) Height	(3) BMI
Panel A: Short Run (First Follow-up)			
Treatment	0.2845*	0.6703	0.0612
	(0.1468)	(0.4069)	(0.1982)
Weight at base line	0.7094***		
	(0.0458)		
Height at base line		0.6240***	
		(0.0611)	
BMI at base line			0.2036***
			(0.0560)
Control Variables	Base Line	Base Line	Base Line
Observations	338	337	335
R-squared	0.8222	0.8666	0.2782
F-First stage (Shea)	123.0	68.34	7.246
F-First stage (Kl-Paap)	114.7	71.36	8.078
Panel B: Medium Run (Second Follow-up)			
Treatment	0.4554*	0.7212*	0.1651
	(0.2490)	(0.4315)	(0.1943)
Weight at base line	0.7628***		
	(0.0804)		
Height at base line		0.5515***	
		(0.0606)	
BMI at base line			0.1790***
			(0.0491)
Control Variables	Base Line	Base Line	Base Line
Observations	314	314	313
R-squared	0.6830	0.7828	0.3058
F-First stage (Shea)	45.91	44.96	6.706
F-First stage (Kl-Paap)	41.81	43.94	6.652
Panel C: Long Run (Third Follow-up)			
Treatment	2.1244***	1.6715**	0.6324***
	(0.6380)	(0.6861)	(0.2301)
Weight at base line	1.3293***		
	(0.2614)		
Height at base line		0.5521***	
		(0.0719)	
BMI at base line			0.2432***
			(0.0805)
Control Variables	Base Line	Base Line	Base Line
Observations	354	353	352
R-squared	0.5344	0.6084	0.3319
F-First stage (Shea)	14.74	34.10	5.884
F-First stage (Kl-Paap)	18.94	36.80	6.213

Notes: Robust standard errors in parentheses. Clustered by municipalities. The estimations include age and region fixed effects.
Significance: *** p< 0.01, ** p< 0.05, * p< 0.1.

Table 2.7: Second Stage Estimation Cognitive development children 0 to 1

	t: First Follow-up			t: Second Follow-up			t: Third Follow-up		
	(1) Peabody Score	(2) Peabody Score	(3) Peabody Z-score	(4) Peabody Score	(5) Peabody Score	(6) Peabody Z-score	(7) Peabody Score	(8) Peabody Score	(9) Peabody Z-score
Method of estimation	OLS	IV	IV	OLS	IV	IV	OLS	IV	IV
Panel A: Weight									
Weight in t follow-up	0.5547	0.9495	0.5742**	0.5177	0.9962*	0.5947**	0.1250	0.5412*	0.3232**
	(0.4958)	(0.5786)	(0.2617)	(0.3280)	(0.5204)	(0.2396)	(0.1367)	(0.2871)	(0.1281)
Age of enrolment	-2.0414	-3.3337**	-1.2494	-1.3345	-2.3595	-0.7739	-1.5154	-2.1844	-0.6594
	(1.6772)	(1.6290)	(0.7898)	(1.5105)	(1.5105)	(0.7138)	(1.4448)	(1.3850)	(0.6655)
Years of education of spouse at baseline	0.7520*	0.8290*	0.3464*	0.6196	0.6663	0.2657	0.6394	0.7034*	0.2935*
	(0.4199)	(0.4379)	(0.1779)	(0.4515)	(0.4740)	(0.1922)	(0.3991)	(0.4190)	(0.1664)
Observations	350	338	338	324	314	314	365	354	354
R-squared	0.4171	0.4160	0.4399	0.4058	0.3945	0.4247	0.4022	0.3864	0.4052
Durbin-Wu-Hausman p-value		0.876	0.660		0.430	0.342		0.189	0.136
F-First stage (Shea)		123.0	109.3		45.91	52.50		14.74	16.04
F-First stage (Kl-Paap)		114.7	109.3		41.81	52.50		18.94	16.04
Panel B: Height									
Height in t follow-up	0.3767***	0.3139*	0.1838**	0.3569***	0.4453**	0.2479***	0.1544	0.3945**	0.2246***
	(0.1261)	(0.1668)	(0.0804)	(0.1330)	(0.1911)	(0.0878)	(0.1039)	(0.1830)	(0.0831)
Age of enrolment	-2.1679	-3.4686**	-1.3445*	-1.3178	-2.4966*	-0.8586	-1.4902	-2.3490*	-0.7643

Continued . . .

Table 2.7 (continued)

	t: First Follow-up			t: Second Follow-up			t: Third Follow-up		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Peabody Score	Peabody Score	Peabody Z-score	Peabody Score	Peabody Score	Peabody Z-score	Peabody Score	Peabody Score	Peabody Z-score
Method of estimation	OLS	IV	IV	OLS	IV	IV	OLS	IV	IV
	(1.6318)	(1.5704)	(0.7532)	(1.4602)	(1.4056)	(0.6636)	(1.4126)	(1.2443)	(0.5974)
Years of education of spouse at baseline	0.7080	0.8361*	0.3456**	0.6062	0.6858	0.2723	0.6140	0.6744	0.2688
	(0.4299)	(0.4432)	(0.1761)	(0.4635)	(0.4942)	(0.1952)	(0.4054)	(0.4240)	(0.1677)
Observations	349	337	337	324	314	314	364	353	353
R-squared	0.4249	0.4215	0.4461	0.4124	0.4019	0.4346	0.4038	0.3955	0.4146
Durbin-Wu-Hausman p-value		0.630	0.800		0.626	0.594		0.183	0.129
F-First stage (Shea)		68.34	55.33		44.96	42.67		34.10	33.15
F-First stage (Kl-Paap)		71.36	55.33		43.94	42.67		36.80	33.15
Panel C: BMI									
BMI in t follow-up	-0.7271	0.9966	0.6594	-0.2704	1.0498	0.7703	0.0893	1.1564	0.7327
	(0.8076)	(1.4107)	(0.6946)	(0.6222)	(1.5895)	(0.8203)	(0.3919)	(1.1622)	(0.6069)
Age of enrolment	-2.3629	-3.1952*	-1.1784	-1.6705	-2.5485	-0.8659	-1.6301	-2.4464	-0.7849
	(1.6535)	(1.7592)	(0.8552)	(1.5319)	(1.6598)	(0.7888)	(1.4766)	(1.5503)	(0.7475)
Years of education of spouse at baseline	0.8167*	0.9038**	0.3950**	0.6704	0.7406	0.3050	0.6553	0.7679*	0.3327*
	(0.4175)	(0.4335)	(0.1769)	(0.4624)	(0.4659)	(0.1912)	(0.4034)	(0.4249)	(0.1710)

Continued . . .

Table 2.7 (continued)

	t: First Follow-up			t: Second Follow-up			t: Third Follow-up		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Peabody Score	Peabody Score	Peabody Z-score	Peabody Score	Peabody Score	Peabody Z-score	Peabody Score	Peabody Score	Peabody Z-score
Method of estimation	OLS	IV	IV	OLS	IV	IV	OLS	IV	IV
Observations	348	335	335	324	313	313	364	352	352
R-squared	0.4183	0.4027	0.4196	0.4026	0.3851	0.4061	0.4007	0.3868	0.4042
Durbin-Wu-Hausman p-value		0.423	0.347		0.476	0.357		0.382	0.348
F-First stage (Shea)		7.246	6.814		6.706	5.209		5.884	4.271
F-First stage (Kl-Paap)		8.078	6.814		6.652	5.209		6.213	4.271

Notes: Robust standard errors in parentheses. Clustered by municipalities. The estimations include age and region fixed effects.

Significance: *** p< 0.01, ** p< 0.05, * p< 0.1.

Table 2.8: IV Estimation Cognitive development children aged 2

	(1) Raven Standard	(2) Raven Standard	(3) Raven Standard
Panel A: Short Run (First Follow-up)			
Weight	0.9406***		
	(0.3313)		
Height		0.2656*	
		(0.1458)	
BMI			4.7307**
			(2.3141)
First stage:	**Weight**	**Height**	**BMI**
Treatment	0.0975	-0.0691	0.2230
	(0.2125)	(0.6083)	(0.2721)
Weight at base line	0.8725***		
	(0.0610)		
Height at base line		0.5374***	
		(0.1087)	
BMI at base line			0.0623
			(0.0528)
Observations	183	182	182
F-First stage (Shea)	105.5	12.42	0.954
Panel B: Medium Run (Second Follow-up)			
Weight	0.7255**		
	(0.2847)		
Height		0.1542	
		(0.1725)	
BMI			4.8024**
			(2.2887)
First stage:	**Weight**	**Height**	**BMI**
Treatment	0.4632	-0.3658	0.2271
	(0.3675)	(0.9991)	(0.2405)
Weight at base line	1.1319***		
	(0.1164)		
Height at base line		0.4502***	
		(0.0889)	
BMI at base line			0.0607
			(0.0401)
Observations	172	156	156
F-First stage (Shea)	65.01	13.46	1.456
Panel C: Long Run (Third Follow-up)			
Weight	0.3304**		
	(0.1548)		
Height		0.1998	
		(0.1464)	
BMI			2.5378**
			(1.0203)
First stage:	**Weight**	**Height**	**BMI**
Treatment	0.4762	0.6794	0.1577
	(1.3239)	(1.2135)	(0.5083)
Weight at base line	2.1615***		
	(0.2423)		
Height at base line		0.1998	
		(0.1464)	
BMI at base line			2.5378**
			(1.0203)
Observations	176	174	174
F-First stage (Shea)	44.93	13.49	1.407

Notes: Robust standard errors in parentheses. Clustered by municipalities. The estimations include age and region fixed effects.
Significance: *** $p< 0.01$, ** $p< 0.05$, * $p< 0.1$.

Table 2.9: Program effect on average household age of enrolment

	(1) Average age of enrolment in the households		(2) Average age of enrolment in the households	
VARIABLES	coef	se	coef	se
Treatment	-0.0988	(0.1005)	-0.0849	(0.0756)
Municipality characteristics				
Number of urban public schools in the municipality	0.0014	(0.0093)	0.0056	(0.0072)
Number of rural public schools in the municipality	0.0005	(0.0021)	-0.0002	(0.0018)
Students per teacher ratio in the municipality	0.0109	(0.0116)	0.0027	(0.0094)
Class square metres per student in the municipality	0.0143	(0.0288)	0.0152	(0.0230)
School attendance rate in the municipality	-0.0069	(0.0127)	-0.0061	(0.0092)
Adults average of years of school in the municipality	0.0371	(0.1080)	0.0040	(0.0717)
Quality life index of the municipality in 1993	-0.0023	(0.0076)	-0.0006	(0.0060)
Pacific region	0.2193	(0.2046)	0.1615	(0.1290)
Central region	0.4309***	(0.1343)	0.3532***	(0.1063)
Atlantic Region	-0.0370	(0.1038)	0.0302	(0.1028)
Household characteristics				
Household has fridge at third follow-up	-0.0047	(0.0706)	-0.0275	(0.0601)
Household has blender at third follow-up	0.0103	(0.0704)	0.0173	(0.0553)
Good quality wall materials at third follow-up	0.0772	(0.0862)	-0.0429	(0.0942)
Medium quality wall materials at third follow-up	0.0546	(0.0971)	-0.0336	(0.0958)
Household has sewage system	0.0070	(0.1944)	-0.0059	(0.1284)
Households has WC connected to sewer or septic tank	0.0761	(0.0815)	0.0143	(0.0591)
Household has waste collection service	0.1577	(0.1708)	-0.0402	(0.0991)
Household has a motorcycle	0.0502	(0.1537)	0.1346	(0.1023)
Good quality floor materials	0.3454**	(0.1556)	0.1654	(0.1332)
Household has other assets	0.1096	(0.0892)	0.0885	(0.0732)
Number of children 0 to 6	0.0970*	(0.0542)	0.0529	(0.0411)
Number of children 7 to 11	-0.0544	(0.0771)	-0.0388	(0.0517)
Number of children 12 to 17	0.0693	(0.0565)	0.0617	(0.0395)
Head and spouse characteristics				
Age head at third follow-up	-0.0066	(0.0048)	-0.0065**	(0.0033)
Female head at third follow-up	-0.2120*	(0.1184)	-0.1123	(0.0950)
Years of education of head at third follow-up	-0.0057	(0.0148)	-0.0263**	(0.0123)
Log wages of head at third follow-up	-0.0107	(0.0098)	-0.0085	(0.0072)
Number of hours worked monthly by head at third follow-up	0.0011	(0.0021)	0.0016	(0.0016)
Head Main activity: Chores at base line	0.3100**	(0.1538)	0.0412	(0.1688)
Age spouse at base line	0.0055	(0.0051)	0.0024	(0.0038)
Years of education of spouse at third follow-up	-0.0418***	(0.0121)	-0.0296***	(0.0093)
Constant	6.2029***	(0.6181)	6.6655***	(0.5169)
Observations	515		723	
R-squared	0.1193		0.0859	

Notes: Robust standard errors in parentheses. Clustered by municipalities. The sample for model in column (1) contains the households used for the estimation of cognitive development. The sample for model in column (2) contains all households in rural areas with children 2 to 6. Control variables used in the estimation contains the variables unbalanced in the baseline and head education and age.
Significance: *** p< 0.01, ** p< 0.05, * p< 0.1.

Table 2.10: IV Estimation Cognitive development children 2 6 including # of older siblings

	(1) Weight in t	(2) Raven Standard	(3) Height in t	(4) Raven Standard	(5) BMI in t	(6) Raven Standard
Panel A: t - First Follow up						
Treatment	0.2054		-0.1962		0.2072	
	(0.1516)		(0.2737)		(0.1388)	
Weight in t follow up		0.3238**				
		(0.1609)				
Weight at baseline	0.9059***					
	(0.0460)					
Height in t follow up				0.1413*		
				(0.0799)		
Height Baseline			0.7300***			
			(0.0812)			
BMI in t follow up						0.8860
						(0.6781)
BMI Baseline					0.2002	
					(0.1341)	
Number of siblings 7 to 17	0.0185	-0.0389	-0.2703***	-0.0029	0.0590**	-0.1499
	(0.0263)	(0.2414)	(0.0932)	(0.2428)	(0.0278)	(0.2548)
Observations	822	822	822	822	821	821
R-squared	0.8489	0.1971	0.8932	0.1987	0.3123	0.1826
DWH p-value		0.269		0.928		0.0277
F-First stage (Shea)	194.3		44.23		1.952	

Continued ...

Table 2.10 (continued)

	(1) Weight in t	(2) Raven Standard	(3) Height in t	(4) Raven Standard	(5) BMI in t	(6) Raven Standard
Panel B: t - Second Follow up						
Treatment	0.5814**		0.2021		0.2572**	
	(0.2304)		(0.4300)		(0.1161)	
Weight in t follow up		0.3701***				
		(0.1216)				
Weight at baseline	1.2456***					
	(0.0799)					
Height in t follow up				0.1781*		
				(0.1065)		
Height Baseline			0.7041***			
			(0.0990)			
BMI in t follow up						1.2866*
						(0.7340)
BMI Baseline					0.1881	
					(0.1283)	
Number of siblings 7 to 17	-0.0607	0.2431	-0.2926**	0.2721	-0.0359	0.1635
	(0.0469)	(0.2671)	(0.1388)	(0.2905)	(0.0320)	(0.2896)
Observations	692	692	645	645	644	644
R-squared	0.7732	0.2184	0.8222	0.2178	0.2532	0.1885
DWH p-value		0.0319		0.594		0.0372
F-First stage (Shea)	125.2		25.58		2.964	

Continued ...

Table 2.10 (continued)

	(1) Weight in t	(2) Raven Standard	(3) Height in t	(4) Raven Standard	(5) BMI in t	(6) Raven Standard
Panel C: t - Third Follow up						
Treatment	1.3635**		0.5450		0.3602*	
	(0.6743)		(0.6858)		(0.2004)	
Weight in t follow up		0.1831***				
		(0.0675)				
Weight at baseline	2.1141***					
	(0.1453)					
Height in t follow up				0.1758**		
				(0.0836)		
Height Baseline			0.7801***			
			(0.1011)			
BMI in t follow up						0.6649*
						(0.3809)
BMI Baseline					0.3052	
					(0.1867)	
Number of siblings 7 to 17	-0.0074	-0.0412	-0.1533	-0.0182	-0.0609	-0.0986
	(0.1729)	(0.2638)	(0.1876)	(0.2682)	(0.0499)	(0.2625)
Observations	712	712	710	710	709	709
R-squared	0.5572	0.1970	0.6227	0.2032	0.1991	0.1783
DWH p-value		0.119		0.497		0.0440
F-First stage (Shea)	109.6		31.66		2.930	

Notes: Robust standard errors in parentheses. Clustered by municipalities. The estimations include age and region fixed effects.

Significance: *** p< 0.01, ** p< 0.05, * p< 0.1.

Table 2.11: Program effect on labour supply

	(1) Number of hours worked per week	(2) Number of hours worked per week	(3) Number of hours worked per week	(4) Number of hours worked per week	(5) Number of hours worked per week	(6) Number of hours worked per week
		OLS			Tobit	
Program effect in the	coef	coef	coef	dy/dx	dy/dx	dy/dx
first follow-up	-0.1893			-0.786		
	(1.3192)			(1.1224)		
second follow-up		0.7655			-0.9676	
		(1.8174)			(1.1427)	
third follow-up			-0.3888			-0.8449
			(2.3285)			(1.0934)
Observations	7,219	6,501	5,302	7,219	6,501	5,302

Notes: Robust standard errors in parentheses. Clustered by municipalities. For the tobit model the marginal effect is presented. Marginal effects (dy/dx in the treatment) are calculated in the means for Numbers of hours worked ¿0. Control variables used in the estimation contains the variables unbalanced in the baseline and head education and age. Clustered errors by municipality.
Significance: *** p< 0.01, ** p< 0.05, * p< 0.1.

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A2 Appendix

A2 - 1 Third Follow-up Cognitive ability assessment description

The assessment of cognitive ability in the third follow-up survey was planned by the government at the time of conception of the long term evaluation. Once the permission to implement an additional test was approved, the cognitive ability measurement section in the survey was divided in two. For children between 3 and 11 at the testing time (2012) the Spanish version of the Peabody Picture Vocabulary Test (PPVT) was applied as an indicator of receptive vocabulary while for children aged between 12 and 16, the Raven's progressive matrices test was applied as an indicator of logical and comprehensive reasoning. The difference between these two tests is not only the differences in abilities assessed but also different aspects of cognition. In this context, psychometric testing can be divided into performance and aptitude testing. Performance testing assesses what the current knowledge of the individual is, while aptitude testing assesses the potential of the individual to develop abilities or the capacity to learn a new ability. In this sense, the PPVT is a *performance* test which reflects the vocabulary that children already possess and the Raven's test is an *aptitude* test which reflects the ability of children to adapt and solve new problems.

The PPVT test measures receptive vocabulary and could be used as an indicator of verbal ability or scholastic aptitude as mentioned, however, this test did not allow us to isolate the environmental effects from the cognitive ability outcome. This is because this test measures language development and thus, factors such as starting school generates changes in this ability as the stimulus received by children increases. In this sense, higher scores can be related with good nutritional conditions but also with a rich language environment and consequently distinguishing the effect between each component can be complicated. The Raven's progressive matrices test is a non-verbal multiple choice test which is often referred to as a general intelligence test. The Raven's test has the advantage of measuring cognitive ability independently of educational factors; as a result the test minimizes the effects of language and culture on performance. Usually, high scores in this test are related to adequate development of the brain in early life (an indicator of adequate nutrition at the same life stage). In this sense, the Raven's test will allow us to relate high scores with nutritional status, as is the objective of the second part of this research project.

The design of the sample for the Raven's test was done with reference to the results of previous evaluations and with consideration for the 'no overlapping' restriction. As the hypothesis to be checked is the relationship between nutrition and cognitive ability in the conditional cash transfer program framework, it was important to take into account that nutritional status was increased only in rural areas as a result of the program (Attanasio et al., 2005). As a result, the Raven's test was only applied in rural areas. Since the research question is to find the effect of physical development on cognitive development, the selection of the age group was done considering a combination of two facts. The first one is that the most crucial stage of physical development of a child is below five years old, determined mainly by the nutrition received in this life-stage and deficiencies are only partially recovered in older stages. The second one is that after 2007 the program was extended to all municipalities, which meant that children in the control group started to receive the nutrition intervention. However, those children who were older than 6 in control municipalities by 2006 would have participated in the education component of the program and not in the nutritional one as the critical stage for nutrition had already passed.

Consequently we were interested in children 10 to 16 years old in treatment municipalities but have to exclude the 10 and 11 age groups due to the overlap restriction and possibility of cross contamination with the education component. Under this scenario we can still compare the cognitive ability of children in treatment municipalities with children in controls as shown in Figure 2.1, since they were not affected in terms of nutrition by the expansion of the program. The advantage of using this approach is that this will allow us to use data on nutritional information from previous waves to estimate the effect of the program on cognitive ability.

A2 - 2 Attrition Bias

Attrition can generate biased estimates if attrition is not random. To test the nature of attrition we perform two types of test. We check that attrition in not related to the treatment status of the households and also that it is not related to the physical development of the children at the baseline.

We first check if attrition is correlated with treatment. If attrition is related to treatment and we find, for example, systematically more attrition in control households, the estimates are biased. In order to test this, we regress attrition status defined as 1 if the household was not traceable in the follow-up as a function of some head of the household, household and municipality characteristics and allocation into treatment. Additionally, due to budget restrictions, only a sample of 70% of the original sample was randomly tracked in the third follow-up. We include in the model that additional feature experienced in the third follow-up including the sampling criteria based on three main variables (random number for tracking, surveying order and attrition in the second follow-up). All households that were in the base line received a random number for tracking. This random number jointly with the traceability of the household in the second follow-up determined the order in which each household was going to be contacted in each municipality. We use a probit estimation and the attrition marginal fixed effect results are presented in Table A2 - 2.1. Columns 1 to 3 show the estimation for all households, columns 4 to 6 for all households that have children in the age range 2 to 6, and columns 7 to 9 include the same households but exclude municipalities where the program started before the baseline and which are excluded from the analysis. For all households we find that there is no significant difference between treatment and control in attrition in any of the follow ups (coefficients 0.010, -0.002 and -0.024; p-values 0.174, 0.900 and 0.206, first, second and third follow-up respectively). The random number for tracking and the order in which the households were interviewed in the third follow-up are good predictors of attrition in the third follow-up but not in any other follow-up as is expected. We replicate the exercise for the different samples to verify that the attrition was random over treatment in the location and nutritional participation domain. We do not find evidence of non-random attrition across treatment and control municipalities.

The second test we perform aims to verify that the attrition is not related to the variables of interest. For example, if the children who are not tracked are nutritionally worse off at the baseline than the children tracked, the estimates will overestimate the real effect of the nutrition intervention. We cannot perform a test on cognitive development as this measure is only available in the third follow-up. If there is not a clear pattern between attrition and physical development, there is less cause for concern over selective attrition in cognitive development. We estimate a model regressing the physical development measures on basic predictive variables, the attrition variable and the interaction between the basic predictive variables and the attrition variable. The intuition behind this is that we are testing that the prediction of nutritional status does not change systematically for those who attrite. Table A2 - 2.2 shows the results. Columns 1 to 3 present the estimation for attrition in the first follow-up, columns 4 to 6 for attrition in the second follow-up and columns 7 to 9 attrition in the third follow-up. The points to highlight are that the attrition and treatment variables are not significant in any case. Performing a joint hypothesis test of the attrition variables and the interactions, we find that only height is affected by attrition in the first follow-up at 5 percent significance, although none of the interactions are individually significant. This is not driving the results as we find no-program effect on children's height.

Table A2 - 2.1: Attrition Estimation

	All households			Households with children 2-6 at baseline			Households in control and treatment without payment with children 2-6 at baseline		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Attrition	Attrition	Attrition	Attrition	Attrition	Attrition	Attrition	Attrition	Attrition
	First	Second	Third	First	Second	Third	First	Second	Third
VARIABLES	Follow-up	Follow-up	Follow-up	Follow-up	Follow-up	Follow-up	Follow-up	Follow-up	Follow-up
Program operation									
Treatment	0.010	-0.002	-0.024	0.005	-0.004	-0.016	0.009	0.003	-0.027
	(0.008)	(0.013)	(0.019)	(0.008)	(0.014)	(0.018)	(0.009)	(0.017)	(0.021)
Sample and survey characteristics									
Random number for tracking	-0.011	-0.010	0.145***	-0.016	-0.026	0.155***	-0.018	-0.033	0.162***
	(0.008)	(0.012)	(0.038)	(0.010)	(0.016)	(0.038)	(0.012)	(0.020)	(0.044)
Surveying order			-0.002***			-0.002***			-0.002**
			(0.001)			(0.001)			(0.001)
Attrition Second Follow-up			0.514***			0.533***			0.491***
			(0.038)			(0.055)			(0.085)
Household head characteristics									
Age	-0.000**	0.001*	0.005***	-0.001***	-0.001***	0.002***	-0.001**	-0.001**	0.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.001)	(0.001)
Single	0.020***	0.043***	0.043***	0.021**	0.031**	0.020	0.012	0.023	0.053**
	(0.006)	(0.009)	(0.016)	(0.009)	(0.013)	(0.021)	(0.011)	(0.016)	(0.025)
No education	0.015	0.026*	0.043	0.003	-0.010	-0.002	0.005	0.025	0.034
	(0.011)	(0.014)	(0.026)	(0.012)	(0.019)	(0.026)	(0.014)	(0.025)	(0.035)
Incomplete primary school	0.002	-0.003	0.027	-0.010	-0.047***	-0.018	-0.015	-0.027	0.028
	(0.008)	(0.013)	(0.021)	(0.011)	(0.018)	(0.024)	(0.013)	(0.022)	(0.031)

Continued . . .

Table A2 - 2.1 (continued)

	All households			Households with children 2-6 at baseline			Households in control and treatment without payment with children 2-6 at baseline		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	Attrition First Follow-up	Attrition Second Follow-up	Attrition Third Follow-up	Attrition First Follow-up	Attrition Second Follow-up	Attrition Third Follow-up	Attrition First Follow-up	Attrition Second Follow-up	Attrition Third Follow-up
Primary school	-0.015*	-0.037***	0.008	-0.017*	-0.060***	-0.039	-0.024**	-0.045**	-0.015
	(0.008)	(0.013)	(0.024)	(0.010)	(0.017)	(0.027)	(0.011)	(0.022)	(0.036)
Incomplete secondary school	0.001	-0.007	0.013	-0.009	-0.048***	-0.017	-0.015	-0.031	-0.004
	(0.012)	(0.017)	(0.028)	(0.012)	(0.018)	(0.028)	(0.013)	(0.024)	(0.037)
Hosehold charcteristics									
Own house	-0.010**	-0.038***	-0.012	-0.014**	-0.044***	-0.025*	-0.020***	-0.053***	-0.031*
	(0.005)	(0.007)	(0.013)	(0.006)	(0.010)	(0.014)	(0.007)	(0.012)	(0.018)
Good wall materials	-0.022**	-0.030*	-0.032	-0.026**	-0.035	-0.022	-0.026**	-0.029	-0.055
	(0.011)	(0.017)	(0.030)	(0.013)	(0.024)	(0.034)	(0.013)	(0.028)	(0.040)
Medium wall materials	-0.022**	-0.016	-0.014	-0.028**	-0.028	-0.007	-0.026**	-0.013	-0.029
	(0.011)	(0.017)	(0.028)	(0.013)	(0.023)	(0.036)	(0.013)	(0.027)	(0.039)
Waste collection service	0.003	0.016	-0.015	0.001	0.022	0.019	-0.004	0.026	0.026
	(0.007)	(0.011)	(0.020)	(0.009)	(0.015)	(0.024)	(0.009)	(0.017)	(0.029)
Sewer service	-0.005	0.009	0.008	-0.008	-0.000	-0.005	-0.009	-0.003	-0.010
	(0.007)	(0.012)	(0.023)	(0.009)	(0.018)	(0.026)	(0.011)	(0.020)	(0.029)
Piped water service	0.003	-0.036*	0.015	-0.008	-0.061**	0.037	0.004	-0.050	0.024
	(0.012)	(0.020)	(0.022)	(0.017)	(0.028)	(0.030)	(0.023)	(0.040)	(0.042)
Piped gas service	0.003	-0.011	0.007	0.004	-0.018	0.011	0.015	-0.020	-0.006

Continued . . .

Table A2 - 2.1 (continued)

	All households			Households with children 2-6 at baseline			Households in control and treatment without payment with children 2-6 at baseline		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Attrition	Attrition	Attrition	Attrition	Attrition	Attrition	Attrition	Attrition	Attrition
	First	Second	Third	First	Second	Third	First	Second	Third
VARIABLES	Follow-up	Follow-up	Follow-up	Follow-up	Follow-up	Follow-up	Follow-up	Follow-up	Follow-up
	(0.010)	(0.013)	(0.028)	(0.015)	(0.015)	(0.034)	(0.020)	(0.021)	(0.040)
Electricity	-0.019**	-0.044***	-0.044**	-0.009	-0.034*	-0.066**	-0.009	-0.038*	-0.093**
	(0.009)	(0.015)	(0.021)	(0.008)	(0.017)	(0.027)	(0.011)	(0.022)	(0.040)
Water fascility in the house	-0.010	0.010	-0.039*	-0.009	0.022	-0.052*	-0.015	0.013	-0.041
	(0.012)	(0.018)	(0.020)	(0.017)	(0.026)	(0.029)	(0.024)	(0.039)	(0.039)
Toilet connected to sewer or septic tank	-0.004	-0.007	0.010	0.002	-0.003	0.005	0.002	-0.010	0.032
	(0.007)	(0.009)	(0.016)	(0.007)	(0.012)	(0.019)	(0.009)	(0.012)	(0.021)
Number of household members	-0.005***	-0.015***	-0.016***	-0.004***	-0.013***	-0.004	-0.005***	-0.013***	-0.004
	(0.001)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)	(0.004)
Municipality and geographical characteristics									
Atlantic	-0.042***	-0.097***	-0.094***	-0.046***	-0.104***	-0.078**	-0.052***	-0.114***	-0.063*
	(0.016)	(0.022)	(0.029)	(0.014)	(0.024)	(0.031)	(0.017)	(0.026)	(0.037)
Central	-0.020*	-0.050**	0.012	-0.025**	-0.055***	0.043*	-0.027**	-0.073***	0.054*
	(0.012)	(0.020)	(0.023)	(0.011)	(0.021)	(0.025)	(0.012)	(0.020)	(0.028)
Pacific	-0.027**	-0.061***	-0.096***	-0.020*	-0.055**	-0.103***	-0.017	-0.051***	-0.109***
	(0.011)	(0.017)	(0.035)	(0.012)	(0.021)	(0.025)	(0.012)	(0.020)	(0.030)

Continued . . .

Table A2 - 2.1 (continued)

	All households			Households with children 2-6 at baseline			Households in control and treatment without payment with children 2-6 at baseline		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	Attrition First Follow-up	Attrition Second Follow-up	Attrition Third Follow-up	Attrition First Follow-up	Attrition Second Follow-up	Attrition Third Follow-up	Attrition First Follow-up	Attrition Second Follow-up	Attrition Third Follow-up
Altitude	-0.000	0.000	-0.000	-0.000*	-0.000	-0.000	-0.000*	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Quality life index	0.001	0.001*	0.000	0.000	0.002*	-0.000	0.001**	0.003***	0.000
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Violence level	0.038**	0.022	0.065**	0.037*	0.024	0.096**	0.031	0.001	0.040
	(0.015)	(0.020)	(0.033)	(0.019)	(0.029)	(0.046)	(0.021)	(0.034)	(0.052)
Observations	11,403	11,403	6,853	5,930	5,930	3,639	4,067	4,067	2,543

Notes: Robust standard errors in parentheses. Clustered by municipalities.

Significance: *** $p< 0.01$, ** $p< 0.05$, * $p< 0.1$.

Table A2 - 2.2: Attrition on Physical Development

VARIABLES	(1) Height	(2) Weight	(3) BMI	(4) Height	(5) Weight	(6) BMI	(7) Height	(8) Weight	(9) BMI
Treatment	-0.205	0.004	0.065	-0.250	-0.018	0.059	-0.288	-0.050	0.033
	(0.284)	(0.096)	(0.079)	(0.295)	(0.099)	(0.081)	(0.282)	(0.116)	(0.094)
	Attrition First Follow-up			***Attrition Second Follow-up***			***Attrition Third Follow-up***		
Attrition	-0.416	-0.834	-0.547	0.091	0.088	-0.032	0.327	0.068	-0.194
	(2.504)	(0.893)	(0.742)	(1.323)	(0.581)	(0.432)	(1.051)	(0.443)	(0.435)
Age = 3 * Attrition	1.352	0.641	0.280	0.202	0.070	0.140	0.427	0.066	0.158
	(1.666)	(0.474)	(0.407)	(0.975)	(0.366)	(0.290)	(0.743)	(0.260)	(0.385)
Age = 4 * Attrition	2.221	0.789*	0.194	0.102	0.190	0.289	-0.278	-0.023	0.305
	(1.695)	(0.447)	(0.345)	(0.929)	(0.351)	(0.276)	(0.646)	(0.231)	(0.373)
Age = 5 * Attrition	-0.801	-0.272	0.124	0.356	0.245	0.173	-0.157	-0.014	0.095
	(0.927)	(0.450)	(0.274)	(0.592)	(0.260)	(0.160)	(0.411)	(0.180)	(0.110)
Age = 6 * Attrition	1.446	0.257	-0.239	-0.231	0.061	-0.019	0.155	0.156	0.013
	(1.412)	(0.742)	(0.467)	(1.055)	(0.742)	(0.397)	(0.578)	(0.286)	(0.176)
Sewer service * Attrition	-0.114	-0.023	0.017	-0.005	0.010	0.017	-0.033	-0.001	0.016
	(0.135)	(0.073)	(0.050)	(0.083)	(0.036)	(0.026)	(0.070)	(0.028)	(0.019)
Piped gas service * Attrition	0.025	0.026**	0.013	-0.012	-0.006	-0.002	-0.005	-0.002	-0.002
	(0.033)	(0.012)	(0.010)	(0.018)	(0.007)	(0.004)	(0.014)	(0.005)	(0.003)
Toilet connected to sewer or septic tank * Attrition	-0.665	-0.901**	-0.630*	0.568	-0.030	-0.193	0.557	0.107	-0.077
	(0.897)	(0.444)	(0.351)	(0.504)	(0.251)	(0.183)	(0.372)	(0.175)	(0.111)
Number of household members * Attrition	-1.440	-0.806*	-0.351	0.143	-0.112	-0.217	0.481	0.094	-0.127
	(1.124)	(0.486)	(0.354)	(0.624)	(0.265)	(0.190)	(0.427)	(0.219)	(0.139)

Continued . . .

Table A2 - 2.2 (continued)

VARIABLES	(1) Height	(2) Weight	(3) BMI	(4) Height	(5) Weight	(6) BMI	(7) Height	(8) Weight	(9) BMI
Head age * Attrition	0.088	-0.026	-0.004	-0.500	-0.086	0.115	-0.099	0.089	0.145
	(0.850)	(0.348)	(0.284)	(0.641)	(0.263)	(0.154)	(0.346)	(0.165)	(0.119)
Mother no education * Attrition	-1.112	-0.112	0.237	-0.617	-0.053	0.165	-0.467	-0.043	0.094
	(0.807)	(0.410)	(0.325)	(0.590)	(0.250)	(0.143)	(0.392)	(0.175)	(0.113)
Central * Attrition	-0.822	-0.266	-0.001	-0.046	-0.099	-0.122	-0.155	-0.309*	-0.253*
	(1.079)	(0.320)	(0.317)	(0.700)	(0.234)	(0.172)	(0.433)	(0.162)	(0.128)
Pacific * Attrition	-1.842*	-0.366	0.188	-0.054	-0.092	-0.114	0.047	-0.010	-0.041
	(1.028)	(0.434)	(0.291)	(0.626)	(0.228)	(0.154)	(0.370)	(0.140)	(0.102)
Treatment * Attrition	-1.064	-0.396	-0.029	0.359	-0.042	-0.182	0.463	0.123	-0.042
	(1.134)	(0.365)	(0.363)	(0.624)	(0.233)	(0.178)	(0.425)	(0.167)	(0.124)
Age = 3	7.177***	1.765***	-0.337***	7.083***	1.746***	-0.320***	6.942***	1.721***	-0.265**
	(0.229)	(0.073)	(0.074)	(0.250)	(0.075)	(0.080)	(0.343)	(0.099)	(0.121)
Age = 4	13.830***	3.443***	-0.777***	13.776***	3.440***	-0.756***	13.855***	3.583***	-0.648***
	(0.222)	(0.080)	(0.058)	(0.239)	(0.089)	(0.061)	(0.310)	(0.120)	(0.085)
Age = 5	19.882***	5.164***	-1.004***	19.800***	5.159***	-0.978***	19.786***	5.158***	-0.973***
	(0.188)	(0.068)	(0.060)	(0.196)	(0.073)	(0.061)	(0.285)	(0.102)	(0.085)
Age = 6	25.435***	7.028***	-1.009***	25.310***	7.011***	-0.981***	25.122***	6.942***	-0.984***
	(0.227)	(0.084)	(0.059)	(0.238)	(0.088)	(0.062)	(0.317)	(0.122)	(0.089)
Sewer service	0.380	0.057	-0.086	0.313	0.025	-0.098	0.444	0.061	-0.130
	(0.241)	(0.091)	(0.066)	(0.261)	(0.096)	(0.067)	(0.310)	(0.129)	(0.097)

Continued . . .

