

A Service of

ZBU

Leibniz-Informationszentrum Wirtschaft Leibniz Information Centre for Economics

Vera Cossio, Diego Alejandro

## Working Paper Dependence or constraints? Cash transfers, labor supply and the process of development

Development Research Working Paper Series, No. 01/2017

**Provided in Cooperation with:** Institute for Advanced Development Studies (INESAD), La Paz

*Suggested Citation:* Vera Cossio, Diego Alejandro (2017) : Dependence or constraints? Cash transfers, labor supply and the process of development, Development Research Working Paper Series, No. 01/2017, Institute for Advanced Development Studies (INESAD), La Paz

This Version is available at: https://hdl.handle.net/10419/177357

#### Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

#### Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.



# WWW.ECONSTOR.EU

**Institute for Advanced Development Studies** 



## 1/2017

# Dependence or Constraints? Cash transfers, labor supply and the process of development

By:

Diego A. Vera Cossio Department of Economics, University of California, San Diego

**Development Research Working Paper Series 01/2017** 

## January 2017

The views expressed in the Development Research Working Paper Series are those of the authors and do not necessarily reflect those of the Institute for Advanced Development Studies. Copyrights belong to the authors. Papers may be downloaded for personal use only.

## Dependence or Constraints? Cash transfers, labor supply and the process of development

Diego A. Vera-Cossio\*

Department of Economics University of California, San Diego

January 14, 2017

#### Abstract

In this study, I use the timing and eligibility criteria of a large-scale conditional cash transfer program in Bolivian public schools to identify the effect of the program on adults' labor supply. I find that adult females increase their labor supply due to the program, mostly through self-employment. To understand these results, I sketch a simple theoretical framework of selection into employment that introduces fixed costs to work and imperfections in capital markets, two main features of the process of development. In this environment, households select into employment only if they are able to self-finance the fixed costs. I derive additional predictions that are empirically tested. First, the positive treatment effects should manifest at the extensive and not the intensive margin. Estimating treatment effects along the cumulative density function of work hours/week, I find that the effects on labor supply come exclusively from the extensive margin. Second, the effects of an income shock should be stronger when capital market frictions are more salient. Using baseline data for the supply of financial services at the municipality level as a third difference, I find that the effects on labor supply are higher for women in more credit-constrained areas. I compare these results with compelling alternative explanations such as increases in local aggregate demand induced by the program and the relaxation of time constraints for mothers due to the condition component of the program. I find no evidence supporting these two alternative mechanisms. Overall, the results suggest that after considering the role of credit and labor market frictions, the first step in climbing the ladder of development is to overcome the barriers households face in simply starting to work.

Keywords: Labor supply, poverty traps, gender, conditional cash transfers. JEL Classification Codes: D13, J46, J21, J22, O12, O18

<sup>\*</sup>I would like to thank Prashant Bharadwaj, Craig McIntosh, Karthik Muralidharan, Krislert Samphantharak and Gordon Dahl for their advice and guidance. I would also like to thank Paul Niehaus and Mitch Downey and participants in UCSD Development-Applied Micro seminars and the LACEA 2016 conference for insightful comments. All errors are my own. Please send any questions or comments to dveracos@ucsd.edu.

#### 1 Introduction

Cash transfers are common tools for tackling poverty, both in developed and developing countries. While these programs show welfare-increasing effects in many dimensions (Fiszbein et al., 2009), a main concern is whether these types of programs can have negative effects on labor supply and create dependence, leading to a trade-off between immediate poverty alleviation and long-term poverty reduction. Studies from cash welfare programs in developed countries suggest that there is a negative effect on labor supply (Hoynes (1996) and Hoynes and Schanzenbach (2012)), consistent with a neoclassical model of labor supply. However, the literature analyzing the effects of cash transfer programs on labor supply in developing countries systematically fails to find significant treatment effects (Skoufias and Maro (2008), Alzua et al. (2013), Banerjee et al. (2015) and, de Brauw et al. (2015)). Moreover, recent evidence on unconditional cash transfers to groups of young potential entrepreneurs finds increases in work hours due to the program (Blattman et al., 2014). Understanding which features of the process of development explain the divergence in results will reconcile the empirical evidence and shed light on the salience of the poverty alleviation-dependence trade-off.

This paper uses the staggered timing and eligibility criteria of a conditional cash transfer program (CCT) in Bolivia to estimate the causal impact of a positive income shock on adults' labor supply through a difference-in-difference approach. The program provided 200 Bolivianos (approximately 25 U.S. dollars) per year to children in Bolivian public schools conditional on 80% attendance during the school year. Using 8 years of Bolivian household surveys, I construct a pooled cross-section dataset of children in public schools in Bolivia<sup>1</sup>. Exploiting the variation in eligibility across school grades and the rollout of the program, I compare changes in work outcomes before and after introduction of the program for adults whose children were enrolled in eligible grades with changes in work outcomes for adults whose children were not beneficiaries of the program.

I find that the program increased the probability of working by 4 percentage points and increased work hours by 2.5 units for adult females (heads of households or their spouses). These effects are small; they represent increases of 6% and 9% with respect to the baseline mean<sup>2</sup>, respectively. I find that 90% of this effect comes from adult females whose children were likely to attend to school even in the absence of the program and were not affected by the condition component of the program. This result suggests that a shift in the budget set induced by an income shock dominates potential increases in the availability of time induced by the condition component of the CCT<sup>3</sup>. The results are robust to a variety of specifications and are consistent with an economy characterized by a large, stagnant gender gap in employment<sup>4</sup>

<sup>&</sup>lt;sup>1</sup>This represents 90% of children enrolled in school during the year preceding the program.

 $<sup>^{2}</sup>$ The effects on work hours represent 6% of the baseline mean, conditional on working.

<sup>&</sup>lt;sup>3</sup>These results also complement existing evidence regarding the role of the condition component (Baird et al. (2011), Benhassine et al. (2015), Filmer and Schady (2011), de Brauw and Hoddinott (2011)).

<sup>&</sup>lt;sup>4</sup>In Bolivia, for every 10 male household heads who work, there are only 7 female household heads or spouses of household heads working. This gap has remained constant over the last decade, according to data from Bolivian household surveys.

To understand why these apparently unusual positive elasticities appear in the context of a developing country, I outline a simple theoretical framework which predicts the result found in my empirical approach and derive additional predictions which I then take to the data. I do so by drawing on a traditional idea behind the process of development: "in a context of capital markets imperfections, economic performance, either prosperity or stagnation, depends on the initial wealth distribution" (Banerjee and Newman, 1993). I sketch a stylized model for labor force participation that includes heterogeneous fixed costs to enter the labor force and frictions in credit markets<sup>5</sup>. In this environment, there is selection into employment based on the initial household wealth; fixed costs generate the need for funds and credit market frictions create difficulties in getting these funds. As a result, households have lower consumption levels because they can't work and they can't work because they are too poor-a poverty trap. The main empirical implication of this model is that an income shock can push people into the labor force, at least for agents who are close to being able to cover their fixed costs.

To test the importance of these two features of the process of development, I derive two additional predictions from the model and take them to the data. First, because of the fixed costs to enter the labor force, the effects of the income shock should affect the decision to enter the labor force but should not affect the intensive margin of labor supply. Estimating treatment effects along the cumulative density function of weekly work hours, I find that the effects on labor supply come from the extensive margin rather than the intensive margin, supporting the fixed-costs hypothesis. Moreover, if the fixed costs are salient, then the results should come from activities that require a fixed cost to work. I find that the effect on employment comes from people shifting from unemployment to self-employment. Second, the effects of an income shock should be stronger when capital market frictions are more salient. Using baseline data for the supply of financial services at the municipality level as a third difference, and controlling for potential treatment effect heterogeneity across urban and rural areas, I find that the effects on labor supply are higher for women from more credit-constrained areas.

Why would agents respond to positive income shocks by increasing labor supply? The evidence provided in this study shows that two particular features of the process of developmentfixed costs to work and imperfections in credit markets-explain why the labor supply responds differently to income shocks in developed countries than in developing countries: In developed countries, capital market frictions and labor market frictions are smaller and households behave as in the neoclassical model. In developing countries, households live in an environment of liquidity constraints and fixed costs, and this explains why dependence on cash transfers might be less likely. Moreover, if the process of development is about occupational choice, the evidence in this study suggests that escaping involuntary unemployment is the first step in this process.

To understand the extent to which other mechanisms could lead to the same empirical

<sup>&</sup>lt;sup>5</sup>I allow these frictions to arise either due to high intermediation costs that result in higher borrowing rates, or simply through constraints in the maximum amount each household can borrow.

results, I compare the empirical evidence and the implications of the theoretical framework with compelling alternative explanations in this context. In particular, I discuss two relevant mechanisms: relaxed time constraints and aggregate demand changes due to the program. First, I analyze whether increases in labor supply may be driven by a relief in the mother's time constraints by the program. Since the program provides resources conditional on attendance at school, it might be the case that the observed treatment effects arise because mothers simply reallocate time from child care to productive activities. Three facts rule out that mechanism. First, the program was implemented in a baseline context of high attendance and enrollment and low dropout rates <sup>6</sup>. Moreover, this mechanism would imply that the treatment effects should come from adults whose children are more likely to be affected by the condition component (marginal children). I find that the responses in labor supply are driven primarily by mothers of children who would have attended school even in the absence of the program. Finally, consistent with the latter facts, I find no evidence of effects of the program on enrollment nor on child labor.

Another possible explanation for the positive effects on labor supply could arise from shifts in aggregate demand at the local (municipality) level <sup>7</sup>. Although the transfers were small, the program reached a large share of the households with school-age children. This cash inflow could modify the business environment, favoring self-employment, or it could increase wages, thereby inducing households to work. Two arguments rule out this potential mechanism. First, if local demand were driving the results, the treatment effects should be a function of the ability of households to take advantage of the new context, and therefore the treatment effects should be higher for households in areas that have better access to credit; I find the opposite. Second, in this study, treatment effects are identified from individual variation within clusters (municipalities) since entry into treatment is orthogonal to locations and is a function of individual characteristics only (years of schooling for children of school age). This design provides a way of replicating the natural experiment in each cluster; if either the economic conditions changed or wages increased, they did so similarly for treatment and control groups.

This paper reconciles evidence regarding labor supply responses to income shocks from developed and developing countries using a basic idea: the interaction of frictions in labor and credit markets, which is core to development economics (Banerjee and Newman, 1993) (Lewis, 1954) (Gollin, 2014). The evidence provided by this study contributes to four strands of the literature. First, it provides novel evidence regarding positive incomeelasticities of work outcomes, suggesting that a trade-off between short-run poverty alleviation and dependence may not be salient in developing economies (Banerjee et al.

<sup>&</sup>lt;sup>6</sup>Reports from the Ministry of Education (Zambrana et al., 2004) show that before the program was implemented, the national attendance rate was above 80%, enrollment was above 90% and the dropout rate was below 10%.

<sup>&</sup>lt;sup>7</sup>One particular challenge in interpreting the reduced form treatment effects from large-scale cash transfer programs is the presence of general equilibrium effects that are confounded with direct income shocks on treatment units (Acemoglu, 2010). Studies such as (Kaboski and Townsend, 2012) and (Muralidharan et al., 2016b) analyzing large-scale programs that imply a significant injection of liquidity into the local economy find evidence of general equilibrium effects manifested through wages, in the case of micro-credit in Thailand and a reduction in leakage of resources from a workfare program in India, respectively.

