Income Effect on Labor Outcomes for People Living in Poverty: the case of PROGRESA

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Income Effect on Labor Outcomes for People Living in Poverty: the case of PROGRESA *

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Abstract

This paper studies the income effect of cash transfers on adult labor outcomes. I use data of PROGRESA, a large cash transfer program in Mexico that provides money to households subject to the condition that school aged kids go to school. I focus on a subsample of the eligibles for whom the conditionality is not a constraint. This allows me to shut-down the substitution effect that the conditionality of the transfer may induce. In practice, it is *as if* PROGRESA was an *un*conditional cash transfer for this subpopulation. Contrary to standard beliefs, I find that the income effect on labor outcomes is not negative.

JEL classification: O12, C93, I32, J22.
Keywords: Conditional Cash Transfers, Poverty, Labor Supply.

1 Introduction

According to standard job-search theory (Chetty, 2008), providing unconditional cash to people has detrimental effects on the probability of finding a job. In the neo-classical theory of labor supply it is standard to assume that leisure is a normal good (see, among many others Becker, 1965, Gahvari, 1994, Cahuc et al., 2014). This implies that if agents receive unearned income, part of the money will be used to buy leisure. In both cases there is a negative "income effect" on labor outcomes. For developed countries there is empirical evidence that supports these theoretical predictions (Chetty, 2008, Card et al., 2007, Basten et al., 2014, Cesarini et al., 2017, Picchio et al., 2018). All this reinforces the belief of economists, policy makers, and the public at large, that unconditional cash transfers (UCT) generate incentives to work less (for data about beliefs in different countries, see for instance Banerjee et al., 2017).

Nevertheless, poor people in developing countries face difficulties to meet basic needs and receive low or no public monetary support. Cash transfers could be used to cope with these difficulties. Because of this, they could allow people to be more willing or capable to work. In fact, there is some recent empirical evidence showing that UCT targeted to poor households in developing countries do not have detrimental effects on labor outcomes of prime-age adults and could even have positive effects (Ardington et al., 2009, Haushofer and Shapiro, 2016, Salehi-Isfahani and Mostafavi-Dehzooei, 2018, Franklin, 2018). This evidence questions the

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standard properties of canonical job-search and neo-classical models of labor supply (Baird et al., 2018, Bosch and Manacorda, 2012).

The purpose of this paper is to contribute to this literature using data of PROGRESA, a randomized control trial (RCT) providing a large (equivalent to 20% of the average wage of the household head) and long-lasting cash transfer to households in rural Mexico, conditional on kids going to school. This rich data set has already been used to analyze the effect of PROGRESA on adult labor outcomes. And it has been found that it did not affect labor outcomes negatively (Skoufias et al., 2001, Skoufias and di Maro, 2008, Rubio-Codina, 2010, Alzua et al., 2013, Banerjee et al., 2017). However, it has been argued that the absence of negative effects on labor outcomes of adults is explained, at least in part, by the conditionality of the program. The conditionality of the transfers has a "cross-substitution effect" on adults: It could induce adults to work more to substitute for child's work (Rubio-Codina, 2010, Parker and Todd, 2017).

This paper restricts the dataset of PROGRESA to a subsample (exogenously defined) of adults for whom, I claim, the conditionality was not binding. It is as if PROGRESA was an unconditional cash transfer for them. I find, using a difference-in-differences (DiD) empirical strategy, that the income effect is not detrimental to labor outcomes on this subsample. If anything, the effects are positive. This evidence helps further to debunk the stereotypes against welfare recipients. And it is useful because it suggests that the background conditions (in particular, the level of income and the degree to which basic needs are covered) are crucial to understand whether receiving cash is or is not detrimental to work.

Throughout the paper I look at the impact of PROGRESA on three different indicators: (1) labor force participation in all types of work, (2) labor force participation in day agricultural and nonagricultural employment (DANAE): this measure excludes self-employed, people who work for a family business or those who work without receiving a payment and (3) the number of hours worked per week.

The remainder of the paper is organized as follows: It starts with a literature review. Section 3 briefly describes the main features of PROGRESA, its design, and the data from the available surveys. Section 4 defines and characterizes the sample with which I work throughout the paper. Section 5 is the main section, where I present the econometric specification and the impact of PROGRESA on work, DANAE, and the number of hours worked per week. Section 6 discusses (and tries to rule out) threats to the identification. Finally, Section 7 concludes.

2 Literature Review

The current work is part of the literature that studies the income effect of cash transfers in labor outcomes of adults. In this section I briefly comment the recent papers, mentioned in the introduction, that look at at this.

In developed countries this has been recently done using data of lottery winners. For instance, Cesarini et al. (2017) look at Swedish data and Picchio et al. (2018) at Dutch data. Both papers find that winning the lottery reduces pre-tax earnings by a small magnitude during several years. The income effect on labor outcome has also been analyzed using data of severance payments (which are received on top of the unemployment compensations): Chetty (2008) do it for the US, Card et al. (2007) for Austria and Basten et al. (2014) for Norway. These studies find that the recipients of the transfers increased their duration in unemployment. None of the previous studies focuses on a population of poor people. Instead, Yang (forthcoming), studies whether poor households in the US adjust their labor supply behavior in response to receiving expected income payments. He exploits the disbursement timing of the earned income tax credit (EITC). He concludes that labor force participation of secondary earners (but not the one of primary earners) is sensitive to the receipt of the EITC. Notice that, when making labor supply choices, people may take into account the effects of their decisions in future EITC reimbursements. Therefore, the identified effect is not a "pure" income effect.