Table A2 - 2.2 (continued)

VARIABLES	(1) Height	(2) Weight	(3) BMI	(4) Height	(5) Weight	(6) BMI	(7) Height	(8) Weight	(9) BMI
Piped gas service	0.783*	0.466***	0.193**	0.829*	0.455**	0.184**	0.754	0.390**	0.174
	(0.410)	(0.168)	(0.091)	(0.453)	(0.178)	(0.089)	(0.491)	(0.190)	(0.133)
Toilet connected to sewer or septic tank	0.210	0.125*	0.074	0.229	0.128*	0.068	0.377	0.143	0.061
	(0.201)	(0.071)	(0.065)	(0.219)	(0.076)	(0.064)	(0.241)	(0.095)	(0.095)
Number of household members	-0.286***	-0.118***	-0.024**	-0.291***	-0.121***	-0.025**	-0.268***	-0.117***	-0.031**
	(0.033)	(0.013)	(0.010)	(0.034)	(0.014)	(0.010)	(0.045)	(0.020)	(0.014)
Head age	0.036***	0.010***	-0.002	0.038***	0.012***	-0.001	0.040***	0.012***	-0.000
	(0.008)	(0.003)	(0.002)	(0.008)	(0.003)	(0.002)	(0.011)	(0.004)	(0.003)
Mothe no education	-1.400***	-0.491***	-0.069	-1.490***	-0.510***	-0.062	-1.842***	-0.545***	0.005
	(0.277)	(0.121)	(0.078)	(0.285)	(0.125)	(0.081)	(0.385)	(0.170)	(0.102)
Central	0.437	0.018	-0.097	0.498	0.024	-0.114	0.618	0.032	-0.133
	(0.336)	(0.136)	(0.088)	(0.371)	(0.147)	(0.087)	(0.400)	(0.156)	(0.110)
Pacific	-0.880	-0.254	0.086	-0.798	-0.232	0.084	-1.376*	-0.330*	0.206
	(0.778)	(0.165)	(0.184)	(0.803)	(0.170)	(0.187)	(0.739)	(0.174)	(0.232)
Constant	87.089***	12.976***	16.893***	87.053***	12.931***	16.873***	86.724***	12.878***	17.005***
	(0.629)	(0.256)	(0.199)	(0.691)	(0.288)	(0.215)	(0.940)	(0.375)	(0.383)
Observations	6,811	6,809	6,790	6,811	6,809	6,790	6,811	6,809	6,790
R-squared	0.736	0.560	0.066	0.735	0.560	0.066	0.736	0.560	0.067
F (Attrition and Interactions)	2.075	1.631	1.180	0.595	0.641	1.585	1.198	1.328	1.004
P-value	0.008	0.056	0.283	0.910	0.874	0.0671	0.268	0.175	0.462

Notes: Robust standard errors in parentheses. Clustered by municipalities.

Significance: *** $p< 0.01$, ** $p< 0.05$, * $p< 0.1$.

A2 - 3 First Stage Estimation Cognitive Development Children 2 to 6

Table A2 - 3.1: WEIGHT

	(1) t: first follow-up	(2) t: second follow-up	(3) t: third follow-up
Treatment	0.2073	0.5846**	1.3638**
	(0.1520)	(0.2310)	(0.6745)
Children variables			
Age 3	0.1888	-0.3859	0.6395
	(0.1910)	(0.2418)	(1.0288)
Age 4	0.3513	-0.3808	2.3408*
	(0.2586)	(0.3447)	(1.2341)
Age 5	0.5028*	-0.1340	1.8446
	(0.2735)	(0.4177)	(1.2848)
Age 6	1.0003***	0.1732	1.1438
	(0.3347)	(0.4276)	(1.1306)
Weight at baseline	0.9038***	1.2528***	2.1150***
	(0.0451)	(0.0785)	(0.1469)
Time duration test	-0.0003	-0.0039	-0.0095
	(0.0014)	(0.0025)	(0.0152)
Attending shool at testing time	-0.1146	-0.4600*	-2.6767**
	(0.1424)	(0.2590)	(1.1705)
Have ever work by testing time	-0.0637	-0.5880***	-0.6577
	(0.1140)	(0.2129)	(0.6963)
Age of enrolment	0.1269	0.0585	0.2783
	(0.0985)	(0.0829)	(0.3240)
Average age of enrolment in the household	-0.1265	-0.1419	-0.7600
	(0.0842)	(0.1284)	(0.4851)
Time attended to nursery at baseline	-0.0041	0.0062	-0.0039
	(0.0043)	(0.0098)	(0.0262)
Head and spouse characteristics at baseline			
Age head	0.0073	-0.0169	-0.0170
	(0.0069)	(0.0108)	(0.0398)
Female head	0.1842	-0.5611	0.2517
	(0.1979)	(0.4905)	(1.8596)
Years of education of head	0.0337	-0.0084	0.0637
	(0.0272)	(0.0465)	(0.1552)
Log wages of head	-0.0029	-0.0003	-0.0371
	(0.0130)	(0.0240)	(0.0670)
Number of hours worked monthly by head	0.0015	-0.0024	0.0158
	(0.0029)	(0.0049)	(0.0155)

Continued ...

Table A2 - 3.1 (continued)

	(1) t: first follow-up	(2) t: second follow-up	(3) t: third follow-up
Head Main activity: Chores	0.6309*	1.8292**	5.1486
	(0.3349)	(0.8375)	(3.2313)
Age spouse	0.0008	0.0144	-0.0012
	(0.0080)	(0.0124)	(0.0530)
Years of education of spouse	0.0338	0.0459	-0.0072
	(0.0220)	(0.0359)	(0.1374)
Household characteristics			
Household has fridge at baseline	-0.1370	0.2692	0.6990
	(0.1079)	(0.2424)	(0.8382)
Household has blender at baseline	0.0989	-0.1277	-0.1987
	(0.1147)	(0.2129)	(0.6442)
Good quality wall at baseline	0.4837	0.8420	1.5955
	(0.4662)	(0.5686)	(1.3999)
Medium quality wall at baseline	0.3487	0.8932	1.9128
	(0.4801)	(0.5511)	(1.3665)
Household has sewage system	0.0894	-0.3423	-1.8561*
	(0.1997)	(0.3018)	(1.0389)
WC connected to sewer or septic tank	0.2519**	0.3254	1.3744**
	(0.1117)	(0.2202)	(0.5366)
Household has waste collection service	-0.2391	-0.2250	1.9647*
	(0.2136)	(0.4120)	(1.1382)
Household has a motorcycle	-0.5124**	-0.6263	-2.3593*
	(0.2451)	(0.4900)	(1.3422)
Good quality floor	0.0540	-0.1363	0.4407
	(0.2448)	(0.3110)	(1.1185)
Household has other assets	-0.0397	-0.1831	-0.1642
	(0.1267)	(0.2569)	(0.7902)
Municipality characteristics			
number of urban public schools	-0.0363*	-0.0763***	-0.0622
	(0.0194)	(0.0222)	(0.0713)
number of rural public schools	-0.0001	-0.0029	0.0050
	(0.0035)	(0.0046)	(0.0137)
Students per teacher ratio	0.0035	0.0132	-0.1053
	(0.0176)	(0.0197)	(0.0682)
class square metres per student	-0.0377	0.0106	0.1209
	(0.0248)	(0.0386)	(0.1715)

Continued ...

Table A2 - 3.1 (continued)

	(1) t: first follow-up	(2) t: second follow-up	(3) t: third follow-up
School attendance rate	-0.0203	-0.0270	-0.0145
	(0.0147)	(0.0203)	(0.0687)
Adults average of years of school	-0.0217	0.0052	0.2579
	(0.1390)	(0.2004)	(0.4691)
Quality life index in 1993	0.0057	0.0198	-0.0098
	(0.0146)	(0.0159)	(0.0486)
Pacific region	-0.2048	-0.1151	1.7025
	(0.2219)	(0.3749)	(1.1174)
Central region	0.1379	0.0441	1.3182
	(0.2322)	(0.3401)	(0.7998)
Atlantic Region	0.5700**	0.3315	-1.3116
	(0.2755)	(0.3083)	(1.2078)
Constant	3.5060***	4.3941***	14.9918***
	(0.9534)	(1.3010)	(4.9716)
Observations	822	692	712
R-squared	0.8489	0.7728	0.5572
F-First stage (Shea)	201.2	130.9	106.9
F-First stage (Kl-Paap)	210.1	138.9	115.4

Notes: Robust standard errors in parentheses. Clustered by municipalities.

Significance: *** p< 0.01, ** p< 0.05, * p< 0.1.

Table A2 - 3.2: HEIGHT

	(1) t: first follow-up	(2) t: second follow-up	(3) t: third follow-up
Treatment	-0.2178	0.2199	0.5516
	(0.2800)	(0.4335)	(0.6916)
Children variables			
Age 3	1.1852	-0.3114	0.8815
	(0.7813)	(0.9159)	(1.3634)
Age 4	1.9462*	-0.3472	1.2814
	(1.0666)	(1.1617)	(1.6383)
Age 5	3.0878**	0.3605	-0.8143
	(1.4527)	(1.7196)	(2.2004)
Age 6	4.3318**	1.5810	-1.2584
	(1.9201)	(2.3465)	(2.6323)
Height at baseline	0.7433***	0.7210***	0.7888***
	(0.0796)	(0.0952)	(0.0963)
Time duration test	0.0004	-0.0022	-0.0075
	(0.0030)	(0.0034)	(0.0081)
Attending shool at testing time	0.0800	-0.1588	-1.5512*
	(0.2067)	(0.4233)	(0.8904)
Have ever work by testing time	-0.0963	-0.7580**	0.2574
	(0.2147)	(0.3107)	(0.6485)
Age of enrolment	0.1802	0.1718	0.3695
	(0.1201)	(0.1521)	(0.3153)
Average age of enrolment in the household	-0.2055	-0.0460	-0.6206
	(0.1522)	(0.2426)	(0.4811)
Time attended to nursery at baseline	0.0010	0.0074	0.0141
	(0.0071)	(0.0110)	(0.0159)
Head and spouse characteristics at baseline			
Age head	0.0055	-0.0204	-0.0125
	(0.0148)	(0.0202)	(0.0352)
Female head	0.7126	-1.3954*	-2.0224
	(0.5621)	(0.7079)	(2.7128)
Years of education of head	0.0322	0.0002	0.1846
	(0.0500)	(0.0768)	(0.1213)
Log wages of head	-0.0339	-0.0314	-0.1251*
	(0.0257)	(0.0405)	(0.0718)
Number of hours worked monthly by head	-0.0024	-0.0014	0.0100
	(0.0079)	(0.0100)	(0.0158)
Head Main activity: Chores	0.0265	0.7610	2.0007
	(0.9941)	(1.0691)	(2.3335)

Continued ...

Table A2 - 3.2 (continued)

	(1) t: first follow-up	(2) t: second follow-up	(3) t: third follow-up
Age spouse	0.0050	0.0117	0.0204
	(0.0126)	(0.0204)	(0.0325)
Years of education of spouse	0.1881***	0.1324*	0.0569
	(0.0696)	(0.0772)	(0.1368)
Household characteristics			
Household has fridge at baseline	-0.2891	0.3645	0.5522
	(0.2945)	(0.5369)	(0.7326)
Household has blender at baseline	0.4627	0.1560	-0.0598
	(0.2962)	(0.4373)	(0.5160)
Good quality wall at baseline	-0.8193	-0.4273	-0.5696
	(1.3240)	(1.2623)	(1.6791)
Medium quality wall at baseline	-0.9374	-0.7122	-0.1207
	(1.3211)	(1.2902)	(1.6719)
Household has sewage system	-0.3749	-1.3471***	-1.7839**
	(0.3511)	(0.3766)	(0.7720)
WC connected to sewer or septic tank	0.2834	0.5977*	0.8580
	(0.3026)	(0.3560)	(0.5560)
Household has waste collection service	0.1371	0.5928	1.5061
	(0.4479)	(0.7599)	(0.9266)
Household has a motorcycle	-1.3917*	-0.7418	-1.7139**
	(0.7687)	(0.6655)	(0.8465)
Good quality floor	-0.6570	-0.0479	-0.2217
	(0.6075)	(0.5318)	(0.8631)
Household has other assets	-0.1345	0.1551	-0.6728
	(0.3517)	(0.5173)	(0.6359)
Municipality characteristics			
number of urban public schools	-0.1130***	-0.0947**	-0.0711
	(0.0343)	(0.0443)	(0.0805)
number of rural public schools	0.0090	0.0128	0.0302*
	(0.0065)	(0.0103)	(0.0158)
Students per teacher ratio	-0.0163	0.0057	-0.0746
	(0.0257)	(0.0339)	(0.0748)
class square metres per student	0.0877*	0.0845	0.1521
	(0.0472)	(0.0633)	(0.1269)
School attendance rate	0.0216	0.0852*	0.0985*
	(0.0322)	(0.0504)	(0.0585)

Continued ...

Table A2 - 3.2 (continued)

	(1) t: first follow-up	(2) t: second follow-up	(3) t: third follow-up
Adults average of years of school	-0.2465	-0.2741	-0.3901
	(0.3120)	(0.4140)	(0.5184)
Quality life index in 1993	0.0539**	-0.0241	0.0220
	(0.0264)	(0.0371)	(0.0571)
Pacific region	0.0131	-0.0519	-0.0544
	(0.4725)	(0.7721)	(1.0930)
Central region	0.9041**	1.3176*	1.6360**
	(0.3930)	(0.6756)	(0.7717)
Atlantic Region	0.1203	-0.4245	-1.3915
	(0.4960)	(0.6715)	(0.9471)
Constant	27.4498***	44.0584***	69.6548***
	(7.8024)	(8.2846)	(8.7324)
Observations	822	645	710
R-squared	0.8912	0.8197	0.6221
F-First stage (Shea)	47.12	28.86	35.64
F-First stage (Kl-Paap)	46.51	29.90	34.35

Notes: Robust standard errors in parentheses. Clustered by municipalities.
Significance: *** p< 0.01, ** p< 0.05, * p< 0.1.

Table A2 - 3.3: BMI

	(1) t: first follow-up	(2) t: second follow-up	(3) t: third follow-up
Treatment	0.2144	0.2579**	0.3609*
	(0.1404)	(0.1164)	(0.2021)
Children variables			
Age 3	-0.2590	-0.2629	0.2939
	(0.2374)	(0.2446)	(0.3353)
Age 4	-0.3154	0.1622	1.4032***
	(0.2884)	(0.2681)	(0.3685)
Age 5	-0.5081*	0.4796	2.0329***
	(0.2911)	(0.3115)	(0.4463)
Age 6	-0.2928	0.9068***	2.4056***
	(0.2847)	(0.2658)	(0.3422)
BMI at baseline	0.1988	0.1882	0.3069
	(0.1338)	(0.1281)	(0.1867)
Time duration test	0.0010	-0.0013	-0.0006
	(0.0017)	(0.0018)	(0.0053)
Attending shool at testing time	-0.1632	-0.3573*	-0.7069**
	(0.1330)	(0.1862)	(0.3359)
Have ever work by testing time	0.0588	-0.1518	-0.1730
	(0.1062)	(0.1476)	(0.2646)
Age of enrolment	0.0545	0.0357	0.0790
	(0.0626)	(0.0493)	(0.0920)
Average age of enrolment in the household	-0.1416*	-0.1974**	-0.2822*
	(0.0844)	(0.0876)	(0.1649)
Time attended to nursery at baseline	-0.0079**	-0.0038	-0.0078
	(0.0036)	(0.0055)	(0.0107)
Head and spouse characteristics at baseline			
Age head	0.0025	-0.0045	0.0026
	(0.0068)	(0.0097)	(0.0159)
Female head	0.1293	0.1374	0.7643
	(0.2183)	(0.2647)	(1.0875)
Years of education of head	0.0119	0.0061	-0.0052
	(0.0197)	(0.0260)	(0.0542)
Log wages of head	-0.0100	-0.0008	-0.0079
	(0.0162)	(0.0197)	(0.0289)
Number of hours worked monthly by head	0.0060*	0.0039	0.0069
	(0.0035)	(0.0035)	(0.0057)
Head Main activity: Chores	0.2650	1.0206**	1.1888
	(0.4420)	(0.5044)	(1.0727)

Continued . . .

Table A2 - 3.3 (continued)

	(1) t: first follow-up	(2) t: second follow-up	(3) t: third follow-up
Age spouse	-0.0005	0.0099	-0.0114
	(0.0091)	(0.0116)	(0.0227)
Years of education of spouse	-0.0149	0.0071	-0.0116
	(0.0262)	(0.0281)	(0.0475)
Household characteristics			
Household has fridge at baseline	0.1654	0.1721	0.3697
	(0.1273)	(0.1657)	(0.2641)
Household has blender at baseline	-0.0583	0.0061	0.0293
	(0.1017)	(0.1251)	(0.2718)
Good quality wall at baseline	0.4631	0.4350	0.7038
	(0.2887)	(0.3089)	(0.5192)
Medium quality wall at baseline	0.3920	0.5418*	0.7204
	(0.2745)	(0.2971)	(0.5046)
Household has sewage system	0.1635	0.0857	-0.4112
	(0.2192)	(0.2310)	(0.4663)
WC connected to sewer or septic tank	0.1258	0.1269	0.4052**
	(0.0959)	(0.1342)	(0.1859)
Household has waste collection service	-0.1726	-0.5209**	0.5607
	(0.2683)	(0.2586)	(0.4867)
Household has a motorcycle	0.0237	-0.1413	-0.6117
	(0.2020)	(0.2326)	(0.3818)
Good quality floor	0.3994*	0.2686	0.4372
	(0.2158)	(0.2310)	(0.4079)
Household has other assets	0.1126	-0.1122	0.2164
	(0.1105)	(0.1381)	(0.3179)
Municipality characteristics			
number of urban public schools	0.0162	-0.0149	0.0163
	(0.0161)	(0.0126)	(0.0279)
number of rural public schools	0.0008	-0.0017	-0.0011
	(0.0034)	(0.0025)	(0.0059)
Students per teacher ratio	-0.0032	-0.0170	-0.0470*
	(0.0194)	(0.0139)	(0.0276)
class square metres per student	-0.0974***	-0.0299	-0.0362
	(0.0336)	(0.0413)	(0.0604)
School attendance rate	-0.0161	-0.0371***	-0.0100
	(0.0150)	(0.0111)	(0.0258)

Continued ...

Table A2 - 3.3 (continued)

	(1) t: first follow-up	(2) t: second follow-up	(3) t: third follow-up
Adults average of years of school	0.1121	0.2739**	0.1614
	(0.1700)	(0.1197)	(0.2201)
Quality life index in 1993	-0.0277	0.0003	-0.0242*
	(0.0170)	(0.0090)	(0.0134)
Pacific region	-0.3410	0.1087	0.6175
	(0.2287)	(0.1773)	(0.3792)
Central region	-0.1037	0.1095	0.3378
	(0.2381)	(0.1749)	(0.3485)
Atlantic Region	0.5216*	0.5754***	-0.4434
	(0.2872)	(0.1522)	(0.4333)
Constant	15.0372***	14.6991***	16.3437***
	(2.6997)	(2.7555)	(3.6277)
Observations	821	644	709
R-squared	0.3088	0.2520	0.1979
F-First stage (Shea)	1.989	2.985	2.944
F-First stage (Kl-Paap)	2.189	3.461	3.170

Notes: Robust standard errors in parentheses. Clustered by municipalities.
Significance: *** p< 0.01, ** p< 0.05, * p< 0.1.

A2 - 4 Second Stage Estimation Cognitive Development Children 2 to 6

Table A2 - 4.1: WEIGHT

	(1)	(2)	(3)	(4)	(5)	(6)
	Raven test	Raven test	Raven test	Raven test	Raven test	Raven test
	t: first follow-up		t: second follow-up		t: third follow-up	
Weight in t follow-up	0.0526	0.0959**	0.0496	0.1012***	0.0228*	0.0542***
	(0.0422)	(0.0478)	(0.0333)	(0.0359)	(0.0128)	(0.0190)
Children variables						
Age 3	0.8322**	0.8385**	1.0058**	1.1637***	0.8096**	0.8952***
	(0.4118)	(0.4125)	(0.4475)	(0.4197)	(0.3687)	(0.3278)
Age 4	1.0514**	1.0301**	1.1579**	1.2373***	1.0214**	0.9927**
	(0.4661)	(0.4621)	(0.4877)	(0.4675)	(0.4063)	(0.4123)
Age 5	1.1843**	1.1240**	1.3634**	1.3474***	1.0591**	0.9771**
	(0.4965)	(0.4987)	(0.5485)	(0.4958)	(0.4757)	(0.4573)
Age 6	1.1607**	0.9765*	1.6752***	1.4236***	1.3472***	1.0279**
	(0.5462)	(0.5495)	(0.5253)	(0.5071)	(0.5045)	(0.5219)
Time duration test	0.0002	-0.0001	0.0009	0.0007	0.0005	0.0007
	(0.0061)	(0.0060)	(0.0066)	(0.0065)	(0.0058)	(0.0054)
Attending shool at testing time	0.5977*	0.6235*	0.4370	0.5055	0.3765	0.4847
	(0.3417)	(0.3336)	(0.3449)	(0.3382)	(0.3929)	(0.3981)
Have ever work by testing time	-0.9130***	-0.9500***	-1.1516***	-1.1513***	-0.9601***	-0.9704***
	(0.2457)	(0.2345)	(0.2744)	(0.2608)	(0.2937)	(0.2795)
Age of enrolment	-0.0777	-0.1722	-0.0537	-0.1181	0.0017	-0.0685
	(0.1378)	(0.1316)	(0.1344)	(0.1325)	(0.1272)	(0.1231)
Average age of enrolment in the household	-0.3645*	-0.2616	-0.5283**	-0.3951*	-0.5712***	-0.4331**
	(0.1834)	(0.1835)	(0.2008)	(0.2018)	(0.1769)	(0.1822)

Continued . . .

Table A2 - 4.1 (continued)

	(1)	(2)	(3)	(4)	(5)	(6)
	Raven test	Raven test	Raven test	Raven test	Raven test	Raven test
	t: first follow-up		t: second follow-up		t: third follow-up	
Time attended to nursery at baseline	0.0082	0.0071	0.0080	0.0099	0.0114	0.0133**
	(0.0070)	(0.0067)	(0.0078)	(0.0074)	(0.0071)	(0.0065)
Head and spouse characteristics at baseline						
Age head	-0.0136	-0.0084	-0.0197*	-0.0197	-0.0104	-0.0139
	(0.0123)	(0.0122)	(0.0117)	(0.0126)	(0.0127)	(0.0129)
Female head	0.3136	0.3704	-0.3896	-0.4208	-0.8572	-0.8352
	(0.8020)	(0.7914)	(0.8378)	(0.8003)	(0.9494)	(0.9432)
Years of education of head	0.0577	0.0342	0.0192	-0.0038	0.0342	0.0115
	(0.0413)	(0.0420)	(0.0419)	(0.0424)	(0.0477)	(0.0484)
Log wages of head	-0.0415	-0.0444	-0.0383	-0.0450	-0.0327	-0.0398
	(0.0274)	(0.0281)	(0.0326)	(0.0326)	(0.0312)	(0.0305)
Number of hours worked monthly by head	0.0005	0.0007	0.0005	-0.0013	0.0012	-0.0010
	(0.0045)	(0.0045)	(0.0053)	(0.0051)	(0.0051)	(0.0050)
Head Main activity: Chores	1.0505	1.2822	1.6051	1.3832	1.9363	1.5800
	(1.0224)	(1.0740)	(1.2110)	(1.2508)	(1.2541)	(1.2659)
Age spouse	-0.0036	-0.0083	0.0047	0.0022	0.0007	0.0033
	(0.0156)	(0.0140)	(0.0158)	(0.0148)	(0.0156)	(0.0137)
Years of education of spouse	0.1072**	0.1169**	0.1241**	0.1323***	0.1119**	0.1292**
	(0.0471)	(0.0461)	(0.0501)	(0.0488)	(0.0536)	(0.0508)
Household characteristics						
Household has fridge at baseline	-0.1797	-0.1981	-0.1260	-0.2715	-0.2655	-0.4246
	(0.2229)	(0.2081)	(0.2801)	(0.2662)	(0.2764)	(0.2708)

Continued . . .

Table A2 - 4.1 (continued)

	(1)	(2)	(3)	(4)	(5)	(6)
	Raven test	Raven test	Raven test	Raven test	Raven test	Raven test
	t: first follow-up		t: second follow-up		t: third follow-up	
Household has blender at baseline	0.4867**	0.4739**	0.2171	0.3013	0.1740	0.2519
	(0.2214)	(0.2106)	(0.2296)	(0.2128)	(0.2275)	(0.2140)
Good quality wall at baseline	0.7912	0.7310	0.8952*	0.8615*	0.7006	0.6404
	(0.5203)	(0.5228)	(0.4939)	(0.5038)	(0.5032)	(0.5173)
Medium quality wall at baseline	0.9578**	0.9404**	1.0147**	0.9762**	0.9965**	0.9239*
	(0.4563)	(0.4541)	(0.4398)	(0.4454)	(0.4784)	(0.4860)
Household has sewage system	0.9898	0.9469	1.1052	1.1360	0.6563	0.7192
	(0.6499)	(0.6552)	(0.7215)	(0.7145)	(0.6507)	(0.6345)
WC connected to sewer or septic tank	-0.0889	-0.0560	-0.1030	-0.1024	0.1117	0.0978
	(0.2729)	(0.2727)	(0.3225)	(0.3177)	(0.3055)	(0.2998)
Household has waste collection service	-1.0431	-0.9351	-1.1551	-1.0474	-0.8009	-0.7756
	(0.6390)	(0.6304)	(0.7543)	(0.7178)	(0.6757)	(0.6283)
Household has a motorcycle	0.1534	0.1565	0.2686	0.3071	0.2645	0.4055
	(0.4454)	(0.4222)	(0.4168)	(0.3730)	(0.4944)	(0.4488)
Good quality floor	-0.1571	-0.1466	-0.2910	-0.3522	-0.1419	-0.1695
	(0.3755)	(0.3883)	(0.4305)	(0.4291)	(0.4160)	(0.4032)
Household has other assets	0.5187**	0.4480*	0.4901	0.4489	0.6285*	0.5358
	(0.2385)	(0.2549)	(0.3031)	(0.2963)	(0.3228)	(0.3283)
Municipality characteristics						
number of urban public schools	0.0052	0.0047	0.0036	0.0051	-0.0009	0.0030
	(0.0295)	(0.0287)	(0.0287)	(0.0277)	(0.0313)	(0.0305)

Continued . . .

Table A2 - 4.1 (continued)

	(1)	(2)	(3)	(4)	(5)	(6)
	Raven test	Raven test	Raven test	Raven test	Raven test	Raven test
	t: first follow-up		t: second follow-up		t: third follow-up	
number of rural public schools	-0.0037	-0.0043	0.0009	0.0010	0.0011	-0.0003
	(0.0048)	(0.0047)	(0.0052)	(0.0054)	(0.0047)	(0.0046)
Students per teacher ratio	-0.0512*	-0.0524*	-0.0495	-0.0443	-0.0594*	-0.0520
	(0.0304)	(0.0307)	(0.0316)	(0.0302)	(0.0328)	(0.0317)
class square metres per student	0.1307**	0.1417**	0.1549***	0.1489***	0.1487***	0.1515**
	(0.0589)	(0.0651)	(0.0491)	(0.0565)	(0.0553)	(0.0648)
School attendance rate	-0.0138	-0.0156	-0.0082	-0.0180	0.0159	0.0040
	(0.0241)	(0.0242)	(0.0237)	(0.0235)	(0.0258)	(0.0247)
Adults average of years of school	0.0122	-0.0395	-0.0949	-0.1402	-0.2862	-0.3549
	(0.2382)	(0.2342)	(0.2273)	(0.2167)	(0.2461)	(0.2386)
Quality life index in 1993	0.0429**	0.0473**	0.0462**	0.0520***	0.0500**	0.0565***
	(0.0197)	(0.0191)	(0.0204)	(0.0197)	(0.0202)	(0.0187)
Pacific region	-0.5846	-0.5769	-0.6991	-0.8592**	-0.7949*	-0.9704**
	(0.4147)	(0.4101)	(0.4229)	(0.4068)	(0.4380)	(0.4134)
Central region	0.2842	0.2547	0.1212	-0.1146	0.0529	-0.1895
	(0.3941)	(0.3972)	(0.4286)	(0.4126)	(0.4358)	(0.4217)
Atlantic Region	-0.1752	-0.1458	-0.0878	-0.2322	-0.2115	-0.2488
	(0.3445)	(0.3489)	(0.3773)	(0.3853)	(0.3437)	(0.3464)
Constant	4.6747***	4.0949**	5.1307***	4.3289**	4.4333***	3.5788**
	(1.6498)	(1.6842)	(1.7563)	(1.8934)	(1.5344)	(1.7055)

Continued ...

Table A2 - 4.1 (continued)

	(1)	(2)	(3)	(4)	(5)	(6)
	Raven test	Raven test	Raven test	Raven test	Raven test	Raven test
	t: first follow-up		t: second follow-up		t: third follow-up	
Observations	853	822	715	692	737	712
R-squared	0.1925	0.1970	0.2097	0.2180	0.1886	0.1967
DWH p-value		0.254		0.0390		0.109
Method of estimation	OLS	IV	OLS	IV	OLS	IV

Notes: Robust standard errors in parentheses. Clustered by municipalities.

Significance: *** $p< 0.01$, ** $p< 0.05$, * $p< 0.1$.

Table A2 - 4.2: HEIGHT

	(1)	(2)	(3)	(4)	(5)	(6)
	Raven test	Raven test	Raven test	Raven test	Raven test	Raven test
	t: first follow-up		t: second follow-up		t: third follow-up	
Height in t follow-up	0.0299	0.0412*	0.0313	0.0457	0.0345**	0.0516**
	(0.0213)	(0.0233)	(0.0219)	(0.0296)	(0.0145)	(0.0234)
Children variables						
Age 3	0.8194*	0.8327**	0.9232**	1.0809***	0.7395**	0.8569***
	(0.4147)	(0.4116)	(0.4113)	(0.3865)	(0.3528)	(0.3271)
Age 4	0.9575*	0.9374*	1.1329**	1.2650**	0.8840**	0.9325**
	(0.5155)	(0.5134)	(0.4727)	(0.4973)	(0.3878)	(0.4393)
Age 5	1.0118*	0.9626	1.2436**	1.2904**	0.9165**	0.9505*
	(0.5811)	(0.5932)	(0.5767)	(0.6137)	(0.4527)	(0.4881)
Age 6	0.9382	0.8020	1.5185**	1.4354**	1.1274**	0.9820*
	(0.6760)	(0.6879)	(0.6033)	(0.7040)	(0.4929)	(0.5899)
Time duration test	0.0006	0.0002	0.0004	0.0004	0.0005	0.0008
	(0.0061)	(0.0058)	(0.0063)	(0.0061)	(0.0059)	(0.0058)
Attending shool at testing time	0.5955*	0.6144*	0.3963	0.3999	0.4268	0.4692
	(0.3416)	(0.3332)	(0.3386)	(0.3263)	(0.3941)	(0.3860)
Have ever work by testing time	-0.8946***	-0.9310***	-1.0548***	-1.0587***	-0.9628***	-0.9768***
	(0.2481)	(0.2372)	(0.2917)	(0.2837)	(0.2949)	(0.2832)
Age of enrolment	-0.0667	-0.1686	-0.1172	-0.1758	-0.0006	-0.0727
	(0.1359)	(0.1304)	(0.1377)	(0.1335)	(0.1280)	(0.1234)
Average age of enrolment in the household	-0.3868**	-0.2810	-0.4262**	-0.3452*	-0.5539***	-0.4420**
	(0.1802)	(0.1806)	(0.2091)	(0.2004)	(0.1772)	(0.1788)
Time attended to nursery at baseline	0.0073	0.0054	0.0090	0.0092	0.0109	0.0102
	(0.0071)	(0.0068)	(0.0080)	(0.0077)	(0.0073)	(0.0072)

Continued ...

Table A2 - 4.2 (continued)

	(1)	(2)	(3)	(4)	(5)	(6)
	Raven test	Raven test	Raven test	Raven test	Raven test	Raven test
	t: first follow-up		t: second follow-up		t: third follow-up	
Head and spouse characteristics at baseline						
Age head	-0.0139	-0.0090	-0.0186	-0.0198	-0.0093	-0.0129
	(0.0125)	(0.0123)	(0.0130)	(0.0139)	(0.0129)	(0.0134)
Female head	0.2804	0.3576	-0.3915	-0.3933	-0.7762	-0.7155
	(0.7923)	(0.7735)	(0.8639)	(0.8131)	(0.9978)	(1.0156)
Years of education of head	0.0547	0.0311	0.0114	-0.0136	0.0315	0.0045
	(0.0414)	(0.0417)	(0.0461)	(0.0466)	(0.0481)	(0.0481)
Log wages of head	-0.0421	-0.0449	-0.0421	-0.0466	-0.0354	-0.0383
	(0.0270)	(0.0276)	(0.0311)	(0.0322)	(0.0302)	(0.0301)
Number of hours worked monthly by head	0.0005	0.0012	-0.0010	-0.0018	0.0007	-0.0000
	(0.0044)	(0.0045)	(0.0055)	(0.0053)	(0.0051)	(0.0048)
Head Main activity: Chores	1.0629	1.3261	1.7198	1.6609	1.8746	1.7213
	(0.9926)	(1.0291)	(1.2827)	(1.3123)	(1.2795)	(1.2994)
Age spouse	-0.0040	-0.0090	0.0062	0.0022	-0.0020	0.0000
	(0.0159)	(0.0144)	(0.0176)	(0.0164)	(0.0157)	(0.0141)
Years of education of spouse	0.1033**	0.1108**	0.1321**	0.1418***	0.1118**	0.1272**
	(0.0476)	(0.0473)	(0.0561)	(0.0547)	(0.0529)	(0.0502)
Household characteristics						
Household has fridge at baseline	-0.1622	-0.1644	-0.0050	-0.1601	-0.2347	-0.3829
	(0.2242)	(0.2119)	(0.2969)	(0.2796)	(0.2809)	(0.2651)
Household has blender at baseline	0.4640**	0.4516**	0.0857	0.1652	0.1886	0.2583
	(0.2219)	(0.2132)	(0.2601)	(0.2494)	(0.2237)	(0.2102)

Continued ...

Table A2 - 4.2 (continued)

	(1)	(2)	(3)	(4)	(5)	(6)
	Raven test	Raven test	Raven test	Raven test	Raven test	Raven test
	t: first follow-up		t: second follow-up		t: third follow-up	
Good quality wall at baseline	0.7809	0.7281	0.9895*	0.9210*	0.7210	0.6515
	(0.5193)	(0.5231)	(0.5343)	(0.5352)	(0.5053)	(0.5132)
Medium quality wall at baseline	0.9478**	0.9349**	1.0423**	0.9965**	0.9616**	0.9016*
	(0.4573)	(0.4583)	(0.4751)	(0.4762)	(0.4767)	(0.4825)
Household has sewage system	1.0114	0.9642	1.2049	1.2528	0.6447	0.6921
	(0.6474)	(0.6514)	(0.7743)	(0.7647)	(0.6553)	(0.6421)
WC connected to sewer or septic tank	-0.0781	-0.0372	-0.1538	-0.1198	0.1229	0.1277
	(0.2755)	(0.2787)	(0.3353)	(0.3397)	(0.3042)	(0.3032)
Household has waste collection service	-1.0580*	-0.9574	-1.2480	-1.1618	-0.8346	-0.7646
	(0.6234)	(0.6196)	(0.7765)	(0.7656)	(0.6793)	(0.6488)
Household has a motorcycle	0.1738	0.1689	0.2897	0.3359	0.2142	0.3352
	(0.4257)	(0.4005)	(0.4500)	(0.4234)	(0.4714)	(0.4376)
Good quality floor	-0.1200	-0.0875	-0.3513	-0.3657	-0.1492	-0.1284
	(0.3784)	(0.3886)	(0.4461)	(0.4480)	(0.4187)	(0.4178)
Household has other assets	0.5427**	0.4895*	0.4024	0.4015	0.6701**	0.6327*
	(0.2376)	(0.2519)	(0.3049)	(0.3007)	(0.3244)	(0.3317)
Municipality characteristics						
number of urban public schools	0.0080	0.0062	-0.0082	-0.0043	0.0019	0.0071
	(0.0294)	(0.0293)	(0.0275)	(0.0272)	(0.0309)	(0.0297)
number of rural public schools	-0.0040	-0.0040	0.0019	0.0017	0.0001	-0.0007
	(0.0048)	(0.0047)	(0.0058)	(0.0065)	(0.0048)	(0.0047)

Continued . . .

Table A2 - 4.2 (continued)

	(1)	(2)	(3)	(4)	(5)	(6)
	Raven test	Raven test	Raven test	Raven test	Raven test	Raven test
	t: first follow-up		t: second follow-up		t: third follow-up	
Students per teacher ratio	-0.0514*	-0.0552*	-0.0375	-0.0340	-0.0586*	-0.0589**
	(0.0297)	(0.0299)	(0.0289)	(0.0284)	(0.0304)	(0.0289)
class square metres per student	0.1272**	0.1303**	0.1422***	0.1366**	0.1416**	0.1402**
	(0.0596)	(0.0663)	(0.0524)	(0.0617)	(0.0551)	(0.0663)
School attendance rate	-0.0161	-0.0179	-0.0297	-0.0394*	0.0076	-0.0035
	(0.0243)	(0.0241)	(0.0237)	(0.0231)	(0.0253)	(0.0246)
Adults average of years of school	0.0098	-0.0204	0.1200	0.0439	-0.2511	-0.2862
	(0.2331)	(0.2344)	(0.2358)	(0.2227)	(0.2478)	(0.2459)
Quality life index in 1993	0.0410**	0.0445**	0.0430*	0.0496**	0.0486**	0.0532***
	(0.0198)	(0.0195)	(0.0218)	(0.0214)	(0.0208)	(0.0197)
Pacific region	-0.5831	-0.5700	-0.7673*	-0.9099**	-0.8059*	-0.8534**
	(0.4209)	(0.4183)	(0.4313)	(0.4311)	(0.4276)	(0.4147)
Central region	0.2712	0.2574	-0.0210	-0.2149	-0.0279	-0.1759
	(0.4054)	(0.4089)	(0.4337)	(0.4343)	(0.4289)	(0.4119)
Atlantic Region	-0.1084	-0.0434	-0.2724	-0.3508	-0.2180	-0.2103
	(0.3439)	(0.3391)	(0.3659)	(0.3793)	(0.3410)	(0.3466)
Constant	2.9147	1.9358	2.9683	2.0471	0.8371	-1.1262
	(2.3128)	(2.4852)	(2.7043)	(3.4948)	(2.3704)	(3.4425)

Continued . . .