(2015), Skoufias and Maro (2008), de Brauw et al. (2015), Alzua et al. (2013) and Fiszbein et al. (2009)), suggesting a win-win scenario for long-run poverty alleviation consistent with Gertler et al. (2012). In particular this paper complements evidence and key insights from Blattman et al. (2014) who also find positive effects on work hours after a randomly assigned cash grant to groups of young entrepreneurs in Uganda. Although the fixed-costs and credit-constraints hypotheses are discussed in that paper, because the study focuses on a particular sample of credit-constrained beneficiaries there is little variation in the sample to test empirically for treatment-effect heterogeneity in that dimension. In this paper I exploit a large-scale nationwide program that captures enough regional variation in credit market imperfections.

Second, the theoretical framework proposed and tested in this paper and the design of the program provide insights for understanding why other studies were not able to find positive responses. Successful, emblematic CCT programs are means-tested and therefore affect a particularly disadvantaged share of the population. If the households that can take advantage of the income shock are only those who are close to covering their fixed costs, as the theoretical framework suggests, programs that focus exclusively on the most disadvantaged agents will fail to capture agents who would potentially use the extra resources as a tool to escape involuntary unemployment. This same logic also explains why Blattman et al. (2014) find strong effects on work hours: They targeted agents who despite being poor according to several metrics, are better off than most of the Ugandan population. The Bolivian program studied in this paper is not means-tested and reaches around 90% of school-age children, capturing the entire distribution of fixed costs and credit constraints and providing the empirical approach with enough power to capture positive responses in labor supply due to the program.

Third, through having an empirical design that minimizes the role of changes in local demand as drivers of the effects on labor supply<sup>8</sup>, this paper focuses on household-level shocks and is related to recent literature providing evidence of the salience of micro-level rather than macro poverty traps(Kraay and McKenzie, 2014). Recent literature regarding low-cost interventions aiming to break these vicious circles focuses on the role of reducing fixed costs that generate low uptake of profitable investments (Bryan et al. (2014),de Mel et al. (2008), Field (2007) and Cascio (2009)). This paper identifies a complementary comparative statics exercise; instead of reducing fixed costs, it modifies non-labor wealth, reducing the salience of these fixed costs. The evidence suggests that when fixed costs are heterogeneous and hard for policy makers to identify, as is most likely the case for large-scale anti-poverty programs, cash transfers are a powerful option that complements other interventions aimed at reducing fixed costs.

Finally, the results complement evidence regarding the importance of credit constraints and capital markets in developing countries. Interventions that attempt to expand credit

<sup>&</sup>lt;sup>8</sup>The design of the program is different from previous experimental evidence from emblematic programs in which random assignment of CCTs is conducted at the cluster level, such as Shultz (2004) in the case of PROGRESA or the studies reviewed by Banerjee et al. (2015), and contributes to the literature with elasticities coming exclusively from increases in the budget sets.

markets have delivered modest results overall Banerjee et al. (2015), however the results of this program suggest that these interventions can be effectively complemented by small grants in areas that are subject to high intermediation costs that result in credit market frictions.

#### 2 The setting

The Bono Juancito Pinto (BJP) program was first announced in October 2006. The program provided a cash transfer (CCT) of 200 bolivianos (approximately 25 U.S. dollars) conditional on 80% school attendance for every child enrolled in public school<sup>9</sup>. As opposed to most programs in the region, this program was not means-tested and the eligibility criterion was based on the grade the child was enrolled in, regardless of their socioeconomic status. This transfer represents around one-third of the monthly minimum wage for the baseline year, 4% of average per capita yearly consumption, around 53% of the yearly per capita education spending in urban areas and more than 100% in rural areas<sup>10</sup>. As of 2005, the school enrollment rate was already high, at 90%. Moreover, dropout and non-passing rates were below 10% before the program was implemented<sup>11</sup>.

In the first stage, the potential beneficiaries were children enrolled in first to fifth grades; children who met the attendance threshold and fulfilled additional documentation requirements<sup>12</sup> received the transfer at the end of the school year (November). The funds were disbursed by personnel from the Armed Forces in each school, leaving very little room for leakage or implementation failures <sup>13</sup>. In October 2007, the program was extended to children in sixth grade, again with disbursement of the funds at the end of the school year. The set of beneficiaries was expanded to children in seventh and eighth grades in July 2008, but the disbursement schedule was changed to two payments in July and November 2008. Although the funds were disbursed in two payments, the total amount given to each student did not change.

The program was implemented in a context of high poverty but steady economic growth. In 2006, Bolivia had a per capita GDP of 4,438 U.S. dollars (PPP), just above the average for lower middle-income countries and about one-third of the average for Latin America and the Caribbean <sup>14</sup>. However, the country experienced an average yearly growth rate of 5% for the years analyzed in this study. The poverty rate has fallen in recent years, from

<sup>&</sup>lt;sup>9</sup>In the baseline year, this accounts for 90% of children enrolled in either private or public schools.

<sup>&</sup>lt;sup>10</sup>Source: Own calculations based on Household Surveys (2005-06) from the National Bureau of Statistics (INE).

<sup>&</sup>lt;sup>11</sup>Source: Ministry of Education, see Zambrana et al. (2004).

<sup>&</sup>lt;sup>12</sup>A birth certificate or ID were required; in addition, children had to be accompanied by a parent or guardian to receive the money, generally the mother. After the second round of the program, children who did not possess a birth certificate or an ID could receive the money if they presented two witnesses who testified to their identity.

<sup>&</sup>lt;sup>13</sup>Although there is evidence of leakage in large-scale transfer programs in contexts of low state capacity (Muralidharan et al., 2016a), this issue is of minor concern in this program as self-reported data from national household surveys show that 90% of eligible children received the transfer in the first stage.

<sup>&</sup>lt;sup>14</sup>Source: World Development Indicators.

60% in 2000 to 30% in 2013. However, poverty reduction has not occurred at the same rate for all: The decrease in poverty was mostly driven by urban areas and women are employed at 80% of the rate for males, a difference that has persisted over the last 10 years. <sup>15</sup>.

Figure 1 shows that for male household heads or male spouses of household heads, the share of individuals that report having worked, performed remunerated activities or tasks for a family business during the week preceding the data collection date is around 95%. On the other hand, the ratio is around 70% for female household heads or female spouses of household heads. Two main lessons are suggested by these results. First, the high employment rates for males suggest that job opportunities exists in this economy. Second, despite job availability, the broad gender gap in employment suggests that women face constraints to entering the labor force. This feature of the Bolivian economy motivates the main question of this paper: Can income shocks mitigate some constraints agents face when deciding whether to work?

#### 3 Data

The data for this study come from national household surveys conducted by the National Bureau of Statistics (INE) for the years 2002-2009. I constructed a pooled cross-section data set based on 8 waves of household surveys. These surveys are independent cross-section samples of individuals drawn from a common sample frame based on the 2001 population census. Surveys for the years 2002 and 2005 to 2009 were conducted between late November and December of each year. The 2003-2004 survey is a continuous survey applied to different households in two rounds: November 2003-April 2004 (2003 round) and May-October 2004 (2004 round).

In this study, I use a sample of children between 7 and 17 years old who have completed at most eighth grade and who do not report being enrolled in a private school; the sample accounts for 90% of the children of school age. For each child, I compute information regarding the adults living in each child's household and labor market variables for the head of household and the head of household's spouse. I focus on household heads and heads' spouses as, on average, they represent most of each household's income. I use two main measures of employment: The first is an indicator of whether the interviewee reports having worked or performed remunerated activities or tasks for a family business during the week preceding the survey. The second measure refers to the average hours worked per week. To construct this measure I use self-reported information regarding the average number of hours worked per day and the number of days worked in the week preceding the interview. In the case of unemployed people, the number of hours is 0. I focus on these two measures as they are the standard measures used in studies analyzing responses of labor supply to cash transfers in developing countries such as (Alzua et al. (2013) and Banerjee et al. (2015)).

I complement this dataset with information regarding the number of branches of financial

<sup>&</sup>lt;sup>15</sup>Computations based on data from the National Bureau of Statistics (INE)- Household Surveys.

institutions and population at the municipality level. Information regarding the number of branches of financial institutions comes from the national regulator, the Authority of Supervision for Financial Institutions (ASFI), and only covers municipalities that are also provincial capitals (112 of 339 municipalities), which account for two-thirds of the observations in my sample. Population data come from the 2001 National Population Census conducted by INE. Summary statistics for 2005, the year preceding the implementation of the program, are presented in Table 2.

#### 4 Identification strategy

I take advantage of the design of the BJP program to estimate its causal effects on adult employment. I use the staggered timing and eligibility criteria of the program as the identifying sources of variation. Although the program was implemented in all regions of the country at the same time, children were included as beneficiaries of the program gradually, based on years of schooling. Thus, the design provides variation over time and across individuals in a given year, suggesting a difference-in-difference approach. The program's design is presented in Table 1.

Years of Schooling	2002-2005	2006	2007	$\geq 2008$
1	С	Т	Т	Т
2	С	Т	Т	Т
3	С	Т	Т	Т
4	$\mathbf{C}$	$\mathbf{T}$	$\mathbf{T}$	$\mathbf{T}$
5	$\mathbf{C}$	$\mathbf{T}$	$\mathbf{T}$	$\mathbf{T}$
6	$\mathbf{C}$	$\mathbf{C}$	$\mathbf{T}$	$\mathbf{T}$
7	$\mathbf{C}$	$\mathbf{C}$	$\mathbf{C}$	$\mathbf{T}$
8	$\mathbf{C}$	$\mathbf{C}$	$\mathbf{C}$	$\mathbf{T}$
>8	С	С	С	С

Table 1: Program design

Note: Columns report the year in which the information was collected. Rows report the grades in which children can be enrolled. The entries in the table represent the treatment status of each group at each moment in time. "C" denotes groups that belong to the control group in a particular year, that is, groups that are not beneficiaries of the program at that moment in time. "T" denotes groups that belong to the control to the treatment group in a given her; that is, children who, given their years of schooling (grades completed) are treated or not in a particular year. Bold letters denote the groups that are included in the main analysis in this study.

In order to identify the causal effects of the program on work measures, I use the timing of the program's announcements, which is arguably exogenous to households' decisions, as a first source of variation. The program includes the entire public-school system. Recall that the program was first announced in October 2006, while two expansions were announced later, in October 2007 and July 2008. These dynamics are represented in the columns of Table 1 <sup>16</sup>. Cash was disbursed at the end of the 2006 and 2007 school years (November)

<sup>&</sup>lt;sup>16</sup>The program was first announced in the first year of the administration elected in November 2005, which suggests that the announcement was unexpected with respect to the set of information the population had in 2005.

and in two payments in July and November of 2008.

Second, the design of the program provides cross-sectional variation at each year based on the program's eligibility criterion. This variation is summarized in the columns from Table 1. In the first round of the program, children from first to fifth grade were eligible (children with 1 to 5 years of schooling in the sample<sup>17</sup>), thus they constitute the treatment group for the first round (2006). The control group are children from sixth to eighth grade (6 to 8 years of schooling). Due to the program's expansion, in the second round children in sixth grade enter the treatment group and in the third round, children in seventh and eighth grades are added to the treatment group. These variations suggest a difference-indifference approach that compares changes over time in the employment rates for parents of children in the treatment group before and after the program with changes over time in the employment rates for parents of children in the control group before and after the program.

In Section 8, I discuss two potential problems with my empirical approach. First, as younger children are more likely to induce different opportunity costs for parents' time than older children, I restrict the sample used in the main analysis to children between fourth and eighth grade. This sample selection is represented in bold letters in the entries in Table 1. Results using the whole sample (first-eighth grade) are presented in Appendix Table 9; these results do not differ from the main results of the paper. Second, the units of observation are children as treatment assignment is at that level. However, note that among the sample of students, it is possible that some treatment children have siblings in the control group; this implies a 40% reduction of the sample and therefore a loss of statistical power. I present the results for the entire sample, acknowledging that my estimates are likely to represent a lower bound. In Appendix Table A.4 I indeed show that the estimates are higher but noisier once I exclude children whose siblings are in a different treatment group.