Table A2 - 4.2 (continued)

	(1)	(2)	(3)	(4)	(5)	(6)
	Raven test	Raven test	Raven test	Raven test	Raven test	Raven test
	t: first follow-up		t: second follow-up		t: third follow-up	
Observations	854	822	664	645	734	710
R-squared	0.1929	0.1987	0.2046	0.2168	0.1936	0.2032
DWH p-value		0.935		0.625		0.473
Method of estimation	OLS	IV	OLS	IV	OLS	IV

Notes: Robust standard errors in parentheses. Clustered by municipalities.

Significance: *** $p< 0.01$, ** $p< 0.05$, * $p< 0.1$.

Table A2 - 4.3: BMI

	(1)	(2)	(3)	(4)	(5)	(6)
	Raven test	Raven test	Raven test	Raven test	Raven test	Raven test
	t: first follow-up		t: second follow-up		t: third follow-up	
BMI in t follow-up	0.0020	0.2656	0.0354	0.3741*	0.0240	0.1962*
	(0.0654)	(0.2019)	(0.0853)	(0.2132)	(0.0396)	(0.1116)
Children variables						
Age 3	0.8895**	1.0264**	1.0010**	1.3068***	0.8698**	0.9871***
	(0.4003)	(0.4532)	(0.4089)	(0.4122)	(0.3697)	(0.3417)
Age 4	1.1996***	1.4221***	1.3587***	1.6006***	1.1741***	1.1732***
	(0.4189)	(0.4673)	(0.4309)	(0.4239)	(0.3952)	(0.3915)
Age 5	1.4112***	1.7241***	1.6112***	1.7501***	1.2689***	1.1878***
	(0.4032)	(0.4741)	(0.4869)	(0.4572)	(0.4489)	(0.4378)
Age 6	1.4931***	1.7180***	2.0148***	1.9307***	1.5727***	1.2906***
	(0.4149)	(0.4585)	(0.4390)	(0.4407)	(0.4633)	(0.4478)
Time duration test	0.0002	-0.0007	0.0002	0.0004	0.0000	0.0001
	(0.0060)	(0.0061)	(0.0063)	(0.0067)	(0.0058)	(0.0051)
Attending shool at testing time	0.5833*	0.6367*	0.3732	0.4758	0.3742	0.4923
	(0.3412)	(0.3360)	(0.3475)	(0.3542)	(0.3909)	(0.4278)
Have ever work by testing time	-0.9118***	-0.9765***	-1.0842***	-1.0597***	-0.9490***	-0.9351***
	(0.2471)	(0.2306)	(0.2951)	(0.2887)	(0.2933)	(0.2859)
Age of enrolment	-0.0694	-0.1817	-0.1081	-0.1720	0.0098	-0.0647
	(0.1386)	(0.1353)	(0.1379)	(0.1412)	(0.1280)	(0.1247)
Average age of enrolment in the household	-0.3772**	-0.2347	-0.4229**	-0.2712	-0.5593***	-0.4139**
	(0.1810)	(0.1842)	(0.2085)	(0.2179)	(0.1735)	(0.1798)
Time attended to nursery at baseline	0.0078	0.0089	0.0100	0.0129	0.0123*	0.0146**
	(0.0072)	(0.0075)	(0.0081)	(0.0078)	(0.0073)	(0.0067)

Continued . . .

Table A2 - 4.3 (continued)

	(1)	(2)	(3)	(4)	(5)	(6)
	Raven test	Raven test	Raven test	Raven test	Raven test	Raven test
	t: first follow-up		t: second follow-up		t: third follow-up	
Head and spouse characteristics at baseline						
Age head	-0.0134	-0.0085	-0.0184	-0.0173	-0.0094	-0.0138
	(0.0122)	(0.0119)	(0.0130)	(0.0141)	(0.0127)	(0.0127)
Female head	0.3471	0.4165	-0.3887	-0.4300	-0.8262	-0.9232
	(0.7955)	(0.8140)	(0.8764)	(0.8333)	(0.9618)	(0.9409)
Years of education of head	0.0590	0.0379	0.0140	-0.0089	0.0406	0.0228
	(0.0409)	(0.0416)	(0.0458)	(0.0461)	(0.0473)	(0.0480)
Log wages of head	-0.0436	-0.0435	-0.0431	-0.0484	-0.0416	-0.0460
	(0.0272)	(0.0293)	(0.0310)	(0.0324)	(0.0313)	(0.0319)
Number of hours worked monthly by head	0.0007	-0.0006	-0.0011	-0.0035	0.0011	-0.0008
	(0.0045)	(0.0051)	(0.0055)	(0.0058)	(0.0051)	(0.0052)
Head Main activity: Chores	0.9888	1.1914	1.6569	1.2311	1.8464	1.4934
	(1.0056)	(1.1574)	(1.3017)	(1.4988)	(1.2946)	(1.3317)
Age spouse	-0.0035	-0.0077	0.0062	-0.0006	-0.0006	0.0046
	(0.0157)	(0.0136)	(0.0175)	(0.0155)	(0.0154)	(0.0133)
Years of education of spouse	0.1130**	0.1250***	0.1392**	0.1480***	0.1161**	0.1342**
	(0.0479)	(0.0476)	(0.0567)	(0.0557)	(0.0547)	(0.0535)
Household characteristics						
Household has fridge at baseline	-0.1661	-0.2205	-0.0021	-0.2071	-0.2095	-0.4080
	(0.2269)	(0.2156)	(0.2986)	(0.2958)	(0.2838)	(0.2823)
Household has blender at baseline	0.5002**	0.5098**	0.1150	0.1931	0.1975	0.2545
	(0.2214)	(0.2086)	(0.2588)	(0.2515)	(0.2288)	(0.2285)

Continued . . .

Table A2 - 4.3 (continued)

	(1) Raven test	(2) Raven test	(3) Raven test	(4) Raven test	(5) Raven test	(6) Raven test
	t: first follow-up		t: second follow-up		t: third follow-up	
Good quality wall at baseline	0.7976	0.7347	1.0076*	0.9580*	0.7371	0.6744
	(0.5216)	(0.5315)	(0.5323)	(0.5627)	(0.4994)	(0.5187)
Medium quality wall at baseline	0.9609**	0.9595**	1.0574**	0.9944**	1.0094**	0.9748**
	(0.4605)	(0.4569)	(0.4718)	(0.4924)	(0.4689)	(0.4792)
Household has sewage system	0.9914	0.9002	1.1474	1.1377	0.5910	0.6735
	(0.6392)	(0.6722)	(0.7815)	(0.7971)	(0.6449)	(0.6249)
WC connected to sewer or septic tank	-0.0689	-0.0603	-0.1286	-0.1482	0.1440	0.0927
	(0.2746)	(0.2714)	(0.3417)	(0.3381)	(0.3077)	(0.3009)
Household has waste collection service	-1.0403	-0.8796	-1.1958	-0.8714	-0.7790	-0.7311
	(0.6478)	(0.6732)	(0.8118)	(0.8392)	(0.6933)	(0.6446)
Household has a motorcycle	0.1485	0.1184	0.3068	0.4020	0.2155	0.4209
	(0.4445)	(0.4722)	(0.4647)	(0.4587)	(0.4961)	(0.4767)
Good quality floor	-0.1223	-0.1966	-0.3332	-0.4119	-0.1359	-0.1704
	(0.3732)	(0.4155)	(0.4510)	(0.4616)	(0.4268)	(0.4170)
Household has other assets	0.5233**	0.3961	0.3966	0.3769	0.6240*	0.4839
	(0.2382)	(0.2599)	(0.3099)	(0.3135)	(0.3256)	(0.3234)
Municipality characteristics						
number of urban public schools	0.0054	-0.0025	-0.0104	-0.0027	-0.0013	0.0005
	(0.0300)	(0.0303)	(0.0282)	(0.0290)	(0.0321)	(0.0332)
number of rural public schools	-0.0033	-0.0036	0.0029	0.0034	0.0019	0.0015
	(0.0047)	(0.0048)	(0.0059)	(0.0062)	(0.0048)	(0.0049)

Continued ...

Table A2 - 4.3 (continued)

	(1)	(2)	(3)	(4)	(5)	(6)
	Raven test	Raven test	Raven test	Raven test	Raven test	Raven test
	t: first follow-up		t: second follow-up		t: third follow-up	
Students per teacher ratio	-0.0530*	-0.0547*	-0.0374	-0.0268	-0.0615*	-0.0526
	(0.0307)	(0.0330)	(0.0301)	(0.0320)	(0.0325)	(0.0345)
class square metres per student	0.1253**	0.1593**	0.1483***	0.1596***	0.1472***	0.1611**
	(0.0597)	(0.0709)	(0.0521)	(0.0578)	(0.0550)	(0.0644)
School attendance rate	-0.0115	-0.0093	-0.0213	-0.0164	0.0162	0.0113
	(0.0243)	(0.0253)	(0.0245)	(0.0257)	(0.0264)	(0.0264)
Adults average of years of school	0.0005	-0.0630	0.0864	-0.0887	-0.2714	-0.3686
	(0.2377)	(0.2563)	(0.2317)	(0.2337)	(0.2547)	(0.2605)
Quality life index in 1993	0.0420**	0.0527**	0.0413*	0.0463**	0.0486**	0.0567***
	(0.0198)	(0.0215)	(0.0217)	(0.0213)	(0.0210)	(0.0196)
Pacific region	-0.6012	-0.4883	-0.7614*	-0.9304**	-0.8256*	-1.0049**
	(0.4151)	(0.4325)	(0.4341)	(0.4230)	(0.4484)	(0.4320)
Central region	0.3119	0.3450	0.0557	-0.1422	0.0551	-0.1241
	(0.3893)	(0.3998)	(0.4142)	(0.4096)	(0.4291)	(0.4315)
Atlantic Region	-0.1716	-0.3133	-0.3474	-0.7123**	-0.3422	-0.3719
	(0.3417)	(0.3536)	(0.3659)	(0.3589)	(0.3715)	(0.3755)
Constant	5.4219***	0.4963	5.4216**	-0.3089	4.8020***	1.3276
	(1.9501)	(4.1012)	(2.2212)	(4.4816)	(1.6639)	(3.0795)

Continued . . .

Table A2 - 4.3 (continued)

	(1)	(2)	(3)	(4)	(5)	(6)
	Raven test	Raven test	Raven test	Raven test	Raven test	Raven test
	t: first follow-up		t: second follow-up		t: third follow-up	
Observations	853	821	664	644	734	709
R-squared	0.1907	0.1814	0.2018	0.1882	0.1872	0.1776
DWH p-value		0.0237		0.0382		0.0416
Method of estimation	OLS	IV	OLS	IV	OLS	IV

Notes: Robust standard errors in parentheses. Clustered by municipalities.

Significance: *** $p< 0.01$, ** $p< 0.05$, * $p< 0.1$.

Chapter 3

Health Spillover Effects of a Conditional Cash Transfer Program

3.1 Introduction

Policy makers in developing countries around the world are increasingly using conditional cash transfers (henceforth CCTs) to improve the health, nutritional and educational outcomes of children in poor households. See WorldBank (2009), DFID (2011), Baird et al. (2011) among others for more on CCT programs. These schemes give stipends and food to the poorest if they meet specific conditions (for example their children attend school, or their babies are vaccinated). The main idea behind such CCTs is to target children living in these poor households by investing in the human capital of children early enough in order to facilitate proper mental and physical development of these children; healthier and better educated children are likely to be more productive adults, thereby breaking the vicious cycle that perpetuates poverty over generations. Indeed the Economist terms CCTs as the *world's* *favourite new anti-poverty device.*

In this chapter we examine the indirect effects or spillovers associated with CCT programs. While it is now accepted that CCT programs have significant direct effects on child nutrition (Behrman and Hoddinott, 2005; Attanasio and Mesnard, 2006), child health (Gertler and Boyce, 2001; Gertler, 2004; Attanasio et al., 2004, 2005b), school participation (Baez and Camacho, 2011; Attanasio et al., 2005a; Fitzsimons and Mesnard, 2008; Attanasio et al., 2010) and consumption (Maluccio and Flores, 2005; Attanasio et al., 2005c), indirect or spillover effects associated in such programs have been less studied.[1] Ignoring these effects may in fact lead to an underestimation of the total effect of the program. We examine whether CCT programs affect the health outcomes of others in the household who are not the direct beneficiaries of the program (for example parents, grandparents and other family members/friends residing in the household).

We provide evidence of this kind of spillover using data from the *Familias en Acción* (henceforth FA) program, a CCT program that has been in operation in Colombia since 2002. The aim of the program is to increase the level of human capital (health, nutrition and educational attainment) of children in the poorest households of the country, by providing monetary transfers to primary caregivers (or *titulars)* in beneficiary families, conditional on having completed specific requirements: (a) children under 6 should be taken to health centres for health and development check-ups, and *titulars* had to attend sessions on nutrition, hygiene and contraception; and (b) children between 7 and 17 years old should regularly attend school.[2]

[1] See Lagarde et al. (2007) for a survey of the direct effects of CCT programs.

[2] The *titular* is the actual recipient of the subsidy. In most cases it is the mother of the child, though there are cases when the actual recipient is the father or the grandmother. This is particularly true if the mother does not reside in the household. Specifically around 82% of the *titulars* are the mother of the child and 95% of the *titulars* are females.

Our focus is on the health and nutrition component of the program that was targeted at households with at least one child aged 0 and 6. Each eligible household that attended a health check-up every two months received a flat-rate monthly monetary supplement of 46,500 pesos (approximately US $20.45 at the 2002 exchange rate) irrespective of the number of children aged $0-6$. There is evidence that the program directly affected the health and nutrition status of young children. Attanasio et al. (2005b) find that the program reduced the occurrence of diarrhoea from 32.6 to 22 percent for children less than 24 months and from 21.3 to 10.4 percent for children aged $24-48$ months, living in rural areas. Additionally they argue that 12-month old boys grew 0.44 centimetres more than if the program had not been operational.

We take advantage of the design of the FA program to identify its effects on those members of the household who are not the direct beneficiaries of the program. Specifically we examine the effects of the health and nutrition component of the program on the health of adults (individuals aged 18 and higher at the baseline). This component of the program is unlikely to have any direct effects on the health of adults: adults, other than the *titulars*, were not required to attend health information sessions and only the *titulars* received useful advice about nutrition and the prevention of common diseases. Any treatment effect on the health of non-targeted members could therefore be viewed as pure externality arising from the program and it is this kind of within household spillovers that is the focus of this chapter.

There can of course be a number of different pathways through which these within household spillovers can arise: *income effects*, *public good effects* and *contagion effects* (to name three). The cash transfer component of the FA program frees up resources for other members creating an income effect; makes information about healthy practices available within the household that can be used by all members creating a household public good effect; and generates a positive contagion as a result of healthier behaviours and more hygienic surroundings. All three effects are expected to be positive and mutually reinforcing, resulting in a positive spillover effect.

We find that there are indeed strong spillover effects within households. In the short run, the strongest effects are on incidence of illness.[3] Non-targeted individuals (adults) in treatment households were significantly less likely to be ill in the 15-days prior to the survey compared to adults in control households. The effects persist over a longer period of time and indeed over time, lead to better long term health and a reduction in the severity of illness, captured by lower rates of hospitalization. Additionally we find that the effects are quite heterogeneous. Our results suggest that it is household level public good and contagion and not a relaxation of the household budget constraint as a result of the cash transfer that is driving the results. From a policy point of view therefore, simply looking at the direct effects results in significant underestimation of the total effect of such CCT programs.

3.2 Relation to the Existing Literature

It is not the case that the literature has not recognized the possibility of spillover effects arising from CCT programs. The focus however has been primarily on spillovers across households. The literature has identified several possible reasons for spillovers between treated (eligible) and non-treated (ineligible) households living in the same community. These include direct transfers from treated to non-treated households in the form of gifts or other transfers (Angelucci and De Giorgi, 2009), an increase

[3]Unfortunately we are somewhat restricted in terms of available health measures in the survey. We discuss the advantages and disadvantages of using the self-reported measures in Section 3.4.

in overall incomes (Angelucci and De Giorgi, 2009), learning from peer interaction (Bobonis and Finan, 2009; Lalive and Cattaneo, 2009), the desire to behave like the eligible population in the hope that they would become eligible (particularly true when the eligibility criteria are not particularly well defined within the treated community). In the context of health, the effect is expected to happen through changes in behavior pertaining to sanitation, hygiene, health practices and life-style or through the reduction in epidemics in the whole population (see Miguel and Kremer, 2004). While Gertler (2004) and Attanasio et al. (2005b) find evidence of significant impact of CCT programs on targeted children, spillover health effects on neigbouring, ineligible households in the same community have not been adequately explored. One exception is Avitabile (2011) who finds some evidence of an increase in preventive cervical cancer screening among women in ineligible households.

However, evidence on within household spillovers is more scarce. Gertler and Boyce (2001) examine the impact of *Progresa* on adults' health and find that there are significant positive short run effects on self-reported health status, specifically the number of kilometers the adult is able to walk without getting tired, reduction in the number of days of difficulty with daily activities due to illness and the number of days in bed because of illness. Behrman and Parker (2013) examine the long run effects of *Progresa*, specifically on the self-reported health status and demand for medical services by adults, and find a significant positive effect, particularly for women. The program conditions – regular check-ups for adults and the attendance at health information sessions, particularly for women who are much more likely to attend these sessions than men – are some of the potential channels through which the program would affect adults' health.[4] However the indirect effects that we are

[4]Income effect as a result of the CCT increasing the household income, increased flow of resources and increased bargaining power at the margin to women tending to lead to more emphasis on using a given level of resources for health and nutrition than for other uses; and the changed incentives for time use for

interested here are different from those discussed by Gertler and Boyce (2001) and Behrman and Parker (2013). In *PROGRESA*, all adults in the treatment households were required to attend health information sessions and get regular medical check-ups as a part of the program requirements. Any effect on the health of adults could actually be considered a direct program effect. So to look at true spillovers, we need to look beyond the literature that uses data from the *PROGRESA* program.

Chaudhuri (2009) using data from Bangladesh, finds a significant positive spillover impact of a particular reproductive health program that targeted only mothers and children in randomly selected treatment areas, on the health of the never-targeted elderly women. Ploeg (2009) finds that in the US children who are age-ineligible for WIC (Women Income Children) program but live in WIC-participating families have healthier diets than similar children in nonparticipating families. Bustelo (2010) examines the spillover effects associated with Nicaragua's *Red de Proteccion* Social CCT program and finds that while the program targets specifically children aged $7-13$ who have not completed 4^{th} grade, there are positive schooling effects within the households for older, non-targeted siblings, with higher impacts for boys than girls. Kazianga et al. (2013) evaluate the impact of two different school feeding programs (school lunches and take home rations) on the health outcomes of pre-school children in Burkina Faso. They find that take home rations have a significant impact on the health of younger siblings within the household. The pre-school children were not directly eligible and therefore any impact on the health of these children could be viewed as a spillover, attained through intra-household reallocation of food. Indeed, as in our case, Kazianga et al. (2013) argue that ignoring such spillovers under-estimates the overall effect of the intervention.[5]

school-aged children result in reallocation of time uses for adults are some of the other possible channels through which the effect could potentially operate.

[5] It is not the case that there is *always* evidence of such spillovers within household. Using data from

3.3 Theoretical Framework

To explain the mechanisms by which a spillover effect might result, we use a standard unitary model developed by Behrman and Deolalikar (1988) and used by Chaudhuri (2009). This is a simple stylistic model that we use to identify the different mechanisms. Consider a household with n members. The utility function that defines the preferences of the household is well behaved and can be written as:

$$U_j = U_j(H_{ij}, X_{ij}, Z_{ij}) \tag{3.1}$$

where U_j is the utility of the j^{th} household, H_{ij} represents the vector of the health of individuals $i = 1, 2, \ldots, n$ in household j and Z_{ij} represents the vector of health inputs and X_{ij} represents the vector of all other consumption goods of household members. Utility maximization is subject to the household budget constraint and the health production functions of all the individuals in the household.

The health production function of the household members can be written as:

$$H_{ij} = H(X_{ij}, Z_{ij}, W_j(F), H_{-ij}; \mu) \tag{3.2}$$

Health production within the household depends on the use of health inputs (Z_{ij}), consumption of all other goods (X_{ij}), household public good (W_j), health of all other members in the household excluding oneself (H_{-ij}) and all the observed and unobserved endowments of the household (μ). We subdivide the household into two groups: the targeted or T members (for example children aged $0-6$ who are the direct beneficiaries of the program) and the other or O members of the household who are not the targeted beneficiaries. Also $H_{-ij} = \{H_{1j}, \ldots, H_{i-1j}, H_{i+1j}, \ldots, H_{nj}\}$

Cambodia, Ferreira et al. (2009) find that while the CESSP Scholarship Program (CSP) program resulted in a significant increase in the likelihood of scholarship recipients being enrolled in school, the school enrolment and work of ineligible siblings was largely unaffected by the program.

and $H_{ij} \in [H_{ij}^T, H_{ij}^O]$. Health inputs ($Z_{ij}$) depends on health inputs provided by the FA program (z^{FA}) and private health inputs (z^P), so that we can write

$$Z_{ij} = Z(z_j^{FA}(F), z_{ij}^P) \tag{3.3}$$

Since z^{FA} is only available to targeted individuals residing in the treatment municipalities, it is a function of the health program (F). Note that the health and nutrition component of the FA program involves a lump sum payment to the household, irrespective of the number of targeted individuals. Hence $z_j^{FA}(F)$ is defined at the household level. Likewise household public good (W) is also a function of F, generated when the program is present in the household. Define $F = 1$ when the program is available (for households with targeted individuals in the treatment municipalities) and $F = 0$ if otherwise. Then $z_j^{FA}(F) = 0$ if $F = 0$ and $z_j^{FA}(F) > 0$ if $F = 1$. Likewise $W_j(F) = 0$ if $F = 0$ and $W_j(F) > 0$ if $F = 1$.

The household budget constraint when Y is the pooled household income, p_{z^P} and p_x are prices of the private health inputs and consumption goods respectively can be written as:

$$\sum_i p_x X_{ij} + \sum_i p_{z^P} z_{ij}^P = Y + z_j^{FA}(F) \tag{3.4}$$

Maximizing utility (given by equation (3.1)) subject to the production constraints (given by equations (3.2 and 3.3)) and the budget constraint (given by equation (3.4)), the reduced form demand functions for health inputs, consumption and outcome variables can be written as:

$$\{H_{ij}^T, H_{ij}^O, Z_{ij}, W_j, X_{ij}\} = f(p_x, p_{z^P}, Y_j; F, \mu_j) \tag{3.5}$$

Program intervention (through F) that changes any of the right-hand side variables will change the allocation of resources and outcomes within the households to conform to the optimizing allocation. The impact of the program on the targeted and non-targeted population can therefore be written as:

$$\frac{\partial H_{ij}^T}{\partial F} = \underbrace{(\frac{\partial H^T}{\partial X_{ij}})(\frac{\partial X_{ij}}{\partial F}) + (\frac{\partial H^T}{\partial z_{ij}^P})(\frac{\partial z_{ij}^P}{\partial F})}_{\text{Income effect}} + \underbrace{(\frac{\partial H^T}{\partial W_j})(\frac{\partial W_j}{\partial F})}_{\text{Household public good effect}} + \underbrace{(\frac{\partial H^T}{\partial H_{-ij}})(\frac{\partial H_{-ij}}{\partial F})}_{\text{Contagion effect}} + \underbrace{(\frac{\partial H^T}{\partial z_j^{FA}})(\frac{\partial z_j^{FA}}{\partial F})}_{\text{Direct effect}} \tag{3.6}$$

$$\frac{\partial H_{ij}^O}{\partial F} = \underbrace{(\frac{\partial H^O}{\partial X_{ij}})(\frac{\partial X_{ij}}{\partial F}) + (\frac{\partial H^O}{\partial z_{ij}^P})(\frac{\partial z_{ij}^P}{\partial F})}_{\text{Income effect}} + \underbrace{(\frac{\partial H^O}{\partial W_j})(\frac{\partial W_j}{\partial F})}_{\text{Household public good effect}} + \underbrace{(\frac{\partial H^O}{\partial H_{-ij}})(\frac{\partial H_{-ij}}{\partial F})}_{\text{Contagion effect}} \tag{3.7}$$

The focus of this chapter is on spillovers, and therefore we are interested in the effects captured through equation (3.7). The first two terms $[(\frac{\partial H^O}{\partial X_{ij}})(\frac{\partial X_{ij}}{\partial F}) + (\frac{\partial H^O}{\partial z_{ij}^P})(\frac{\partial z_{ij}^P}{\partial F})]$ denote the *income effect*, the third term $[(\frac{\partial H^O}{\partial W_j})(\frac{\partial W_j}{\partial F})]$ denotes the *household public good effect* and the last term $[(\frac{\partial H^O}{\partial H_{-ij}})(\frac{\partial H_{-ij}}{\partial F})]$ denotes the *contagion effect*.[6] Health inputs are assumed to be normal goods and under the assumption that private and publicly provided health inputs (z^P and z^{FA} respectively) are perfect substitutes, by providing some of the necessary health inputs to the targeted members for free, the program would reduce the expenditure on privately provided health inputs. This would make more household resources available for the non-targeted members to increase consumption of their private heath inputs. This additional income can

[6]Note that the total effect on the targeted individuals (equation 3.6) has an additional term which is the direct effect of the program on those targeted (given specifically by the program requirement – regular attendance and check-ups in the health clinics).

also be used to purchase or produce more of the composite goods for all household members; this is, in-turn, likely to result in a positive income effect on the health of the non-targeted individuals within the household. The FA program provides information about health, nutrition and hygiene practices in the household, thereby enhancing the basket of household public good. The third component of the spillover effect is the positive biological contagion, which is generated by the reduction of diseases transmission as a result of healthier individuals within the household. This positive contagion has a multiplier effect for all household members since better health of an individual would affect the health of other members, which in turn would affect the individual and so on.

Typically such a reduced form framework does not permit separate measurement of each component of the transmission mechanism. It can clearly predict the overall spillover effect of the program on the non-targeted individuals. Since all the components are positive and mutually reinforcing, the total spillover effect is expected to be positive. Note that neither Chaudhuri (2009) nor Ploeg (2009) are able to identify the channels. Specifically Ploeg (2009) writes that *it is not possible to tell whether this is due to increased food benefits that are then shared with the non participating children in the family or whether the income offset by the WIC benefits is used to improve the diets of nonparticipating members with other foods* (page 425). However our data enables us to go one step further. While we cannot separate out the effects of the household public good and the contagion effects, we can identify whether the effects are driven by the income effect or by a combination of the public good and contagion effect.

3.4 The Program

The overall aim of the FA program is to increase the level of human capital in the poorest households (those in the first quintile of the income distribution), by providing monetary transfers to *titulars* in beneficiary families, conditional on having completed specific requirements. The program was first targeted geographically. Of the 900 odd municipalities in Colombia, 622 were chosen by *Fondo de Inversiones para la Paz* (FIP), as targets. The targeted municipalities were required to meet all of the following requirements: (i) have less than 100,000 individuals, should not be the capital of a regional department and should not be in the coffee growing region that received special help following the 1995 earthquake; (ii) have at least one bank; (iii) have a minimum level of health and education infrastructure; and (iv) the local authorities must have shown interest in participating in the FA program and have complied with the administrative tasks necessary to participate in the program, which included providing a list of the SISBEN 1 beneficiaries.[7] In the case of FA, only households belonging to SISBEN 1 and having children aged $0-17$ as of December 1999 were eligible and these households constitute approximately the bottom twenty percent of Colombian households (see Velez et al., 1998). The program started, with some exceptions, in the second half of 2002 and the take up among eligible households was over 90 percent.

The evaluation survey was conducted by first choosing a stratified random sample

[7]The SISBEN is an indicator of economic well-being that is used throughout Colombia for targeting welfare programs. In theory, each Colombian household is classified into one of six levels, on the basis of an indicator determined by the value of several variables periodically measured. SISBEN stands for (in Spanish) Identification and Classification System for social programs potential Beneficiaries. In Colombia, Families were surveyed by the municipal authorities and classified into one of the six categories according to their level of measured poverty. The poorest families were classified in level 1, and the richest in level 6. A recategorization of each household was done in 2007 when the SISBEN version II was launched. This has not affected the estimations in this chapter, but I will explain the recategorization's implications in Chapter 4.

of targeted municipalities. The stratification was done on the basis of geographic areas and the level of health and education infrastructure, resulting in a total of 25 strata. Within each of these strata, the evaluation team chose control municipalities that were as similar as possible (in terms of size, population, an index of quality of life as well as health and education infrastructure) to the treatment municipalities. In each municipality in the sample, 10 geographic clusters were randomly drawn, with weights proportional to the population, of which three clusters were urban and seven rural. Finally, in each of the clusters, about 20 households were randomly drawn from the SISBEN 1 lists. See IFS (2004) for more on the survey methodology. Given non response rates and household mobility, about 10 households per cluster were included in the final evaluation sample, which was, in the end, made up of about 11,500 households living in 122 municipalities, of which 57 were treatment municipalities and 65 were control municipalities. A household was eligible (for the health component of the program) if there was at least one child aged $0-6$ within the household at the baseline.

We define a household to be a *treatment* household if it is eligible and resides in a treatment municipality and a household to be a *control* household if it is eligible but resides in a control municipality. This gives us a final estimating sample of 6648 households: 3993 in treatment and 2655 in control municipalities. We are specifically interested in the effects of the program on individuals aged 18 or higher (at the baseline) who would not have been exposed to either the health or the education components of the FA program.

Since the assignment of municipalities to treatment and control groups is not strictly random, the treatment and control samples could end up being different in a number of different dimensions. In the main regression results we present the difference-in-difference estimates, controlling for baseline observables, bearing in mind that there might be some bias in the estimates. To analyze the extent of this bias, we examine the robustness of the results using propensity score matching in the comparison of treatment and control households. These results (presented in Tables 3.9 and 3.10) are very similar to the standard difference-in-difference regression results - the effects are very similar both in terms of direction and magnitude. This gives us confidence in the difference-in-difference estimates.

As the primary aim of the program was the child's health, the surveys were designed to evaluate the effectiveness of the program and did not include extensive measures of health of other (non targeted) members of the household. We are therefore restricted in terms of what variables we can use to measure health impacts of the program. Specifically we use the following two variables:

1. Self reported illness in the last 15 days (was the individual ill during the 15 days prior to the survey?)
2. Was the individual hospitalized in the last year?

Several studies have used self reported illness as a measure of health status arguing that self-reported health reliably predicts actual morbidity and mortality even when other risk factors are controlled for (see Idler and Benyamini, 1997; Haddock et al., 2006; Brook et al., 1984). Having said this, the binary nature of self reported illness makes it less informative. Additionally, an individual's self-reported health status is subjectively affected by his/her social and cultural background, given their objective health. Schultz and Tansel (1997) argue that this is because of *cultural conditioning*: the threshold of what is considered good health varies systematically across a society, controlling for their objective health status. For example, individuals who are more educated, wealthier and from socially advantaged groups, are typically more aware of the limitations imposed on them by their health status and are more likely to report themselves (and their family) as being of poor health. However, in the context of this paper, this is not a major problem as all households in the sample (both in the treatment and control municipalities) are drawn from the poorest income quantile. The second measure (whether the individual had been hospitalized in the one year prior to the follow-up survey) is a longer-term measure of health and is based on an objective assessment by a health care professional. This variable is less likely to suffer from the cultural conditioning problem that we have discussed above. While it is generally possible that hospitalization is affected by a person's social background, this is unlikely to be a major issue here as the entire sample is relatively poor (from the lowest wealth quintile). Additionally, hospitalization could be regarded as a measure of the severity of the illness.[8]

3.5 Estimation Methodology, Data and Descriptive Statistics

The data used in this paper come from the first three rounds of the panel data collected for the evaluation of the impact of the FA program. The data collection was done in three rounds. The first one in 2001, in order to establish the baseline before the start of the program; the first follow up conducted one year later with the primary aim of obtaining the short run impact of the program. The second follow-up survey was conducted in 2006, with the aim of assessing the medium term

[8]The survey also asks whether the individual was in bed as a result of the illness; this is likely to be more informative because in this case illness is considered severe enough to affect the individual's regular activities (including earnings) and is less likely to suffer from the cultural conditioning problem. On the other hand, this measure could also be viewed as an increase in preventative care; for example, when individuals take time off to reduce the intensity of the effect of sickness. It is difficult, if not impossible, to determine which of the two effects is operating here.

impact of the program.[9] Recall the baseline sample consisted of 6648 households; the attrition rate was approximately 6 percent in the first follow up (conducted in 2002), with 6255 households being re-interviewed. The attrition rate was slightly higher for the treatment households (6.5 percent) compared to the control households (5.1 percent). The overall attrition rate is higher in the second follow up (conducted in 2006): 5609 households were re-interviewed, which translates to an attrition rate of 15.6 percent relative to the baseline. The attrition rate was similar for treatment and control households (15.6 percent and 15.7 percent, respectively).

Table 3.1 presents the differences between treatment and control households at the baseline. Of particular interest are household income, access to services that can potentially affect the health of members and educational attainment within the household, which could affect how information is used within the household. We see that households in control municipalities are richer, more likely to have access to piped water, access to waste collection services and more likely to use piped water for cooking. The household head is more likely to be a single parent in a treatment household. There is very little difference in the educational attainment of the household head or the spouse of the household head between the treatment and the control households. There are some significant differences at the baseline in terms of asset ownership, and the means suggest that the control households were relatively better off.

Table 3.1 also presents the unconditional means for the two outcome variables of interest. Overall, at the baseline, individuals in the treatment households are worse off compared to individuals in control households – both the intensity and severity of illness is significantly higher for individuals in treatment households. This implies

[9] See http://www.dnp.gov.co for more details. A fourth round was collected in 2011 but not used in this chapter as adults' health was not included in the survey.

that the difference-in-difference (program) effects that we present below give us the lower bounds of the program impacts, even after controlling for a full set of observable characteristics.

While the baseline survey was designed to obtain pre-program information about the households, for political reasons the program actually started in 26 of the 57 treatment municipalities prior to the baseline survey. These were the *early treatment municipalities* and households in these municipalities were already receiving the cash transfers at the time the baseline survey was conducted. We examined the robustness of our results by excluding the early treatment sample and restricting the sample to those households residing in the 31 treatment municipalities where the baseline survey was conducted prior to the program becoming operational. We show in the robustness section that the spillover effects are unchanged when we exclude the *early treatment municipalities*[10].

As a part of government policy the program was expanded in 2005 and a second follow up survey was conducted in 2005-2006. But the program was extended to 13 of the previous control municipalities (in the second follow up these 13 municipalities can be thought of as being treatment municipalities). We call these the *converted municipalities*. In the results that we present below, we continue to include these *converted municipalities* as control municipalities − the argument being that the change happened not long before the second follow up survey. However we examine the robustness of our results by excluding these *converted municipalities* from the estimation sample[11].

[10] The corresponding regression results, will be presented in columns 1 – 2 of Table 3.8.

[11] The results are presented in columns 3 – 4 of Table 3.8 and in the robustness section show that including these *converted municipalities* does not make any difference to our results.

3.5.1 Estimation Methodology

We use a difference-in-difference model to estimate the intent-to-treat (ITT) estimates of the program on non-targeted individuals in treatment households. The panel dimension of the data for the health outcome of interest allows us to control for any initial differences across groups. Our primary estimating equation takes the following form:

$$H_{ict} = \beta_0 + \beta_1 Treatment_c + \beta_2 Year_t + \beta_3 Treatment_c \times Year_t + \mathbf{X}_{ic}^{'}\gamma + \varepsilon_{ict} \quad (3.8)$$

Where H_{ict} is an outcome of interest (for example health of an adult in household i residing in municipality c at time t); $Treatment_c$ is a dummy variable for the treatment group or community; $Year_t$ is an indicator variable for the post-intervention period; $Treatment_c \times Year_t$ is an indicator variable for assignment into the program (this variable takes the value of 1 for treatment municipalities in the post intervention period); $\mathbf{X}_{ic}$ is a set of baseline individual, household and cluster (or municipality) characteristics to control for any remaining pre-treatment differences and ε_{ict} is a random disturbance term. Standard errors are clustered at the municipality level. The causal estimate of assignment to the program on the health of individuals in the household is given by β_3, which gives us the ITT estimates of the program. Recall that we are not talking about direct program effects here: rather we are focusing on indirect effects on non-treated members.

3.5.2 Attrition

The attrition rate in the first follow up was around 5.9 percent but increases to 15.6 percent in the second follow up (relative to the baseline). Attrition, if non-random (particularly if the likelihood of attrition is correlated with the baseline variable of interest) could result in biased estimates. To examine the issue of attrition in more detail, we first (following the methodology proposed by Fitzgerald et al., 1998) estimate an attrition probit model on a set of baseline observables and including a set of quality of fieldwork at the baseline as additional explanatory variables.[12] The dependent variable in this regression is $ATTRITE_s$, a dummy variable that takes the value of 1 if the household is not surveyed in the first ($s = 1$) or the second ($s = 2$) follow up survey[13]. While a number of observable (household, geographical and interview) characteristics significantly affect the likelihood of attrition, the $Treatment$ dummy is not statistically significant, indicating that there is no evidence of differential attrition across the treatment and control households.

Is the initial health status of attriting households different from non-attriting households? To examine this, we regress the two outcome variables of interest for the baseline sample, on the baseline observables, the attrition dummy ($ATTRITE$) and a set of interaction terms between the attrition dummy and each of the explanatory variables. The non-interacted coefficients give us the effects for the (eventually) non-attriting households while the interacted coefficients give us the difference between the attriters and non-attriters at the baseline. A test of the joint significance of the $ATTRITE$ dummy and the interaction terms tells us whether the attriting households are different from the non-attriting households. The results show that the null hypothesis –that the attriting households are not different from the non-attriting households – can never be rejected. There is therefore no evidence to suggest that attrition is non random.

[12]We include the number of visits to complete the interview, the number of enumerators to complete the interview, the number of supervisors of the enumerators and if the interview was incomplete as measures of the quality of the interview. We also include dummies for the supervisor code and the percentage of attrition in the municipality.