### 5 Labor supply responses and the CCT program

In this section I provide evidence of positive treatment effects of the program on female employment through an event-study approach and a difference-in-difference approach. I interpret the result from both approaches as reduced form effects (intention-to-treat effects). Figure 2 shows cash reception rates after the program announcement. Compliance is high in this context for all the policy years.

Figures 3 and 4 show that there was an increase in the total number of hours/week dedicated to work by adults right after their children entered the treatment group. A similar pattern is observed for the total number of adults who report working during the week before the interview. Figure 5 shows that the hours dedicated to work and the proportion of adult females (heads of household or spouses) who report working during the week preceding the interview jump abruptly during the first period in which their children enter the treatment

<sup>&</sup>lt;sup>17</sup>For this study, preschool is not considered in the computation of years of schooling.

group. Work outcomes for adult males (heads of household or spouses) exhibit a smooth trajectory over time. These results suggest that there were increases in work outcomes for adult females as a consequence of the program. To test this hypothesis more rigorously, I estimate a flexible difference-in-difference model using the following specification:

$$Y_{ismt} = \alpha_0 + \mu_m + \delta_t + \theta_s + \sum_{j=-6}^{j=-2} \beta_j \mathbb{1}[\tau_{st} = j] + \sum_{k=0}^{k=4} \beta_k \mathbb{1}[\tau_{st} = k] + \epsilon_{ismt}$$
(1)

 $Y_{ismt}$  represents the work outcome of interest for the head of household or head's spouse from child *i*'s household.  $\theta_s$  denotes child *i*'s years of schooling fixed effects,  $\mu_m$  denotes municipality fixed effects and  $\delta_t$  denotes year fixed effects. Time to treatment is denoted by  $\tau_{st}$ . The omitted category is  $\tau_{st} = -1$  which denotes the year before a child with *s* years of schooling enters the treatment group. Standard errors are clustered at the municipality level.

Figures 6 and 7 plot the point estimates for  $\beta_j$  and their respective confidence intervals for work outcomes. Again, Figure 7 shows that there is a significant jump in the hours/week worked and employment status for adult females.

To assess the validity of the common trends assumption, I test two null hypotheses. First, I test whether the sum of the difference-in-difference coefficients  $\beta_j$  for the periods preceding the program is different than zero. Panel A in Table 3 shows that it is not possible to reject the null hypothesis of  $\beta_{-6} + \ldots + \beta_{-2} = 0$  for all the work outcomes. Complementarily, Panel B in Table 3 shows that it is not possible to reject the null hypothesis that all the difference-in-difference coefficients for periods preceding the program are jointly zero  $(\beta_{-6} = \ldots = \beta_{-2} = 0)$ .

To capture the average impact of the program for all the periods following the intervention and to increase statistical power, I estimate treatment effects following a standard differencein-difference approach:

$$Y_{ismt} = \alpha_0 + \mu_m + \delta_t + \theta_s + \beta T_{st} + X_{ismt} \gamma + \epsilon_{ismt}$$
<sup>(2)</sup>

Again  $Y_{ismt}$  denotes the outcome of interest.  $T_{st}$  is an indicator that takes the value of 1 for the periods in which children with s years of schooling enter the treatment group (i.e.  $\tau_{st} \geq 0$ ).

Table 4 presents treatment effects for working outcomes; the results are robust even after including group-specific linear time trends. Considering suggestive evidence of differential shocks between the treatment and control group in period  $\tau_{st} = -5$ , depicted in Figures 6 and 7), I explicitly include a dummy that takes the value of 1 whenever  $\tau_{st} = -5$ . Results are also robust to this specification and are presented in Appendix Table A.1.

Two results are worth considering: first, there is no evidence of negative effects on work outcomes. In most specifications it is possible to reject the null of negative treatment effects  $\beta < 0$  at 10%. In the case of work outcomes for adult males, the point estimates are

precisely estimated zeros. These results confirm evidence from previous studies of CCT programs (Alzua et al. (2013), Banerjee et al. (2015) and Skoufias and Maro (2008)).

More importantly, there is evidence of positive treatment effects for females both at the extensive and intensive margins. I find that the program increases the number of hours/week that female household heads report by 2.5 units and it induces an increase of 4 percentage points in the probability of being employed for female heads. These effects represent 9% of the baseline mean in the case of work hours (6% conditional on working) and 6% in the case of employment. The effects are small, and consistent with a small income shock induced by the CCT program. These results are also consistent with previous evidence found by Alzua et al. (2013) and Skoufias and Maro (2008) in the context of the *PROGRESA* program in Mexico for work hours for females. The results also complement suggestive evidence of positive effects on employment from the *Bolsa Familia* program in Brazil (de Brauw et al., 2015)<sup>18</sup>. Consistent with a context in which there is a large, stagnant gender gap in employment, the positive effect of the cash transfer program manifests in the most disadvantaged population: adult females who are household heads or heads' spouses.

#### 5.1 Cash or condition?

To have a better understanding of the nature of the shock and analyze the extent to which the increase in labor supply was driven by either the cash or condition component of the program, I test for heterogeneity in treatment effects based on how binding the condition component of the program was. Understanding which feature of the program induced the treatment effects observed in the previous section will shed light on the interpretation of the program as either an income shock (cash) or a relief of adult females' time constraint (condition). Evidence regarding the role of condition in CCT programs is mixed: de Brauw and Hoddinott (2011) and Filmer and Schady (2011) provide evidence of a stronger role of the condition component of these programs. Yet Baird et al. (2011) show that even an unconditional cash transfer (UCT) can induce changes in behavior in the direction intended by the condition component of CCT programs; Benhassine et al. (2015) show that simply labeling a UCT as a CCT is enough to encourage the intended behavior. In order to provide a deeper understanding of the type of shock induced by the program, I test whether the impact comes from parents of children for whom the condition component was binding or from parents of children who didn't modify behavior in order to receive the transfer.

The condition component of the program required that children attend 80% of school days during the school year in order to receive the transfer. To test whether the treatment effect comes from marginal or inframarginal agents, it would be ideal to compute treatment effects for children whose baseline attendance rate is below 80% (marginal agents) and for those whose baseline attendance rate is above 80%. Since the dataset in this study does

<sup>&</sup>lt;sup>18</sup>de Brauw et al. (2015) use a propensity score re-weighting approach that relies on selection on observable characteristics. The empirical approach in this study contributes with novel evidence from a natural experiment.

not follow children over time, I do not observe the attendance rate in the absence of the program or at baseline. Nevertheless, I use the 2004 round of the household surveys, a baseline year, to estimate a probit model for attendance using demographic characteristics. I then use the coefficients to predict the 2004 attendance rate for all the children in the study sample <sup>19</sup>.I interpret this predicted attendance rate as the average attendance rate a child would have, had the child been observed in the 2004 sample; this is a counterfactual baseline attendance rate.

The 2004 round of the survey is particularly useful for two reasons. First, the information was collected during the months of May to November of 2004, covering most of the school year. In other years, the household survey data was collected in December, once the school year had ended. Since the period of reference in the surveys is the week before the survey interview, most interviewees respond that children didn't attend school because of summer vacation.<sup>20</sup>. However, this is not the case in the 2004 wave as it covers a period that co-incides with the school year. Second, the 2004 wave provides information regarding school attendance based on several months rather than just a single month as opposed to the rest of the surveys. As the sample is random, for each child interviewed in month m of the 2004 wave, there is another similar child interviewed in the upcoming months; this means that this attendance rate also captures variation across months within the school year. Figure 8 depicts the distribution of the baseline counterfactual attendance rate above the condition.

Table 6 reports triple differences estimates using the predicted baseline attendance rate as a third difference (columns (1) and (4)). I interpret this third difference as a measure of the salience of the condition component in the program. For children with a low baseline attendance rate, the shock induced by the program is interpreted as a mix between cash and condition; for children with a high baseline attendance rate, the shock induced by the program is interpreted as a mix between cash school in the absence of the program. The results suggest that the treatment effects on work outcomes are an increasing function of the baseline attendance rate. The treatment effects evaluated at the 90th percentile of attendance rate are 3.4 hours (p-value=0.002) and 0.05 percentage points (p-value=0.001) for hours and the probability of work respectively. Estimates at the 10th percentile are very small and statistically not different from zero in both cases (see bottom panel of Table 6).

To test this hypothesis with higher power, I estimate a triple-difference model using an indicator of whether child *i*'s attendance rate is below the condition threshold (0.8, see columns (2) and (5)) and whether child *i*'s attendance rate is below the median (columns (3) and (6)). Results show that work outcomes for adults related to inframarginal children

<sup>&</sup>lt;sup>19</sup>The probit model was estimated using a full set of dummy variables regarding age and years of schooling; household indicators, including indicators for whether the household is located in a rural or urban area, the number of people in the household, and whether the head of household is male; children's characteristics such as gender; and indicators for speaking Spanish as a first language and whether the survey respondent self-identified as indigenous. Appendix Figure A.4 shows that the model has good out-of-sample prediction power across all the age categories.

<sup>&</sup>lt;sup>20</sup>For those children not on vacation, the average attendance rate is 98%.

are higher. The effects for marginal children are even null in the case of hours/week and not significant in the case of employment. In general, the positive impact on employment for adult females related to inframarginal children accounts for 90% of the overall treatment effect computed in Table 4. This result is not surprising as schooling outcomes were already high before the program was implemented. Moreover, the announcements of the implementation and expansion of the program were made once the school year was close to its end; for example, the first announcement was made in October 2006, a month before the school year was over, leaving reduced scope for behavior adjustment in order to meet the conditions. Appendix Table A.5 shows that there were no overall effects on employment for children and small effects on enrollment that vanish once I allow for group-specific trends. All together, the evidence suggests that the effects of the program on labor supply come mostly from an income shock.

#### 6 Dependence or constraints?

The results from the preceding sections contradict evidence from developed countries showing small negative responses in labor supply after exposure to cash welfare programs (Hoynes (1996) and Hoynes and Schanzenbach (2012)) and are consistent with evidence from developing countries that fails to find negative effects of cash transfer on adults' labor supply (Alzua et al. (2013), Banerjee et al. (2015)). In this section, I outline a simple framework that unifies these divergent results. I do so by referring to a traditional idea behind the process of development: In a context of imperfections in capital markets, economic performance (either prosperity or stagnation) depends on the initial wealth distribution (Banerjee and Newman, 1993). I sketch a stylized model for labor force participation that includes fixed costs to enter the labor force and frictions in capital markets. In this environment, there is selection into employment based on initial wealth. The model suggests three testable implications: i) an income shock can push people into the labor force, consistent with the evidence presented in the previous section; ii) the effects of an income shock should be bigger when capital market frictions are more salient; and, iii) the effects of an income shock should affect the decision to work and not the intensive margin of labor supply.

Consider a household composed of one individual deriving utility u from consumption  $c_i$ . For simplicity, let the utility function be  $u(c) = c_i^{21}$ . The household is endowed with initial wealth  $v_i$  and allocates hours of labor inelastically to the only possible job always available in this economy, receiving earnings equal to w. There is a cost  $p_i$  of entering the labor force. The timing is as follows: In period t = 0 the household decides whether to cover the fixed cost using its initial wealth  $v_i$  or borrowing  $a_i$ , using funds available in complete financial markets at a zero real interest rate. In period t = 1, conditional on its decision in period t = 0, the household maximizes utility subject to its budget constraint. I assume that if a household decides to cover the fixed cost, the household finds a job instantaneously. For example, this can be the case of self-employment. Let  $\lambda_i \in \{0, 1\}$  denote the decision of

<sup>&</sup>lt;sup>21</sup>I choose this utility function since I want to analyze a model for labor supply decisions at the extensive rather than at the intensive margin. Introducing a trade-off between consumption and leisure doesn't modify the main predictions of the model. Moreover, I assume that, conditional on working, agents behave according to the neoclassical model.

investing in the fixed cost. If the household decides to invest, then  $\lambda_i = 1$ ; if the household doesn't invest then  $\lambda_i = 0$ ; in the latter case, the household member stays outside the labor force.

This framework is consistent with several fixed costs or frictions discussed in the development economics literature and tries to capture heterogeneity in fixed costs over households. In some cases  $p_i$  can be the market value of the minimum caloric intake necessary to conduct a task and be chosen by employers as in Dasgupta and Ray (1986). Alternatively,  $p_i$ could represent the cost of attaining the minimum consumption of comfort goods that are necessary for women to focus on working rather than exclusively on household chores as in Banerjee and Mullainathan (2008). Alternatively,  $p_i$  could represent the cost of sending children to preschool and therefore free up time to be allocated to labor (Cascio, 2009). In contexts of high salience of self-employment,  $p_i$  could represent the value of capital necessary for agriculture or a family business as in de Mel et al. (2008) or Blattman et al. (2014). Fixed costs can also be present outside self-employment;  $p_i$  could represent the price of a bus ticket in the context of seasonal migration (Bryan et al., 2014). Fixed costs can be nonpecuniary:  $p_i$  could represent the cost of paperwork to obtain land/house titles, as the absence of title could result in lack of labor force participation (Field, 2007).

The household maximizes:

$$egin{array}{ll} \max_{c,a,\lambda} & u(c_i) = c_i \ {
m s. t.} & c_i = w - a_i & {
m if } \lambda = 1 {
m and } t = 1 \ c_i = v_i & {
m if } \lambda = 0 {
m and } t = 1 \ v_i + a_i = p_i & {
m if } t = 0 \ c_i > 0 \end{array}$$

Using backward induction, the household will decide to invest in the fixed cost and therefore work if and only if  $w \ge p_i$ . In this setting, even with frictions in the labor market, working decisions do not depend on initial wealth. Note, however, that with heterogeneity in fixed costs, households that face higher fixed costs will only work if wages are high enough to make it profitable. For instance, in an economy with higher fixed costs for females, there would be a higher employment rate for males at the same market wage. This is consistent with the Bolivian gender gap in employment as discussed in Section 2 (see Figure 1).

Consider now an environment in which there are intermediation costs for the lender that lead to a risk premium over the interest rate that the household head would earn when depositing her money in a savings account or investing in a risk-free asset. Denote this premium as r. Note that now the household can either decide to self-finance the fixed cost and invest the remaining funds in a zero-real-interest-rate, riskless asset or borrow some money from either a bank or an informal lender at rate r > 0. There are no exogenous credit constraints in this economy but there is a spread between lending and saving interest rates that reflects potential frictions in the credit market. In period t = 0 the household faces the same budget constraint but depending on whether  $a_i > 0$  or not, the household member will face different budget constraints in period t = 1.

$$egin{array}{ll} c_i = w - a_i(1+r) & ext{if } \lambda_i = 1 ext{, } t = 1 ext{ and } a_i > 0 \ c_i = w - (v_i - p_i) & ext{if } \lambda_i = 1 ext{, } t = 1 ext{ and } a_i \leq 0 \ c_i = v_i & ext{if } \lambda_i = 0 ext{ and } t = 1 \ v_i + a = p_i & ext{if } t = 0 \end{array}$$

Suppose household *i* faces fixed costs  $p_i < w$  and is endowed with an initial wealth  $v_i \ge p_i$ . As borrowing and self-financing are perfect substitutes, this household picks the least expensive option: self-financing. On the other hand, if  $v_i < p$  the household can only cover the fixed cost by borrowing at rate r in an amount equal to  $a_i = p_i - v_i$ . Consider now a household with a high initial wealth  $v_H$  such that  $v_H \ge \tilde{p}$ . This household enters the labor force if and only if  $w \ge \tilde{p}$ . Thus, this household lives in a context where financial market frictions are not salient. However, the story is different for a household facing the same wages (w) and fixed costs  $(\tilde{p})$  but with low initial wealth  $v_L$  such that  $v_L < \tilde{p}$ . In order to work, this household has to finance the fixed cost by borrowing at a rate r. This means that this household will only work if  $w \ge \tilde{p} + (\tilde{p} - v_L)r$ .

Let  $\bar{w}_H = \tilde{p}$  and  $\bar{w}_L = \tilde{p} + (\tilde{p} - v_L)r$  denote the reservation wage corresponding to households with high and low income, respectively. Since  $\tilde{p} - v_L > 0$ , we have that  $\bar{w}_L > \bar{w}_H$ . This means that households with lower wealth need a higher market wage in order to decide to work. This difference arises because of the interaction of frictions in the labor market (fixed costs p) and frictions in the financial market r > 0. In this case poor households have low consumption levels because they can't work, and they can't work because they are simply too poor. Minimal assumptions were needed to generate the possibility of a poverty trap: as in Banerjee and Newman (1993), economic performance, either prosperity or stagnation, depends on where in the distribution of initial wealth a household is located. In this environment, there are three testable predictions from the model.

Prediction 1: A positive income shock can increase the probability of working. Consider a shock  $\epsilon_i$  such that  $\epsilon_i \geq p_i - v_L$ . This income shock pushes the new income  $v'_i = v_L + \epsilon_i$  above the fixed cost. In this case, poor agents can self-finance its entrance to the work force and will work as long as the market wage w is greater than the fixed cost. This income shock pushes the household from an equilibrium of involuntary unemployment to one with employment. This prediction is consistent with the results found in Section 5: An income shock can push people into the labor force. However, note that this effect has a local nature as only the households for whom the income shock is large enough to cover the gap between their fixed costs and wealth endowments will be pushed into the labor force (individuals at the margin); less fortunate households will face binding constraints even after the shock.

Heterogeneity in wealth and fixed costs could explain some stylized facts in the empirical literature on CCTs. Emblematic CCT programs aim to help the most disadvantaged part of the population. In particular, means-testing or proxy-means-testing mechanisms are popular targeting tools<sup>22</sup>. To the extent that these programs effectively target the least advantaged population (i.e., the ones with higher  $p_i - v_i$ ) it could be the case that studies of the impact of CCT programs on labor supply fail to find effects on employment as, given an income shock, the gap between wealth and fixed costs is simply too large. In this study, eligibility for the program is fairly orthogonal to wealth and fixed costs as its design does not involve a means-tested targeting mechanism; therefore the evaluation captures the entire distribution of  $p_i - v_i$ . In the same spirit, Blattman et al. (2014) find positive impacts of random assignment of grants on work hours among program applicants. Although poor by any metric, the applicants in that study were better off than the average agents in Uganda. These agents were credit-constrained and proposed start-ups in sectors with low fixed costs; in other words, these were agents for whom  $p_i - v_i$  was low.

Prediction 2: The effect of an income shock  $\epsilon_i$  should be higher when there are borrowing constraints. Despite evidence supporting the role of informal sources of credit in replacing formal institutions as a risk-sharing tool (Besley et al. (1993), Kinnan and Townsend (2012), Angelucci and De Giorgi (2009)), the hypothesis of perfect consumption smoothing is generally rejected Townsend (1994). This suggests that some households are creditconstrained even when there are informal credit markets. Consider the case of a household with non-labor income  $v_L$  such that  $v_L < p_i$ . This household would borrow from the bank or informal lender if  $\bar{w}_L = p_i + (p_i - v_L)r$ . Let  $\bar{a} > 0$  denote the maximum amount a household can get from the informal lender. This household solves:

$$egin{array}{lll} \max & u(c) = c_i \ {
m s. t.} & c_i = w - a_i(1+r) & {
m if } \lambda = 1 \ , \, t = 1 \ c_i = v_L & {
m if } \lambda = 0 \ {
m and } t = 1 \ v_L + a_i = p_i & {
m if } t = 0 \ a \leq ar a_i \ c < 0 \end{array}$$

In the interior solution, when the credit constraint is not binding, this household uses the same decision rule as in the unconstrained case and there is still selection into employment arising from the interaction of fixed costs and other frictions in capital markets. Moreover, when credit constraints bind, although it is profitable to work, the household member won't be able to work because of her inability to cover the fixed cost. In a context of credit constraints the problem households face is even more complicated: Even if r is small, households that would like to borrow at the current rate wouldn't be able to borrow optimally; those households facing a credit constraint  $\bar{a}_i$  such that  $v_L + \bar{a}_i < p_i$  will not work. However, note that an income shock  $\epsilon_i$  such that  $v_L + \epsilon_i + \bar{a}_i = p_i$  will push households into the labor force. In this model, both types of financial frictions interact with

<sup>&</sup>lt;sup>22</sup>Fiszbein et al. (2009) provide a comprehensive summary of targeting mechanisms for CCT programs. Large-scale programs such as PROGRESA and BOLSA FAMILIA follow this approach.

labor market frictions and yield a result in which households sort into the labor force based on their initial wealth. The increase in the effect of an income shock comes from households who find it profitable to borrow at rate r but can't borrow as much as they would like.

Prediction 3: Income shocks should affect labor supply positively only at the extensive margin. So far, the model sketched in this section doesn't consider labor supply at the intensive margin. This approach was chosen in order to focus only on corner solutions. Interior solutions in a model with a trade-off between consumption and leisure should behave as in the neoclassical model once the agent decides to work; conditional on working, a household chooses how many hours to work, equalizing marginal rates of substitution between consumption and leisure with the real market wage. In this context, an income shock has non-increasing effects on hours worked. Note, however, that a positive effect on hours worked can be observed in a richer model in which time off work can be productive for household consumption as in Becker (1965); in this case, the positive effect requires that households substitute away from time-intensive goods.

#### 6.1 Testing the implications of the model

#### 6.1.1 Labor supply and fixed costs to work

In this section, I test for the salience of fixed costs to enter the labor force. I do this in two steps. First, I show that despite finding effects at both the intensive and extensive margin of work for adult females, the effects come mainly from responses at the extensive margin. Second, I show that these effects are associated with increases in the probability of being self-employed due to the program, suggesting that the responses in employment come from small businesses, a sector that faces small but salient fixed costs.

The theoretical framework sketched in this paper suggests that the impacts of an income shock should be related to the extensive margin of labor supply rather than the intensive margin, as I assume that once the decision to work is taken, the agents behave according to a neoclassical model. So far, the results presented in Table 5 show significant impacts on hours/week worked by females. Yet the measure of work hours includes zeros for females who do not work. Although fixed costs are unobserved and heterogeneous, if they are salient they should manifest in the labor supply responses to an income shock only at the bottom of the distribution of work hours. To empirically test this hypothesis, I estimate treatment effects along the cumulative distribution function of work hours.

Let  $H_i$  denote the hours worked weekly by child *i*'s mother. Let  $Y_i^x$  be an indicator function  $Y_i^x = 1[h_i > x]$  denoting whether child *i*'s mother reported working more than x hours the week before the interview ( $x \in [0, \bar{h}]$ ).

$$Y_{ismt}^{x} = \alpha + \mu_m + \delta_t + \theta_s + \beta(x)T_{st} + X_{ismt}\gamma + e_{ismt}$$
(3)

The parameter of interest is  $\beta(x)$ , which represents the difference-in-difference estimate for the ITT effect on the cumulative density function of hours/week worked evaluated at x. If

there are fixed costs to enter the labor force, then treatment effects should only manifest through the extensive margin. Formally, this means that  $\beta(x)$  is a non-increasing function of x with  $\beta(0)$  as intercept. Figure 9 plots the estimated coefficients  $\hat{\beta}(x)$  from (3) against x for the case of adult females. Note that the treatment effects, for most values of x, are significant and constant at  $\beta(0)$ . Although there are some increases around  $x = 20^{-23}$ , the biggest jump in the treatment effects comes at the bottom of the distribution of working hours, confirming the fixed-cost hypothesis. This result complements evidence from recent literature that analyzes variation in particularly salient fixed costs to work such as Bryan et al. (2014) and Field (2007). This paper identifies a complementary comparative statics exercise; instead of reducing fixed costs, it modifies non-labor wealth and reduces the salience of these fixed costs.