[13]We examine attrition in period 1 and 2 in separate estimations. The results are presented in the Table A3 - 1 in the appendix.

3.6 Results

Tables 3.2 – 3.3 present the ITT estimates for the program effect denoted by *Program*. In each case Panel A presents the short run results (comparing the baseline to the first follow-up) and Panel B presents the medium term results (comparing the baseline to the second follow-up). We present estimates for the full sample and for different sub-samples: young adults aged $18-25$, working age adults aged $26-59$ and the elderly aged 60 and older. This is done to examine whether the program effects are different across the different sub-samples. The stratification is done on the basis of age at the baseline. We present the marginal effects as they are interpreted more easily. Table 3.2 presents the ITT estimates for illness (*Was the individual ill during the 15 days prior to the survey?*); Table 3.3 presents the ITT estimates for hospitalization (*Was the individual hospitalized in the last year?*). In all regression results that are presented we include a full set of controls (individual, household and municipality level controls) to capture pre-treatment differences and standard errors are clustered at the municipality level. Note that the full set of results (for all controls) are presented in Tables A3 - 2 and A3 - 3.[14]

3.6.1 Short Run Results

We start with a discussion of the short run effects. The program effect is given by the coefficient estimate associated with *Program.* Individuals (adults aged 18 and higher) in treatment households are almost 3 percentage points less likely to be ill in the 15 days prior to the survey (post intervention), compared to individuals in control households (Table 3.2, Panel A, Column 1). Given the mean incidence

[14]Our results are not driven by the timing of the survey. In unreported regressions we control for the month of the interview as an additional explanatory variable. These month effects are not significant and the program effects are not affected by the inclusion of the month of interview.

of illness of 20 percent for adults in the comparison group, this translates to a 15 percent drop in the incidence of illness at the mean. In the short run there is no statistically significant program effect on the likelihood of being hospitalized in the one-year prior to the survey (Table 3.3, Panel A, Column 1).

Heterogeneity by Age

The estimation results presented in column 1 in Tables 3.2 and 3.3 essentially give us the overall spillover effects. But it is quite possible that the spillover effects vary across the different age groups (i.e., the effects are heterogenous). We therefore conduct and present in columns $2-4$ of each table the corresponding difference-in-difference estimates for the different age specific sub-samples: $18-25$ or young adults including young parents (column 2), $26-59$ or working age adults (column 3) and 60 and higher or the elderly (column 4).

We start with the sub-sample estimates for being ill in the 15 days prior to the survey (Table 3.2). While the coefficient estimate associated with *Program* is always negative, it is statistically significant only in the case of working age adults and the elderly (columns 4 and 5). Adults aged $26-59$ belonging to treatment households are more than 3 percentage points less likely to be ill in the 15 days prior to the survey compared to a similar aged adult in a control household. The magnitude of the effect is even larger in the case of the elderly: the results in column 5 show that individuals aged 60 or higher in treatment households are almost 9 percentage points less likely to be ill in the 15 days prior to the follow-up survey relative to those in control households. The sub-sample estimation for being in a hospital in the year preceding the survey (Panel A in Table 3.3) shows that there are no program effects, irrespective of the age group. The entire spillover effect in the short run therefore operates through the effect on the likelihood of being ill in the 15 days prior to the survey.

Heterogeneity by Gender

Next we seek to examine whether there are any gender effects of the program. The *titular*, the person who accompanies the child to the health centre and attends the conferences and workshops on health, hygiene and nutrition, is typically a woman (recall from footnote 2 that 95% of the *titulars* are females) and it is worth examining whether the spillover effects are concentrated along gender lines. If, for example, women have more information on the behavior of other women and of children and can advise them, the peer effects could be stronger for women. On the other hand, if men are better able to internalize the information about health improvements, the effects could be stronger for men.[15] Table 3.4 presents the gender specific regressions (columns 1 and 3 for women and columns 3 and 4 for men). Panel A presents the results for all individuals aged 18 and higher; Panel B restricts the sample to young and working age adults (males and females aged $18-59$) and finally Panel C examines the effects for the elderly (males and females aged 60 or higher). Females aged $18-59$ are more likely to have children enrolled in the program and any effects on the elderly are important from a public health point of view.

Women in treatment households are 3 percentage points less likely to be ill compared to women in control households (Panel A, column 1). The effect is similar for men (at 2.9 percentage points) in treatment households (Panel A, column 3). Also, while there is a 2 percentage point reduction on the likelihood of being hospitalized for women in treatment households, there is no corresponding treatment effect on men.

[15] We find the level of education is, in general, higher for men than for women; and this pattern is stronger for the elderly.

The results presented in Panel B show that irrespective of the gender of the adult under consideration, there is no program effect on the $18-59$ year olds in the likelihood of illness. There is however a reduction in the likelihood of hospitalization for females; there is no corresponding effect for males aged $18-59$. On the other hand, the program effects are very strong on the incidence of illness for both elderly males and females and the program effects are similar for elderly males and females (Panel C). The fact that there is such a strong effect on the health of the elderly can have substantial implications on public health, given that in most countries, expenditure on health of the elderly is a substantial component of the health budget, both at the macro and the micro level. Finally, in general there is no short run program effect on hospitalization for the elderly.

Are the effects driven by the *titulars*?

Recall that the *titulars* accompany their children to the health centres. The *titulars* are directly exposed to the program by accompanying their children to the health centres, having direct interactions with health practitioners and attending sessions on health, nutrition, and hygiene. So it is worth examining whether the *titulars* are affected differently compared to the other similarly aged women in the household. To examine this we restrict the sample to treatment households and conduct the following regression:

$$H_{ict} = \alpha_0 + \alpha_1 Titular_i + \alpha_2 Year_t + \alpha_3 Titular_i \times Year_t + \mathbf{X}_{ict}^{'}\gamma + \epsilon_{ict} \qquad (3.9)$$

Here $Titular$ is a dummy variable that takes the value of 1 if the woman is the *titular*, 0 otherwise. The estimated coefficient α_3 gives us the differential effect of being in the treatment group on *titulars*.[16] The regression results presented in Table 3.5, Panel A show that *titulars* are not benefitting any more compared to the non-titulars within the treatment households: the estimated coefficient α_3 is never statistically significant. The program effects are therefore unlikely to be driven by attendance at the clinics (i.e., by the potentially improved health of the *titulars*).[17]

Further even when we exclude the *titulars* from the treatment households, the treatment effect continues to be statistically significant (see Panel B in Table 3.5). The results therefore are not driven by isolated improvements in the health of the titulars (through the information they gain by visiting the clinics), rather through what they bring back to the household.

To summarize our results: in the short run, there is evidence of spillover effects within the household, occurring through a significantly reduced likelihood of illness in the 15 days prior to the date of the survey. The overall effects are driven by the improvements in the health of the elderly (males and females). Additionally the effects are not driven by improvements in the health of the *titulars*.

3.6.2 Medium Run Effects

The medium run effects are however quite different from the short run effects. Panel B in Tables 3.2 and 3.3 present the medium run ITT estimates of being ill in the 15 days prior to the survey and being hospitalized in the one year prior to the

[16] We consider different definitions of *titular*. In the results presented in Table 3.5 *titulars* are defined as those who answered the *titular* specific Module (Module 2) in the questionnaire in the treatment municipalities at the baseline. The results are however consistent across the different definitions. The alternative definitions are i) those who answered the *titular* specific Module (Module 2) in the questionnaire in the treatment municipalities at the first follow-up and ii) those who were clasified as titulars in the roster of the first follow-up survey.

[17] In unreported regressions we also account for household fixed effects. The linear probability regression results are qualitatively similar to those presented in Panel A in Table 3.5.

survey respectively. The program effects on the incidence of illness are not statistically significant over the longer run. The medium run effects are stronger when we consider the severity of illness (measured by a reduction in hospitalization). Individuals in treatment households are 1.8 percentage points less likely to be ill in the 15 days prior to the survey compared to individuals in control households (see Table 3.2, Panel B, Column 1), down from 3 percentage points in the short run, and no longer statistically significant. Unlike in the short run, we find a statistically significant treatment effect in the likelihood of hospitalization – individuals in treatment households are 1.6 percentage point less likely to have been hospitalized compared to individuals in control households (see Table 3.3, Panel B, Column 1). The effect is statistically significant at the 5 percent level of significance and given the control mean of 7 percent, this corresponds to a 23 percent drop in the likelihood of hospitalization at the mean[18].

The impacts are again quite heterogeneous across the different age subsamples. There is a strong and statistically significant effect for individuals aged $18-25$. Individuals in this age group are 3.4 percentage points less likely to be ill in the 15 days prior to the survey and 5.2 percentage points less likely to be in a hospital in the one year prior to the survey.[19] The elderly (individuals aged 60 and higher) are 4.5 percentage points less likely to have been hospitalized in the one year prior to the survey, indicative of a significant improvement in the long term health (measured by a reduction in the intensity of illness) of the elderly in the treatment households. This is important because in the control households, the severity of illness (rates of hospitalization) of both young adults and the elderly has actually increased over the

[18]Note that the program was only implemented in municipalities that had adequate health infrastructure. It is not clear whether we should expect a larger or smaller effect on health in municipalities with worse infrastructure.

[19]These hospitalization effects are not driven by a reduction in fertility for the treatment households. In unreported regressions we find that there is no program effect on children born to members of the household.

period under consideration. We find a flip in the program effect between the short run and the medium run for some age groups. For example a reduction of illness in the short run for adults aged 26-59 and the elderly and no effects in the medium run. It is not clear why this is the case.

The gender specific medium run effects for the different sub-samples are presented in Table 3.6. The impact is surprisingly almost always stronger on the health of men and while this holds across all age groups, the largest impact is on the health of the elderly men. This stronger effect on men than women can possibly be related to differences in self-perception of health across genders – there are biological differences in interpreting health conditions and symptom recognition across gender. Women typically report rates of illness higher than for men: this is true for both the FA sample (used in our analysis) and the overall Colombian context.[20] The results presented in Table 3.7 suggest that *titulars* are not differently affected and surprisingly *non-titulars* appear to have benefitted more (compared to women in control households) in the form of a reduction in the severity of illness.

3.6.3 Robustness

We conduct a number of different robustness checks. First, we examine the robustness of our results by

(1) excluding the *early treatment municipalities*, i.e. the set of municipalities that received the payment before the baseline survey was conducted; and

(2) by excluding the *converted municipalities*, i.e., the control municipalities that

[20] At the national level in 2000, 14.5 percent of women reported to be sick while only 11.7 percent of men did. The rate of hospitalization in the same year was reported as 5.6 percent and 3.8 percent for women and men, respectively (Guarnizo-Herreno and Agudelo, 2008).

actually got converted to treatment municipalities in 2005.

The corresponding regression results for the first and second follow up presented in Table 3.8 are very similar to those presented in Tables 3.2 and 3.3.

3.6.4 Difference-in-Difference Propensity Score Matching

The evaluation followed a quasi-experimental methodology, which meant that the assignment of municipalities into treatment and control groups was not fully random (the treatment municipalities were randomly chosen from the list provided by the government and the control municipalities were matched). Therefore the treatment and control municipalities could be different along a number of different dimensions. In the difference-in-difference regressions (above) we control for the baseline characteristics, to account for possible pre-treatment differences in observables. To ensure that the results presented thus far are not biased, we also compute the average treatment effect on ineligible members of the household using a difference-in-difference propensity score (DID-PSM) estimator. We can obtain the indirect effect of the program on adults' health by comparing the average of the health indicators after matching households using their propensity score. This procedure follows the Rosenbaum and Rubin (1983) methodology, which suggests that finding and comparing *similar* households in control and treatment municipalities, based on their propensity scores (rather than using a full set of observable variables), will generate consistent estimators.[21] In this case, we use two sets of observable characteristics to find the household propensity score. The first one (scenario A) gives us a balanced distribution of the propensity score between households in control and treatment municipalities. As the estimated bias can be affected by the set of variables used to

[21] Of course this methodology relies on the assumption of no significant differences between treatment and control municipalities in terms of the unobservables.

estimate the propensity score (Smith and Todd, 2005), we include a second set of variables (scenario B) based on the variables used by Attanasio et al. (2010) in their propensity score matching estimation and used this as an additional sensitivity test. The variables used in the two scenarios are listed in section A3 - 4 in the Appendix.

The propensity score matching method allows us to find comparable households in treatment and control municipalities, using information on the observed baseline characteristics. This is defined as the region of support. Figure 3.1 shows the distribution of the propensity score of all households within the common support region for the short run estimations, while Figure 3.2 shows the corresponding distribution for the medium run. We argue that using a PSM estimator can help us reduce the bias based on observables, given that the information comes from a similar economic setting and the measurement of the outcomes is done in the same way. The use of the difference-in-difference matching estimators also allows us to control for time invariant characteristics.

We use a probit model to estimate the propensity score. Then we use the kernel non-parametric matching estimator to estimate the average indirect effect on health. The advantage of the kernel estimator is that it does not impose any structure on the functional form of the propensity score distribution. However, for purposes of sensitivity analysis we also estimate parametrically the indirect impact on health using the nearest neighbour and the caliper matching estimators.[22] We also take advantage of the variation on the common support definition, modifying the matching estimations using a trimming specification to determine the support region.[23]

[22]We also estimate the average treatment effect using the local linear estimator and variations of the nearest neighbour estimator and we find very similar results.

[23]Using this variation is important because the non-parametric regression estimators of the counterfactual mean outcome are unreliable when evaluated at points where the estimated density is close to 0.

Tables 3.9 and 3.10 present the results of the DID-PSM estimation (for the short run and medium run respectively). The estimates are very similar to those presented in Tables 3.2 and 3.3, not only in terms of the magnitude but also in in terms of the significance of the impact. We can therefore conclude that in the short run the program impacts non-targeted individuals by reducing the incidence of illness while in the medium run it reduces both the incidence and severity of illness. Alternative specifications of the matching technique do not change the results. The results presented in Tables 3.9 and 3.10 therefore make us more confident of the DID regression results that we have discussed so far[24].

3.7 Mechanisms

Recall that there can be a number of different pathways through which the within household spillovers can arise: the cash transfer component of the FA program frees up resources for other members creating an *income effect*, produces a public good such as health information that creates a household *public good effect* and generates a positive *contagion effect* as a result of healthier and more hygienic surroundings within the household. The three components are mutually reinforcing but from a policy point of view it is important to know which effect is the strongest.

While we do not have data that will allow us to separate the public good effect from the contagion effect, the eligibility condition of the conditional cash transfer components can potentially allow us to partially identify the income effect. To do this, we conduct a falsification test. We utilize the education component of the FA program, which was targeted at households with children aged 6 to 17. Eligible households

[24]The balanced achieved between treatment and control after matching is presented in Table A3 - 5 in the Appendix.

satisfying the attendance requirement received a per-child monthly subsidy of 14,000 pesos (US \$6.15) and 28,000 pesos (US \$12.30) for each child attending primary and secondary school respectively.

We restrict the sample to households with no children aged $0-5$, but have at least one child aged $6-17$. In this sample, the only way in which exposure to the program can affect adult health is through the income effect.[25] We estimate a specification where we include a set of interaction effects interacting *Program* with dummies for the number of children aged $6-17$ in the household. The variation in the number of children aged $6-17$ in the household allows us to identify the income effect arising from the FA program. The results for the non-interacted term (*Program*) and the difference estimates are presented in Table 3.11; so ξ_i gives the additional effect of having i children in the household, $i = 2, 3, 4, 5, 6$ and higher. The non-interacted term *Program* gives us the effect for households with one child aged $6-17$. Neither in the short run, nor in the medium run is the joint test $\beta_3 + \xi_i = 0$ rejected, indicating that additional income (through the FA program) does not have any effect on either of the two health measures that we consider. There is therefore no evidence of an income effect and the treatment effect is driven by a combination of the public good effect and the contagion effect. Lack of data prevents us from further decomposition.

3.8 Conclusions

Conditional Cash Transfer programs are increasingly becoming the policy makers' vehicle of choice to provide benefits to poor households that can potentially break

[25] In all regressions we control for labor supply including the number of hours worked. This helps us to isolate the program income effect from any labor supply income effect. The IFS-Econometria-SEI (2006) program report shows an increase in the job market participation by adults but no program effect on the number of hours worked.

the vicious inter-generational cycle of poverty. While the stated aims of most CCT programs is to improve the health and nutritional status and educational attainment of children in poor households, we argue that there are strong within household spillovers that can arise as a result of the introduction of such programs. The total program effects go beyond the direct effects on the health of children and it would be incorrect to evaluate the program solely on the basis of the direct effect. We illustrate this using data from the *Familias en Acción* program that has been in operation in Colombia for more than a decade now.

Our results show that there are indeed strong spillover effects within households. In the short run, the strongest effects are on self-reported illness. Non-targeted individuals (adults) in treatment households were significantly less likely to be ill in the 15 days prior to the survey compared to adults in control households. The effects persist over a long period of time and indeed over time it leads to better long term health and a reduction in the severity of illness, captured by lower rates of hospitalization. Given that the health of individuals in treatment households was actually poorer at the baseline, these are possibly the lower bounds of the true program effects. Additionally we find that the effects are quite heterogeneous and are stronger for men in the medium term only and the elderly. Our findings suggest that it is household level public good and contagion, happening through changes in behavior and not a relaxation of the household budget constraint as a result of the cash transfer that is driving the results.

All of this has significant effects on the inter-generational poverty cycle. Healthier adults are more productive and this increase in productivity of adults is likely to positively affect the human capital of the next generation. None of this is captured by examining only the effects on the targeted group. From the policy point of view therefore simply looking at the direct effects results in significant underestimation of the effect of such CCT programs. Proper cost-benefit analysis of such CCT programs needs to take into account the improved health of the non-targeted individuals and the consequent reduction in both the incidence and severity of illness, resulting in improvements in long term health.

3.9 Tables

Table 3.1: Baseline Descriptive Statistics

	Control (1)	Treatment (2)	Difference‡ (3 = 2 − 1)
Outcome Variables			
Ill	0.1840	0.2230	0.0394***
	(0.0051)	(0.0046)	(0.0070)
Hospitalization	0.0790	0.0990	0.0200***
	(0.00361)	(0.00331)	(0.0050)
Individual, Household and Municipality level Variables			
Household income per capita	56244	53378	-2865.70**
	(951.7)	(849.9)	(1299.45)
Household has piped gas service	0.0752	0.0726	-0.0025
	(0.0051)	(0.0041)	(0.0066)
Household has piped water	0.625	0.594	-0.0315**
	(0.0094)	(0.0078)	(0.0122)
Household has sewage system	0.247	0.251	0.0041
	(0.0084)	(0.0069)	(0.0108)
Household has waste collection service	0.351	0.285	-0.0658***
	(0.0093)	(0.0072)	(0.0116)
Household has access to any telephone	0.0906	0.0877	-0.0029
	(0.0056)	(0.0045)	(0.0071)
Household uses piped water for cooking	0.618	0.578	-0.0403***
	(0.0094)	(0.0078)	(0.0123)
Water receives treatment before drinking	0.591	0.613	0.0220*
	(0.0096)	(0.0077)	(0.0122)
Household has WC connected to sewer or septic tank	0.509	0.508	-0.0015
	(0.0097)	(0.0079)	(0.0125)
Status of ownership of this house	1.687	1.655	-0.0327
	(0.0175)	(0.0139)	(0.0222)
Household has other assets	0.0998	0.0967	-0.0032
	(0.0058)	(0.0047)	(0.0074)
Household has fridge	0.301	0.274	-0.0278**
	(0.0089)	(0.0071)	(0.0113)
Household has sewing machine	0.0836	0.0846	0.0010
	(0.0054)	(0.0044)	(0.0070)

Continued . . .

Table 3.1 (continued)

	Control (1)	Treatment (2)	Difference (3 = 2 − 1)
Household has black-white TV	0.253	0.244	-0.0083
	(0.0084)	(0.0068)	(0.0108)
Household has radio	0.457	0.404	-0.0537***
	(0.0097)	(0.0078)	(0.0124)
Household has bicycle	0.346	0.367	0.0207*
	(0.0092)	(0.0076)	(0.0120)
Household has motorcycle	0.0328	0.0501	0.0173***
	(0.0035)	(0.0035)	(0.0051)
Household has fan	0.373	0.319	-0.0542***
	(0.0094)	(0.0074)	(0.0119)
Household has blender	0.414	0.41	-0.0037
	(0.0096)	(0.0078)	(0.0123)
Household has color TV	0.363	0.342	-0.0215*
	(0.0093)	(0.0075)	(0.0119)
Household has kerosene lamp	0.102	0.0624	-0.0400***
	(0.0059)	(0.0038)	(0.0067)
Household has boat	0.0508	0.0238	-0.0270***
	(0.0043)	(0.0024)	(0.0046)
Household has energy plant	0.0075	0.0088	0.0012
	(0.0017)	(0.0015)	(0.0023)
Household has livestock	0.635	0.683	0.0480***
	(0.0094)	(0.0074)	(0.0118)
Household member born in the last 12 months	0.217	0.202	-0.0153
	(0.008)	(0.0064)	(0.0102)
Household member died in the last 12 months	0.0309	0.0389	0.0080*
	(0.0034)	(0.0031)	(0.0046)
Household member is pregnant	0.0912	0.0812	-0.0100
	(0.0056)	(0.0043)	(0.0070)
Small municipality center	0.28	0.333	0.0524***
	(0.0087)	(0.0075)	(0.0116)
Medium municipality center	0.403	0.287	-0.1169***
	(0.0095)	(0.0072)	(0.0117)
Atlantic region	0.434	0.385	-0.0488***
	(0.0096)	(0.0077)	(0.0123)
Central region	0.215	0.273	0.0583***
	(0.008)	(0.0071)	(0.0108)

Continued . . .

Table 3.1 (continued)

	Control (1)	Treatment (2)	Difference (3 = 2 − 1)
Pacific region	0.142	0.134	-0.0079
	(0.0068)	(0.0054)	(0.0086)
Household lives in grouped populated rural area of the municipality	0.365	0.444	0.0797***
	(0.0093)	(0.0079)	(0.0123)
Household lives in sparsely populated rural area of the municipality	0.0731	0.11	0.0369***
	(0.0051)	(0.005)	(0.0073)
Household member migrated in the last 12 months	0.106	0.11	0.0039
	(0.006)	(0.005)	(0.0078)
Walls of good quality	0.455	0.426	-0.0289**
	(0.0097)	(0.0078)	(0.0124)
Walls of poor quality	0.0418	0.0523	0.0105**
	(0.0039)	(0.0035)	(0.0054)
Number of children under 7	1.868	1.866	-0.0015
	(0.0188)	(0.0156)	(0.0245)
Number of children between 7-11	1.064	1.08	0.0156
	(0.0193)	(0.0155)	(0.0247)
Number of children between 12-17	0.909	0.851	-0.0580**
	(0.02)	(0.0161)	(0.0256)
Number of of household members	6.779	6.631	-0.1484**
	(0.0519)	(0.0402)	(0.0650)
Number of female adults	1.5	1.482	-0.0181
	(0.0164)	(0.0127)	(0.0205)
Access to health system by the household	0.124	0.203	0.0783***
	(0.0064)	(0.0064)	(0.0094)
Age of the household head	43.32	42.54	-0.7736**
	(0.255)	(0.207)	(0.3286)
Age of the household spouse	37.39	36.5	-0.8897***
	(0.254)	(0.196)	(0.3165)
Household head has no education	0.234	0.218	-0.0154
	(0.0082)	(0.0065)	(0.0104)
Household head did not complete primary school	0.131	0.148	0.0163*
	(0.0066)	(0.0056)	(0.0087)
Household head completed primary school	0.407	0.422	0.0155
	(0.0095)	(0.0078)	(0.0123)
Household head did not complete secondary school	0.0331	0.0343	0.0012
	(0.0035)	(0.0029)	(0.0045)

Continued ...

Table 3.1 (continued)

	Control (1)	Treatment (2)	Difference (3 = 2 − 1)
Spouse has no education	0.188	0.176	-0.0128
	(0.0083)	(0.0067)	(0.0106)
Spouse did not complete primary school	0.156	0.163	0.0070
	(0.0077)	(0.0065)	(0.0101)
Spouse completed primary school	0.414	0.443	0.0286**
	(0.0105)	(0.0087)	(0.0136)
Spouse did not complete secondary school	0.0366	0.0399	0.0033
	(0.004)	(0.0034)	(0.0053)
Single parent or head	0.166	0.185	0.0181*
	(0.0072)	(0.0061)	(0.0096)
Number of observations	2,655	3,993	
Number of municipalities	65	58	

Standard deviation in parentheses.
Significance: *** : 1%;** : 5%;* : 10%
‡: Difference = Treatment − Control

Table 3.2: Program effects: on being ill in the 15 days prior to the follow-up survey

	All (1)	18 − 25 (2)	26 − 59 (3)	60 or Higher (4)
Panel A: First Follow up				
Treatment	0.0323***	0.0204	0.0338***	0.0474
	(0.0116)	(0.0160)	(0.0109)	(0.0366)
Year	0.1790	-0.1010	0.2630**	-0.3770
	(0.1320)	(0.1470)	(0.1310)	(0.4580)
Program	-0.0296*	-0.0024	-0.0307*	-0.0898**
	(0.0153)	(0.0205)	(0.0162)	(0.0423)
Sample size	26,884	5,535	20,112	2,188
Mean Control	0.20	0.13	0.20	0.36
Panel B: Second Follow up				
Treatment	0.0327***	0.0229	0.0304***	0.0426
	(0.0115)	(0.0152)	(0.0114)	(0.0349)
Year	0.0055	-0.1250	0.0704	-0.2660
	(0.1670)	(0.1730)	(0.1720)	(0.5240)
Program	-0.0180	-0.0338*	-0.0099	-0.0439
	(0.0191)	(0.0202)	(0.0195)	(0.0481)
Sample size	23,338	4,446	17,764	1,821
Mean Control	0.18	0.14	0.18	0.28

Notes:
Significance: *** : 1%;** : 5%;* : 10%. Standard errors clustered by municipality. Regressions control for a set of individual, household and municipality characteristics. Full set of results corresponding to column (1) are presented in Tables A3 - 2 and A3 - 3

Table 3.3: Program effects: on being hospitalized in the year prior to the survey

	All	18 − 25	26 − 59	60 or Higher
	(1)	(2)	(3)	(4)
Panel A: First Follow up				
Treatment	0.0184***	-0.0003	0.0153***	0.0161
	(0.00557)	(0.0124)	(0.0057)	(0.0215)
Year	0.0157	0.0154	0.0329	-0.0299
	(0.0618)	(0.1620)	(0.0606)	(0.2890)
Program	-0.0054	-0.0155	-0.0062	0.0140
	(0.0075)	(0.0129)	(0.0083)	(0.0278)
Sample size	27,884	5,522	20,081	2,183
Mean Control	0.08	0.08	0.08	0.09
Panel B: Second Follow up				
Treatment	0.0197***	0.0064	0.0160***	0.0182
	(0.0057)	(0.0130)	(0.0059)	(0.0213)
Year	-0.0523	-0.0723	-0.0825	0.4430
	(0.0619)	(0.1400)	(0.0641)	(0.3360)
Program	-0.0164**	-0.0524***	-0.0041	-0.0453*
	(0.0076)	(0.0125)	(0.0083)	(0.0265)
Sample size	23,338	4,438	17,733	1,816
Mean Control	0.07	0.09	0.06	0.12

Notes:
Significance: *** : 1%;** : 5%;* : 10%. Standard errors clustered by municipality. Regressions control for a set of individual, household and municipality characteristics. Full set of results corresponding to column (1) are presented in Tables A3 - 2 and A3 - 3

Table 3.4: Gender Specific Short Run Effects

	Ill (1)	Hospitalized (2)	Ill (3)	Hospitalized (4)
Panel A	Female 18 and higher		Male 18 and higher	
Treatment	0.0215*	0.0317***	0.0390***	0.0070
	(0.0124)	(0.0084)	(0.0147)	(0.0061)
Year	0.1727	0.0397	0.0813	-0.0183
	(0.1566)	(0.0927)	(0.1363)	(0.0608)
Program	-0.0303*	-0.0187*	-0.0291*	0.0002
	(0.0175)	(0.0105)	(0.0172)	(0.0079)
Sample size	14,406	14,455	13,754	13,792
Mean Control	0.21	0.10	0.19	0.05
Panel B	Females 18 − 59		Males 18 − 59	
Treatment	0.0219*	0.0306***	0.0380***	0.0062
	(0.0120)	(0.0089)	(0.0139)	(0.0059)
Year	0.1814	0.0638	0.1129	-0.0259
	(0.1504)	(0.0964)	(0.1348)	(0.0612)
Program	-0.0267	-0.0195*	-0.0227	-0.0023
	(0.0173)	(0.0109)	(0.0177)	(0.0075)
Sample size	13,483	13,524	12,505	12,535
Mean Control	0.20	0.10	0.17	0.05
Panel C	Females 60 and higher		Males 60 and higher	
Treatment	0.0416	0.0218	0.0352	0.0081
	(0.0549)	(0.0315)	(0.0427)	(0.0246)
Year	0.3621	-0.3302	-0.2269	0.1672
	(0.6461)	(0.5669)	(0.6292)	(0.3699)
Program	-0.1201*	0.0097	-0.1063**	0.0240
	(0.0635)	(0.0452)	(0.0532)	(0.0340)
Sample size	883	887	1,227	1,229
Mean Control	0.39	0.11	0.35	0.08

Notes
Significance: *** : 1%;** : 5%;* : 10%. Standard errors clustered by municipality. Regressions control for a set of individual, household and municipality characteristics.

Table 3.5: Direct and Indirect Short Run Effects on Women aged $18-59$.

	Ill, past 15 days (1)	Hospitalized (2)
Panel A: Titulars and Non-Titulars in Treatment Households		
Titular	-0.0029	0.0142*
	(0.0120)	(0.0083)
Year	0.3290*	0.0184
	(0.1770)	(0.1090)
Program	0.0046	0.0065
	(0.0156)	(0.0136)
Sample size	7,991	7,967
Mean Control (Non Titular)	0.20	0.10
Panel B: Non-Titulars in Treatment and All Women in Control Households		
Treatment	0.0313**	0.0218*
	(0.0159)	(0.0115)
Year	0.1200	-0.0331
	(0.1620)	(0.1370)
Program	-0.0320*	-0.0182
	(0.0178)	(0.0122)
Sample size	8,546	8,297
Mean Control	0.20	0.10

Notes
Significance: $^{***}:1\%;^{**}:5\%;^{*}:10\%$. Standard errors clustered by municipality. Regressions control for a set of individual, household and municipality characteristics. *Titulars* are defined as those who answered the *titular* specific Module (Module 2) in the questionnaire in the treatment municipalities at the baseline.

Table 3.6: Gender Specific Medium Run Effects

	Ill	Hospitalized	Ill	Hospitalized
	(1)	(2)	(3)	(4)
Panel A	Female 18 and higher		Male 18 and higher	
Treatment	0.0165	0.0304***	0.0373***	0.0114*
	(0.0134)	(0.0085)	(0.0141)	(0.0063)
Year	0.0426	-0.0290	-0.0075	-0.0516
	(0.1965)	(0.0895)	(0.1755)	(0.0858)
Program	-0.0095	-0.0175*	-0.0303	-0.0198**
	(0.0227)	(0.0099)	(0.0192)	(0.0084)
Sample size	12,672	12,722	11,717	11,757
Mean Control	0.19	0.09	0.17	0.06
Panel B	Females 18 − 59		Males 18 − 59	
Treatment	0.0185	0.0290***	0.0376***	0.0104*
	(0.0129)	(0.0087)	(0.0137)	(0.0061)
Year	0.0422	-0.0250	0.0321	-0.1156
	(0.1813)	(0.0942)	(0.1778)	(0.1010)
Program	-0.0105	-0.0181*	-0.0266	-0.0145*
	(0.0210)	(0.0104)	(0.0192)	(0.0083)
Sample size	11,903	11,942	10,684	10,716
Mean Control	0.19	0.09	0.15	0.05
Panel C	Females 60 and higher		Males 60 and higher	
Treatment	0.0387	0.0280	0.0338	0.0093
	(0.0559)	(0.0310)	(0.0401)	(0.0254)
Year	0.1408	0.0157	-0.6895*	0.7110**
	(0.8454)	(0.5842)	(0.4191)	(0.3575)
Program	0.0265	0.0161	-0.0824*	-0.0708**
	(0.0886)	(0.0537)	(0.0467)	(0.0295)
Sample size	715	715	1,001	1,003
Mean Control	0.26	0.08	0.29	0.14

Notes
Significance: *** : 1%;** : 5%;* : 10%. Standard errors clustered by municipality. Regressions control for a set of individual, household and municipality characteristics.

Table 3.7: Direct and Indirect Medium Run Effects on Women aged $18 - 59$.

	Ill, past 15 days (1)	In bed (2)	Hospitalized (3)
Panel A: Titulars and Non-Titulars in Treatment Households			
Titular	0.0018	-0.0081	-0.0026
	(0.0134)	(0.0393)	(0.0083)
Year	0.0050	-0.0711	-0.1300
	(0.1700)	(0.6250)	(0.1180)
Program	-0.0093	-0.0315	0.0220
	(0.0221)	(0.0618)	(0.0142)
Sample size	7,067	1,508	7,040
Mean Control (Non Titular)	0.21	0.62	0.08
Panel B: Non-Titulars in Treatment and All Women in Control Households			
Treatment	0.0205	0.0220	0.0236**
	(0.0166)	(0.0382)	(0.0114)
Year	0.1510	0.5180	-0.1200
	(0.2270)	(0.4030)	(0.1260)
Program	-0.0148	0.0886	-0.0221*
	(0.0254)	(0.0572)	(0.0118)
Sample size	7,448	1,435	7,172
Mean Control	0.19	0.53	0.09

Notes
Significance: *** : 1%;** : 5%;* : 10%. Standard errors clustered by municipality. Regressions control for a set of individual, household and municipality characteristics. *Titulars* are defined as those who answered the *titular* specific Module (Module 2) in the questionnaire in the treatment municipalities at the baseline.

Table 3.8: Robustness

	Excluding *early treatment municipalities* Short run		Excluding *converted municipalities* Medium run	
	Ill	Hospitalized	Ill	Hospitalized
	(1)	(2)	(3)	(4)
Treatment	0.0377**	0.0243***	0.0245*	0.0213***
	(0.0155)	(0.0085)	(0.0138)	(0.0065)
Year	0.1947	-0.0005	0.1439	-0.0339
	(0.1277)	(0.0754)	(0.1540)	(0.0549)
Program	-0.0338**	-0.0106	-0.0210	-0.0247***
	(0.0163)	(0.0103)	(0.0182)	(0.0073)
Sample Size	18,619	19,246	20,717	21,143

Notes:
Significance: *** : 1%;** : 5%;* : 10%. Standard errors clustered by municipality. Regressions control for a set of individual, household and municipality characteristics. In columns 1 – 2, estimating sample excludes the *early treatment municipalities* that received the payment before to the baseline survey was conducted. Only short run effects are presented. In columns 3 – 4 the estimating sample excludes the *converted municipalities* control municipalities that became treatment municipalities before the second follow-up survey was conducted. Only medium run effects are presented.

Table 3.9: DID-PSM Estimation Results. Short Run

	Ill		Hospitalized	
	A	B	A	B
DID Estimation by Kernel	-0.0232**	-0.0226**	-0.0138**	-0.0138**
Common support‡	(0.0098)	(0.0096)	(0.0063)	(0.0061)
Unmatched Sample				
Mean Absolute Standardized Bias	9.4619	5.3515	9.4619	5.3515
Median Absolute Standardized Bias	7.4457	4.6146	7.4457	4.6146
Pseudo R2	0.047	0.017	0.047	0.017
LR χ^2	815.58***	297.08***	815.58***	297.08***
Matched Sample				
Mean Absolute Standardized Bias	1.3733	1.2828	1.3733	1.2828
Median Absolute Standardized Bias	1.218	0.8801	1.218	0.8801
Pseudo R2	0.002	0.001	0.002	0.001
LR χ^2	36.13	23.13	36.13	23.13
Sensitivity analysis				
Parametric				
Nearest Neighbour Matching	-0.0215*	-0.0244**	-0.0189***	-0.0166**
($N = 5$) common support	(0.0117)	(0.0112)	(0.0079)	(0.0075)
Nearest Neighbour Matching	-0.0166	-0.0271**	-0.013	-0.0143*
($N = 5$) trimming	(0.0124)	(0.0122)	(0.0084)	(0.0083)
Caliper Matching (radius=0.001)	-0.0164	-0.0244***	-0.0142*	-0.0137**
Common support	(0.0107)	(0.0102)	(0.0072)	(0.0069)
Caliper Matching (radius=0.001)	-0.0164	-0.0262***	-0.0126*	-0.012
trimming	(0.0113)	(0.0109)	(0.0076)	(0.0074)
Non-parametric				
Kernel Matching (bandwidth=0.06)	-0.0210**	-0.0275**	-0.0115	-0.0134*
(bandwidth=0.06) trimming‡	(0.0100)	(0.0112)	(0.0081)	(0.0069)

Notes: Scenario A matches using the variables shown in Table A3 - 4. Scenario B contains the variables used by Attanasio et al. (2010) in their propensity score matching estimation. ‡: Bootstrapped Standard Errors. Significance: *** : 1%;** : 5%;* : 10%.