The fixed-costs to work hypothesis suggests that the positive effects on work outcomes should come from a measure of labor markets deeply related to business activity: selfemployment. Table 5 provides evidence of positive treatment effects of the cash transfer program on self-employment for adult females (heads of household or head's spouse). These effects are not related with work inside the household. The dependent variable is an indicator function that takes the value of 1 for self-employed females and 0 for unemployed females; it measures the transition from unemployment to self-employment. As the cash transfer relieves liquidity constraints, this finding complements mild positive results on self-employment and business start-up from interventions expanding the supply of microcredit (Banerjee et al., 2015) (Kaboski and Townsend, 2012). Moreover, previous evidence from Mexico (Gertler et al., 2012) shows that the long-term gains in consumption due to the OPORTUNIDADES program can be explained by an increase in productive investment induced by the program. The increases in employment for females mostly related to self-employment complements these long-term results with short-term responses in labor supply. Similarly, this set of results complements recent evidence on increases in work hours due to a random allocation of grants to groups of credit-constrained start-up applicants with projects with low fixed costs as in Blattman et al. (2014). Overall, if the process of development is about a reallocation of resources from subsistence agricultural production to entrepreneurship, moving people from unemployment to self-employment could be the first step in that process.

#### 6.1.2 Labor supply responses and credit markets

To test whether the impact of the program is higher for individuals who are either more credit-constrained or face stronger credit market imperfections, I estimate a triple-difference model that extends the difference-in-difference model from equation (2) by including a third source of variation: the number of financial institution branches per 100,000 individuals in each municipality at baseline. These data are only available for municipalities that are provincial capitals (112 out of 339 municipalities), however two-thirds of my sample belong to these localities. I interpret this cross-municipality variation as a shift in credit market imperfections: Areas with low supply of financial services have a limited set of financing

<sup>&</sup>lt;sup>23</sup>These extra increases at x = 20 are consistent with a context of under-employment or agents overcoming fixed costs for a second occupation.

options for local households, leading to higher credit constraints; they also exhibit less competition for informal lenders, allowing repayment rates to be potentially higher. Columns (1) and (4) from Table 7 report triple-difference estimates for hours/week and the probability of working the week prior to the interview. The results show that the effect is higher for females in areas with high credit-market imperfections.

To show that heterogeneity in treatment effects does not come from the fact that rural areas are more credit-constrained than urban areas, I estimate a model that includes a full set of interactions between rural-urban dummies, years of schooling and years fixed effects: a triple-difference coefficient using urban-rural dummies. Columns (3) and (6) show that even accounting for potential treatment-effect heterogeneity across urban and rural areas, the negative slope with respect to access to financial services remains strong and hence the results are not simply driven by treatment-effect heterogeneity due to geography. The results in this paper suggest that the cash transfers were more salient for households that were more likely to face credit constraints. These results complement evidence from Blattman et al. (2014); in that study, the pool of potential beneficiaries of the cash grant, although credit-constrained, was very homogeneous, providing little variance in terms of credit constraints. In this study, although I don't observe baseline cash holdings, the cross-municipality variation allows me to capture significant treatment-effect heterogeneity based on credit-market imperfections.

#### 7 Potential alternative mechanisms

In this section I discuss alternative mechanisms that could explain the positive labor supply responses to the program; I also discuss the plausibility of these channels given the evidence found in the empirical exercises presented in this study. I present two alternative explanations: an aggregate demand mechanism induced by the injection of cash into the local economy and the relaxation of adult females' time constraints due to the condition component of the program.

One particular challenge in interpreting the reduced-form treatment effects from studies that evaluate the impact of large-scale cash transfer programs is the presence of general equilibrium effects that are confounded with direct income shocks on treatment units (Acemoglu, 2010). The Bolivian program, despite providing a small transfer to each beneficiary child, injected money into the local economy in a short period of time. If this transfer increased aggregate demand in the local economy and hence wages, then it could be the case that some agents decided to work at that higher wage. This mechanism has been documented in the development economics literature that analyzes general equilibrium effects after large-scale interventions<sup>24</sup>. However, the nature of the shock studied in this paper differs from the shocks induced by other CCT interventions analyzed by Alzua et al. (2013)

<sup>&</sup>lt;sup>24</sup>Alzua et al. (2013) find positive effects of the *PROGRESA* CCT program on wages for males. Similarly, Kaboski and Townsend (2012) and Muralidharan et al. (2016b) find increases on wages after the implementation of the Million Baht Fund program in Thailand and a large-scale public works program (NREG) in Andha Pradesh, India, respectively.

and Banerjee et al. (2015): in those studies the treatment is randomly assigned across clusters and their estimates are based on cross-cluster comparisons. In this study, the treatment effects are identified using arguably exogenous individual and time series variation within clusters, as both specifications in equations (1) and (2) include municipality fixed effects. This means that potential effects through prices are isolated as comparisons are performed within clusters. If there was an increase in wages, this increase affected the treatment and control groups similarly. Moreover, if the effects were driven by increases in wages, then households who are less exposed to credit-market imperfections should be better able to respond as they can borrow to cover the fixed cost of working. The evidence found in Section 6.1.2 (see Table 7) suggests the opposite, as the treatment effect is a decreasing function of the degree of credit-market imperfections.

Second, since the program's main objective was to increase attendance and enrollment among the children who were the beneficiaries, the increase in labor supply for adult females could be explained by the relief of a time constraint rather than an income shock. Two pieces of evidence from this study suggest that this may not be the case. First, the positive treatment effects are driven by beneficiaries who would have attended school even in the absence of the program as discussed in Section 5.1. Second, after controlling for differential trends, I can't find evidence supporting increases in enrollment due to the program. Appendix Table A.5 shows difference-in-difference estimates of the program on the probability of enrolling in school the year after each cohort was exposed and the probability of working the year the transfer was disbursed. The evidence suggests that there were not effects on outcomes for children.

### 8 Robustness checks and methodological issues

In this section I discuss two empirical challenges and conduct two robustness checks that rule out potential threats to my identification strategy and my results. First, the main analysis includes children from fourth to eighth grade only, excluding younger children as they may have differential trends arising from differential opportunity costs for parents' time. In Section A.2 of the appendix, I replicate the main graphical evidence from this study but including younger children (see figures A.1 and A.2). Regression results using the whole sample (first-eighth grade) are presented on Appendix Table A.2. The results are fairly similar in all of the specifications. Note that in this case, I am able to detect significant increases in total household labor supply, measured by the total number of work hours for all adults in the household (See panel A in Appendix Table A.2).

Second, since treatment assignment is at the child level, the units of observation in my dataset are children. However, note that among the sample of students, it is possible that some treatment children have siblings in the control group; this would imply that data for their parents is counted both in the treatment and control group. This could be a source of downward bias of the estimates. Since excluding children with siblings with differential treatment status implies reducing the sample by 40% with the resulting loss of statistical power, I present the results for the entire sample, acknowledging that my estimates are

likely to be a lower bound. In Section A.3 of the Appendix (Table A.4) I show that the estimates are higher but noisier once I exclude children whose siblings are in a different treatment group.

#### 9 Concluding remarks and discussion

This paper analyzes whether positive income shocks can cause increases in labor supply using a large-scale conditional cash transfer program implemented through Bolivia's public schools. Contrary to predictions from the neoclassical model and the evidence from cash welfare programs in developed economies, I find that an income shock can push people into the labor force. In particular, I find that this is so for adult females, either household heads or heads' spouses. This result is consistent with systematic evidence from CCT programs in developing countries of non-negative income labor supply elasticities (Alzua et al. (2013) and Banerjee et al. (2015)). I also find evidence that the positive impact of the program on adult females' labor supply comes from women whose children would have attended school in the absence of the program, suggesting that the cash rather than the condition component of the program explains the effects. This result rules out responses in labor markets due to the relief of time constraints for adults.

To understand the economics behind these results, I provide a simple explanation that unifies the results from developed and developing countries. Once I introduce fixed costs to enter the labor force and credit-market imperfections that lead to either high repayment interest rates or borrowing constraints into a stylized labor-force participation model, selection into employment is based on initial wealth. In this environment, two equilibria are present in the economy: one in which agents are rich enough to self-finance the fixed costs to work and another in which the agents are simply too poor to work-a poverty trap. In this context, an income shock can move agents from an equilibrium with involuntary unemployment to one with employment, consistent with the main result of this paper. I find that the program increased the probability of working by 4 percentage points and the weekly hours worked by 2.5 hours for female household heads. These effects are associated with similar impacts on self-employment, a sector with fixed costs. The effects are small as the income shock is small, and are consistent with the theoretical approach in this paper suggesting that the effects come from agents at the margin.

Why do labor supply studies in developed countries find negative income elasticities, but this is not the case for developing countries? The theoretical framework developed in this study suggests that if agents don't face fixed costs and credit constraints, then their behavior should be consistent with the neoclassical model. This should be the case for countries that are far along in the process of development. However, the reality may be quite different in countries that are further down the ladder in this process. Underdevelopment comes with strong barriers to work and credit markets that are far from perfect. When cash aid reaches agents in this environment, some agents may use that money to cover basic needs, while others will find the extra liquidity needed to begin moving out of poverty. As discussed in the theoretical framework, those agents who are lucky enough to be close to covering their fixed costs will exhibit positive labor supply responses.

Why then have other studies in developing countries not found positive effects of income on labor supply? The theoretical framework proposed and tested in this paper and the design of the program provide insights for understanding why other studies were not able to find positive responses. Successful, emblematic CCT programs are means-tested and therefore affect a particularly disadvantaged share of the population. If the households that can take advantage of the income shock are only those that are close to covering their fixed costs, as the theoretical framework suggests, programs that focus exclusively on the most disadvantaged agents will fail to capture agents who would potentially use the extra resources as a tool to escape involuntary unemployment. Studies such as Alzua et al. (2013) and Banerjee et al. (2015) focus on contexts in which the program beneficiaries are simply too poor to take advantage of the shock. This same logic also explains why Blattman et al. (2014) find strong effects on work hours; they targeted agents who despite being poor according to several metrics, are better off than most of the Ugandan population. The Bolivian program studied in this paper is not means-tested and reaches around 90% of children of school age, capturing the entire distribution of fixed costs and credit constraints. This provides an empirical approach with enough power to capture positive responses in labor supply due to the program.

Altogether, the results suggests that an apparent trade-off between immediate poverty reductions and long-term poverty alleviation might not be salient in contexts of fixed costs to work and credit constraints, two key features of developing economies. This potential trade-off would arise from dependence generated by these income transfers; nonetheless, the results suggest that constraints rather than dependence may explain vicious circles of poverty. Consistent with recent evidence regarding investments in human capital and skills after winning cash grants (Blattman et al., 2014) and long-term improvements in consumption driven by agricultural investment in Mexico (Gertler et al., 2012), the results suggest that the first step to climbing the ladder of development is overcoming the barriers households face to simply start working.