Table 3.10: DID-PSM Estimation Results. Medium Run

	Ill		Hospitalized	
	A	B	A	B
DID Estimation by Kernel	-0.0340***	-0.0214**	-0.0249***	-0.0224**
Common support‡	(0.0116)	(0.0105)	(0.0065)	(0.0077)
Unmatched Sample				
Mean Absolute Standardized Bias	9.2514	4.8855	9.2514	4.8855
Median Absolute Standardized Bias	7.3329	3.7688	7.3329	3.7688
Pseudo R2	0.05	0.016	0.05	0.016
LR χ^2	723.91***	228.90***	723.91***	228.90***
Matched Sample				
Mean Absolute Standardized Bias	1.4493	1.3412	1.4493	1.3412
Median Absolute Standardized Bias	1.1248	1.0324	1.1248	1.0324
Pseudo R2	0.002	0.001	0.002	0.001
LR χ^2	33.01	18.53	33.01	18.53
Sensitivity analysis				
Parametric				
Nearest Neighbour Matching	-0.0463***	-0.0104	-0.0177**	-0.0251***
($N = 5$) common support	(0.0126)	(0.0121)	(0.0087)	(0.0083)
Nearest Neighbour Matching	-0.0422***	-0.0105	-0.0201**	-0.0197**
($N = 5$) trimming	(0.0133)	(0.0133)	(0.0091)	(0.009)
Caliper Matching (radius=0.001)	-0.0372***	-0.0158	-0.0191***	-0.0248***
Common support	(0.0118)	(0.0111)	(0.0081)	(0.0077)
Caliper Matching (radius=0.001)	-0.0357***	-0.0124	-0.0208***	-0.0183**
trimming	(0.0123)	(0.0119)	(0.0085)	(0.0082)
Non-parametric				
Kernel Matching (bandwidth=0.06)	-0.0317***	-0.0175	-0.0234***	-0.0187**
(bandwidth=0.06) trimming‡	(0.0120)	(0.0120)	(0.0084)	(0.0086)

Notes: Scenario A matches using the variables shown in Table A3 - 4. Scenario B contains the variables used by Attanasio et al. (2010) in their propensity score matching estimation. ‡: Bootstrapped Standard Errors. Significance: *** : 1%;** : 5%;* : 10%.

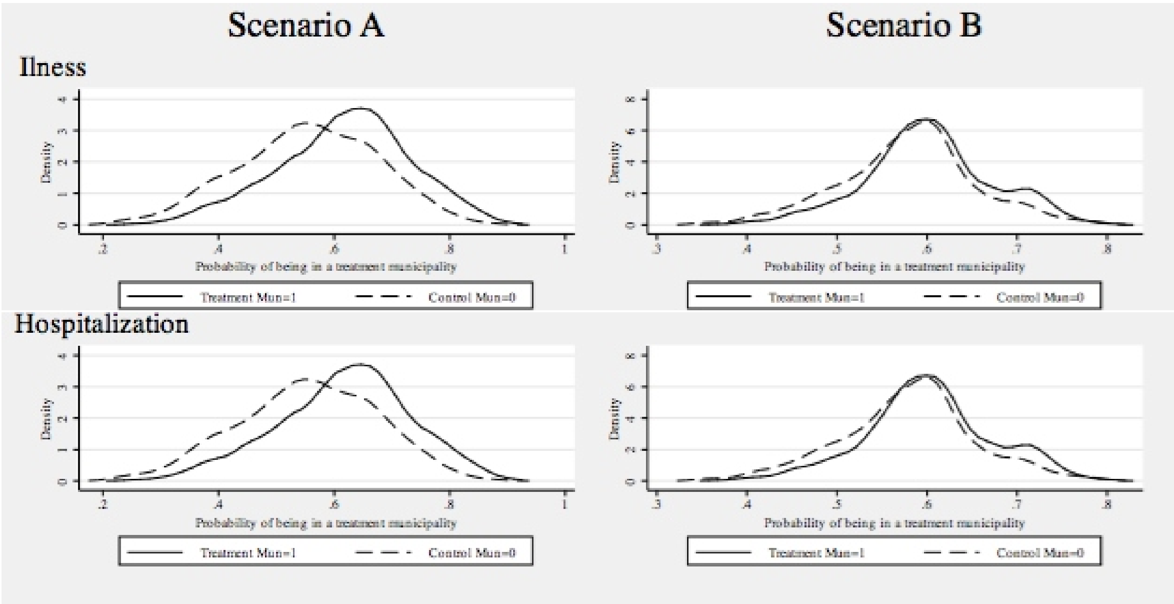
Table 3.11: Is the spillover effect operating through an income effect?

	Short Run		Medium Run	
	Ill	Hospitalized	Ill	Hospitalized
	(1)	(2)	(3)	(4)
$Program(\beta_3)$	-0.0129	-0.0147*	-0.0168	-0.0191**
	(0.0228)	(0.00849)	(0.0281)	(0.00962)
$Program \times$ #Children$(6-17) = 2(\xi_2)$	-0.00657	0.00190	0.0193	0.00888
	(0.0193)	(0.0118)	(0.0245)	(0.0126)
$Program \times$ #Children$(6-17) = 3(\xi_3)$	-0.0242	0.0168	0.00674	0.00612
	(0.0174)	(0.0109)	(0.0225)	(0.0127)
$Program \times$ #Children$(6-17) = 4(\xi_4)$	0.0144	0.0241*	0.0263	-0.00601
	(0.0256)	(0.0145)	(0.0295)	(0.0125)
$Program \times$ #Children$(6-17) = 5(\xi_5)$	-0.0168	-0.00372	-0.0509	0.0181
	(0.0309)	(0.0151)	(0.0422)	(0.0207)
$Program \times$ #Children$(6-17) \geq 6(\xi_6)$	0.0193	-0.0543***	0.00797	-0.0125
	(0.0502)	(0.0119)	(0.0390)	(0.0344)
Sample Size	17,106	17,343	12,352	12,489
Joint Test				
$\beta_3 + \xi_2 = 0$	0.738	3.928	0.748	3.548
$\beta_3 + \xi_3 = 0$	3.364	3.637	0.352	3.781
$\beta_3 + \xi_4 = 0$	0.441	4.471	0.868	5.574
$\beta_3 + \xi_5 = 0$	0.774	3.356	1.868	3.567
$\beta_3 + \xi_6 = 0$	0.465	7.938	0.373	3.563

Notes:
Significance: *** : 1%;** : 5%;* : 10%. Standard errors clustered by municipality. Regressions control for a set of individual, household and municipality characteristics. Sample restricted to households with no children aged $0-5$.

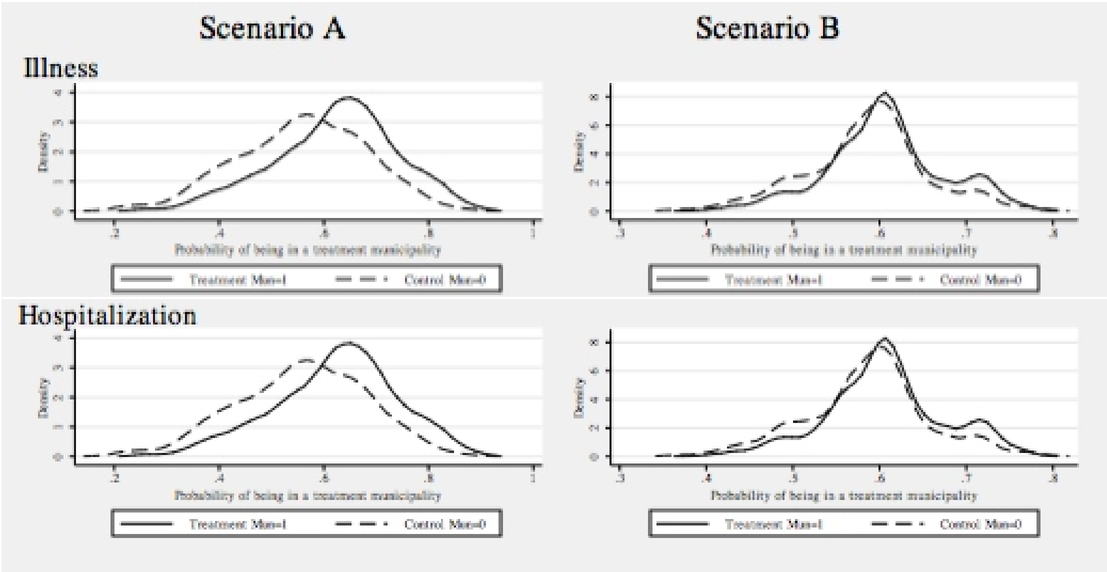
3.10 Figures

Figure 3.1: Distribution of Propensity Scores Short Run



Scenario A matches using the variables shown in Table A3 - 4. Scenario B contains the variables used by Attanasio et al. (2010) in their propensity score matching estimation.

Figure 3.2: Distribution of Propensity Scores Medium Run



Scenario A matches using the variables shown in Table A3 - 4. Scenario B contains the variables used by Attanasio et al. (2010) in their propensity score matching estimation.

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A3 Appendix

Table A3 - 1: Does attrition vary with treatment status?

	Attrition First Follow up (1)	(2)	**Attrition Second follow up** (3)	(4)
Status group				
Treatment	0.0031	0.0019	-0.0007	-0.0044
	(0.0054)	(0.0045)	(0.0081)	(0.0075)
Household head characteristics				
Age	-0.0003	-0.0002	-0.0068***	-0.0068***
	(0.0014)	(0.0015)	(0.0020)	(0.0020)
Education level: None	0.0135	0.0143	0.0438***	0.0437***
	(0.01000)	(0.0102)	(0.0166)	(0.0162)
Education level: Incomplete primary school	-0.0146*	-0.0144*	-0.0240	-0.0243
	(0.00800)	(0.00809)	(0.0169)	(0.0168)
Household characteristics				
Any livestock	-0.0083	-0.0069	-0.0187**	-0.0175**
	(0.0053)	(0.0053)	(0.0092)	(0.0087)
% household members aged 0 - 6	-0.0001	-0.0001	-0.0017***	-0.0016**
	(0.0004)	(0.0004)	(0.0006)	(0.0006)
% household members aged 7 - 14	-0.0004	-0.0004	-0.0025***	-0.0025***
	(0.0004)	(0.0004)	(0.0006)	(0.0006)
% household members aged 15 - 25	2.4e-05	-2.3e-05	-0.0013**	-0.0013**
	(0.0004)	(0.0004)	(0.0006)	(0.0006)
% household members aged 26 - 59	-0.0003	-0.0004	-0.0020***	-0.0019***
	(0.0004)	(0.0004)	(0.0006)	(0.0006)
Income percapita	1.2e-08	1.6e-08	2.4e-07***	2.3e-07***
	(5.2e-08)	(5.1e-08)	(7.2e-08)	(7.2e-08)
Geographical characteristics				
Small municipality center	-0.0050	-0.0007	-0.0052	0.0004
	(0.0064)	(0.0054)	(0.0081)	(0.0085)
Distance to the capital of the department	1.1e-05	1.7e-05	1.7e-05	1.3e-05
	(2.6e-05)	(1.7e-05)	(4.2e-05)	(3.6e-05)
Shocks				
Number of violent actions	-2.5e-05	-6.7e-06	-8.1e-05	-2.1e-05
	(8.8e-05)	(9.7e-05)	(0.0001)	(8.8e-05)
Interview characteristics				
Interview incomplete (dummy)	0.0553*	0.0520	0.0859**	0.0878**
	(0.0324)	(0.0339)	(0.0420)	(0.0430)
% of attrition in the mun. in the 2nd followup			0.0082***	0.0080***
			(0.0006)	(0.0005)
% of attrition in the mun. in the 1st followup	0.0053***	0.0051***	-0.0002	-4.1e-06
	(0.0006)	(0.0005)	(0.0009)	(0.0008)
Observations	6,631	6,631	6,631	6,631

Notes:Probit at the household level, marginal effects reported. Columns 2 and 4 include regional dummies; gender, access to health system and labour participation of the head of the households; and household wealth indicators such as ownership of basic appliances and fuel for cooking.
Significance: *** : 1%;** : 5%;* : 10%. Standard errors clustered by municipality.

Table A3 - 2: Program effects in the First Follow up: Full results for All adults

	Ill	Hospitalization
Treatment	0.0323***	0.0184***
	(0.00771)	(0.00479)
Year	0.179*	0.0157
	(0.105)	(0.0618)
Program	-0.0296***	-0.00547
	(0.00993)	(0.00622)
Municipality literacy	-0.000254	-0.000537
	(0.000759)	(0.000507)
Municipality poverty indicator	0.000816*	5.15e-05
	(0.000427)	(0.000276)
Percentage piped water	0.0550**	0.0179
	(0.0263)	(0.0172)
Percentage piped sewer	0.00682	0.0157
	(0.0224)	(0.0151)
Number of health centres	0.00330	0.000290
	(0.00259)	(0.00157)
Number of hall candidates	-0.00424**	-0.000287
	(0.00211)	(0.00113)
Distance to the main city of the state	8.56e-05***	7.48e-05***
	(2.81e-05)	(1.80e-05)
Extension of the department	1.35e-07	1.17e-06
	(2.76e-06)	(1.85e-06)
Municipality literacy × Time	-0.000476	0.000349
	(0.00100)	(0.000601)
Municipality poverty indicator × Time	-0.00164***	-0.000423
	(0.000551)	(0.000312)
Percentage piped water × Time	-0.000993	-0.0562***
	(0.0342)	(0.0209)
Percentage piped sewer × Time	-0.0738***	0.0212
	(0.0283)	(0.0176)
Number of health centres × Time	-0.00274	-0.00268
	(0.00326)	(0.00212)
Number of hall candidates × Time	-0.00892***	0.00222
	(0.00258)	(0.00138)
Number of children 0 to 5	-0.00311	0.000615
	(0.00345)	(0.00215)
Number of children 6 to 17	-0.00106	-0.00175
	(0.00189)	(0.00121)

Continued ...

Table A3 - 2 (continued)

	Ill	Hospitalization
Household income per capita (log)	-1.61e-07**	
	(6.26e-08)	
Household has sewage system		0.000353
		(0.00545)
Household has blender	-0.00874	
	(0.00636)	
Household has color TV	-0.00990	
	(0.00652)	
Household member born in the last 12 months		0.0351***
		(0.00522)
Household member is pregnant		0.0301***
		(0.00699)
Small municipality center	-0.0275***	
	(0.00921)	
Medium municipality center	0.000249	0.00523
	(0.00790)	(0.00431)
Household lives in grouped populated rural area of the municipality		-0.0189***
		(0.00459)
Household lives in sparsely populated rural area of the municipality		-0.0220***
		(0.00616)
Household member migrated in the last 12 months	0.0139	0.0118*
	(0.00956)	(0.00631)
Walls of good quality		0.00535
		(0.00412)
Main fuel for cooking: gas in cylinder		0.00594
		(0.00454)
Main fuel for cooking: wood	0.0214***	
	(0.00699)	
Gender	0.0180***	0.0337***
	(0.00698)	(0.00440)
Age	0.00338***	0.000393***
	(0.000220)	(0.000149)
Marital Status (Married or Partnership)		0.00255
		(0.00416)
Adult has no access to health system	-0.0247***	
	(0.00828)	
Adult has access to Private health system		0.0201*
		(0.0109)

Continued . . .

Table A3 - 2 (continued)

	Ill	Hospitalization
Adult can write	-0.0173**	
	(0.00674)	
Adult education Level: None		-0.0200***
		(0.00531)
Adult education Level: Incomplete Primary	-0.0109	-0.0114**
	(0.00739)	(0.00531)
Adult education Level: Complete Primary		-0.0139***
		(0.00461)
Adult was working last week	-0.0464***	-0.0239***
	(0.00685)	(0.00462)
Adult was not working last week but has job	0.0690***	0.0267*
	(0.0215)	(0.0160)
Adult has disability to work		0.136***
		(0.0281)
Adult was studying last week		-0.0536***
		(0.00783)
Head of the household	0.0366***	
	(0.00730)	
Sample Size	26,884	27,688
Mean Control	0.20	0.08

Significance: *** : 1%;** : 5%;* : 10%. Standard errors clustered by municipality.
Regressions control for a set of individual, household and municipality characteristics.

Table A3 - 3: Program effects in the Second Follow up: Full results for All adults

	Ill	Hospitalization
Treatment	0.0327***	0.0197***
	(0.00794)	(0.00478)
Year	0.00549	-0.0523
	(0.113)	(0.0700)
Program	-0.0180*	-0.0164**
	(0.0109)	(0.00659)
Municipality literacy	-0.000143	-0.000308
	(0.000790)	(0.000501)
Municipality poverty indicator	0.000650	0.000251
	(0.000451)	(0.000286)
Percentage piped water	0.0608**	0.0162
	(0.0270)	(0.0173)
Percentage piped sewer	0.00168	0.0128
	(0.0232)	(0.0151)
Number of health centres	0.00424	0.000297
	(0.00268)	(0.00154)
Number of hall candidates	-0.00610***	-0.00113
	(0.00217)	(0.00115)
Distance to the main city of the state	6.40e-05**	5.02e-05***
	(2.92e-05)	(1.76e-05)
Extension of the department	1.94e-06	-7.42e-07
	(2.95e-06)	(1.69e-06)
Municipality literacy × Time	0.00167	0.000918
	(0.00107)	(0.000658)
Municipality poverty indicator × Time	-0.00126**	-3.55e-05
	(0.000594)	(0.000360)
Percentage piped water × Time	-0.218***	-0.0586**
	(0.0374)	(0.0228)
Percentage piped sewer × Time	0.108***	0.0349*
	(0.0311)	(0.0196)
Number of health centres × Time	-0.00570	-0.00486*
	(0.00348)	(0.00251)
Number of hall candidates × Time	0.0102***	0.00148
	(0.00253)	(0.00149)
Number of children 0 to 5	-0.00119	0.00275
	(0.00364)	(0.00216)
Number of children 6 to 17	-0.000875	-0.00298**
	(0.00197)	(0.00118)

Continued ...

Table A3 - 3 (continued)

	Ill	Hospitalization
Household income per capita (log)	-7.03e-08	
	(6.44e-08)	
Household has sewage system		0.00289
		(0.00542)
Household has blender	-0.0113*	
	(0.00662)	
Household has color TV	-0.0123*	
	(0.00692)	
Household member born in the last 12 months		0.0353***
		(0.00521)
Household member is pregnant		-0.00942
		(0.00634)
Small municipality center	-0.0120	
	(0.00985)	
Medium municipality center	0.0157*	0.00132
	(0.00866)	(0.00422)
Household lives in grouped populated rural area of the municipality		-0.0105**
		(0.00470)
Household lives in sparsely populated rural area of the municipality		-0.0216***
		(0.00562)
Household member migrated in the last 12 months	0.0257**	0.0110*
	(0.0102)	(0.00611)
Walls of good quality		0.00790*
		(0.00415)
Main fuel for cooking: gas in cylinder		0.00748
		(0.00462)
Main fuel for cooking: wood	0.0174**	
	(0.00739)	
Gender	0.0287***	0.0331***
	(0.00751)	(0.00454)
Age	0.00314***	0.000401***
	(0.000234)	(0.000150)
Marital Status (Married or Partnership)		0.00646
		(0.00416)
Adult has no access to health system	-0.0134	
	(0.00902)	
Adult has access to Private health system		0.0166*
		(0.00997)

Continued ...

Table A3 - 3 (continued)

	Ill	Hospitalization
Adult can write	-0.0152**	
	(0.00719)	
Adult education Level: None		-0.0109**
		(0.00544)
Adult education Level: Incomplete Primary	-0.00884	-0.0101*
	(0.00777)	(0.00531)
Adult education Level: Complete Primary		-0.0118**
		(0.00466)
Adult was working last week	-0.0429***	-0.0160***
	(0.00729)	(0.00466)
Adult was not working last week but has job	0.0762***	0.0276*
	(0.0216)	(0.0157)
Adult has disability to work		0.131***
		(0.0293)
Adult was studying last week		-0.0307**
		(0.0124)
Head of the household	0.0316***	
	(0.00792)	
Sample Size	23,338	23,862
Mean Control	0.18	0.07

Significance: *** : 1%;** : 5%;* : 10%. Standard errors clustered by municipality.
Regressions control for a set of individual, household and municipality characteristics.

Table A3 - 4: Observables used in DID-PSM Estimations

Observable Characteristics Scenario A	Observable Characteristics Scenario B[‡]
Household income per capita	*Health Insurance of head:*
Household has piped water	Private health system
Household has waste collection service	Covered by the health system
Water facility is inside the house	Public health system
Water receives treatment before drinking	Age of the head
Household has fridge	Age of the spouse
Household has radio	Single parent
Household has bicycle	*Education Level of the head:*
Household has motorcycle	No education
Household has fan	Incomplete primary
Household has color TV	Complete primary
Household has kerosene lamp	Incomplete secondary
Household has livestock	*Education level spouse:*
Household member died in the last year	No education
Small municipality center	Incomplete primary
Medium municipality center	Complete primary
Atlantic Region	Incomplete secondary
Central region	*House walls:*
Household lives in grouped populated rural area of the municipality	Good quality wood
Household lives in sparsely populated rural area of the municipality	Poor quality wood
Walls of good quality	Mud, Cardboard/none
Walls of poor quality	Household has piped gas service
Number of children between 12 and 17	Household has piped water
Number of household members	Household has sewage system
Age of the head of the Household	Household has waste collection service
Age of the spouse of the head of the Household	Household has telephone access
Head with no partner	Household has WC connected
	to sewer or septic tank
Education level of the spouse: Complete primary	Ownership of this house
No access to health system by the Household	Household suffered from violence 2000-2002

Notes:
‡: See Attanasio et al. (2010)

Table A3 - 5: Balance of the treatment and control groups after matching

Variable	U / M	Mean Treated	Mean Control	%bias	%reduct $\|bias\|$	t-test t	t-test p> $\|t\|$
PANEL A: Scenario A First Follow-up							
Household has piped water	U	0.57	0.61	-9.8		-5.38	0
	M	0.57	0.56	1.3	87	0.77	0.44
Household has waste collection service	U	0.25	0.34	-21.3		-11.74	0
	M	0.25	0.26	-1.7	92	-1.07	0.28
Water facility is inside the house	U	0.55	0.61	-12.1		-6.61	0
	M	0.55	0.55	0.2	98.6	0.1	0.92
Water receives treatment before drinking	U	0.61	0.58	4.9		2.68	0.01
	M	0.61	0.62	-2	58.1	-1.25	0.21
Household has fridge	U	0.28	0.31	-6.5		-3.55	0
	M	0.28	0.27	1.5	76.6	0.94	0.35
Household has radio	U	0.42	0.47	-9.9		-5.4	0
	M	0.42	0.42	-0.7	92.9	-0.42	0.67
Household has bicycle	U	0.4	0.37	6		3.28	0
	M	0.4	0.39	1.6	73.3	0.96	0.34
Household has motorcycle	U	0.06	0.03	11.5		6.15	0
	M	0.06	0.05	4.4	62.1	2.45	0.01
Household has fan	U	0.32	0.38	-13		-7.15	0
	M	0.32	0.32	1.5	88.4	0.94	0.35
Household has color TV	U	0.35	0.38	-4.8		-2.66	0.01
	M	0.35	0.34	3.1	37.1	1.87	0.06
Household has kerosene lamp	U	0.06	0.1	-14.7		-8.22	0
	M	0.06	0.06	1.6	88.9	1.15	0.25
Household has livestock	U	0.74	0.66	17.4		9.62	0
	M	0.74	0.73	0.7	95.8	0.46	0.64
Household member died in the last year	U	0.03	0.03	-0.1		-0.04	0.97
	M	0.03	0.03	0.2	-221.6	0.14	0.89
Small municipality center	U	0.35	0.29	14		7.64	0
	M	0.35	0.36	-2.5	82.2	-1.47	0.14
Medium municipality center	U	0.29	0.38	-19.2		-10.56	0
	M	0.29	0.29	1.2	93.9	0.74	0.46
Atlantic Region	U	0.4	0.46	-12		-6.58	0
	M	0.4	0.4	-0.3	97.3	-0.2	0.85
Central region	U	0.27	0.2	16.8		9.1	0
	M	0.27	0.26	2.4	85.9	1.37	0.17
Household lives in grouped populated rural area	U	0.49	0.38	22.3		12.15	0
	M	0.49	0.48	0.6	97.5	0.34	0.74
Household lives in sparsely populated rural area	U	0.11	0.07	14.5		7.8	0
	M	0.11	0.11	1.4	90.3	0.78	0.44
Walls of good quality	U	0.41	0.46	-9.7		-5.33	0
	M	0.41	0.41	-0.4	95.4	-0.27	0.79

Continued ...

Table A3 - 5 (continued)

Variable	U M	Mean Treated	 Control	 %bias	%reduct $\|bias\|$	t-test t	 p> $\|t\|$
Walls of poor quality	U	0.05	0.04	6.3		3.43	0
	M	0.05	0.06	-2.1	67.1	-1.17	0.24
Number of children between 12 and 17	U	0.91	0.97	-5.6		-3.05	0
	M	0.91	0.92	-0.7	88.1	-0.4	0.69
Number of household members	U	6.86	7.06	-7.3		-4	0
	M	6.86	6.87	-0.4	95	-0.23	0.82
Age of the head of the Household	U	42.94	43.42	-3.9		-2.11	0.04
	M	42.93	42.98	-0.4	89.1	-0.26	0.8
Age of the spouse of the head of the Household	U	37.64	38.38	-6.5		-3.58	0
	M	37.64	37.55	0.8	88.4	0.46	0.64
Education level of the spouse: Complete primary	U	0.46	0.42	8.6		4.72	0
	M	0.46	0.46	0.8	90.7	0.49	0.63
No access to health system by the Household	U	0.86	0.79	17.5		9.72	0
	M	0.86	0.86	-0.6	96.7	-0.38	0.7
PANEL B: Scenario A Second Follow-up							
Household has piped water	U	0.57	0.62	-9.6		-4.82	0
	M	0.57	0.57	1	90	0.54	0.59
Household has waste collection service	U	0.25	0.34	-20		-10.19	0
	M	0.25	0.26	-1.9	90.3	-1.14	0.26
Water facility is inside the house	U	0.55	0.61	-11.5		-5.79	0
	M	0.55	0.56	-0.3	97.7	-0.15	0.89
Water receives treatment before drinking	U	0.61	0.58	5.5		2.77	0.01
	M	0.61	0.61	-1.8	67.8	-1	0.32
Household has fridge	U	0.27	0.31	-8.3		-4.19	0
	M	0.27	0.26	2.1	74.1	1.24	0.22
Household has radio	U	0.41	0.46	-10.7		-5.41	0
	M	0.41	0.41	0.1	99	0.06	0.95
Household has bicycle	U	0.41	0.37	6.3		3.15	0
	M	0.41	0.4	1.9	70.3	1.04	0.3
Household has motorcycle	U	0.06	0.03	10.8		5.31	0
	M	0.06	0.05	4.7	56.3	2.51	0.01
Household has fan	U	0.33	0.39	-13.1		-6.6	0
	M	0.33	0.32	1.8	86.4	1.03	0.31
Household has color TV	U	0.35	0.38	-4.9		-2.46	0.01
	M	0.35	0.34	3.5	29.1	1.97	0.05
Household has kerosene lamp	U	0.06	0.11	-17.5		-9.05	0
	M	0.06	0.06	1.4	92.2	0.91	0.36
Household has livestock	U	0.74	0.66	15.7		7.95	0

Continued ...

Table A3 - 5 (continued)

Variable	U M	Mean Treated	 Control	 %bias	%reduct $\|bias\|$	t-test t	 p> $\|t\|$
	M	0.74	0.73	1.1	92.8	0.65	0.51
Household member died in the last year	U	0.03	0.02	0.5		0.25	0.8
	M	0.03	0.02	1.3	-155.8	0.73	0.47
Small municipality center	U	0.35	0.3	10.8		5.39	0
	M	0.35	0.36	-3.2	70.2	-1.76	0.08
Medium municipality center	U	0.3	0.38	-18.8		-9.52	0
	M	0.3	0.29	1.3	93.3	0.74	0.46
Atlantic Region	U	0.41	0.46	-11		-5.53	0
	M	0.41	0.41	-0.1	98.6	-0.08	0.93
Central region	U	0.27	0.2	17		8.47	0
	M	0.27	0.26	2.3	86.5	1.23	0.22
Household lives in grouped populated rural area	U	0.48	0.38	21.9		10.98	0
	M	0.48	0.48	0.4	98.3	0.21	0.84
Household lives in sparsely populated rural area	U	0.11	0.07	15.2		7.48	0
	M	0.11	0.11	1.2	92.4	0.59	0.55
Walls of good quality	U	0.41	0.46	-9.5		-4.8	0
	M	0.41	0.42	-0.7	92.9	-0.38	0.7
Walls of poor quality	U	0.05	0.04	6.2		3.08	0
	M	0.05	0.06	-1.8	71	-0.94	0.35
Number of children between 12 and 17	U	0.91	0.97	-5.5		-2.75	0.01
	M	0.91	0.92	-1.1	80.5	-0.61	0.55
Number of household members	U	6.86	7.02	-5.7		-2.9	0
	M	6.86	6.86	-0.1	98.2	-0.06	0.95
Age of the head of the Household	U	42.8	43.38	-4.8		-2.4	0.02
	M	42.78	42.84	-0.5	89.1	-0.29	0.77
Age of the spouse of the head of the Household	U	37.53	38.23	-6.2		-3.15	0
	M	37.51	37.44	0.6	90	0.35	0.72
Education level of the spouse: Complete primary	U	0.46	0.42	7.4		3.72	0
	M	0.46	0.46	0.2	96.8	0.13	0.89
No access to health system by the Household	U	0.86	0.8	15.5		7.9	0
	M	0.86	0.86	-0.6	95.8	-0.39	0.69
PANEL C: Scenario B First Follow-up							
Private health system	U	0.21	0.14	18.9		10.15	0
	M	0.21	0.19	5.1	73.1	2.9	0
Covered by the health system	U	0.03	0.05	-9.8		-5.47	0
	M	0.03	0.03	0.6	93.5	0.44	0.66
Public health system	U	0.64	0.68	-6.9		-3.78	0

Continued ...

Table A3 - 5 (continued)

Variable	U M	Mean Treated	 Control	 %bias	%reduct $\|bias\|$	t-test t	 p> $\|t\|$
	M	0.64	0.66	-3.7	46.6	-2.23	0.03
Age of the head	U	42.95	43.41	-3.7		-2.04	0.04
	M	42.94	43.03	-0.8	79.8	-0.46	0.65
Age of the spouse	U	37.66	38.38	-6.3		-3.45	0
	M	37.65	37.79	-1.3	79.6	-0.79	0.43
Education Level of the head: No education	U	0.24	0.25	-3.6		-1.99	0.05
	M	0.24	0.24	-1.5	58.7	-0.91	0.36
Education Level of the head: Incomplete primary	U	0.15	0.14	2.3		1.28	0.2
	M	0.15	0.14	1.5	36.1	0.9	0.37
Education Level of the head: Complete primary	U	0.45	0.42	4.5		2.47	0.01
	M	0.44	0.44	0.3	93.1	0.19	0.85
Education Level of the head: Incomplete secondary	U	0.03	0.03	-0.6		-0.31	0.76
	M	0.03	0.03	0.5	14	0.3	0.77
Education level spouse: No education	U	0.2	0.21	-2.5		-1.34	0.18
	M	0.2	0.2	-1.1	54	-0.69	0.49
Education level spouse: Incomplete primary	U	0.16	0.16	-1.2		-0.64	0.52
	M	0.16	0.16	0.3	76.5	0.17	0.87
Education level spouse: Complete primary	U	0.46	0.42	8.8		4.78	0
	M	0.46	0.46	0.4	95.5	0.24	0.81
Education level spouse: Incomplete secondary	U	0.04	0.03	0.9		0.48	0.63
	M	0.04	0.03	1.3	-51.1	0.82	0.41
Walls of good quality wood	U	0.41	0.46	-9.9		-5.43	0
	M	0.41	0.41	-1.1	88.8	-0.68	0.5
Walls of mud, cardboard/none	U	0.05	0.04	6.2		3.36	0
	M	0.05	0.05	2.6	57.5	1.55	0.12
Household has piped gas service	U	0.07	0.07	-3.2		-1.75	0.08
	M	0.07	0.06	1.2	63.2	0.74	0.46
Household has piped water	U	0.56	0.61	-9.9		-5.38	0
	M	0.56	0.57	-0.2	98.4	-0.09	0.93
Household has sewage system	U	0.21	0.25	-8.5		-4.66	0
	M	0.21	0.22	-0.6	93.5	-0.34	0.73
Household has waste collection service	U	0.25	0.34	-21.2		-11.72	0
	M	0.25	0.24	0.8	96.2	0.51	0.61
Household has telephone access	U	0.07	0.09	-5.5		-3.04	0
	M	0.07	0.07	-0.3	94.1	-0.21	0.84
Household has WC connected to sewer or septic tank	U	0.48	0.52	-7.9		-4.29	0
	M	0.48	0.49	-0.9	88.4	-0.55	0.58
Ownership of this house	U	0.64	0.63	2.9		1.57	0.12
	M	0.64	0.63	1.1	60.4	0.69	0.49

Continued . . .

Table A3 - 5 (continued)

Variable	U / M	Mean Treated	Mean Control	%bias	%reduct $\|bias\|$	t-test t	t-test p> $\|t\|$
PANEL D: Scenario B Second Follow-up							
Private health system	U	0.21	0.14	19.6		9.67	0
	M	0.21	0.19	3.9	80.2	2.04	0.04
Covered by the health system	U	0.03	0.05	-8.8		-4.52	0
	M	0.03	0.03	0.6	93.7	0.35	0.73
Public health system	U	0.65	0.69	-8.8		-4.43	0
	M	0.65	0.66	-2.7	69.3	-1.51	0.13
Age of the head	U	42.81	43.36	-4.5		-2.27	0.02
	M	42.81	42.9	-0.7	83.6	-0.42	0.68
Age of the spouse	U	37.56	38.21	-5.8		-2.95	0
	M	37.56	37.67	-1	83	-0.57	0.57
Education Level of the head: No education	U	0.24	0.24	-1.3		-0.67	0.51
	M	0.24	0.24	-1.7	-30.1	-0.97	0.33
Education Level of the head: Incomplete primary	U	0.15	0.14	2.2		1.09	0.28
	M	0.15	0.14	1.8	15.9	1.03	0.3
Education Level of the head: Complete primary	U	0.44	0.43	1.9		0.95	0.34
	M	0.44	0.44	0.1	97.1	0.03	0.98
Education Level of the head: Incomplete secondary	U	0.03	0.03	0.2		0.09	0.93
	M	0.03	0.03	0.6	-235.6	0.32	0.75
Education level spouse: No education	U	0.2	0.2	-2.2		-1.09	0.28
	M	0.2	0.2	-2	6.1	-1.14	0.25
Education level spouse: Incomplete primary	U	0.16	0.16	-0.1		-0.05	0.96
	M	0.16	0.16	0.1	49	0.03	0.98
Education level spouse: Complete primary	U	0.46	0.42	7.6		3.81	0
	M	0.46	0.46	0.9	87.7	0.52	0.6
Education level spouse: Incomplete secondary	U	0.04	0.03	2.3		1.13	0.26
	M	0.04	0.04	1.3	41.4	0.74	0.46
Walls of good quality wood	U	0.41	0.46	-9.6		-4.84	0
	M	0.41	0.42	-1.1	88.4	-0.63	0.53
Walls of mud, cardboard/none	U	0.05	0.04	6.1		3.01	0
	M	0.05	0.05	3.1	48.5	1.71	0.09
Household has piped gas service	U	0.06	0.07	-3.5		-1.79	0.07
	M	0.06	0.06	0.8	76.7	0.48	0.63

Continued ...

Table A3 - 5 (continued)

Variable	U M	Mean Treated	 Control	 %bias	%reduct $|bias|$	t-test t	 p> $|t|$
Household has piped water	U	0.57	0.62	-9.4		-4.73	0
	M	0.57	0.57	-0.2	97.6	-0.13	0.9
Household has sewage system	U	0.21	0.24	-7.1		-3.58	0
	M	0.21	0.21	-0.3	96.4	-0.15	0.88
Household has waste collection service	U	0.25	0.34	-20		-10.16	0
	M	0.25	0.24	0.7	96.4	0.43	0.67
Household has telephone access	U	0.07	0.09	-7.6		-3.86	0
	M	0.07	0.07	-0.3	95.4	-0.21	0.84
Household has WC connected to sewer or septic tank	U	0.48	0.52	-7.4		-3.75	0
	M	0.48	0.49	-0.6	92.2	-0.33	0.75
Ownership of this house	U	0.64	0.64	1.1		0.57	0.57
	M	0.64	0.64	1.8	-55.4	0.99	0.32

Notes: U represents Unmatched and M represents Matched sample.

Scenario A matches using the variables shown in Table A3 - 4.

Scenario B contains the variables used by Attanasio et al. (2010) in their propensity score matching estimation.

Chapter 4

Can a Conditional Cash Transfer program change parents'underlying behavioural parameters and hopes for their children?

4.1 Introduction

During the last two decades a considerable number of developing countries have incorporated Conditional Cash Transfers (CCTs) in their social policies in order to improve human capital in poor households and break the intergenerational transmission of poverty. CCT programs consist of regular stipends given to poor households, on the condition that they invest in the human capital of their children, in general, promoting child health, nutrition and schooling. Given the evidence of low investment in human capital in poor households, the conditionality and the monetary incentive can be justified for a couple of reasons. In developing countries, observed low investments in children's human capital formation could be a result of low parental internalization of the positive social externalities of education (De Janvry & Sadoulet, 2005); parental agency problems where parents make decisions over children's education and labour, where they do not consider children's future wellbeing (Edmonds, 2007); parental impatience, irrationality and low self-control (Basu, 2003; J. Das, Do, & Özler, 2005; M. Das, 2007); liquidity or credit constraints (Lawrance, 1991); or low expectations of returns of education (Attanasio & Kaufmann, 2009; Jensen, 2010). Previous research has shown that health care utilization and school attendance has certainly increased with CCT programs. Fiszbein, Schady, and Ferreira (2009) give a review. However the question remains: Is this a result of the condition, the monetary transfer incentive or changes in parents' decision-making processes? If CCT programs change underlying parents' decision-making then we would expect to see a sustained increase in enrolments and nutrition even if the conditions and the cash payment were to stop. In contrast, if the CCT merely relaxed a liquidity constraint with conditions attached, if the program were to stop parents would no longer choose to send their children to school.

Using a Regression Discontinuity (RD) design, we explore if participation in a CCT has an effect on parents' time preferences and aspirations for their children's education. We use the term time preferences to refer to the preference for immediate utility over delayed utility; high time preference is also referred to as impatience or a high discount rate. To examine time preference and aspirations, we use information on hypothetical questions to elicit the discount rate, the number of years of education parents would like their children to complete and the probability parents place on their child completing each school level (secondary and tertiary education).

Living in poverty is often associated with high time preferences and low educational aspirations (Bauer & Chytilová, 2010, 2013; Becker & Mulligan, 1997; Kirby et al., 2002; Lawrance, 1991) mainly due to financial constraints, poor access to services and low levels of education. These factors limit people's ability to plan or invest, and to consider a wider range of options for themselves and their children or delay gratification. Conditional Cash Transfer programs offer people additional income and behavioural conditions. The income transfer relaxes the budget constraint of the household, which once they are living above subsistence levels creates the possibility for them to invest (in education or productive activities, for example) and plan present-consumption versus future-consumption. The budget relaxation can also increase the set of goods the households can afford, or create demand for new items previously not considered. Even if the consumption of these goods is only achievable with the new higher level of income, when income drops, the preference (aspiration) for these goods may have changed due to the experienced access. If changes in investments in human capital are due to relaxation of the budget constraint, the observed effect of the program in changing behaviour will be temporary. In contrast, if preferences have changed then the behaviour may change permanently.

Poor participant households have to take their children to the doctor, send their children to school, and caregivers have to attend educational talks in order to receive the monetary transfer. These conditions "force" them to change their behaviour. With time, they may create the habit of actually delaying present gratification and increasing the act of investing in human capital (L. Carvalho, Prina, & Sydnor, 2013). Additionally, the information they receive in the educational talks may create awareness of the importance of education, nutrition, health and general child care, making them change their preferences for human capital investment. If they are meeting the requirements because of the budget constraint relaxation, then the effect on their preferences for human capital investment for their children will be temporary, but if they form the habit of investing (sending their children to school or taking them to the doctor) and the new knowledge gained has changed their preferences for human capital, then the preference for higher human capital investment will be permanent.

Changing parents' preferences could potentially have a double effect on boosting human capital of children in poor households. If the program has an effect on parents' underlying preferences, parents will choose to keep investing in the human capital of their children, even in the absence of the program or in addition to it. Additionally, these parameters could potentially be transmitted generation to generation as the evidence suggests (Dohmen, Falk, Huffman, & Sunde, 2012; Volland, 2013; Zilibotti & Doepke, 2014; Zumbuehl, Dohmen, & Pfann, 2013). Although the focus of this chapter is the effect of the program on parents preferences and aspirations for their children, the program may have an impact on childrens preferences (in addition to increasing their human capital). If patience and aspirations are somehow learned by children from parents and they are also associated with human capital investment decisions, changes in parents' preferences would have an impact not only on parents' decisions over children's schooling but also on children's own decisions. If children are more patient they may also, for example, aspire to jobs with better wages after a period of training or education, as opposed to lower paying jobs (Lawrance, 1991). They may also choose to invest more in their own children. This effect could then break the cycle of poverty.

4.2 Literature review

Understanding how underlying behavioural parameters are formed and how they evolve (if they do evolve), is important to predict and evaluate public policies and institutional arrangements (Bowles, 1998). Changing some parameters amongst poor households, like time preferences or educational aspirations, is likely to boost human capital development. There are very few empirical studies examining this in developing countries.

How are preferences formed? Are they learned? Are they fixed? Or do they evolve? The psychological literature recognizes that humans are born impatient and later learn to be future oriented and choose actions with postponed rewards (Mischel, Shoda, & Rodriguez, 1989) Also, preferences are to some extent biologically determined but not immutable as they are sublimated by parental teaching, social pressure and circumstances (Anderson, Dietz, Gordon, & Klawitter, 2004). Doepke & Zilibotti (2008) and Zilibotti & Doepke (2014) provide a theoretical framework where children's preferences are learnt from parents, where different parenting styles are considered in the model. Empirical evidence exists of intergenerational transmission of behaviours like work attitudes (Fernandez & Fogli, 2009) or leisure activities (Volland, 2013); and underlying preferences like risk and trust (Dohmen et al., 2012; Zumbuehl et al., 2013) or academic aspirations and motivation (Benner & Mistry, 2007; Kirk, Lewis-Moss, Nilsen, & Colvin, 2011). These similarities in preferences within families may partially explain substantial intergenerational persistence of different economic outcomes like income, wealth and education (Bowles, Gintis, & Groves, 2009).

Even if the preferences are to a degree learned from parents, there are other factors that influence them to a certain extent. For example poverty has been associated with high time preferences and low educational aspirations. High time preferences have been associated with low education, wealth and income, household composition, gender and age (Bauer & Chytilová, 2010, 2013; Becker & Mulligan, 1997; Kirby et al., 2002; Pennings & Garcia, 2005; Tanaka, Camerer, & Nguyen, 2006). Becker and Mulligan (1997) argue that education can be an investment in patience. As individuals grow older and their level of education increases, education can help them to form a picture of life, its pleasures and difficulties. Constant practice of problem solving will also enhance the process of anticipation as individuals learn the art of scenario simulation. In that sense, education helps people to perceive future pleasures as less remote. However, poor households are usually characterized by low education, income and wealth and tend to be more impatient favouring present over future consumption. Lawrance (1991) finds that poor households in the US have time preferences 5 percentage points higher than richer households. L. S. Carvalho (2010), using information from rural Mexico finds that the poor seem to be very impatient. Women and older people seem to be more patient. Using hypothetical information in Vietnam, Anderson et al. (2004) find women more patient than men but no differences coming from income variation. Rubalcava, Teruel, and Thomas (2009) using evidence on intertemporal preferences from the Mexican Family Life Survey[1] indicate that women are more patient than men when thinking about the future. They also find that additional income from cash transfers given to married women is spent on small livestock, improved nutrition, and child goods in Mexico. This suggests that balancing the bargaining power between men and women in the household allows the allocation of more resources toward future investments. Tanaka et al. (2006) provides some evidence on the positive relationship between

[1] This is a panel survey in Mexico but this does not provide the information used for the CCT impact evaluation.

household and mean village income and patience of individuals in Vietnam. In the same line, Bauer and Chytilová (2013) also find that women in Indian villages are more patient than men. Equally they find heterogeneity in patience according to household composition, for example parents with younger children are the ones who exhibit lower time preferences. Dean and Sautmann (2014) find that changes in preferences are related to changes in non-labour income rather than to labour income.

Parents' aspirations are consistently related to children's ultimate school achievements (Benner & Mistry, 2007; Chiapa, Garrido, & Prina, 2012; Spera, Wentzel, & Matto, 2009). Low levels of parents' education, income and expected returns to education have been shown to be associated with parents having low schooling aspirations for their children (Attanasio & Kaufmann, 2014; Davis Kean, 2005; Halle, Kurtz-Costes, & Mahoney, 1997; Sosu, 2014; Spera et al., 2009). While the economic literature on the relationship between poverty and parental educational aspirations for children is still limited, the education and psychology literature provides more information. Halle et al. (1997) using a sample of low-income minority families in the US finds that mothers with high education had higher aspirations for their children's academic achievement. Even when parents' expectations[2] reflect parents' aspirations adjusted by constraints to attain them, Davis Kean (2005) similarly finds using data from the US that parents' education shapes their expectations over children's educational achievement. However ultimately the way these aspirations are formed is unclear. A possible mechanism could be the beliefs of the decision makers about the nature of private investment and their possible future returns. For example, parents may believe that earnings respond to education less elastically than they

[2] Educational aspirations refer to the desired level of education (free of real world constraints) while educational expectations refer to the assessment of the real possible educational achievement of the child.

actually do, which may result in parents placing low value on schooling. Attanasio and Kaufmann (2009) tested this hypothesis using data from Mexico and found that earnings obtained for young adults between 15 and 25 years old are higher than the expected returns, especially among children of parents with low education levels.

Household income can either enable or limit parents educational aspirations for their children. The range of options individuals aspire to is influenced by the options they can realistically contemplate. The options and opportunities (or lack of them) are usually related to their own income and wealth as well as the income and wealth of their neighbours or people with whom they interact. For example, comparing own standards of living or achievements with neighbouring people can influence ones own aspirations (Appadurai, 2004; Ray, 2006). Overall, if age, household composition, income, wealth and interactions with others are related to time preferences and aspirations and they adjust over time, then preferences are likely to change to reflect current circumstances (Becker & Mulligan, 1997; Bowles, 1998).

This chapter contributes to the literature that examines how changes in circumstances such as changes in income, knowledge of a wider range of options and changes in regular behaviour (sending children to school or taking them to the doctor) can potentially change underlying behavioural parameters of individuals. We look in particular at time preferences and aspirations in education. Empirical evidence of their evolution is still limited mainly due to lack of longitudinal studies. Kirby et al. (2002), using incentivized choices of immediate or delayed gains collected information every three months for 2 years in a sample of Tsimane' Amerindians in 2 villages in rural Bolivia. They find that high time preferences, high rate of consumption and impulsive behaviour, although relatively stable characteristics are also influenced by situational factors like income variation. Using tax return information from individuals in Boston, Meier and Sprenger (2014) find that discount parameters are stable over the period of 2 years. While the parameters over time are far from perfectly correlated, the differences are uncorrelated with levels and changes in socio-demographic variables. This evidence however is limited by the short period of analysis, the particular population included and the small size of the changes in the socio-demographic variables considered. Discount parameters may take longer and require bigger changes to adjust and may depend on the initial conditions or context of the individuals, like living in poverty or in developing countries. Dean and Sautmann (2014) use panel data to evaluate the impact of a randomized trial for a health care program for children in Mali. They find persistence in discount choices of about 69%. However, they find that the marginal rate of intertemporal substitution varies systematically with income, consumption, spending shocks and savings. For example, individuals increase their discount rates right before pay-day or if they just have experienced an adverse shock. In the same line Cameron and Shah (Forthcoming), using data on individuals in Indonesia show that exposure to negative shocks such as natural disasters makes individuals exhibit more risk-averse choices and changes their subjective beliefs (occurrence and severity of future events). They cannot definitively show that risk preferences change (as distinct from changes in beliefs about the state of the world) but they acknowledge that changes in the underlying preferences is one possible cause of these changes.

Finally, availability, experience and knowledge of services or goods can change individuals' preferences for them. For example availability of financial institutions and the habit of making regular choices like saving can affect preferences for risk taking or experiencing better health or education. Exposing people to these options could increase the preference for education and health. L. Carvalho et al. (2013) provide evidence that the option of opening a savings account and the act of saving, seems to increase risk attitudes and the willingness to delay gratification among poor households in Nepal. Although they are not able to distinguish whether the effect comes from increased wealth or a change in the preference for saving, they state that access to savings accounts and saving itself could change the marginal utility of consumption affecting present-bias choices. Additionally, they find that access to savings accounts changes preferences towards considering broader options. Chiapa et al. (2012) provide some similar empirical evidence in this line in terms of parents' educational aspirations over their children. Using information from Mexico they find that a CCT that increases parents' exposure to and interaction with more educated individuals than themselves can explain, at least in the short term, an increase in parent's aspirations for their children's education. Additional income and behavioural conditions coming from the *Familias en Acción* program in Colombia are the potential channels through which parents' time preferences and aspirations for their children's schooling can change.

There is not much evidence of changes in parameters in the CCT literature. Using data from an unconditional cash transfer in Kenya targeting households with one deceased or chronically ill parent, Martorano, Handa, Halpern, and Thirumurthy (2014) do not find differences in intertemporal choice of the parent or caregiver when comparing treatment and control locations two years after exposure to the program. However, the identification of this effect was not clear as, by the time of the evaluation, both treatment and control locations were receiving the treatment. Chiapa et al. (2012) study changes in parents' aspiration for their children's years of education in the Mexican CCT. They find that in the short-term *Progresa* is associated with an increase in educational aspirations of about a third of a school year. They explain this positive effect from mandated exposure to educated professionals (doctors and nurses).

The CCT program can have an effect on children's preferences and aspirations aspirations through other channels, not only via learning from their parents (Kirk et al., 2011; Zilibotti & Doepke, 2014). Preferences may also change as a result of higher education attainment and higher potential income resulting from the program's conditions. There are at least four potential channels for time preferences to be reduced if education is increased. First, schooling may promote increased cognitive skills and the ability to simulate and plan for the future (Becker & Mulligan, 1997). Second, education may allow individuals to develop mechanisms to control present consumption such as savings and investment (Duflo, 2006). Third, education might promote health and reduce mortality risk (Cutler & Lleras-Muney, 2010) and this would make individuals more willing to delay their spending. Finally, more educated individuals may face fewer income constraints that allow them to live above subsistence income levels lowering the pressure for present consumption (Bauer & Chytilová, 2010).

4.3 The program

Colombia, following the trend of a large number of Latin-American countries, implemented the program '*Familias en Acción*', a Conditional Cash Transfer program in poor rural areas from 2002. The overall aim of this initiative was the creation of human capital in households experiencing extreme poverty through monthly transfer payments. Households were classified as extremely poor if they belonged to the first quintile of a poverty index. We will take advantage of this selection criterion for our identification strategy, assuming that participation in the program is discontinuous at an exogenously defined cut-off on this poverty index. The transfer was expected to improve the education levels via improved school attendance, as well as improved health and nutrition of children via regular medical check-ups in rural households. The transfer represented on average 16% to 25% of the total monthly income of the household and was received by the caregivers.

The program included a health and nutrition component that was offered to households with at least one child aged between 0 and 6. Each household received approximately US$25 regardless of the number of children they had in the 0 to 6 years age bracket. This transfer was conditional upon children attending growth and development check-ups every two months, and a vaccination program. Primary caregivers[3] needed to attend talks on hygiene, diet and contraception. The program also included an educational component that was offered to households with children aged between 7 and 17. The transfer was approximately US$8 for each child in primary school, and US$16 for each child in secondary school[4]. The monetary transfer was subject to an average school attendance rate of more than 80% per child. In essence, the program consisted of a monetary transfer to the caregiver, contingent on medical check-ups for children aged 0 to 6, school attendance for children 7 to 17 and information sessions for caregivers. In those information sessions topics like health care for children, nutrition, sanitation of the household, care of the household members and contraception were discussed.

The program was originally offered only in some municipalities[5] in the country. The selection of municipalities was based on population and infrastructure characteristics. Municipalities had to i) have a population of less than 100,000 and not be the principal town of the state, ii) have enough health and education infrastructure, iii)

[3] The caregivers are primarily the mother of the child receiving the benefit. In some cases the father, the grandmother or other household members are the caregivers if the mother does not live in the household. This case represented around 6% of the households participating.

[4] The Colombian school system is divided into five years of primary school followed by four years in basic secondary and then two years of middle secondary school, for a total of six years of secondary education. After finishing secondary school, individuals can pursue vocational or tertiary education.

[5] Municipalities refer to the major governmental administrative units in Colombia

have a bank branch, and iv) the town government had to show interest in being part of the program as well as completing the required documents and providing the identification numbers of the possible beneficiaries of the program. As the aim of the program was to improve human capital among the poorest families, the selection of households was based on the poverty level and presence of children. A household was eligible if i) it was classified as poor by the poverty index SISBEN, ii) it had at least one child aged 0 to 17 and iii) it lived in a municipality where the program was going to operate (treatment municipalities). The take-up rate of the program was over 90%.

The SISBEN index is the instrument designed by the government to rank households and identify them as a target population for social expenditure. This index was first constructed in 1999 as a proxy indicator[6] of the resources of households according to their life conditions with values between 0 and 100. Values close to 0 represented the poorest households while those close to 100 the rich. The threshold imposed to be classified as poor and participate in the program was different in rural and urban areas[7].

The program became operational at the end of 2002 including 691 municipalities out of 1060 in Colombia. Household participation in the following ten years after the program started was mainly defined by geographical location, the poverty index score and presence of children aged 0 to 17 in the household. Due to success with respect to health, nutrition and education outcomes achieved by the program, in 2007 it was extended to other rural municipalities, indigenous populations, households who were internally displaced, and urban municipalities of the country. Meanwhile, an

[6] This indicator is the first principal component of four factors of household characteristics including education and social security; demographic characteristics and income; dwelling quality and equipment; and available utilities.

[7] In Urban areas households had to have a SISBEN score lower than 34 and in rural areas a score lower than 14.

updated version of the poverty index (SISBEN II) was used to classify households. This new measure was intended to update the score excluding some variables and including new variables to reduce chances of manipulation, increase identification of poor households, increase relevance of the variables selected to indicate poverty and stability of these variables over time to reflect structural poverty rather than transitory poverty[8]. The program grew from reaching about 320,000 households in 2002 to around 2.8 million in around 900 municipalities in 2010, making *Familias en Acción* one of the biggest programs for poor households implemented by the Colombian government.[9]

If preferences are highly stable over generations, this limits the role for public policy to improve school attendance, attainment or other outcomes. However, programs such as school vouchers or CCTs have shown that external incentives can either change parents' schooling decisions over their children's education or help them to free up credit constraints and so achieve previously unfeasible educational aspirations. Given that *Familias en Acción* provides households with income, behavioural conditions and some information, it is expected that the program will have positive effects on the preferences of the parents. If a poor parent not only has less ability, but also less willingness to invest in children's human capital formation even with public education provision (M. Das, 2007), we expect that a sustained cash transfer and the habit of experiencing health care and schooling would expand parents' schooling choices and increase their aspirations for their children's schooling. Furthermore, we expect that the habit of delaying present consumption (sending

[8] In 2008 the index was revised again as a result of evidence of lack of ability to identify the poor and manipulation. I will refer to this issue in the data section.

[9] Note that the SISBEN score is also used for targeting of subsidised health care and a childrens nursery program. However, the childrens nursery program is targeted at younger children. Further, the threshold for subsidised health care is higher than that for *Familias en Acción* thus most families in our control and treatment groups are likely to have access to subsidised health care (around 80%). Hence, we are confident we are identifying the impact of *Familias en Acción* on preferences, not the impact of overlapping programs.

children to school and taking them to medical centres) and the information provided in the information sessions on general family care would not only increase the range of possibilities available and experienced by parents but also change their preferences towards them. There is evidence of changes in habits for this population. *Familias en Acción* has increased the enrolment rate of children aged 14 to 17 by 5.6 percentage points (Attanasio et al., 2010), the probability of finishing high school in rural areas by 6 percentage points (Baez & Camacho, 2011) and the number of children who have regular visits to medical check-ups by around 28% (Attanasio, Battistin, Fitzsimons, & Vera-Hernandez, 2005).

If the program only affects the choices via a relaxation of the budget constraint or the choices are merely a product of the conditions of receiving the money, it is likely that the effect on behaviour would last only while the transfer is being received. In contrast, if the observed choices come from a change in the preferences towards education and delayed consumption, the effect of the program on preferences may be permanent. This change in preferences would not only increase participants' willingness to invest more in their children's schooling, but also in other important outcomes of human capital development such us health, nutrition, mental stimulation and/or leisure.

4.4 Data and Methods

4.4.1 Data

The data used in this paper comes from a follow-up survey implemented in 2012, 10 years after the program started. This survey provides our only data on the discount rates and parents' aspirations for their children's schooling attainment. It also provides information on socio-demographic characteristics of the households, and children's schooling history.

We measure time preferences using a hypothetical set of four questions to elicit the implicit discount rate. These questions are asked of only one adult per household (mostly the household head or his/her spouse), where the person is faced with a choice of being paid a hypothetical amount today or a larger sum in the future. Table 4.1 presents the four choices. The first one is the possibility of choosing between 100,000 COP today and 105,000 COP in a month. If the person chooses the 105,000 COP then the other options are not presented but if the person chooses 100,000 COP then the second option is presented. The second option offers 120,000 COP in a month (versus 100,000 COP today), the third option offers 150,000 COP and the fourth one 200,000 COP. The offering process continues until the person switches from the present option to the future option or until the options are exhausted. This procedure allows us to calculate five intervals of the monthly discount rate. For example someone choosing 105,000 COP in a month rather than 100,000 COP today exhibits a discount rate between 0 and 0.05 (most patient), but someone who did not choose the 105,000 COP offer and switches when the second option is offered (120,000 COP in a month) exhibits a discount rate between 0.05 and 0.2 (less patient). Figure 4.1 shows the density distribution for our sample in our five intervals. We observe that for this sample the most chosen option exhibits an underlying discount rate between 1 and ∞ (associated with choosing 100,000 even when 200,000 is offered in a month's time). The average monthly household income in the sample at the time of the interview was 830,000 COP[10]. Hence, the amount offered 'to be received today' is about 12% of average monthly income.

[10] Using an exchange rate of 1900, the average monthly household income is U$ 436.

A low discount rate represents a low preference for present consumption and we will refer to this interchangeably as low time preferences or more patience. We will use a midpoint of the interval as our best estimate of the person's discount rate, similar to the strategy used by Bauer and Chytilová (2010). Someone who exhibits a discount rate between 0 and 0.05 will be associated with a discount rate of 0.025. We will have 5 possible underlying discount rates: 0.025, 0.125, 0.350, 0.75 and 1. Note that given that the last interval is open to the right (1 - ∞) we use for that case the lower value of the interval. This gives us the lower bound of the real discount rate for this sample.

To measure parents' aspirations for their children's schooling, we use three indicators reported only for the oldest child aged 6 to 17 in the household. This question is asked of all the caregivers of that child, in most cases this is the parents. The first indicator is their aspiration for the number of years of education this child will attain. We calculate the number of years of education, combining the information on the believed highest level of education the child will achieve and the number of years in that level[11]. For example, if a parent indicates that he aspires for his child to reach the third year of secondary school, then the aspirational number of years for education will be 8 years. In Colombia there are five main levels: Preschool level which is not compulsory; Primary school level (5 years); Secondary school (6 years) which includes Basic level (4 years) and Vocational level (2 years); and Superior level. Superior level refers to either technical (2 years), technological (4 years) and university (5 years). For a person to have a complete secondary education, that person must complete all the grades of primary, basic secondary and vocational for a total of 11 years of education. People can only access the superior level upon

[11] In Spanish the questions were "¿Cuál cree usted que es el grado de educación más alto que alcanzará el menor?" and "¿Cuál es la cantidad de años que usted cree que el menor aprobará en ese nivel?"

completion of secondary level. Traditionally, someone with a university degree has completed 16 years of education.

The second and third indicators are the caregiver reported probability of the child completing secondary education by age 18 and superior education. The hypothetical questions to elicit these probabilities come from the questions (i) By the time the child is 18 years old what is the probability he/she will have completed secondary education?[12] And at any age (ii) What is the probability that he/she will graduate from superior education?[13] For example, the person may believe the probability the child has finished secondary education by age 18 is 90% but the probability of finishing superior education is 40%.

Because time preferences were asked from the household head or his/her spouse in all households while aspiration questions were asked from all the caregivers of the child in households with school age children, we have a sample size of 3065 for time preferences and 4000 for caregivers' aspirations. The sample is also evenly distributed between urban and rural areas. We disaggregate at this level because traditionally education demand and supply is lower in rural areas and also because the eligibility criteria for participation was different in urban and rural areas. We explain this in the next section. Table 4.2 presents descriptive statistics for our variables of interest. Panel A presents the descriptive statistics for the discount rate. Around 40% of the sample has high time preferences, as they always chose the 100,000 COP (values the present more), which is equivalent to having a high discount rate. Only around 12% of the individuals selected the most patient option (105,000 COP in the first offer). Individuals living in urban areas appear more

[12] In Spanish the question was "¿Qué tan posible es que el menor, cuando tenga 18 años, haya terminado el bachillerato completo?"

[13] In Spanish "¿Qué tan posible es que el menor se gradúe de educación superior?"

impatient than those living in rural ones. The mean monthly discount rate for the sample is 0.62 which means that on average individuals are willing to give up \$100 today if they get \$162 in one month.

Table 4.2 also reports the aspiration questions for all the child's caregivers, which includes parents as well as older siblings or other relatives in the household (Panel B)[14]; and also presents it disaggregated for mothers (Panel C) and fathers (Panel D). We find that in all groups the average number years of education aspired to for a child is about 13 years. Fathers in urban areas have slightly higher average aspirations: just over 14 years. The average probability that the child finishes secondary education by age 18 reported by caregivers is 82% and the probability that they finish superior education is 65%. Aspirations of people living in rural areas are consistently lower than for people living in urban areas. If the program is affecting aspirations we might expect that mothers' aspirations would be higher than fathers' given that they attend the educational talks and also primarily received the monetary transfer; however we do not find a systematic pattern in the mean.

The baseline data shows that 43% of individuals aged 12 to 30 did not complete primary education. If we look at the age 18 to 30 (by age 18, 67% of the individuals are no longer studying), we find that 53% of them did not complete primary school in rural areas and 34% in urban areas. Figure 4.2 presents the distribution of level achieved by cohort ages of 5 years from 12[15] to 30. An increase in the probability of completing secondary and tertiary education is an increase of the lined and black areas that were very small particularly in rural regions. The average number of years of education in rural areas for individuals 18 to 20 and 21 to 25 is 5.6 and 5.0 while

[14] Only around of 10% of the reporters are other people other than the father or mother of the child

[15] We restrict the starting point to 12 as by that age children are supposed to have finished primary school.

in urban areas it is 7.2 and 6.8. This indicates that on average people complete primary school but do not go much further. Note that the aspirations reported in the survey by far exceed actual educational attainment in these areas. Hence, they seem to reflect the hopes of the parents and caregivers, and not necessarily the financial and other constraints faced by these households.

4.4.2 Identification strategy

The initial design for the evaluation of the program, established by government jointly with the evaluation team, was a comparison of municipalities where the program was operating (treatment municipalities) with similar municipalities where the program was not operating (control municipalities)[16]. However, given that the time preference and discount rate data are only available in the 2010 follow up survey and after 2007 the program was offered in all eligible municipalities including the control municipalities, we are unable to identify program effects on time preferences and parents' aspirations for their children's education by a comparison of control and treatment municipalities. Instead, we will use a Regression Discontinuity (RD) design based on the household eligibility criteria of the presence of children 0 to 17 and the poverty score (SISBEN) for identification.

To create a new control group to evaluate the long-run effect of the program, the

[16] The selection of the control municipalities was not random. To identify control municipalities the firm in charge of the evaluation used a matching process on observables to select similar municipalities. The characteristics taken into account for the matching were the population size, the level of the quality of life index, health and education infrastructure. However the presence of a bank branch was not a requirement. To preserve the balance in the sampling process among municipalities a Primary Sampling Unit – PSU was defined under the condition of a minimum of eligible households per unit. Usually one PSU corresponded to one municipality, but where the number of eligible households was less than that required; geographically contiguous and substantially similar municipalities were added to the same PSU to achieve the minimum threshold (similar is defined as having the same index for health and school infrastructure). In total 50 treatment PSUs were chosen to be compared to 50 control PSUs. As such, from the 691 participating municipalities; 57 were part of the treatment sample and 67 more were chosen as non-participating, control, counterparts.

government and the evaluation team decided to interview in the 2010 follow-up additional non-eligible households living in the same municipalities of the initial sample design. These additional households, although having children 0 to 17, were not eligible because they were just above the cut-off point of the poverty index for eligibility. The intuition behind this is that households who are just above the cut-off are similar enough to those who are just below. The survey included a random sample of 70% of the households who were interviewed when the program started, as well as a new sample of households who were ineligible to participate in the same municipalities with a poverty score above the cut-off point. This sample was selected from all the households who were above the cut-off point in both rural and urban areas. The cut-off was different in rural and urban areas and the bandwidth used for sampling was different as well. In rural areas the cut-off point was at SISBEN score 14 and the bandwidth for sampling was 4. This means that the households of comparison are households between 10 and 18 in the SISBEN index in rural areas. In urban areas the cut-off was established at the SISBEN score of 34 and the bandwidth used was 6, which generates a sample of households with SISBEN scores between 28 and 40.

The key point for this identification strategy is that the participation in the program is determined at least partially by the poverty score lying at each side of the fixed cut-off (Imbens & Lemieux, 2008). Eligibility in essence is determined by the poverty score and the cut-off, thus we expect to find a discontinuity in program participation at the cut-off point. For the analysis hereafter we centre the score to 0 where negative scores represent eligibility while positive ones represent ineligibility. Centring the score to zero allows us to make a combined analysis of the total sample as the cut-off point for eligibility was different in rural and urban areas. However, as urban and rural areas differ in a variety of ways, we also break down the sample by urban/rural in the analysis.

This setup gives us a sample of around 880 individuals with poverty scores above the cut-off (ineligibles) and 2800 below the cut-off who are eligible individuals. Table 4.3 shows the number of observations of eligible and ineligible households of the variables of interest. Please note that to allow us to focus on the area around the cut-off, all the visual inspections will be truncated to the poverty score interval [-4, 4] hereafter. The full sample however, goes from -14 to 4 for rural areas and -34 to 6 in urban areas. Figure 4.3 presents the distribution of the poverty score for this sample.

Graphically, we can see a jump in the probability of participation around the cut-off point (poverty score equal zero) for both samples, time preferences and caregivers' educational aspirations for the children (see Figure 4.4). Comparing discount rates or educational aspirations for the children of individuals who were just above and below the cut-off point eliminates selection and omitted variables bias and also allows us to estimate the causal effect of the program at poverty score equal to zero that determines eligibility. Figure 4.5 presents the same jump in the probability of participation disaggregated by rural and urban areas and figure 4.6 for mothers and fathers separately in the educational aspirations sample. We include this break down for the education aspirations sample as the program gave the transfer to the mother[17] and they also attended educational talks, so mothers' exposure to the program differs from fathers'.

Besides the contamination of the initial control municipalities by the 2010 survey, a new methodology to calculate the poverty score (SISBEN II) was also implemented in 2006. Using this new poverty score, households became eligible when the program

[17]The transfer is made to primary caregivers who are mostly mothers.

was offered in the control municipalities. We use this new poverty score (SISBEN II) for identification in those initially designated control municipalities. Appendix A4 - 1 shows the graph of the probability of participation. Contrary to the previous graphs, in this case we do not see a clear jump around the cut-off for eligibility. One possibility for this result is that the office which operated the program used the old version of the poverty score for eligibility, but it seems unlikely. The most plausible explanation is that the score failed to clearly identify poverty and individuals were able to manipulate the score[18]. Manipulability of the score basically implies a violation of the exogeneity of the score for identification. The figures and the descriptive statistics presented above refer only to the sample of individuals in municipalities that were identified in the initial design as treatment and where the program started to operate from 2001[19]. Due to the inability of the second version of the poverty score to clearly identify poverty, households in municipalities which were originally control municipalities are excluded from this analysis.

4.5 Econometric framework

The Regression Discontinuity design takes advantage of the randomization given by the discontinuous change in the probability of program enrolment with some continuous variable (Z). The causal treatment effect on a potential outcome (Y_i) can be obtained by the difference between the outcome when exposed to the program $Y_i(1)$ and the outcome without exposure to the program $Y_i(0)$. The traditional problem in causal inference is that we do not observe both states at the same time. We only

[18] The government found evidence that manipulation of the score occurred, as a consequence they started a new redesign of the score in 2008 which was used in new municipalities from 2010.

[19] This new poverty index could be slightly problematic for identification if some initially ineligible households became eligible or eligible ones became ineligible in the treatment municipalities. However, we find that eligibility is consistent for 70% of the sample and the jump in eligibility is clear in figures 3, 4 and 5.

observe the outcome related to the treatment received by the individual. If we define $D_i \in \{0, 1\}$ where $(D_i = 0)$ denotes non-participation or $(D_i = 1)$ participation in the program, the outcome observed can be described as

$$Y_i = (1 - D_i)\, Y_i\,(0) + D_i Y_i\,(1) = \begin{cases} Y_i\,(0) & \text{if } D_i = 0 \\ Y_i\,(1) & \text{if } D_i = 1 \end{cases} \tag{4.1}$$

Additionally, we also observe pre-program covariates Z_i and X_i. The main characteristic of these covariates is that they are known and not affected by the program. The RD design relies on the principle that the probability of participation in the treatment changes discontinuously in the continuous covariate Z_i, which means that individuals lying just above a fixed threshold c are similar to those individuals that are just below, with the only difference being that the former do not participate in the program while the latter do. Z_i could be itself associated with the potential outcomes but this association is assumed to be smooth.

We can define the individual participation as[20]

$$D_i\,(Z_i) = I\,[Z_i \leq c] \tag{4.2}$$

In the case of complete compliance, the value of D_i is a deterministic function of Z_i and the probability of participation jumps from 0 to 1 at the threshold. If this is the case we could use a *sharp* RD design. But this is not our case. Figure 4 shows that participation in the program jumps from about 60% of households to above 90% of households at the poverty score cut-off. Initial compliance was around 90% but with the change in the poverty score around 35% of the households changed their

[20] In our specification the poverty score Z is allocated at the household level and not by individual level

eligibility status.

In the case of imperfect compliance -our case- a *fuzzy* RD design is used that allows for a smaller jump in the probability of assignment to the treatment at the threshold[21]

$$\lim_{z \downarrow c} Pr\left(D_i = 1 | Z_i = z\right) \neq \lim_{z \uparrow c} Pr\left(D_i = 1 | Z_i = z\right) \tag{4.3}$$

Where $\lim_{z \downarrow c} Pr\left(D_i = 1 | Z_i = z\right)$ is the limit of the probability of participation when the value of z gets closer to c from the right and $\lim_{z \uparrow c} Pr\left(D_i = 1 | Z_i = z\right)$ when z gets closer to c from the left. In this case, following Hahn, Todd, and Van der Klaauw (2001), the average causal effect of the treatment is obtained from the ratio of the discontinuity of the outcome to the discontinuity of the participation at the eligibility threshold.

$$\tau = \frac{\lim_{z \downarrow c} E\left(Y | Z = z\right) - \lim_{z \uparrow c} E\left(Y | Z = z\right)}{\lim_{z \downarrow c} E\left(D | Z = z\right) - \lim_{z \uparrow c} E\left(D | Z = z\right)} \tag{4.4}$$

Interpreting τ as the average treatment effect when participation near the threshold is random implies that the discontinuity in the conditional distribution of the outcome comes from the discontinuity in the conditional distribution in the participation. This estimator allows heterogeneous effects and then identifies a Local Average Treatment Effect (LATE) evaluated at the cut-off for eligibility. The interpretation of this ratio as a causal effect imposes monotonicity and excludability in the program participation function, as is mentioned in Imbens and Angrist (1994). As they suggest, a LATE should be estimated using a 2SLS. In this case, ignoring the

[21] A detailed explanation of RD design can be found in Imbens and Lemieux (2008) and Lee and Lemieux (2010).

effect of eligibility on participation and estimating an OLS equation of the outcome variable on the eligibility around the cut-off would generate a biased estimator of the true treatment effect due to omitted variables and self-selection into participation.

4.5.1 Estimation equation

Following Lemieux and Lee (2014) and Lee and Lemieux (2010), we estimate the treatment effect in our fuzzy RD design using an IV methodology in a 2SLS estimation. We use a flexible parametric model to estimate the effect of program participation D_i on time preferences or educational aspirations Y_i instrumenting D_i with a dummy for eligibility in the program E_i. The probability of treatment is defined by

$$Prob\,(D=1|Z=z)=\gamma+\delta E+g_D(z-c) \tag{4.5}$$

Where $E=1\,[Z\leq c]$ indicates the eligibility in the program as the poverty score is lower than the eligibility threshold. We centre the poverty score Z_i to zero and in that case c is equal to zero. The function $g_D(z-c)$ indicates a flexible polynomial regression function of order p that allows the slope to be different to the left (β_l) and the right (β_r) of the threshold. The equations are then defined by

$$Y_i=\gamma_1+\tau D_i+E_i\sum_{p=1}^{P}\beta_{lp}Z_i^p+(1-E_i)\sum_{p=1}^{P}\beta_{rp}Z_i^p+\varepsilon_{ih} \tag{4.6}$$

$$D_i=\gamma_2+\delta E_i+E_i\sum_{p=1}^{P}\beta_{lp}Z_i^p+(1-E_i)\sum_{p=1}^{P}\beta_{rp}Z_i^p+\upsilon_{ih} \tag{4.7}$$

The program causal effect is given by τ. We expect the program to reduce the time preferences through relaxation of budget constraints and/or the habit of delaying present consumption. That is, we expect τ to be negative (reduced discount). In contrast, we expect the program to increase caregivers' aspirations for the children's education due to the relaxation of the budget constraint, the habit of sending children to school and attendance at information sessions by the main caregiver. In this case, we expect τ to be positive.

We cluster the error terms in both equations at the household level to allow for heteroscedasticity and to account for the poverty score being calculated at the household level defining eligibility and participation at that level. Although monotonicity is itself not verifiable, it appears a sensible assumption. This implies that household participation in the program is a monotonically decreasing function of the poverty score, where for example households who choose to participate when they are ineligible would still choose to participate if they became eligible. Additionally, being at either side of the cut-off point does not have an impact on individuals' discount rates or education aspirations for the children except through participation in the program.

As the treatment effect is obtained by comparing conditional expectations of Y_i when approaching from the left and from the right to the cut-off, the correct specification of the regression function is important. We use a cross-validation procedure suggested by Lee and Lemieux (2010) based on the Akaike information criterion (AIC) for model selection. Assuming that the empirical specification is the true functional form of the underlying data, the LATE provides an efficient estimator of the treatment effect using individuals' different distances from the discontinuity threshold.

4.6 Results

This section presents the results in three parts. We first present the result of the first stage and the selection of the polynomial order. Then we present the estimated causal treatment effect of the second stage and finally some tests of the econometric specification. The analysis will be presented for the total sample and also disaggregated by urban and rural regions and for the educational aspirations by mother/father[22].

4.6.1 Effect of eligibility on program participation

In figure 4.4 we observed a discontinuity of about 35% in the probability of participation in *Familias en Acción* at the eligibility poverty score cut-off for the time preference sample and also the adults' aspirations sample. This jump is a necessary condition to be able to estimate the causal treatment effect on our outcomes of interest. Table 4.4 and table 4.5 show the size of the discontinuities in the probability of participation varying the polynomial order. This is the first stage δ value of eligibility on participation in equation (4.7). We use a cross-validation procedure suggested by Lee and Lemieux (2010) based on the Akaike information criterion (AIC) for model selection. The preferred specification is the one with the lowest AIC. In most cases the preferred specification has the same polynomial order at both sides of the cut-off; however we allow for flexibility in the model by also estimating different polynomial orders at each side of the cut-off and present these results if that is the preferred specification. For example, in table 4.4 we found that the preferred specification for the aggregated sample is a first order polynomial at both sides of the cut-off. This

[22] This is for the sample of parents that we could match with the characteristics of the child for whom the aspirations were reported.

result suggests that program eligibility is associated with an increase of 47% in the probability of participation in *Familias en Acción*. For those living in rural or urban areas the probability is 44% and 51% respectively[23]. The parameter is in all cases strongly significant.