#### References

- Acemoglu, D. (2010). Theory, general equilibrium, and political economy in development economics. The Journal of Economic Perspectives 24(3), 17-32.
- Alzua, M. L., G. Cruces, and L. Ripani (2013). Welfare programs and labor supply in developing countries: experimental evidence from latin america. Journal of Population Economics 26(4), 1255-1284.
- Angelucci, M. and G. De Giorgi (2009). Indirect effects of an aid program: How do cash transfers affect ineligibles' consumption? *American Economic Review 99*(1), 486-508.
- Baird, S., C. McIntosh, and B. Ozler (2011). Cash or condition? evidence from a cash transfer experiment. The Quarterly Journal of Economics 126(4), 1709-1753.
- Banerjee, A., R. Hanna, G. Kreindler, and B. Olken (2015, December). Debunking the Stereotype of the Lazy Welfare Recipient: Evidence from Cash Transfer Programs Worldwide. Working Paper Series 076, Harvard Kennedy School.
- Banerjee, A., D. Karlan, and J. Zinman (2015, January). Six randomized evaluations of microcredit: Introduction and further steps. American Economic Journal: Applied Economics 7(1), 1-21.
- Banerjee, A. V. and S. Mullainathan (2008). Limited attention and income distribution. American Economic Review 98(2), 489-93.
- Banerjee, A. V. and A. F. Newman (1993, April). Occupational Choice and the Process of Development. *Journal of Political Economy 101*(2), 274-98.
- Becker, G. S. (1965). A theory of the allocation of time. *The Economic Journal* 75(299), pp. 493-517.
- Benhassine, N., F. Devoto, E. Duflo, P. Dupas, and V. Pouliquen (2015). Turning a shove into a nudge? a "labeled cash transfer" for education. American Economic Journal: Economic Policy 7(3), 86-125.
- Besley, T., S. Coate, and G. Loury (1993). The economics of rotating savings and credit associations. The American Economic Review 83(4), pp. 792-810.
- Blattman, C., N. Fiala, and S. Martinez (2014). Generating skilled self-employment in developing countries: Experimental evidence from uganda. The Quarterly Journal of Economics 129(2), 697-752.
- Bryan, G., S. Chowdhury, and A. M. Mobarak (2014). Underinvestment in a profitable technology: The case of seasonal migration in bangladesh. *Econometrica* 82(5), 1671– 1748.
- Cascio, E. U. (2009). Maternal labor supply and the introduction of kindergartens into american public schools. Journal of Human Resources 44(1), 140-170.

- Dasgupta, P. and D. Ray (1986). Inequality as a determinant of malnutrition and unemployment: Theory. *The Economic Journal 96*(384), pp. 1011-1034.
- de Brauw, A., D. O. Gilligan, J. Hoddinott, and S. Roy (2015). Bolsa família and household labor supply. *Economic Development and Cultural Change* 63(3), 423-457.
- de Brauw, A. and J. Hoddinott (2011). Must conditional cash transfer programs be conditioned to be effective? the impact of conditioning transfers on school enrollment in mexico. Journal of Development Economics 96(2), 359 - 370.
- de Mel, S., D. McKenzie, and C. Woodruff (2008, November). Returns to Capital in Microenterprises: Evidence from a Field Experiment. The Quarterly Journal of Economics 123(4), 1329-1372.
- Field, E. (2007). Entitled to work: Urban property rights and labor supply in peru. The Quarterly Journal of Economics 122(4), 1561-1602.
- Filmer, D. and N. Schady (2011). Does more cash in conditional cash transfer programs always lead to larger impacts on school attendance? Journal of Development Economics 96(1), 150 - 157.
- Fiszbein, A., N. Schady, F. H. Ferreira, M. Grosh, N. Keleher, P. Olinto, and E. Skoufias (2009). Conditional Cash Transfers : Reducing Present and Future Poverty. Number 2597 in World Bank Publications. The World Bank.
- Gertler, P. J., S. W. Martinez, and M. Rubio-Codina (2012, January). Investing cash transfers to raise long-term living standards. *American Economic Journal: Applied Economics* 4(1), 164-92.
- Gollin, D. (2014, August). The lewis model: A 60-year retrospective. Journal of Economic Perspectives 28(3), 71-88.
- Hoynes, H. W. (1996). Welfare transfers in two-parent families: Labor supply and welfare participation under afdc-up. *Econometrica* 64(2), 295-332.
- Hoynes, H. W. and D. W. Schanzenbach (2012). Work incentives and the food stamp program. Journal of Public Economics 96(1-2), 151 162.
- Kaboski, J. P. and R. M. Townsend (2012, April). The impact of credit on village economies. American Economic Journal: Applied Economics 4(2), 98-133.
- Kinnan, C. and R. Townsend (2012). Kinship and financial networks, formal financial access, and risk reduction. The American Economic Review, 289-293.
- Kraay, A. and D. McKenzie (2014). Do poverty traps exist? assessing the evidence. Journal of Economic Perspectives 28(3), 127-48.
- Lewis, W. A. (1954). Economic development with unlimited supplies of labour. The Manchester School 22(2), 139-191.

- Muralidharan, K., P. Niehaus, and S. Sukhtankar (2016a). Building state capacity: Evidence from biometric smart cards in india. *American Economic Review Forthcoming*.
- Muralidharan, K., P. Niehaus, and S. Sukhtankar (2016b, May). Smartcards: Identifying General Equilibrium Effects from Randomized Controlled Trials. Technical report, University of California, San Diego.
- Shultz, P. T. (2004, June). School subsidies for the poor: evaluating the Mexican Progresa poverty program. Journal of Development Economics 74(1), 199-250.
- Skoufias, E. and V. D. Maro (2008). Conditional cash transfers, adult work incentives, and poverty. The Journal of Development Studies 44(7), 935-960.
- Townsend, R. M. (1994, May). Risk and Insurance in Village India. *Econometrica* 62(3), 539-91.
- Zambrana, O. M., G. Z. Cruz, F. A. Velasco, A. C. Callisaya, and E. M. Idiáquez (2004). LA EDUCACIÓN EN BOLIVIA INDICADORES, CIFRAS Y RESULTADOS (2nd ed.). Ministerio de Educación Bolivia.

## 10 Figures

#### 10.1 Gender gap in employment



Figure 1: Gender disparities in employment

The figure depicts employment rates for female and male heads of household or heads' spouses, on the left axis. Employment rate is measured as the share of people of working age who report having worked the week prior to the survey interview.

#### 10.2 Treatment compliance



The figure shows the proportion of beneficiary children who report having received the transfer for each year before and after the exposure of child i to the program. Time to treatment is equal to 0 in the first period in which treatment kicks in. Uptake rates are computed based on self-reported information regarding the year preceding the survey interview.

#### 10.3 Household labor market participation before and after the program



Figure 3: Total hours worked (per week) - household adults

The figure depicts means for the total weekly hours worked by adults in child i's household before and after child i is exposed to treatment. Time to treatment is equal to 0 in the first period in which treatment kicks in.





The figure depicts means for the number of employed adults in child i's household before and after child i is exposed to treatment. Time to treatment is equal to 0 in the first period in which treatment kicks in.



Figure 5: Employment and hours worked (weekly) for adults

The top panels depict employment rate for adult males (heads of household or spouses) and adult females (heads of household or spouses) in child i's household before and after child i is exposed to treatment. The bottom panel depicts weekly hours for both adult males and females. Time to treatment is equal to 0 in the first period in which treatment kicks in.

### 11 Flexible difference-in-difference estimates





The figure depicts OLS coefficients from equation (1). Left-hand panel: Ech coefficient estimates differences in differences on hours worked by adults between the treatment and control group with respect to the period just before the program was implemented ( $\tau = -1$ ). The dependent variable measures the total number of hours worked by adults in child *i*'s household. Standard errors are clustered at the municipality level. Right-hand panel: Each coefficient estimates differences in differences on adult employment between the treatment and control group with respect to the period just before the program was implemented ( $\tau = -1$ ). The dependent variable measures of adults employment between the treatment and control group with respect to the period just before the program was implemented ( $\tau = -1$ ). The dependent variable measures the number of adults employed in child *i*'s household. Standard errors are clustered at the municipality level.



Figure 7: Treatment effects on employment and work hours for adults

The figure depicts OLS coefficients from equation (1). Each coefficient estimates differences in differences on the relevant measure of labor supply between the treatment and control group with respect to the period just before the program was implemented ( $\tau = -1$ ). The top panel depicts effects on the probability of working, the bottom panel depicts effects on weekly work hours. The plots on the left are the results for adult males while those on the right are results for adult females. Standard errors are clustered at the municipality level.

#### 11.1 Counterfactual attendance



The figure plots the cumulative probability function for the counterfactual attendance rate. The vertical line denotes the cutoff determined by the condition component of the CCT program, while the horizontal line denotes the proportion of the sample located below the condition cutoff.

#### 11.2 Treatment effects along the distribution of work hours



Figure 9: Treatment effects on the CDF of weekly work hours for adult females

The figure depicts treatment effects estimated through OLS based on (3). Each coefficient estimates differences in differences on the probability of working at least x hours between adult females belonging to households from treated children and control children, before and after the program. Standard errors are clustered at the municipality level.

#### 12 Tables

#### **12.1** Summary statistics

	N	Mean	Std.Dev	$\mathbf{Min}$	Max		
Panel A: Work Outcomes	s (Adu	ılts - Ho	usehold)				
Total hours/week	2520	76.51	46.68	0	211		
Number of adults who worked last week	2556	1.69	0.89	0	4		
Number of self-employed adults	2536	0.69	0.64	0	2		
Number of adults working at home	2534	0.05	0.22	0	1		
Panel B: Work Outcomes (Female h	ouseh	old head	ls / heads'	spous	es)		
Total hours/week	2397	26.27	25.54	0	84		
Total hours/week (conditional on working)	1566	40.20	20.92	1	84		
Worked last week	2417	0.66	0.48	0	1		
Self-employed	2417	0.29	0.45	0	1		
Works at home	2417	0.05	0.22	0	1		
Panel C: Work Outcomes (Male household heads / heads' spouses)							
Total hours/week	2090	47.35	21.41	0	91		
Total hours/week (conditional on working)	1977	50.06	18.69	2	91		
Worked last week	2119	0.95	0.22	0	1		
Self-employed	2119	0.47	0.50	0	1		
Works at home	2119	0.02	0.14	0	1		
Panel D: Work/Schooling Outcom	mes (c	hildren -	- 7-18 year	rs old)			
Total hours/week	2560	6.88	14.27	0	60		
Total hours/week (conditional on working)	729	24.15	17.28	2	60		
Worked last week	2560	0.28	0.45	0	1		
Enrolled in school	2560	0.91	0.28	0	1		
Panel E: Household	d Char	racteristi	ics				
Urban Area	2560	0.51	0.50	0	1		
Self-identified as Indigenous	2560	0.63	0.48	0	1		
Spanish as first language	2119	0.54	0.50	0	1		
Number of household members	2560	5.93	2.12	1	18		
Number of adults in household	2560	2.27	1.06	0	9		
Number of children under 5 in household	2560	0.62	0.85	0	5		

Table 2:	Summary	Statistics	at	Baseline
----------	---------	------------	----	----------

Note: The table presents summary statistics for children with 4 to 8 years of schooling as of 2005, the year preceding the program. Panel A presents statistics regarding aggregate data at the household level for household members older than 18. Panels B and C present statistics for the household head or spouse in the case of adult females and males, respectively. Panel D reports information regarding children between 7 and 18 years old. The variables regarding employment are computed based on

indicators of whether or not each person in the household reported working in the week before the interview. Hours worked are computed with self-reported information regarding the average number of working hours per day and the average number of days worked in the week before the interview.