These magnitudes are comparable to the ones estimated for the educational aspirations sample. The best fit for the first stage of caregivers' aspirational years of education for their children is mostly a first order polynomial except for the aggregated sample, where a quadratic functional form is preferred. For mothers, we found that the best fit is a first order polynomial for the sample that is on the left of the cut-off and a second order polynomial for the sample on the right. When looking at the first stage for the other outcomes of interest the results are very similar.

We present the first stage estimates for different polynomial order specifications and even though the size of the effect decreases as the polynomial order increases; it is still similar and significant at the 1% significance level. This gives confidence in the strength of the first stage and the power of the eligibility at the cut-off point as an instrument.

4.6.2 Effect of program participation on Time preferences and Educational aspirations

Using the preferred polynomial order, Figures 4.7 - 4.9 present the discontinuity in the expected value of time preferences and education expectations at the threshold for eligibility. Using a binwidth of 0.05, the estimated line of the conditional expectation helps us to visualize the identification strategy. The time preferences

[23] This is a weighted probability (weighted by the distance from the threshold) and so differs from the unweighted probability shown in the figures.

representation suggests two things. The first and most important one is that there is no evident jump in the discount rate from the left to the right of the threshold for eligibility. This suggests no program effect on time preferences. The second is that we expected the slope of the line to be negative as it is suggested in the literature that wealth and income are related to lower time preferences. It seems that in the overall sample we find this pattern but to the left of the eligibility threshold the line has a slightly positive slope.

For educational aspirations, we expect that as the poverty score increases (less poor) the aspiration increases, giving a positive slope. We do see a jump at the threshold for the years of education the caregivers aspire the child to attend and the probability of completing higher education; however these jumps are in the opposite of the anticipated direction. This result, if significant, would suggest that program participation is having a negative effect on those aspirations.

The IV (2SLS) estimation of the causal effect parameters of program participation on time preferences and education expectations is presented in Table 4.6. For time preferences, a negative sign on the coefficient suggests a decrease in the time preferences (more patient individuals) due to participation in the program; our results show that this is the case for the whole sample both in rural and urban areas. However, none of these coefficients are statistically significant. The hypothesis is that a sustained increase in income as well as the habit of delaying present consumption (act of investing in children's health and education) could potentially change individuals' underlying preferences and reduce their impatience. The biggest reduction of around 0.04 in the discount factor was found among urban households (a 6.25% decrease in impatience) but this is insignificant. These findings are consistent with the null effect for this sample found in figure 4.7. This suggests no program participation effect on time preferences. In figures 4.8 and 4.9 we present the continuity/discontinuity in urban and rural areas.

The results for caregivers' aspirations for their child's education are more surprising. A common assumption is that children do not go to school due to monetary constraints or because long term investments (e.g. child education) are not highly valued. The program seems to relax budget constraints, incentivise attendance at school among children as a result of the conditionality, and provide information to the main caregiver about the importance of education (Attanasio, Fitzsimons, & Gomez, 2005; Attanasio & Mesnard, 2006). Our hypothesis is that those factors may in turn lead to increased educational aspirations for children, especially among mothers given that they are the ones receiving the transfer and attending educational talks. We however find very little effect on educational aspirations. There is no significant effect across the whole sample. We find a marginally significant negative effect among individuals living in urban areas and for fathers (expecting around 1 year less, and significant at the 10% level).

Program participation is found to not have a significant effect on the reported probability of finishing secondary school by the age of 18. Most of the coefficients are negative and in all cases the standard deviation is large. The effect in rural areas is the only case where the coefficient on program participation is positive but it is also insignificant.

The only significant program effect is for the probability of finishing higher education, however the coefficient in this case is negative. This suggests that participating in the program is associated with a 14.3 pp decrease in the perceived probability of finishing higher education. While non-participating caregivers on average attach a 76% probability to their children completing higher education, participating caregivers on average report a probability of 62%. We find a larger negative effect for urban areas, females and women significant at 1%. This result is consistent with the lower expected years of education found before.

The program effect on caregivers' expectations for their children's education was to lower the probability of children completing tertiary education. This is the opposite of what was expected. This could be because they come to see their children's schooling attainment as being associated with program support, but when this is no longer available (after secondary schooling) they attach a lower probability to being able to continue than those households who are not exposed to the program. In addition to this, if children were behind in school progression when they started to participate in the program (were old for their school year); parents might reason that they would be unable to complete high school while eligible for participation (under the age of 17). Similarly, this reasoning would reduce the expected probability of graduating from tertiary education.

In summary, we find no evidence that time preferences change with exposure to the program. We also find no effect on the aspirations of parents or caregivers for their children's education measured by years of schooling and the probability of finishing secondary school. We find, however, a negative program effect on the probability that children will finish higher education. Thus it appears that the program effect depends largely on the ongoing receipt of the cash transfer and associated constraints.

4.6.3 Specification Tests

Polynomial order

The accuracy of the estimation in the RD design is based on the correct polynomial order specification of the true functional form of the underlying data used. As a sensitivity test, we evaluate our results using the second-best specification according to the AIC[24]. Table 4.7 shows these results. For time preferences we find larger coefficients but they are still insignificant. The overall conclusion remains for expected years of education and probability of finishing tertiary education, although the parameters seem to be less precisely estimated using a higher order polynomial order.

Covariates

The inclusion of additional covariates besides the poverty score in the RD design estimation can be used to eliminate sample biases present in the basic specification, help to establish the validity of the RD design and improve precision (Imbens & Lemieux, 2008; Lee & Lemieux, 2010). A general challenge for the probabilities of finishing school levels is that standard errors are very high. For those variables including covariates in the estimations, may be particularly useful. We include as covariates the gender, age, years of education and the region (rural or urban) of the respondent. Table 4.8 presents the results for each outcome and the coefficient of the first stage of eligibility on participation. We find that the first stage in all the cases continues to be as robust as before.

[24] We additionally calculate the BIC selection criteria. As both criteria are sensitive to the number of observations and the number of parameters estimated, the AIC tends to select models with fewer parameters while BIC tends to select models with more parameters. In our case we find that in 60% of the cases they selected the same best-fit polynomial order.

In the time preference estimations, we do not find that age or education are associated with time preferences. This may be a consequence of the low variation in the sample as the population is poor, education is low and the age of household heads/spouses is similar. Some studies suggest that low wealth, low education and unemployment are related to high time preferences (Bauer & Chytilová, 2010; Kirby et al., 2002). We find individuals living in urban zones exhibit less patience while females are consistently more patient as is suggested in the literature. Across the different subsamples, the initial negative non-significant result on time preferences holds. If time preferences are a stable parameter, then this is not a surprising result. Dean & Sautmann, (2014) show that time preferences look to be relatively invariant to changes in income, education and labour status; while they are more responsive to adverse shock events.

When looking at caregivers' aspirations for their children we find, as expected, that the role of parents' own years of education seems to be very important in the three indicators reported. For example, one additional year of a mother's education increases the expected probability of their children finishing secondary and tertiary education by 1.7 and 2.4 percentage points respectively. These magnitudes are smaller for fathers but still positive and significant. The aspirations are on average higher when looking at the urban sample. The coefficient is particularly high for the probability of finishing higher education where it is more likely that that level is offered. Finally, the age and gender of the respondent are not consistently a predictor. The significant negative effect on the aspirations of children's years of education for fathers and individuals living in urban areas disappears once we control for reporters' characteristics. For the probability of finishing tertiary education, we find that the program effect is of a smaller magnitude but still negative and statistically significant for exactly the same sub-samples. We do not find any effect on the probability of finishing secondary school but in this case all the coefficients are positive.

In table 4.9 we present the estimation of the basic model but now including covariates of children's characteristics like gender, school progression, age of enrolment and attendance at nursery for the sample of parents[25]. Being a female or currently attending school are children's characteristics positively associated with parents' aspirations, while the number of grades repeated is negatively related[26]. Overall, higher school performance is associated with higher parents' aspirations.

4.7 Internal validity and Robustness Test

The validity of the RD design is based on the assumption that eligibility in the program is determined exogenously (Imbens & Lemieux, 2008). In our case this is given by the exogeneity of the poverty index and the threshold for eligibility. This implies that individuals to the left of the cut-off for eligibility are similar to individuals to the right and also that the poverty index is continuous at the cut-off.

This section first presents a visual test of the continuity of the poverty index at the cut-off following the procedure proposed by McCrary (2008). Second, we present some evidence of local balance of baseline variables on both sides of the cut-off. We test internal validity at the household level as the poverty score and participation was defined at that level. We also disaggregate by rural and urban areas as the cut-off point for eligibility was different as explained in the data section. Finally, it is

[25] This sample contains only mothers or fathers reporting information on educational aspirations for their child.

[26] As program participation is defined by present or past participation, not all the households classified as participating have children attending school. We however find that from the households that are not currently participating, around 50% are not because they did not meet the attendance condition.

possible that time preferences are affected by the receipt of the cash transfer during the period in which the transfer is received, but revert to the original preferences once the cash transfer is no longer received. To examine this, we present a robustness test where we compare the results for time preferences for those currently receiving the cash transfer with those who had received the cash transfer in the past.

4.7.1 Continuity in the poverty score

If individuals or households are able to manipulate the poverty score they receive in order to become eligible to participate in the program, the RD design will not be valid as the assignment would not be locally random. We first test using a visual inspection of the continuity on the density of the poverty score and second, we treat the frequency counts as a dependent variable in a local linear regression. Continuity in the poverty score at the threshold is evidence of absence of manipulation. For the first test, we divide the poverty score into equally spaced bins and calculate the frequency in each of them. Figure 4.10 shows the density of the poverty score for the time preferences and education aspirations samples. While the density is continuous at the threshold for eligibility in the time preferences sample, indicating no manipulability in the poverty classification, there is a small discontinuity in the education aspirations sample. In Figure 4.11 we present the density for this sample disaggregating by rural and urban region. We find that there is continuity in the density distribution in rural areas but there is some evidence of discontinuity in urban areas.

To test if there is continuity in the frequency counts at the threshold, we also present the estimation of a local linear regression[27] in Table 4.10, as suggested by McCrary

[27] We estimate an OLS model using the counts in bins of size 0.05 from -1.5 to 1.5 in the poverty score.

(2008). We find that the difference in the number of respondents is not statistically different above and below the cut-off for eligibility in the aggregate or in rural or urban areas. However, we find that on average there are two additional counts in the bins that are closer to the cut-off on the left (eligible households) in the sample of education aspirations for the aggregate. This seems to be driven by households living in urban areas.

This finding could however be consistent with a household composition change rather than manipulation of the poverty score. While all households reported information on time preferences, education aspiration questions were only asked of households who had at least one child aged 6 to 17. More households qualifying to report information on aspirations may reflect that children stay at home longer in participating households as a result of the program. To examine this further, in Table 4.11 panel A we present some results on household composition. We find no difference in the number of caregivers or the proportion of females in the household (potential caregivers). We however find some difference in the household composition that determines the number of households who answer the educational aspiration questions. Those questions were answered by all the caregivers in households with children 6 to 17 and referring to the oldest child in that age interval. We find that the number of children 6 to 17 is higher in the aggregated sample while not statistically significant when disaggregated by rural and urban area. When looking at the number of children aged 12 to 17 which are the most likely group to contain the child for whom the information is provided, we find that participant households in urban areas have on average 0.5 more children than non-participant ones. This difference is significant at the 10% level. Other group ages seem to be similar. This finding is consistent with the fact that there is a discontinuity in the counts density function in urban areas. If participant households in urban areas are more likely to have children 12 to 17 then we expect to have more households to the left of the eligibility cut-off. Thus, those differences in counts do not come from manipulation of the poverty score but from changes in demographic composition resulting from the program. It seems that older children of eligible households are deciding to remain in the households to guarantee households' eligibility for the program. This result is in line with the program aim to increase school enrolment and attendance of older children, who are usually at risk of dropping out of school.

In summary, given that when for the time preferences sample (all households) we find the distribution is continuous, we are confident that manipulation was not occurring.

4.7.2 Baseline covariates

An important assumption in order to identify the program effect is that individuals to the left and right of the cut-off were identical before the program started and that participation in the program was random around the poverty score cut-off. To empirically test this we would expect to find no discontinuities at the cut-off for covariates, except program eligibility, before the program started. Unfortunately, we do not have information at the baseline for individuals above the cut-off.

Comparing covariates at the end line could simply reflect program effect on the covariates. We examine the local balance on either side of the threshold for eligibility using variables that we consider unlikely to have been affected by the program. We use age and level of education of the household head.

If the RD design is valid, we should find that characteristics of the household head are continuous at the threshold. We present these results in Table 4.11 panel B. We find no significant differences in age or education[28] for the household heads of participant and non-participant households living in urban and rural areas. When looking at the aggregate result, the coefficient on education is different than zero but this is mainly driven by educational differences between rural and urban household's heads. This means that we need to control for differences in education when analysing the aggregated sample. These results were presented in the covariates section. We interpret this as evidence of RD design validity, as households at the threshold were similar at the baseline over those characteristics.

4.7.3 Time preferences and current income transfer

The final robustness test we conduct examines whether time preferences are affected by the current receipt of the conditional cash transfer. As eligibility is defined by the poverty score, the treatment group contains households who were participating in the program at the time of the interview and also those who had participated in the past. Additional income diminishes the pressure for present consumption and allows households to delay consumption and plan better (Becker & Mulligan, 1997; L. S. Carvalho, 2010; Kirby et al., 2002; Tanaka et al., 2006). If this is the most important component, disaggregating the sample, we may find an effect on current participants' time preferences. But this does not apply in households that participated in the program in the past but are no longer receiving the extra income.

We separate the sample into two subsamples of households who were participating at the time of the interview (current participants) and households who had participated in the program but are no longer participating because there are no longer children aged 0 to 17 in the household (past participants) and estimate the coefficients again

[28] Adults in this population are very unlikely to reenrol at school. As shown, the probability of being enrolled at school by age 21 is very small.

for each sample. If we find an effect on time preferences for the current participants, it may suggest that the relaxation of the budget constraint is the main mechanism to affect the discount rate for this sample. The results are presented in table 4.12. The effects are insignificant in both samples. This suggests that even current receipt of the transfer is not affecting time preferences.

4.8 Conclusions

We conclude that the program does not change participants' time preferences and we find no positive effects of the program on caregivers' aspirations for their children's education. However, we systematically find that participating parents attach a lower probability to their children finishing higher education. These results suggest that participant households' motivation for sending their children to school is related to the monetary transfer they received rather than to changes in preferences and aspirations. Observed changes in behaviour and choices are thus a consequence of the incentive and conditions.

One possible conclusion is that time preferences and aspirations are largely fixed. Although there is growing evidence that preferences do respond to negative shocks (Cameron & Shah, Forthcoming; Dean & Sautmann, 2014), there is little evidence on the effect of positive shocks.

If the program changes the preferences and hopes of participant households, from a public policy perspective this would mean that temporary exposure to the program would enhance future human development investment in children. However, we do not find evidence that participants' preferences or hopes of educational attainment for their children increase. This suggests that if the program were to stop or the budget run out, the program is unlikely to have long term effects on parents' investment in the educational attainment and health of the household. This however, does not imply that the program is not effective overall as it has been shown in Attanasio, Battistin, et al. (2005), Attanasio et al. (2010), Baez and Camacho (2011) and Attanasio, Battistin, and Mesnard (2012) to improve children's health status and educational attendance. It is possible that these increases in education may increase children's own time preferences and increase their own educational aspirations even if their parents' preferences remain the same. This is an area for future research.

4.9 Tables

Table 4.1: Hypothetical time preference questions

	Today	Later	Period	discount rate	Time preference
1	100000	105000	1 month	0.05	high (most patient)
2	100000	120000	1 month	0.2	
3	100000	150000	1 month	0.5	
4	100000	200000	1 month	1	low (most impatient)

Table 4.2: Frequency of Interval of discount rate in percentages and adults' aspirations for child's education

	Total	Rural	Urban
Panel A: Discount rate Interval[a]			
0 - 0.05	0.12	0.13	0.11
0.05 - 0.2	0.11	0.12	0.1
0.2 - 0.5	0.19	0.19	0.19
0.5 - 1	0.18	0.17	0.19
1 - ∞	0.4	0.39	0.41
N	3065	1603	1462
Non-eligible Mean	0.62	0.60	0.64
Panel B: Caregivers' Aspirations for the children[b]			
Aspired years of education	13.47	13.11	13.86
N	4095	2132	1963
Non-eligible Mean	13.95	13.54	14.5
Probability of finishing secondary school	81.44	79.95	83.03
N	3945	2036	1909
Non-eligible Mean	84.5	82.11	87.74
Probability of finishing higher education	64.81	60.53	69.36
N	3877	2000	1877
Non-eligible Mean	69.11	64	76.13
Panel C: Mothers' Aspirations for their children			
Aspired years of education	13.42	13.1	13.77
N	2548	1339	1209
Non-eligible Mean	13.36	12.8	13.94
Probability of finishing secondary school	80.97	79.35	82.73
N	2460	1283	1177
Non-eligible Mean	81.5	80.05	82.94
Probability of finishing higher education	64.84	61.02	69.01
N	2417	1262	1155
Non-eligible Mean	61.54	56.34	66.87
Panel D: Fathers' Aspirations for their children			
Aspired years of education	13.52	13.05	14.06
N	1089	582	507
Non-eligible Mean	13.25	12.98	13.58
Probability of finishing secondary school	82.52	80.87	84.37
N	1046	552	494
Non-eligible Mean	79.91	78.4	81.46
Probability of finishing higher education	63.6	58.55	69.21
N	1029	542	487
Non-eligible Mean	63.51	59.75	67.38

[a] Results from hypothetical questions asked to one member of the household.
[b] Caregivers include any adult of the household including older siblings.
Notes: In around 90% of the cases the caregivers are the parents.

Table 4.3: Sample by eligibility criteria and variables of interest

	Score around the eligibility cut-off	
	above	below
Discount rate	890	2175
Expected years of education	907	3188
Probability of finishing secondary school	877	3068
Probability of finishing higher education	864	3013

Notes: Above include all individuals who have a poverty score above the cut-off point in the interval (cut-off value, ≈+5]. Below the cut-off includes individuals who have a poverty score in the interval [0, cut-off value]. For both cases, the counts are of individuals in rural or urban areas that reported the variable of interest.

Table 4.4: Discontinuity estimates of program participation at poverty score eligibility cut-off for Time Preferences sample

Polynomial order on both sides of the eligibility cut-off	All	Rural	Urban
1	0.4732*** [a]	0.4438*** [a]	0.5082*** [a]
	(0.0219)	(0.0296)	(0.0338)
2	0.4526***	0.4256***	0.4892***
	(0.0310)	(0.0424)	(0.0488)
3	0.3973***	0.3756***	0.4307***
	(0.0383)	(0.0556)	(0.0603)
4	0.3380***	0.3349***	0.3529***
	(0.0451)	(0.0663)	(0.0714)
5	0.3212***	0.2937***	0.3324***
	(0.0511)	(0.0769)	(0.0827)
Number of observations	3,065	1,603	1,462

Notes: The estimates come from the first stage estimation of the IV estimation. We calculated increasing polynomial level to the left and right by a unit, same polynomial order at both sides are presented. Household clustered standard errors in parentheses.
[a] This is the preferred specification of the polynomial order based on the AIC criterion.
Significance: *** p< 0.01, ** p< 0.05, * p< 0.1.

Table 4.5: Discontinuity estimates of program participation at poverty score eligibility cut-off for educational aspirations sample

Polynomial order on both sides of the eligibility cut-off	All	Rural	Urban	Mothers	Fathers
Panel A: Years of education					
1	0.4278***	0.4296*** [a]	0.4393*** [a]	0.4173***	0.4886*** [a]
	(0.0249)	(0.0348)	(0.0375)	(0.0260)	(0.0446)
1 left, 2 right	0.4233***	0.4047***	0.4324***	0.4151*** [a]	0.4989***
	(0.0257)	(0.0366)	(0.0388)	(0.0270)	(0.0452)
2	0.4378*** [a]	0.4322***	0.4412***	0.4239***	0.5123***
	(0.0332)	(0.0475)	(0.0500)	(0.0347)	(0.0593)
3	0.4048***	0.4430***	0.3853***	0.3935***	0.4471***
	(0.0420)	(0.0618)	(0.0651)	(0.0436)	(0.0783)
4	0.3706***	0.4149***	0.3087***	0.3498***	0.4155***
	(0.0504)	(0.0708)	(0.0804)	(0.0517)	(0.0938)
5	0.3641***	0.3851***	0.3052***	0.3443***	0.4303***
	(0.0564)	(0.0805)	(0.0942)	(0.0574)	(0.1124)
Number of observations	4,095	2,132	1,963	2,548	1,089
Panel B: Probability of finishing secondary school					
1	0.4302*** [a]	0.4256*** [a]	0.4483***	0.4169*** [a]	0.4926*** [a]
	(0.0251)	(0.0349)	(0.0378)	(0.0263)	(0.0453)
2	0.4446***	0.4291***	0.4607*** [a]	0.4280***	0.5186***
	(0.0333)	(0.0478)	(0.0498)	(0.0351)	(0.0601)
3	0.4145***	0.4367***	0.4131***	0.4003***	0.4550***
	(0.0422)	(0.0625)	(0.0649)	(0.0442)	(0.0798)
4	0.3822***	0.4163***	0.3400***	0.3588***	0.4281***
	(0.0507)	(0.0721)	(0.0803)	(0.0527)	(0.0958)
5	0.3768***	0.3965***	0.3336***	0.3553***	0.4543***
	(0.0569)	(0.0819)	(0.0938)	(0.0585)	(0.1151)
Number of observations	3,945	2,036	1,909	2,460	1,046
Panel C: Probability of finishing higher education					
1	0.4312***	0.4253***	0.4514*** [a]	0.4177*** [a]	0.4976***
	(0.0252)	(0.0350)	(0.0381)	(0.0265)	(0.0454)
2	0.4444*** [a]	0.4277*** [a]	0.4628***	0.4289***	0.5200*** [a]
	(0.0335)	(0.0479)	(0.0504)	(0.0354)	(0.0608)
3	0.4140***	0.4355***	0.4153***	0.4026***	0.4545***
	(0.0426)	(0.0630)	(0.0660)	(0.0448)	(0.0803)
4	0.3839***	0.4135***	0.3505***	0.3651***	0.4322***
	(0.0511)	(0.0727)	(0.0808)	(0.0532)	(0.0964)
5	0.3815***	0.3965***	0.3496***	0.3649***	0.4573***
	(0.0573)	(0.0827)	(0.0943)	(0.0592)	(0.1152)
Number of observations	3,877	2,000	1,877	2,417	1,029

Notes: The estimates come from the first stage estimation of the IV estimation. We calculated increasing polynomial level to the left and right by a unit, same polynomial order at both sides are presented. Household clustered standard errors in parentheses.
[a] This is the preferred specification of the polynomial order based on the AIC criterion.
Significance: *** p< 0.01, ** p< 0.05, * p< 0.1.

Table 4.6: Effect of Program Participation on Time Preferences and Education Aspirations

	All	Rural	Urban	Mothers	Fathers
Time preferences					
Discount rate	-0.0148	-0.0252	-0.0355	NA	NA
	(0.0450)	(0.0705)	(0.0579)	NA	NA
Number of observations	3,065	1,603	1,462		
Education Aspirations					
Years of education	-0.5775	-0.5272	-0.9701*	-0.5172	-1.1329*
	(0.5828)	(0.6415)	(0.5554)	(0.5615)	(0.6850)
Number of observations	4,095	2,132	1,963	2,548	1,089
Probability of finishing secondary school	-2.9927	4.1882	-9.9530	-5.3741	-2.2762
	(3.8790)	(5.4902)	(6.2391)	(4.7487)	(5.9548)
Number of observations	3,945	2,036	1,909	2,460	1,046
Probability of finishing higher education	-14.2513**	-11.1006	-17.8300***	-17.1438***	-11.1198
	(6.2775)	(9.9204)	(6.1064)	(5.6964)	(9.1037)
Number of observations	3,877	2,000	1,877	2,417	1,029

Notes: This table includes the second stage of the IV estimation that gives the values of τ that is interpreted as a LATE estimator. The estimations include the number of polynomials at each side of the threshold found to be of best fit using the AIC criterion.The estimations do not include covariates. Household clustered standard errors in parentheses.
Significance: *** $p< 0.01$, ** $p< 0.05$, * $p< 0.1$.

Table 4.7: Program effect on Time Preferences and Education Aspirations in the second-best polynomial order

	All	Rural	Urban	Mothers	Fathers
Time preferences					
Discount rate	-0.0676	-0.0984	-0.0722	NA	NA
	(0.0642)	(0.1008)	(0.0849)	NA	NA
Eligibility on participation coefficient	0.4526***	0.4256***	0.4892***		
	(0.0310)	(0.0424)	(0.0488)		
Education Aspirations					
Years of education	-0.9167**	0.1665	-1.2369	-0.1784	-0.9244
	(0.4321)	(0.9231)	(0.9756)	(0.6590)	(0.9034)
Eligibility on participation coefficient	0.4278***	0.4322***	0.3853***	0.4239***	0.5123***
	(0.0249)	(0.0475)	(0.0651)	(0.0347)	(0.0593)
Probability of finishing secondary school	3.2808	1.5086	-7.1427	-0.1473	4.0670
	(5.0833)	(7.8978)	(5.3911)	(6.0377)	(7.4791)
Eligibility on participation coefficient	0.4446***	0.4291***	0.4483***	0.4280***	0.5186***
	(0.0333)	(0.0478)	(0.0378)	(0.0351)	(0.0601)
Probability of finishing higher education	-16.3150***	-17.2148**	-22.2868***	-19.6008***	-23.8279***
	(4.9642)	(7.8315)	(8.1820)	(7.4526)	(8.0233)
Eligibility on participation coefficient	0.4312***	0.4253***	0.4628***	0.4289***	0.4976***
	(0.0252)	(0.0350)	(0.0504)	(0.0354)	(0.0454)

Notes: This table includes the second stage of the IV estimation that gives the values of τ that is interpreted as a LATE estimator. The eligibility on participation is the first-stage parameter δ. The estimations include the number of polynomials at each side of the threshold found to be the second best fit using the AIC criterion.The estimations do not include covariates. Household clustered standard errors in parentheses.
Significance: *** p< 0.01, ** p< 0.05, * p< 0.1.

Table 4.8: Program effect on Time Preferences and Education Aspirations including covariates

	All	Rural	Urban	Mothers	Fathers
Time preferences					
Discount rate	-0.0136	-0.0234	-0.0620	NA	NA
	(0.0462)	(0.0707)	(0.0595)		
Respondant Characteristics					
Female	-0.0535***	-0.0436**	-0.0674***		
	(0.0137)	(0.0198)	(0.0190)		
Age	0.0010*	0.0003	0.0019**		
	(0.0006)	(0.0009)	(0.0008)		
Years of education	-0.0023	-0.0017	-0.0031		
	(0.0021)	(0.0032)	(0.0028)		
Lives in urban area	0.0512***				
	(0.0150)				
Eligibility on participation coefficient	0.4610***	0.4403***	0.4979***		
	(0.0220)	(0.0294)	(0.0341)		
Education Aspirations					
Years of education	-0.6460	-0.1418	-0.6877	-0.0551	-0.7977
	(0.5558)	(0.6174)	(0.5611)	(0.5595)	(0.6372)
Respondant Characteristics					
Female	-0.1071	0.0796	-0.3406***		
	(0.0959)	(0.1388)	(0.1292)		
Age	0.0098**	0.0101	0.0101*	0.0133*	-0.0090
	(0.0045)	(0.0070)	(0.0057)	(0.0069)	(0.0087)
Years of education	0.2131***	0.2647***	0.1755***	0.2473***	0.1864***
	(0.0146)	(0.0243)	(0.0181)	(0.0178)	(0.0276)
Lives in urban area	0.5817***			0.3906***	0.9014***
	(0.1146)			(0.1310)	(0.1948)
Eligibility on participation coefficient	0.4357***	0.4237***	0.4331***	0.4041***	0.4792***
	(0.0329)	(0.0347)	(0.0371)	(0.0268)	(0.0444)
Probability of finishing secondary school	1.5017	8.6559	0.8432	0.3034	1.2606
	(3.7947)	(5.4832)	(7.2694)	(4.7463)	(5.6751)
Respondant Characteristics					
Female	-1.9052**	-2.5225*	-1.5377		
	(0.9232)	(1.3298)	(1.2702)		
Age	-0.1012**	-0.1450**	-0.0625	-0.1235**	-0.1407*
	(0.0418)	(0.0623)	(0.0571)	(0.0608)	(0.0774)

Continued ...

Table 4.8 (continued)

	All	Rural	Urban	Mothers	Fathers
Years of education	1.5463***	1.9360***	1.2694***	1.6502***	1.6119***
	(0.1394)	(0.2172)	(0.1877)	(0.1759)	(0.2328)
Lives in urban area	1.9892*			1.7962	2.0508
	(1.1049)			(1.3231)	(1.8814)
Eligibility on participation coefficient	0.4226***	0.4197***	0.4580***	0.4057***	0.4826***
	(0.0252)	(0.0349)	(0.0493)	(0.0262)	(0.0453)
Probability of finishing higher education	-14.7113**	-7.5763	-14.6267**	-10.8208*	-15.6070*
	(6.0615)	(9.8267)	(6.1037)	(5.6790)	(9.0722)
Respondant Characteristics					
Female	1.2341	2.8796*	-0.9737		
	(1.0951)	(1.5884)	(1.5099)		
Age	0.1181**	0.1496*	0.0813	0.1833**	0.0604
	(0.0512)	(0.0785)	(0.0680)	(0.0749)	(0.1027)
Years of education	2.0806***	2.5916***	1.7054***	2.3844***	1.7645***
	(0.1633)	(0.2554)	(0.2104)	(0.1972)	(0.2979)
Lives in urban area	6.2663***			3.9219**	8.7898***
	(1.3234)			(1.5396)	(2.3086)
Eligibility on participation coefficient	0.4423***	0.4224***	0.4453***	0.4066***	0.5172***
	(0.0333)	(0.0476)	(0.0378)	(0.0264)	(0.0600)

Notes: This table includes the second stage of the IV estimation that gives the values of τ that is interpreted as a LATE estimator. The eligibility on participation is the first-stage parameter δ. The estimations include the number of polynomials at each side of the threshold found to be of best fit using the AIC criterion.The estimations do not include covariates. Household clustered standard errors in parentheses.
Significance: *** p< 0.01, ** p< 0.05, * p< 0.1.

Table 4.9: Program effect on Parents' Education Aspirations for their children including child covariates

Variables	Years of education	Probability of finishing	
		secondary school	higher education
		Mothers	
Participation on the outcome	-0.4262	-0.1631	-13.2668*
	(0.6287)	(5.8036)	(7.1347)
Child characteristics			
Female	0.4585***	4.0013***	3.9118**
	(0.1290)	(1.3171)	(1.6008)
Attended nursery	0.4476***	2.0438	2.2148
	(0.1307)	(1.3321)	(1.6573)
Attending school	2.5200***	23.1262***	20.9174***
	(0.2365)	(2.3208)	(2.4456)
Age of enrolment	-0.2633***	-3.5389***	-3.7532***
	(0.0831)	(0.8375)	(1.0135)
Number of grades repeated	-0.5482***	-8.5456***	-4.6706***
	(0.0802)	(0.8868)	(0.9611)
Number of years defered	-0.4643***	-8.2217***	-4.3576**
	(0.1513)	(1.5679)	(1.7024)
Eligibility on participation	0.3470***	0.3522***	0.3546***
	(0.0284)	(0.0277)	(0.0280)
Number of observations	2,033	1,984	1,948
		Fathers	
Participation on the outcome	-0.5317	5.6562	-10.3391
	(0.6554)	(7.1687)	(10.1928)
Child characteristics			
Female	0.6221***	3.7681*	5.2912**
	(0.1940)	(1.9760)	(2.4206)
Attended nursery	0.4970**	-1.4640	2.3291
	(0.2061)	(2.0904)	(2.5319)
Attending school	2.4229***	18.7497***	17.9113***
	(0.3936)	(3.6315)	(3.8134)
Age of enrolment	-0.1581	-1.4058	-0.4957
	(0.0994)	(1.2227)	(1.2852)
Number of grades repeated	-0.7721***	-8.0049***	-6.8096***
	(0.1318)	(1.3471)	(1.4331)
Number of years defered	-0.2900	-9.5479***	-5.0341*
	(0.2074)	(2.9146)	(2.8128)
Eligibility on participation	0.4732***	0.4789***	0.5132***
	(0.0499)	(0.0504)	(0.0655)
Number of observations	855	826	813

Notes: This table includes the second stage of the IV estimation that gives the values of τ that is interpreted as a LATE estimator. The eligibility on participation is the first-stage parameter δ. The estimations include the number of polynomials at each side of the threshold found to be the second best fit using the AIC criterion.The estimations do not include covariates. Household clustered standard errors in parentheses.
Significance: *** p< 0.01, ** p< 0.05, * p< 0.1.

Table 4.10: Local linear regression of the poverty score counts at the cut-off for eligibility

	Time Preferences			**Education Expectations**		
	All	Urban	Rural	All	Urban	Rural
Eligibility	-0.0667	0.7333	-0.8000	2.0333**	1.6333***	0.4000
	(0.9491)	(0.5465)	(0.7061)	(0.8961)	(0.5505)	(0.5777)
Constant	8.7667***	3.2667***	5.5000***	6.9000***	2.5333***	4.3667***
	(0.6711)	(0.3865)	(0.4993)	(0.6336)	(0.3892)	(0.4085)
Observations	60	60	60	60	60	60
R-squared	0.0001	0.0301	0.0217	0.0815	0.1318	0.0082

Notes: We estimate an OLS model using the counts in bins of 0.05 from -1.5 to 1.5 in the poverty score. Standard errors in parentheses.
Significance: *** p< 0.01, ** p< 0.05, * p< 0.1.

Table 4.11: Discontinuities on the baseline covariates at the poverty score cut-off for eligibility

	All	Rural	Urban
Panel A: Households Composition			
Number of children 6 to 17	0.5466***	0.3080	0.4635
	(0.1821)	(0.3298)	(1.1153)
Number of children 0 to 6	-0.2793	-0.3117	-0.2552
	(0.2089)	(0.2206)	(0.5640)
Number of children 7 to 11	0.2261	0.2022	0.2232
	(0.3141)	(0.3151)	(0.4700)
Number of children 12 to 17	0.3616***	0.2774	0.4789*
	(0.1271)	(0.1863)	(0.2647)
Number of females in the household	0.0789	0.2339	-0.0373
	(0.2292)	(0.3606)	(0.3715)
Number of people in the household	0.6219*	0.1371	0.5760
	(0.3230)	(0.5655)	(0.8916)
Number of caregivers	0.1325	0.1497	0.2521
	(0.0858)	(0.2654)	(0.2371)
Panel B: Households Head Characteristics			
Age of the household head	1.9794	3.1518	1.8091
	(1.5627)	(2.8880)	(2.0944)
Years of education of the household head	-1.0873**	-0.5908	-0.5743
	(0.4777)	(0.5703)	(0.8868)

Notes: This table includes the second stage of the IV estimation that gives the values of τ that is interpreted as a LATE estimator. The estimations include the number of polynomials at each side of the threshold found to be of best fit using the AIC criterion.The estimations do not include covariates. Household clustered standard errors in parentheses.
Significance: *** p< 0.01, ** p< 0.05, * p< 0.1.

Table 4.12: Effect of Program Participation on Time Preferences for current and past eligible households

	Current Participants			**Past Participants[1]**		
	All	Urban	Rural	All	Urban	Rural
Program effect	-0.0215	-0.0573	0.0014	-0.0708	-0.0703	-0.0173
	(0.1400)	(0.1430)	(0.1540)	(0.2506)	(0.2955)	(0.0839)
Discount rate for non-participants	0.7177***	0.6731***	0.7507***	0.7712***	0.7395***	0.6963***
	(0.0937)	(0.0827)	(0.1119)	(0.1066)	(0.0840)	(0.0327)
Eligibility on participation coefficient	0.4319***	0.4679***	0.4567***	0.3370***	0.3286**	0.6089***
	(0.0664)	(0.0682)	(0.0843)	(0.1137)	(0.1348)	(0.0675)
Number of observations	1,993	1,035	958	788	376	412

Notes: This table includes the second stage of the IV estimation that gives the values of τ that is interpreted as a LATE estimator. The eligibility on participation is the first-stage parameter δ. The estimations include the number of polynomials at each side of the threshold found to be of best fit using the AIC criterion.The estimations do not include covariates. Household clustered standard errors in parentheses.
[1] Participating in the past but no longer eligible because there are no longer children aged 0 to 17 in the household.
Significance: *** p< 0.01, ** p< 0.05, * p< 0.1.