#### 12.2 Testing for parallel pre-trends

	(1)	(2)	(3)	(4)	(5)	(6)
	Total	Adults	Adult l	Females -hh heads	Adult M	ales - hh heads
	Hours	Works	Hours	Works	Hours	Works
	Pane	$I A: H_0:$	$eta_{-6}+eta$	$\beta_{-5} + + \beta_{-2} = 0$		
$\hat{eta}_{-6}+\hat{eta}_{-5}++\hat{eta}_{-2}$	-4.50	0.03	-0.47	0.15	<b>-</b> 4.84	-0.02
Fstat	0.27	0.03	0.01	3.02	2.43	0.16
Pval	0.61	0.86	0.93	0.18	0.30	0.69
	Pa	anel B: H	$I_0:eta_{-6}=$	$= = eta_{-2} = 0$		
Fstat	2.00	1.46	1.26	1.54	1.22	1.05
Pval	0.08	0.20	0.28	0.08	0.12	0.39

#### Table 3: Testing for parallel trends

The table presents tests for common pre-trends between treatment and control groups based on the flexible difference-in-difference model described in (1). Standard errors are clustered at the municipality level. Panel A tests the null hypothesis that the sum of all the coefficients capturing differential trajectories between the control and treatment groups from each year preceding the implementation of the program with respect to the year preceding entrance to treatment. Panel B, tests the null hypothesis that all pre-trend coefficients are jointly equal to zero.

Table 4:	Difference	e-in-differe	ence estin	nates on e	mployme	nt
	(1)	(2)	(3)	(4)	(5)	(6)
Par	nel A: Wo	rk Outcon	nes (Adul	ts - House	hold)	
	Tot	tal hours/w	eek	Total	working a	dults
TE (DD)	1.938	1.950	2.200	0.019	0.013	0.012
	(1.732)	(1.533)	(1.638)	(0.036)	(0.030)	(0.032)
Observations	18,194	$17,\!434$	17,434	18,309	17,543	17,543
R-squared	0.008	0.160	0.161	0.006	0.250	0.250
Mean DV	79.37	79.37	79.37	1.732	1.732	1.732
Panel B: Worl	k Outcom	es (Female	e househol	ld heads $/$	heads' sp	oouses)
		Hours/weel	2	Wo	rked last w	eek
TE (DD)	2.591***	2.507***	2.336***	0.039***	0.039***	0.034**
	(0.751)	(0.715)	(0.804)	(0.014)	(0.014)	(0.016)
Observations	$17,\!459$	$17,\!450$	17,450	17,687	17,678	17,678
R-squared	0.011	0.095	0.095	0.004	0.094	0.095
Mean DV	27.39	27.39	27.39	0.662	0.662	0.662
Panel C: Wor	•k Outcom	ies (Males	househol	d heads /	heads' sp	ouses)
		Hours/weel	2	Wo	rked last w	eek
TE (DD)	0.738	1.147	1.369*	-0.002	-0.002	0.001
	(0.759)	(0.783)	(0.753)	(0.008)	(0.007)	(0.007)
Observations	15,505	14,747	$14,\!747$	15,777	15,010	15,010
R-squared	0.006	0.092	0.092	0.002	0.074	0.075
Mean DV	47.86	47.86	47.86	0.949	0.949	0.949
Controls	NO	YES	YES	NO	YES	YES
Municipality FE	NO	YES	YES	NO	YES	YES
Group Trend	NO	NO	YES	NO	NO	YES
Clusters	290	290	290	290	290	290
	*** p	< 0.01, **	p < 0.05, *	p < 0.1		

#### 12.3 Treatment effects on employment

p < 0.01; p < 0.00; p < 0.11

Note: The table presents OLS estimates for a difference-in-difference model. The coefficients represent differential changes in labor supply before and after the program between exposed and nonexposed children. Standard errors, clustered at the municipality level, are presented in parentheses. Panel A presents treatment effects concerning aggregate data at the household level. Panels B and C, present treatment effects regarding employment for females heads of household or spouses and males heads of household or spouses, respectively.

Table 5: Effects on self-employment: Adult females						
	(1)	(2)	(3)	(4)	(5)	(6)
	Se	lf-employ	red	Wo	rks at he	ome
TE (DD)	0.046**	0.042**	0.034*	0.022	0.020	0.004
	(0.018)	(0.017)	(0.019)	(0.016)	(0.014)	(0.015)
Observations	11,117	11,116	11,116	6,723	6,723	6,723
R-squared	0.004	0.121	0.121	0.015	0.130	0.132
Controls	NO	YES	YES	NO	YES	YES
Municipality FE	NO	YES	YES	NO	YES	YES
Group Trend	NO	NO	YES	NO	NO	YES
Clusters	279	279	279	254	254	254
Mean DV	0.463	0.463	0.463	0.123	0.123	0.123
	*** n	-0.01 ** •	0.05 *	n < 0.1		

#### Effects coming from self-employment 12.4

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

Note: The table presents OLS estimates for a difference-in-difference model. The coefficients represent differential changes in self-employment rate before and after the program between female head of households from exposed and non-exposed children. Standard errors, clustered at the municipality level, are presented in parentheses for adult females. The dependent variable is denoted as 1 if the head of household is self-employed and 0 if they did not report working the week preceding the survey.

#### 12.5 Cash or condition

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	1	lours/wee	k	Wor	ked last w	/eek
		a sa adadada	a a sandadada			
TE (DD)	-5.684	3.156***	4.117***	-0.089	0.043***	0.043**
	(5.083)	(0.744)	(0.930)	(0.101)	(0.016)	(0.019)
TE x Attendance rate (DDD)	9.471			0.150		
	(5.755)			(0.114)		
TE x 1[Attendance rate<0.8] (DDD)		-4.120*			-0.028	
		(2.339)			(0.045)	
TE x 1[Attendance rate <median] (ddd)<="" td=""><td></td><td></td><td>-4.153**</td><td></td><td></td><td>-0.012</td></median]>			-4.153**			-0.012
			(1.697)			(0.034)
			. ,			. ,
Observations	14,563	17,450	17,450	14,750	17,678	17,678
R-squared	0.113	0.096	0.098	0.111	0.096	0.097
Clusters	288	289	289	289	290	290
Mean DV	27.39	27.39	27.39	0.662	0.662	0.662
Mean Covariate	0.853	0.154	0.423	0.853	0.154	0.423
1st Decile Covariate	0.659			0.659		
9th Decile Covariate	0.964			0.964		
TE at Percentile 10	0.561			0.0102		
p-val	0.714			0.737		
TE at Percentile 90	3.443***			0.056***		
p-val	0.002			0.007		
TE at CV=1		-0.964	-0.0357		0.0155	0.0305
p-val		0.664	0.978		0.706	0.247

Table 6: Adult females: Heterogeneous treatment effects by counterfactual attendance rate

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

Note: The table presents OLS estimates for a triple-difference model. Standard errors, clustered at the municipality level, are presented in parentheses. The coefficients in the first row represent treatment effects when the relevant covariate equals 0 (DD). Interactions, located in the second, third and fourth rows denote differential treatment effects with respect to the TE presented in row 1 (DDD). Columns (1) and (4) report heterogeneity by counterfactual predicted attendance rate based on a probit model estimated for the 2004 sample. Columns (2) and (5) report heterogeneity for adult females belonging to households from inframarginal (1[Attendance < 0.8] = 0) and marginal (1[Attendance < 0.8] = 1) children. Columns (3) and (7) report heterogeneity for adult females belonging to households with children whose attendance rate is above the median (1[Attendance < median] = 0) and below the median.

Table 7: Adult females: Heterogeneous treatment effects by access to credit							
	(1)	(2)	(3)	(4)	(5)	(6)	
	Н	ours/weel	ς.	Woi	ked last v	veek	
TE (DD)	4.074***	3.485***	3.839	0.085***	0.079***	0.083*	
	(1.335)	(1.249)	(2.368)	(0.025)	(0.027)	(0.045)	
TE x $\#$ branches per 100000 people (DDD)	-0.073	-0.052	-0.078	-0.004*	-0.004*	-0.004**	
	(0.109)	(0.111)	(0.116)	(0.002)	(0.002)	(0.002)	
Observations	12,818	12,809	12,809	13,011	13,002	13,002	
R-squared	0.007	0.045	0.051	0.003	0.049	0.057	
Controls	NO	YES	YES	NO	YES	YES	
Municipality FE	NO	YES	YES	NO	YES	YES	
Group Trend	NO	YES	NO	NO	YES	NO	
Area-cohort-year FE	NO	NO	YES	NO	NO	YES	
Clusters	98	98	98	98	98	98	
Mean DV	0.662	0.662	0.662	0.662	0.662	0.662	
Mean # branches per 100000 people	9.341	9.341	9.341	9.341	9.341	9.341	

#### 12.6 Treatment effects and credit constraints

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

Note: The table presents difference-in-difference estimates (DD) and triple-difference estimates (DDD) in the first and second row, respectively. The number of financial branches per 100,000 individuals in each municipality is used as a third source of variation. Data regarding financial branches corresponds to 2005, the year before the program's implementation, and is only available for the municipalities that are province capitals. The sample for these regressions accounts for one-third of the clusters' sample but two-thirds of the total observations. Standard errors, presented in parentheses, are clustered at the municipality level.

## A Appendix

#### A.1 Treatment effects after controlling for potential pre-trends

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Hours/week (Total adults)	# of adults working	Hours/week (Adult females)	Worked last week (Adult females)	Hours/week (Adult males)	Worked last week (Adult males)
DD	1.572	0.009	2.287***	0.035**	1.086	-0.002
	(1.564)	(0.030)	(0.730)	(0.014)	(0.873)	(0.007)
Observations	17,434	17,543	17,450	17,678	14,747	15,010
R-squared	0.161	0.250	0.095	0.094	0.092	0.074
Controls	YES	YES	YES	YES	YES	YES
Municipality FE	YES	YES	YES	YES	YES	YES
Sample	4-8th	4-8th	4-8th	4-8th	4-8th	4-8th
Clusters	290	290	289	290	289	289
Mean DV	79.37	1.732	27.39	0.662	47.86	0.949
*** p < 0.01	** p < 0.05	* $p < 0.1$				

Table A.1: Treatment effects controlling for potential differences in pre-treatment period

Note: The table presents OLS estimates for a difference-in-difference model under alternative specifications. The coefficients represent differential changes in labor supply before and after the program between exposed and non-exposed children. Standard errors, clustered at the municipality level, are presented in parentheses. Note that all the results control for potential differential shocks five periods before each cohort entered the treatment, through inclusion of an indicator that takes the value of one whenever  $\tau_{st} = -5$ .

Table A.2: Trea	tment effe	ects includ	ling childr	en from	1st to 8t	h grade
	(1)	(2)	(3)	(4)	(5)	(6)
Pane	el A: Worl	k Outcom	es (Adults	s - House	hold)	
	To	tal hours/w	veek	Total	working a	dults
TE (DD)	3.322**	2.638**	2.941**	0.045	0.025	0.026
	(1.380)	(1.295)	(1.346)	(0.028)	(0.025)	(0.026)
Observations	30.618	20 502	20 502	30 701	20 663	20 663
B squared	0.007	0 155	0 156	0.005	23,000 0 2/3	23,000 ∩ 2/3
Mean DV	78 24	78.24	78.24	1 7 1 2	1 712	1 712
Panel B: Work	Outcome	s (Fomalo	househol	hoads/	hoads' st	
	Outcome				licado sp	
(תת) שש	0 / 10***	Hours/wee	S 0.002***	0.022**	rked last w	
	2.410	(0,600)	∠.063**** (0.652)	$(0.033^{++})$	$(0.031^{++})$	$0.025^{+}$
	(0.000)	(0.629)	(0.053)	(0.013)	(0.012)	(0.013)
Observations	29,533	29,518	29,518	29,917	29,902	29,902
R-squared	0.013	0.101	0.102	0.006	0.099	0.099
Mean DV	26.45	26.45	26.45	0.650	0.650	0.650
Panel B: Worl	k Outcom	es (Male l	nousehold	heads / 1	heads' sp	ouses)
		Hours/weel	ĸ	Wo	rked last w	veek
TE (DD)	1.176	1.450**	1.608**	0.005	0.007	0.009*
	(0.749)	(0.672)	(0.697)	(0.006)	(0.006)	(0.006)
<b></b>						
Observations	26,368	25,251	25,251	26,829	25,699	25,699
R-squared	0.007	0.088	0.088	0.003	0.066	0.067
Mean DV	47.50	47.50	47.50	0.948	0.948	0.948
Controls	NO	YES	YES	NO	YES	YES
Municipality FE	NO	YES	YES	NO	YES	YES
Group Trend	NO	NO	YES	NO	NO	YES
Clusters	293	293	293	293	293	293
	*** I	o<0.01, **	p<0.05, * p	<0.1		

#### A.2Effects on employment using all children in primary

Note: The table presents OLS estimates for a difference-in-difference model. The coefficients represent differential changes in labor supply before and after the program between exposed and non exposed-children. Standard errors, clustered at the municipality level, are presented in parentheses. Panel A presents treatment effects concerning aggregate data at the household level. Panels B and C present treatment effects regarding employment for female heads of household or spouses and male heads of household or spouses, respectively. The sample includes children from 1st to 8th grade.

Figure A.1: Treatment effects on total household labor supply (adults): Total weekly hours worked (left) and number of adults working



The figure depicts OLS coefficients from equation (1). Left-hand panel: each coefficient estimates differences in differences on hours worked by adults between the treatment and control group with respect to the period just before the program was implemented ( $\tau = -1$ ). The dependent variable measures the total number of hours worked by adults in child *i*'s household. Standard errors are clustered at the municipality level. Right-hand panel: Each coefficient estimates differences in differences on adult employment between the treatment and control group with respect to the period just before the program was implemented ( $\tau = -1$ ). The dependent variable measures the number of adults employment between the treatment and control group with respect to the period just before the program was implemented ( $\tau = -1$ ). The dependent variable measures the number of adults employed in child *i*'s household. The estimation sample includes all potential beneficiary children from 1st grade to 8th grade. Standard errors are clustered at the municipality level.



Figure A.2: Treatment effects on employment and hours worked for adults

The figure depicts OLS coefficients from equation (1). Each coefficient estimates differences in differences on the relevant measure of labor supply between the treatment and control group with respect to the period just before the program was implemented ( $\tau = -1$ ). The plots on the left present results for adult males, while the plots on the right present results for adult females. Standard errors are clustered at the municipality level. The estimation sample includes all potential beneficiary children from 1st grade to 8th grade.

#### A.3 Effects excluding children with siblings with different treatment status



Figure A.3: Employment and work hours (weekly) for adults

The figure replicates the main event-study analyses focusing in a reduced sample of children whose siblings treatment status is the same as theirs. The top panels depict employment rate for adult males (heads of household or spouses) and adult females (heads of household or spouses) in child i's household before and after child i is exposed to treatment. The bottom panel depicts weekly hours for both adult males and females. Time to treatment is equal to 0 in the first period in which treatment kicks in. Children who have siblings with different treatment status are excluded from the sample.

	(1)	(2)	(3)	(4)	(5)	(6)
Pane	el A: Worl	k Outcome	es (Adults	- House	hold)	
Total hours/week Total working adults						
TE (DD)	3.847	1.616	2.387	0.069	0.007	0.014
	(2.825)	(2.597)	(2.912)	(0.056)	(0.057)	(0.065)
Observations	9,112	8,624	8,624	9,178	8,687	8,687
R-squared	0.010	0.192	0.194	0.010	0.287	0.290
Mean DV	75.04	75.04	75.04	1.654	1.654	1.654
Panel B: Work	Outcome	es (Female	househol	d heads/ $ $	heads' sp	oouses)
		Hours/weel	ζ.	Wor	ked last v	veek
TE (DD)	4.853***	4.700***	4.743***	0.069**	0.061*	0.058
	(1.419)	(1.403)	(1.539)	(0.031)	(0.032)	(0.036)
Observations	8,639	8,632	8,632	8,766	8,759	8,759
R-squared	0.016	0.116	0.117	0.007	0.117	0.119
Mean DV	25.97	25.97	25.97	0.634	0.634	0.634
Panel B: Wor	k Outcom	ies (Male 1	household	heads/h	eads' spo	ouses)
		Hours/week	C C	Wor	ked last v	veek
TE (DD)	-0.298	-0.397	0.459	0.001	-0.005	0.007
	(1.498)	(1.587)	(1.566)	(0.019)	(0.016)	(0.017)
Observations	7,562	7,075	7,075	7,685	7,193	7,193
R-squared	0.010	0.110	0.111	0.008	0.104	0.106
Mean DV	46.51	46.51	46.51	0.934	0.934	0.934
Controls	NO	YES	YES	NO	YES	YES
Municipality FE	NO	YES	YES	NO	YES	YES
Group Trend	NO	NO	YES	NO	NO	YES
Clusters	286	286	286	286	286	286

Table A.3: Effects excluding children with siblings with different treatment status

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

Note: The table presents OLS estimates for a difference-in-difference model under alternative specifications. The table replicates the main results excluding those children who have siblings in a different treatment group. The coefficients represent differential changes in labor supply before and after the program between exposed and non-exposed children. Standard errors, clustered at the municipality level, are presented in parentheses. Panel A presents treatment effects concerning aggregate data at the household level. Panels B and C present treatment effects regarding employment for female heads of household or spouses and male heads of households or spouses, respectively.

# A.4 Heterogeneous treatment effects by counterfactual attendance rateA.4.1 Probit model for attendance rate



Figure A.4: Attendance rate and predicted attendance rate in and out of sample

This picture depicts attendance rates as a function of age for the 2004 wave for the estimation sample and the validation sample. Attendance rate is depicted for actual and predicted data. The probit model included age fixed effects, years of schooling fixed effects, and demographic characteristics. 80% of the 2004 observations were randomly assigned to an estimation sample, the remaining were assigned to a validation sample. The table shows that the model performs well when it comes to out-of-sample prediction.

0	(1)	(2)	(3)	(4)
	Total adults		Males hh	heads
VARIABLES	$\mathbf{Hours}/\mathbf{week}$	Worked	$\mathbf{Hours}/\mathbf{week}$	Worked
TE (DD)	2.679	0.033	0.737	-0.005
	(1.834)	(0.034)	(0.848)	(0.007)
TE x 1[Attendance rate<0.8] (DDD)	-5.878	-0.144*	1.812	0.015
	(4.800)	(0.077)	(2.064)	(0.018)
Observations	17,434	17,543	14,747	15,010
R-squared	0.164	0.254	0.093	0.075
Clusters	290	290	289	289
Mean DV	79.37	1.732	47.86	0.949
Mean Covariate	0.154	0.154	0.154	0.154
TE at CV=1	-3.199	-0.111*	2.549	0.0103
p-val	0.423	0.0878	0.183	0.546

#### A.4.2 Heterogeneity for alternative outcomes

 $Ta\underline{ble \ A.4: \ Heterogeneous \ treatment \ effects \ by \ counterfactual \ attendance \ rate}$ 

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

Note: The table presents OLS estimates for a triple-difference model. Standard errors, clustered at the municipality level, are presented in parentheses. The coefficients in the first row represent treatment effects when the relevant covariate equals 0 (DD) (inframarginal children). The estimates in the second row report heterogeneity by counterfactual predicted attendance rate based on a probit model estimated for the 2004 sample. Treatment effects for marginal children are presented in the bottom panel.

#### A.5Treatment effects on children's outcomes

Panel A: Enrollment and child employment								
	Enrollment			Worked last week				
TE (DD)	0.037**	0.040***	0.017	0.010	0.004	0.007		
	(0.017)	(0.014)	(0.013)	(0.018)	(0.015)	(0.016)		
Observations	15.164	14.519	14.519	18.447	17.678	17.678		
R-squared	, 0.015	, 0.321	, 0.322	, 0.011	, 0.315	, 0.315		
Mean DV (Baseline)	0.900	0.900	0.900	0.294	0.294	0.294		
Controls	NO	YES	YES	NO	YES	YES		
Municipality FE	NO	YES	YES	NO	YES	YES		
Group Trend	NO	NO	YES	NO	NO	YES		
Clusters	289	289	289	290	290	290		
*** m < 0.01 ** m < 0.05 * m < 0.1								

Table A.5: Treatment effects on enrollment and employment (Children)

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

Note: The table presents difference-in-difference estimates (DD) for the probability of enrollment and the probability that child i reported working the week preceding the interview. Note that since the surveys report enrollment at the beginning of each school year, treatment effects on enrollment are identified using eligibility in the year preceding the survey. For example, a child who has completed 5th grade in 2005 and is observed in the 2006 wave will be in the control group for that year. Conversely, a child who completed 4th in 2005 and is observed in the 2006 sample will be in the treatment group.

Table A.6: Adult females: Heterogeneous treatment effects by access to credit					
(1)	(2)	(3)	(4)		
Total adults		Males hh heads			
Hours/week	Worked	Hours/week	Worked		
2.850	0.013	3.301	0.023		
(2.219)	(0.015)	(4.602)	(0.080)		
0.140	-0.001	-0.101	-0.007		
(0.092)	(0.001)	(0.229)	(0.005)		
10,738	10,967	12,811	12,895		
0.088	0.077	0.144	0.233		
YES	YES	YES	YES		
YES	YES	YES	YES		
NO	NO	NO	NO		
YES	YES	YES	YES		
98	98	98	98		
0.662	0.662	0.662	0.662		
9.341	9.341	9.341	9.341		
	eneous treatm (1) Total ad Hours/week 2.850 (2.219) 0.140 (0.092) 10,738 0.088 YES YES YES NO YES 98 0.662 9.341	eneous treatment effect           (1)         (2)           Total adults           Hours/week         Worked           2.850         0.013           (2.219)         (0.015)           0.140         -0.001           (0.092)         (0.001)           10,738         10,967           0.088         0.077           YES         YES           YES         YES           98         98           0.662         0.662           9.341         9.341	eneous treatment effects by access to (1)         (2)         (3)           Total adults         Males hh           Hours/week         Worked         Hours/week           2.850         0.013         3.301           (2.219)         (0.015)         (4.602)           0.140         -0.001         -0.101           (0.092)         (0.001)         (0.229)           10,738         10,967         12,811           0.088         0.077         0.144           YES         YES         YES           YES         YES         YES           YES         YES         YES           98         98         98           0.662         0.662         0.662           9.341         9.341         9.341		

#### A.6 Treatment effects and credit constraints for extended outcomes

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

Note: The table presents difference-in-difference estimates (DD) and triple-difference estimates (DDD) in the first and second row, respectively. The number of financial branches per 100,000 individuals in each municipality is used as a third source of variation. Data regarding financial branches correspond to 2005, the year before the program's implementation, and is only available for the municipalities that are provincial capitals. The sample for these regressions accounts for one-third of the clusters' sample but two-thirds of the total observations.

Table A.7: Effects on self employment-Adult females					
	(1)	(2)	(3)	(4)	
	Total adults		Male hh heads		
	Self-employed	Works at home	Self-employed	Works at home	
TE (DD)	0.038*	-0.003	0.008	-0.083	
	(0.020)	(0.008)	(0.012)	(0.063)	
Observations	17,506	17,554	7,923	1,128	
R-squared	0.117	0.071	0.143	0.332	
Controls	YES	YES	YES	YES	
Municipality FE	YES	YES	YES	YES	
Group Trend	YES	YES	YES	YES	
Clusters	290	290	281	116	
Mean DV	0.691	0.0520	0.898	0.309	

#### A.7 Effects on self-employment: Adult males and household members

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

Note: The table presents OLS estimates for a difference-in-difference model. The coefficients represent differential changes in the number of self-employed adults in the household before and after the program for exposed and non-exposed children, and differential changes in the self-employment probability before and after the program between male heads of household from exposed and non-exposed children, respectively. Standard errors, clustered at the municipality level, are presented in parentheses. The dependent variable is denoted as 1 if the head of household is self-employed and 0 if they did not report working the week preceding the survey.