4.10 Figures

Figure 4.1: Histogram of the sample discount rate interval

Figure 4.2: Education level distribution at baseline by age cohort and region

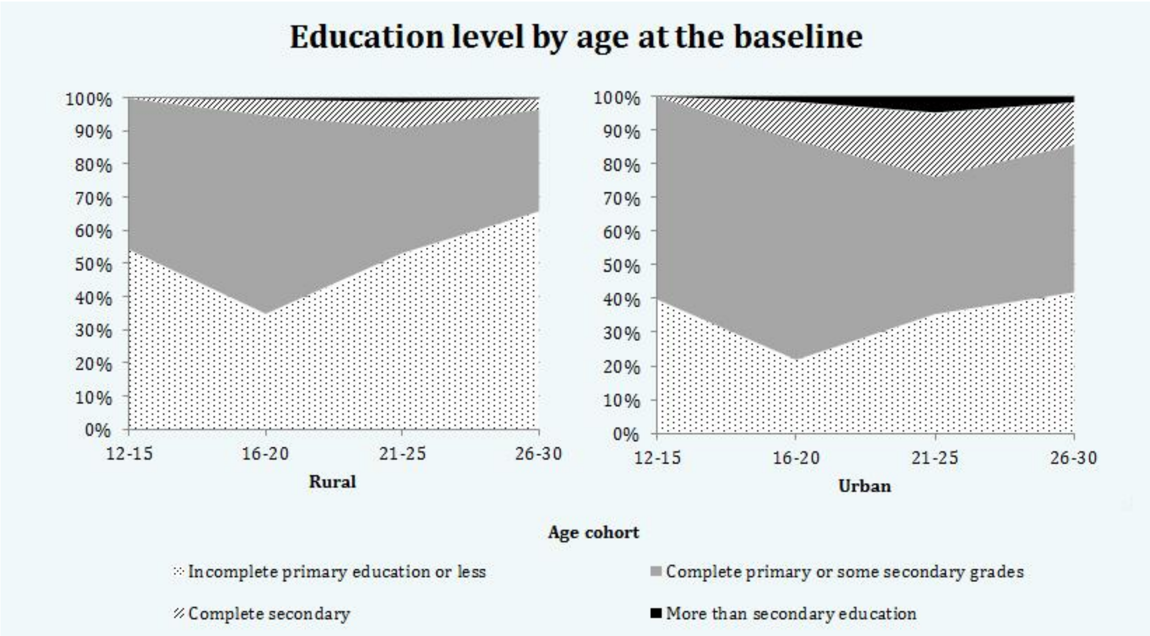


Figure 4.3: Range of sample distribution on the poverty score by region

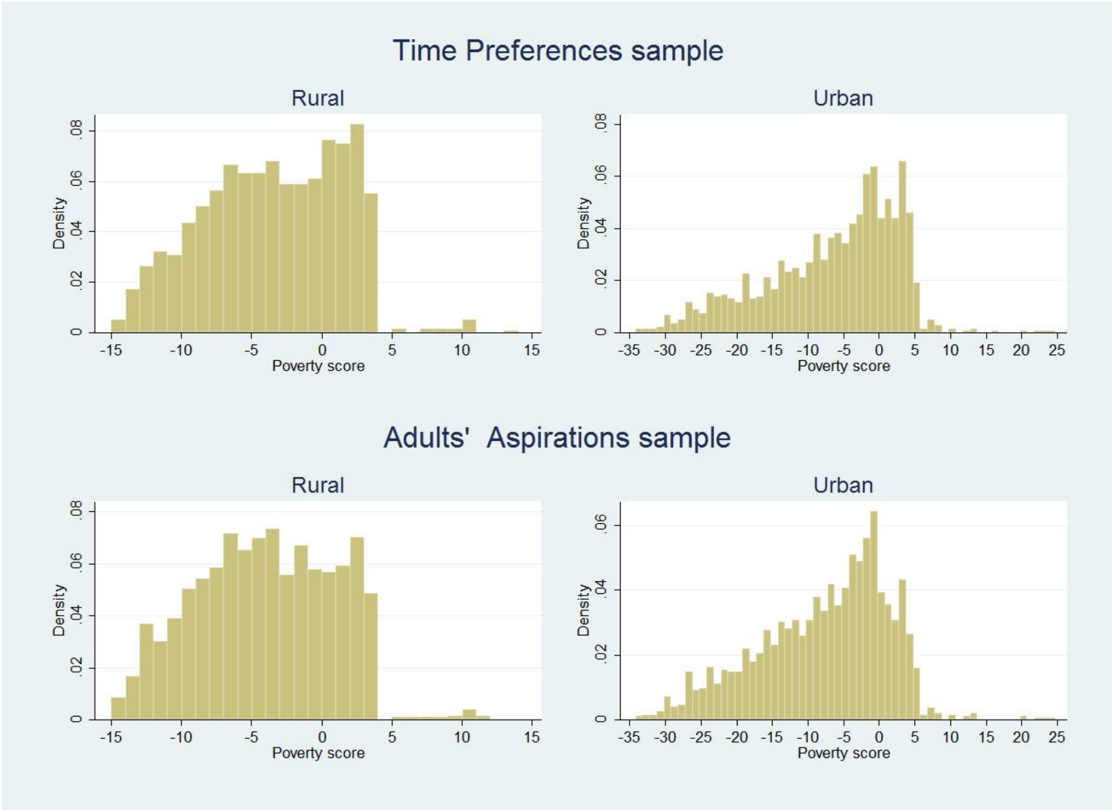


Figure 4.4: Probability of program participation by poverty score

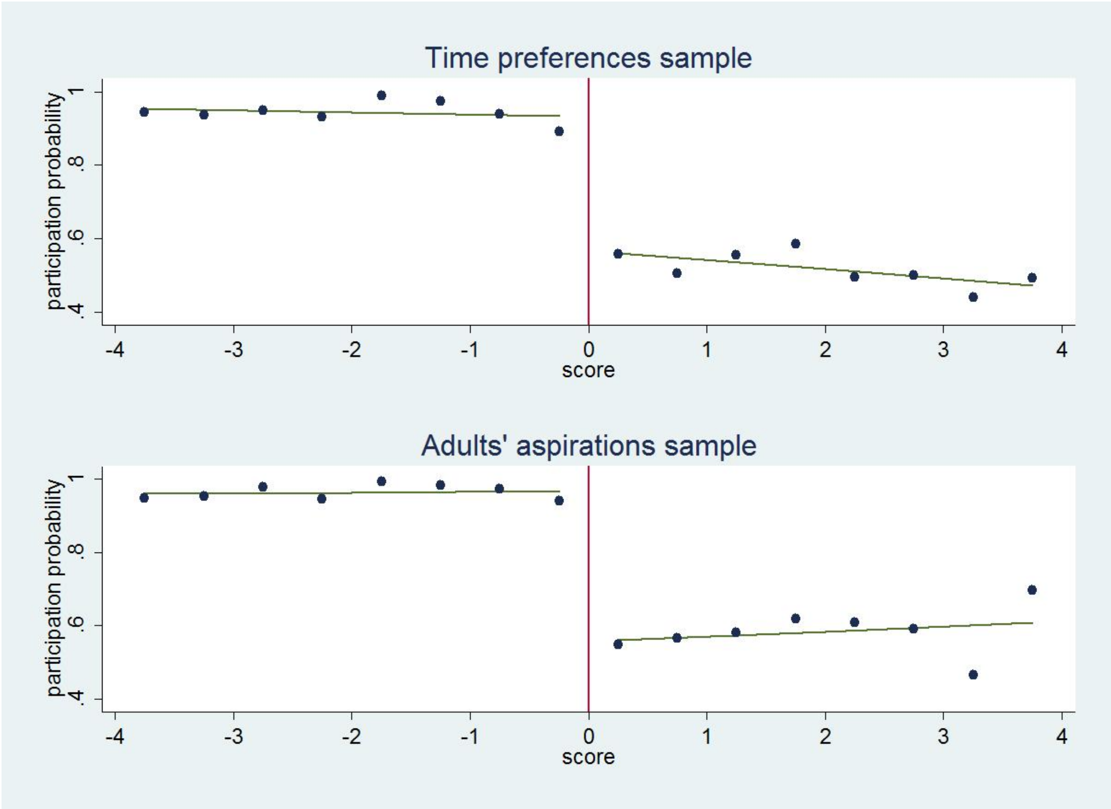


Figure 4.5: Probability of program participation by poverty score and Urban/Rural region

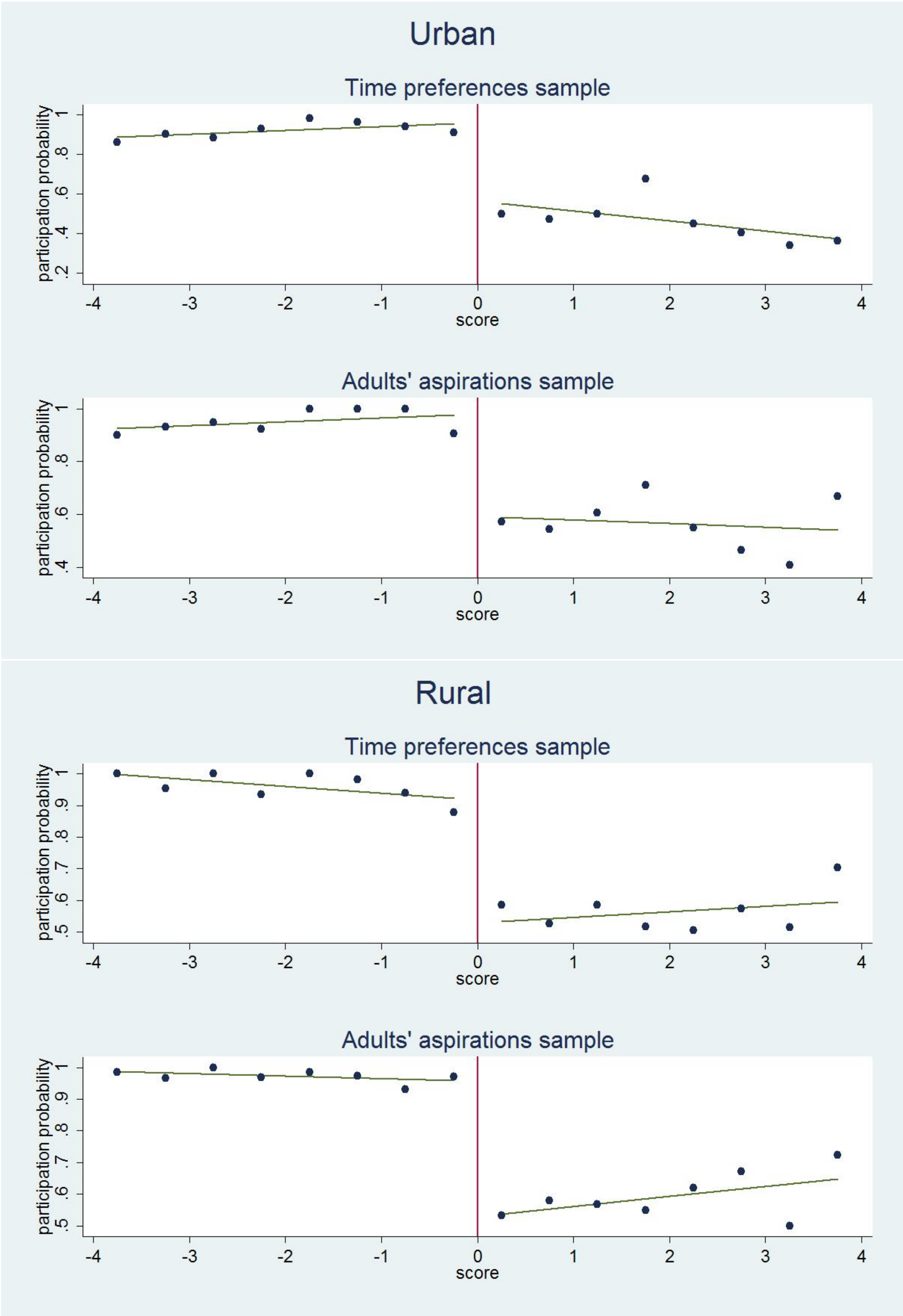


Figure 4.6: Probability of program participation by poverty score for Parents in the aspirations sample

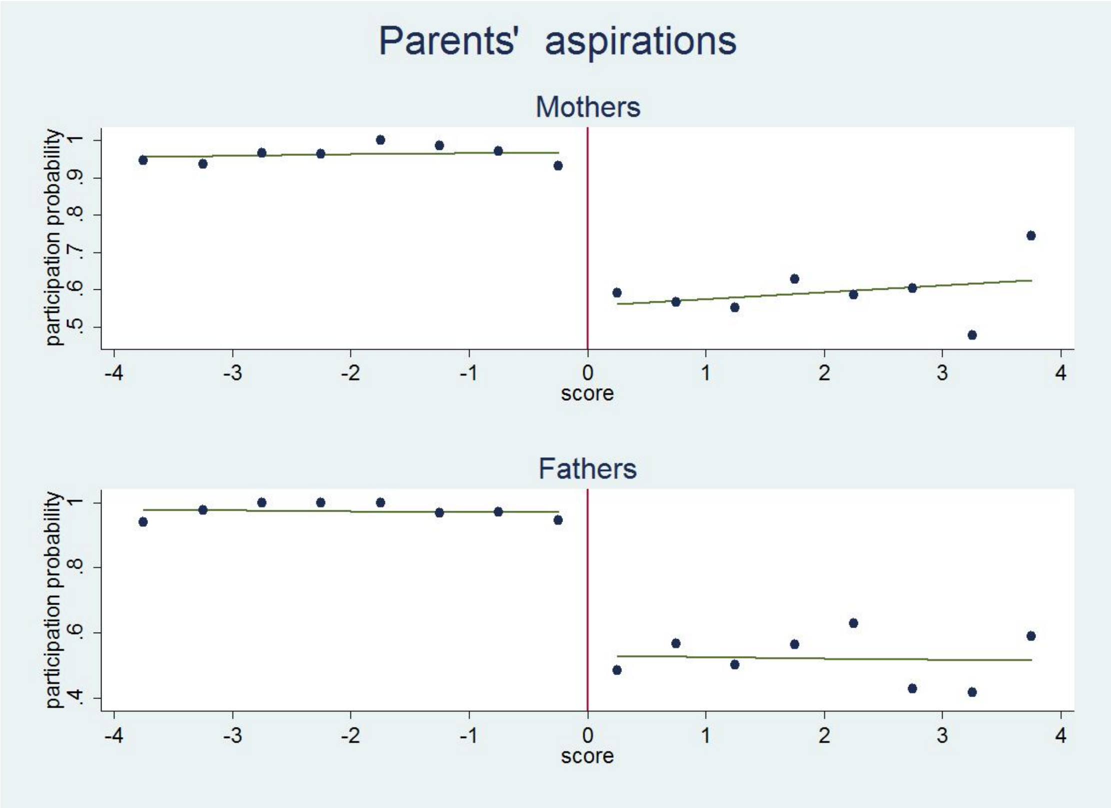


Figure 4.7: Effect of program participation on outcomes

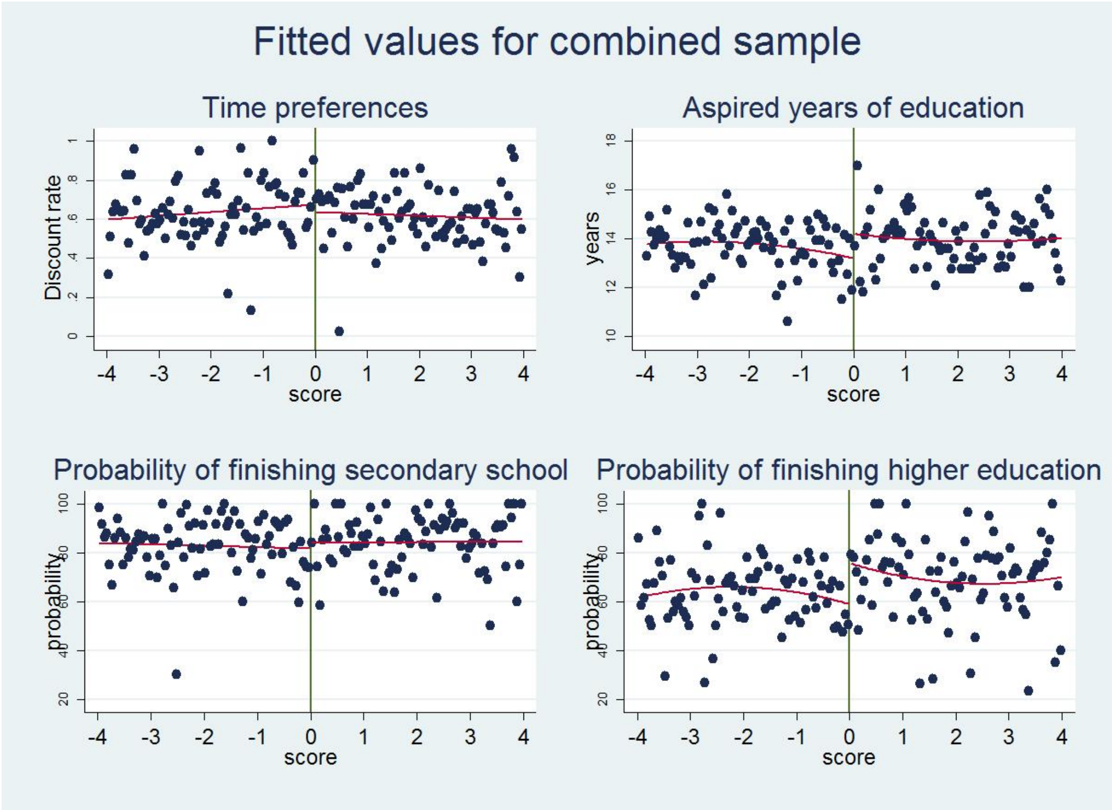


Figure 4.8: Effect of program participation on outcomes for individuals in urban areas

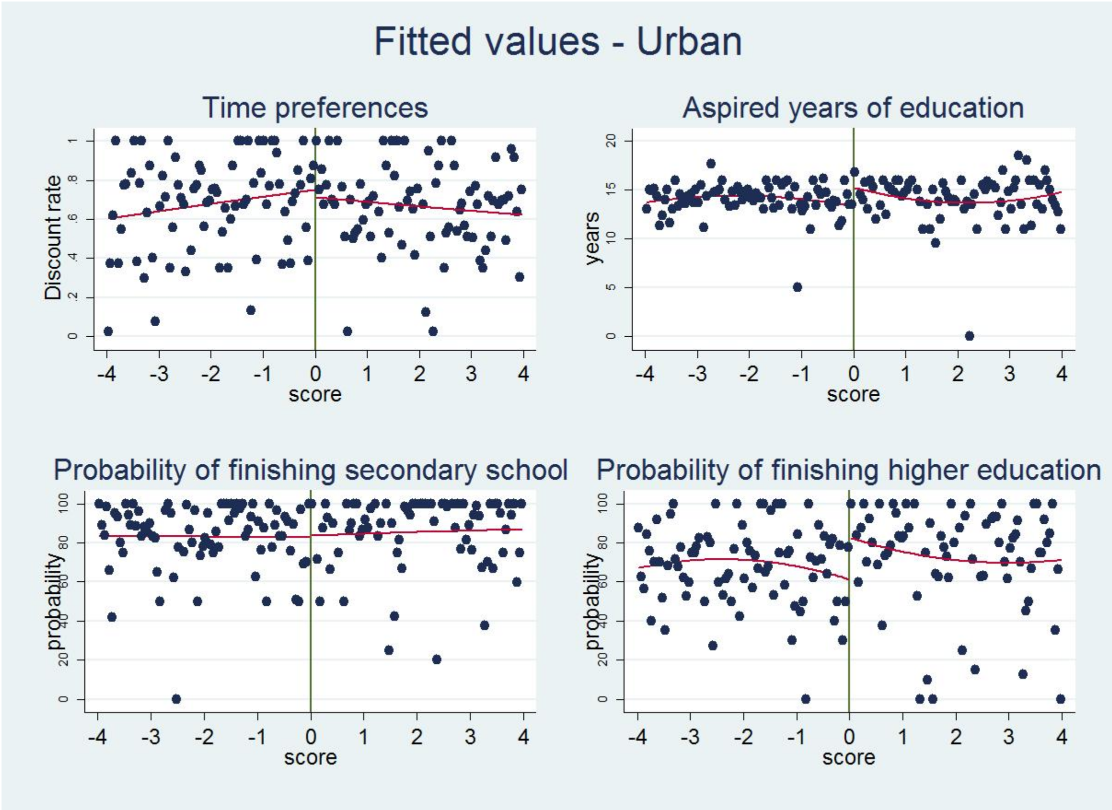


Figure 4.9: Effect of program participation on outcomes for individuals in rural areas

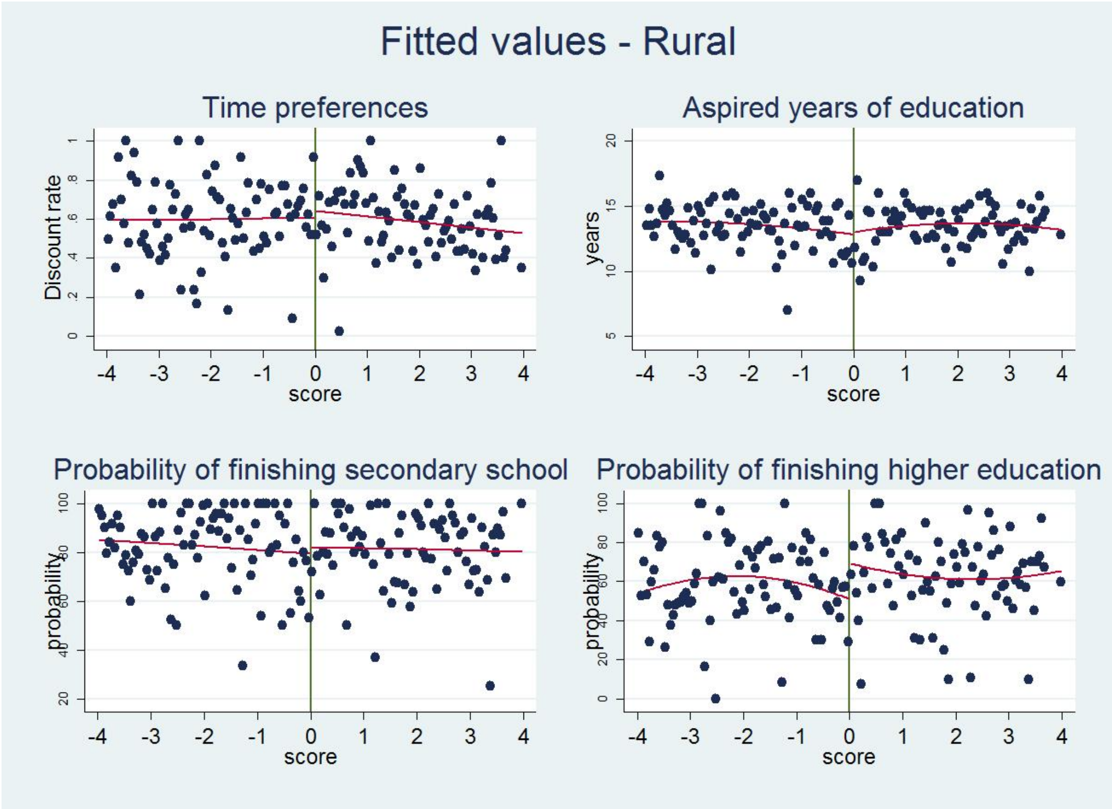


Figure 4.10: Density of the poverty index in treatment municipalities by variable of interest

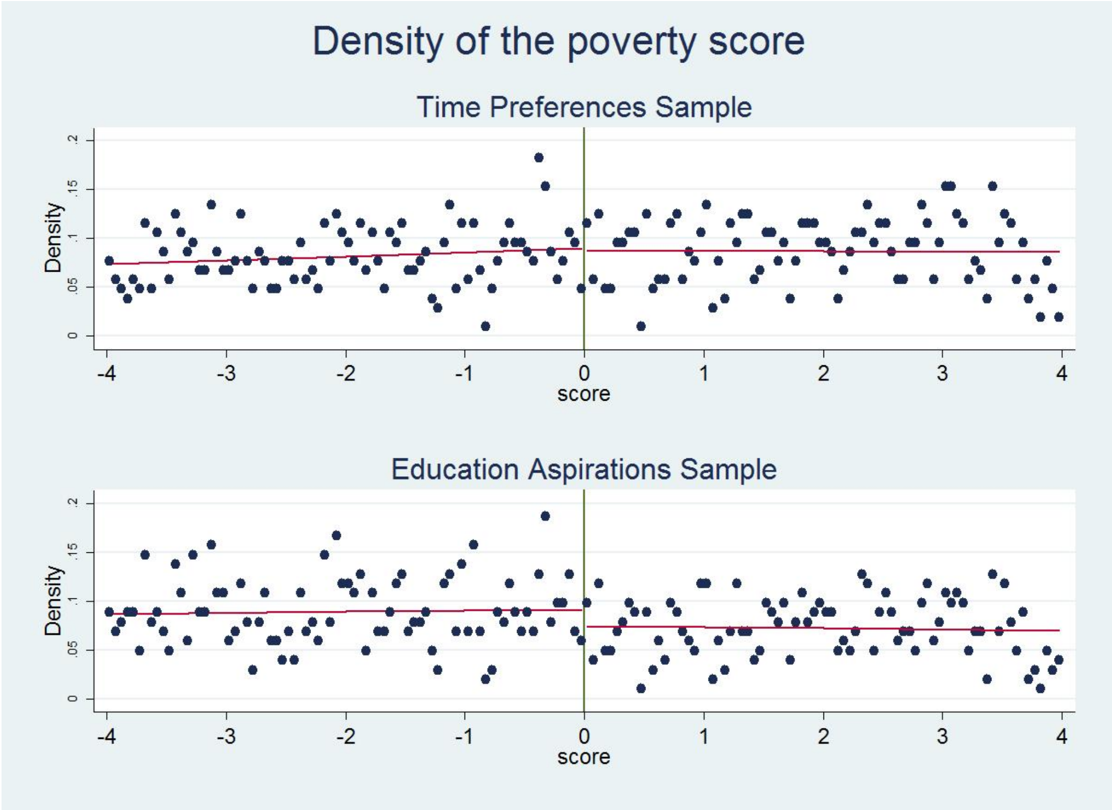
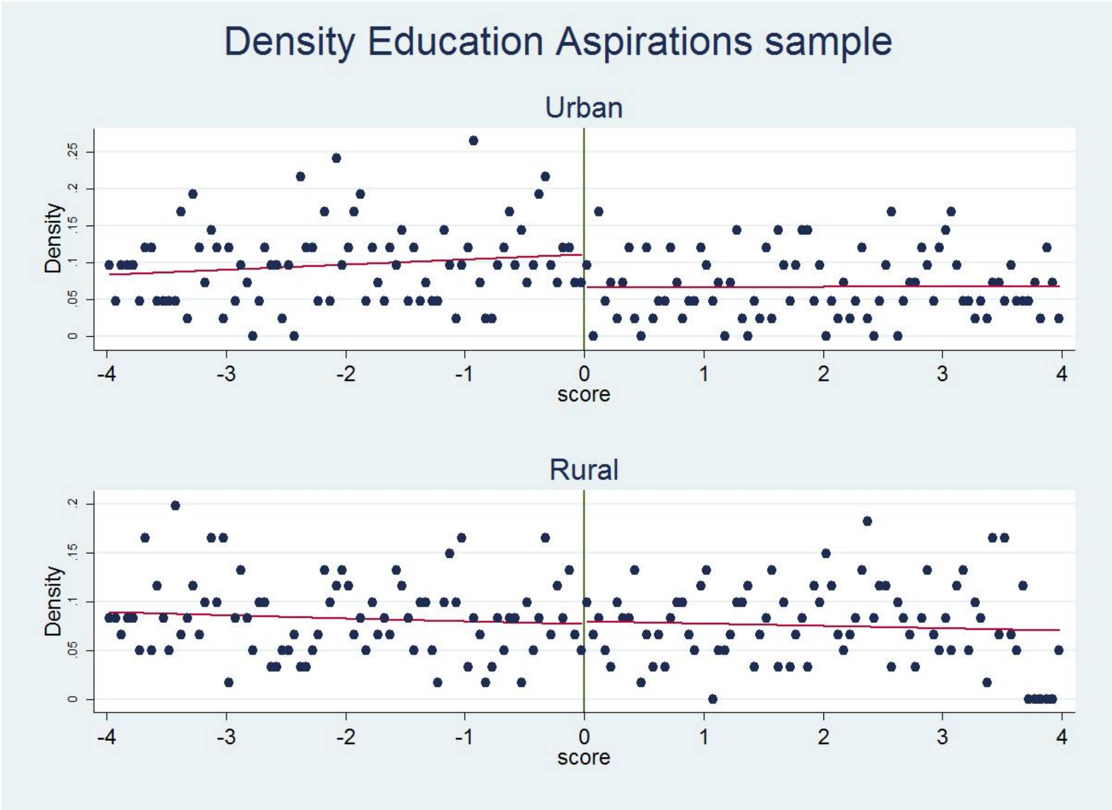


Figure 4.11: Density of the poverty index for Education Expectation sample by zone



4.11 References

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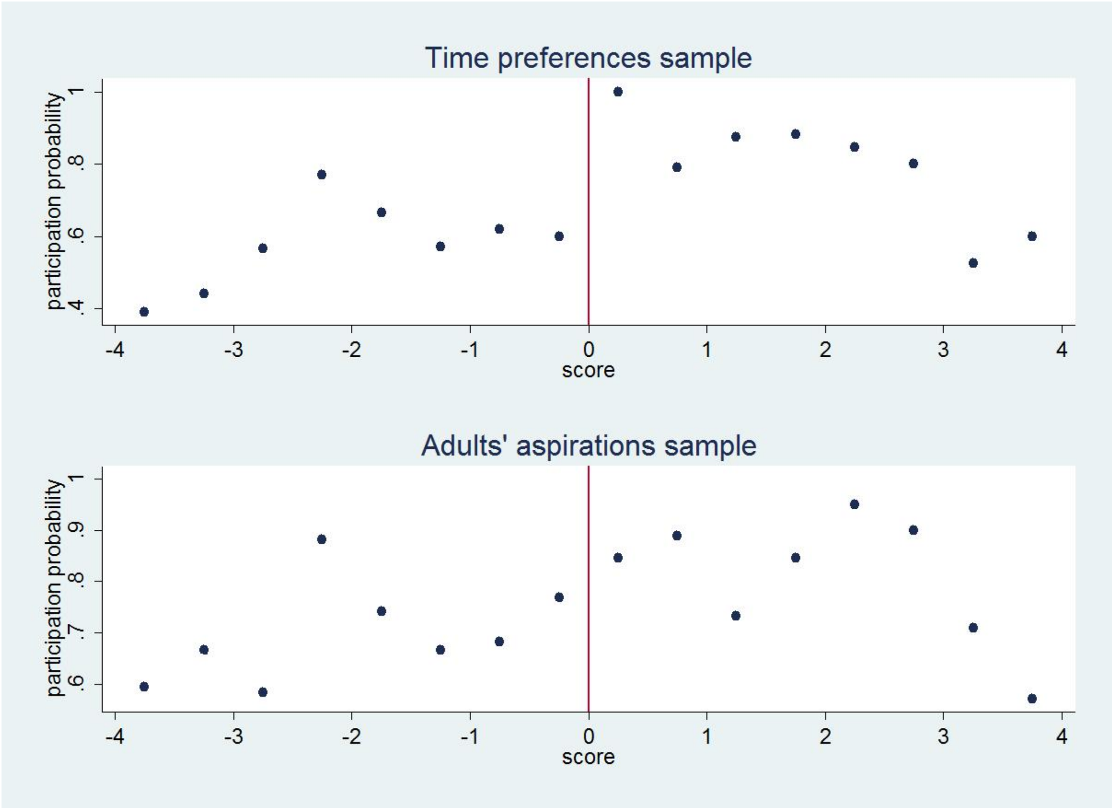
A4 Appendix

Figure A4 - 1: Probability of program participation by poverty score (SISBEN II) in control municipalities of the initial evaluation design



Chapter 5

Conclusions

Developing countries face the challenge of high poverty rates and low human capital. Conditional Cash Transfer (CCT) programs have been implemented widely as a strategy to reduce poverty and increase human capital among the poor. These programs offer poor households a monetary transfer contingent on children attending regular visits to the doctor and school attendance. These conditions and transfers intend to improve (in most cases) childrens health, nutrition and education. A large amount of research has been conducted and finds that demand for health and education has in general increased. The effects on health, nutritional and educational outcomes are still somewhat mixed. Studying the potential channels via which outcomes are (or are not) achieved is important to gain an understanding of how CCTs can boost long run human capital and reduce poverty transmission from generation to generation. This is particularly important as more and more countries have started to implement CCT programs and given the large amount of resources devoted to these programs all around the world.

The main aim of this research was to contribute to our understanding of channels through which the program can, not only improve children's human capital (the main target of such programs), but also the entire family's human capital. If the family's human capital increases as a result of the program, this could potentially increase their capabilities to increase their own human capital and in the long run overcome poverty and stop the transmission of poverty to the next generation. We focused on the CCT program implemented in Colombia – *Familias en Acción*. This program has been in operation from 2001 and currently reaches around 2.6 million poor families in the country. Poor families with children aged 0 to 17 are eligible for a nutritional/health transfer or for an educational transfer according to the age of the children. The nutritional/health transfer offers a lump sum of approximately US$25[1] for households with children 0 to 6 years old. This transfer was conditional upon attendance every two months at growth and development check-ups; adherence to an immunization schedule; and participation at hygiene, diet and contraception conferences by primary caretakers. This transfer aims to improve food consumption as well as the health and nutritional status of the children. The educational transfer was of approximately US$8 for each child in primary school, and US$16 for each child in secondary school[2] and is offered to households with children aged 7 to 17. The transfer is conditional on an average school attendance rate of more than 80% per child as the program's main aim is to improve childrens schooling. On average the transfer received by families represents between 16-25% of their monthly income.

We have focused the research on the effect of the program on improving children's and adults' health, children's cognitive development and adults' preferences and hopes for their children. We found increased health status of adults and children in the short, medium and long run. Adults in eligible households reduced their

[1] At 2001 prices.

[2] In the Colombian school system children are expected to start school at 6 years old and to complete high school they are required to complete 5 years of primary school and six years of secondary school for a total of 11 years. Under the scenario of normal progression children by age 17 should be able to graduate from secondary school and can access higher education.

self-reported incidence of illness in the short run and the severity of illness in the medium run when compared to control adults. For children who were eligible for the program when they were under the age of 2, we found that they have on average an additional 1.6 cm in height ten years later compared to children who were not eligible. Similarly, children who were eligible for program participation between ages 0 to 6, have a sustained gain in weight of about 2 kg ten years later, compared to children the same age who did not participate. While the effect on health and nutritional status on children is a direct effect of the program, improving adults' health is a positive spillover effect of the program. These results are important for several reasons. First, good health and nutrition constitute a fundamental element of life development and quality of life. Second, in CCT programs there is no control over parents' decisions (e.g. feeding practices or home-based health care) besides taking children to medical check-ups and caregivers' attendance at information talks. Better health and nutrition practices and outputs reflect positive internalization of the information received from the program and better household environments that reduce disease transmission. Finally, better health and nutritional status allows increasing working productivity for adults and school readiness for children. Both effects potentially increase future wages for adults and for children.

In the long run, the potential total expected gain in wages is higher for children than for adults, as children increase their health and nutritional status (physical development) and also their schooling. As a result, we looked at the long run gains in cognitive development through improved weight and height for children exposed to the nutrition/health incentive of the program from age 0 to 6. We find that increased weight and height lead to higher cognitive development. Additionally, we find that those nutritional gains are sustained over time and in fact when physical development improvements are measured over a longer period, the estimated gains in cognitive development are higher. These results reveal that the program is increasing the nutritional and health status of children and also their school readiness and cognitive development. Cognitive development has been recognized to be associated with positive long term outcomes like higher schooling, wages, patience, willingness to invest on education or save, and reduction of crime involvement (Alderman, Hoddinott, & Kinsey, 2006; Becker & Mulligan, 1997; Grantham-McGregor et al., 2007; James J Heckman, 2006; James J. Heckman, Stixrud, & Urzua, 2006; Nores & Barnett, 2010).

Finally, we looked at changes in parents' underlying preferences and hopes for their children's educational attainment. This is an additional mechanism through which the program can increase households human capital in the short and long run even if the program were to cease. We do not find supportive evidence of changes in time preferences of educational aspirations except a reduction in the associated probability of children completing higher education. This result suggests that if the program were to stop, parents would probably stop investing in their children's human capital and some of the potential long term benefits would not eventuate. The most likely effects would be on outcomes directly linked to the transfer and the condition, such as school attendance and nutritional status of younger children. However, as education and cognitive development reduce time preferences, the program can potentially lead to changes in participant children's preferences even if parent's preferences remain constant. If the program were to stop, children who are participating or have participated in the past, and whose educational attainment has increased are however more likely to have higher preferences for delaying present consumption, increase their educational aspirations and increase investments in training that favour their own human capital. Hence, in the long run the program may impact on participating children's human capital and potentially also their future families' human capital investment by changing their own preferences and aspirations.

CCT programs are still seen as very promising for improving outcomes for poor families; however the channels and range of potential outcomes are still largely unknown. This is particularly true for long term outcomes. One motivation for this research was to contribute to understanding why CCTs have not homogeneously contributed positively towards learning outcomes. From our findings, if children who participated from birth in the program have better nutrition and health status and also better cognitive development, we would expect that their learning outcomes would have improved. This is an area for future examination.

In the quest for how to improve learning outcomes, we also explored parents preferences and hopes for their children's education. Some questions raised from that research are whether indeed children are more patient than their parents given participation in the program. And, how strongly related are parents' and children's educational aspirations with children's performance at school? Additionally, we found that information given to caregivers is a potential channel via which to improve health outcomes. But why does this not seem to influence parents' educational preferences? Finally, it would be worth examining the role played by the education supply side. For example, how is the quality of schooling related to educational achievement of participating children? What children are more (and less) likely to perform better, graduate and access higher education?

We found that in Colombia the program is improving the health status of children, especially for children who participate from age 0 to 2. Improving the health and nutritional status of children and adults has been a valuable outcome of the program from the beginning. We recognize that there is still scope to improve. The literature suggests that in-utero is a vital period for children's development (Barham, Macours,

& Maluccio, 2013). This stage seems not to be targeted by the program. The inclusion of compulsory maternal health care during pregnancy could be considered.

We found that cognitive development of children has also improved. Parents play an important role in learning outcomes; the program could potentially increase the parents' level of involvement in stimulating children's learning. The information sessions seem to be a useful instrument to educate caregivers. Providing caregivers with tools to promote early childhood learning as well as the means to support older children in their school work could contribute to increased learning outcomes.

There remain many ways to improve lives and help the most disadvantaged populations, and so plenty of work remaining for universities, governments and non-governmental agencies. This general aim leads us to work with enthusiasm and hope in order to positively contribute to the world in which we all live.

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