As for literature about developing countries, Ardington et al. (2009) analyze the effect of social (means tested) old-age pension on the labor supply of the prime age members of the household in South Africa. Their results suggest that the pension plays a role in lessening both credit and childcare constraints, allowing prime-aged adults to migrate for work. Haushofer and Shapiro (2016) study the impact of an UCT to poor people in Kenya, using a RCT. The transfer was relatively high (at least twice the average monthly household consumption in the area) and paid over a short period of time. They look at the effects of these transfers on a large number of outcomes. Regarding labor supply, they find that the transfers did not reduce the probability of having a casual job or a salaried job. Moreover, they find a positive effect on the number of income-generating activities reported by the household. Franklin (2018) develops an experiment in Ethiopia where he provides young jobless people with money (intended to cover transportation costs). He finds that four months after the start people who received the subsidy were seven percentage points more likely to have a permanent work. The effect was stronger for relatively poor and cash constrained people. Salehi-Isfahani and Mostafavi-Dehzooei (2018) use a DiD strategy to analyze the effect of an UCT (that replaces energy subsidies) in Iran starting in 2011. Transfers amounted to 29% of the median household income. They look at the average effects and at the effects on the bottom 40% of the income distribution. They find no evidence that cash transfers reduced labor outcomes. To the contrary, they find positive effects on the labor supply of women.

3 The PROGRESA Experiment

3.1 Brief Description

PROGRESA is a Spanish acronym for "Program of Education, Health and Nutrition". It started in 1997 in rural villages in Mexico and and changed its name to "Oportunidades" in 2000. .[1] It targeted benefits directly to people living in extreme poverty in rural areas of Mexico. As its name suggests, the program had a multiplicity of objectives. Its aim was to improve the education, health, and nutrition status of poor families.

Eligibility to the program was determined in two main stages. First, 506 localities were selected using a means index based on census data. Second, within the selected localities, households were chosen using survey data collected at the household level. In this second step, the income of the household was considered first to perform a preliminary classification. Then, a discriminant analysis was performed to incorporate other household characteristics. The underlying motive was to use a multi-dimensional approach to poverty. Households classified as "poor" were eligible to receive the benefits. Skoufias et al. (1999) provide a detailed description of the selection procedure and an evaluation of the methods.[2]

Cash transfers were given every two months to the female head of the household. They had two main components. First, the nutritional grant was received by all beneficiary households

[1] Since I use data from 1997 to 1999, I refer to the program as PROGRESA, the name it had during that period.

[2] The original classification scheme classified around 52% of the households of the selected localities as poor. I use this original classification. By July 1999 PROGRESA added new households to the list of beneficiaries since it was felt that some households were unduly excluded. As a result of this process (called "densification") 78% of the sample was classified as poor (Skoufias, 2005).

conditional on attending medical check-ups, which were free.[3] Second, an educational grant was provided to mothers of kids younger than 18 years old conditional on attending school a minimum of 85% of the time and on not repeating a grade more than twice.[4] The educational grant varies according to the grade, for kids in secondary school according to gender. On top of that, kids received an annual stipend to pay for school materials. Table 1 shows the transfer structure in nominal pesos in three different moments. To prevent individual migration into the household only kids who were living in the household at the time of the initial household survey were eligible for the school transfers (Gertler et al., 2012).

Table 1: Nominal Monthly Amount of Transfers

	Oct 1998	May 1999	Nov 1999
Education Grant in Primary School per Kid			
Third Grade	70	75	80
Fourth Grade	80	90	95
Fifth Grade	100	115	125
Sixth Grade	135	150	165
Education Grant Secondary School per Kid			
Girls			
Seventh	210	235	250
Eight	235	260	280
Ninth	255	285	305
Boys			
Seventh	200	220	240
Eight	210	235	250
Ninth	220	245	265
School Materials per Kid (once a year)			
Primary (September)			210
Primary (January)		45	
Secondary (September)	170		205
Nutritional Grant (per Household)	100	115	125
Maximum Grant (per Household)	625	695	750

Note: The data to construct this table is taken from Skoufias (2005).
Amounts are in nominal pesos per kid. According to the Bank of Mexico, the Consumer Price Index in October 1998 was 50.4, in May 1999 it was 55.94, and in November 1999 it was 58.43.

3.2 Design and Data Collection

Due to budgetary constraints the Government did not enroll all eligible families at the same time. The full sample used in the evaluation of PROGRESA consists of panel data for 24000 households in 506 localities in seven states. From the 506 localities 320 were randomly assigned to treatment and 186 to control (Behrman and Todd, 2000 analyzed the quality of the randomization and concluded that treatment and control samples were, all in all, very well

[3]According to Skoufias (2005), people aged 17 or older are required to have one annual check-up; kids between 5 and 16 two check-ups a year; kids between 2 and 4 three check-ups a year; kids between 4 months and 24 months eight check-ups. Finally, babies from 0 to 4 months are required to have three annual check-ups.

[4]Kids were required to maintain an attendance record of 85% or better. Parents were supposed to receive a form (E1), the form was taken to the teacher who signed for the register of the child, and parents were supposed to return the signed E1 forms to the PROGRESA officials. Nevertheless, de Brauw and Hoddinott (2011) report that some households did not receive the E1 form but, according to administrative records, received the educational grant.

balanced). Eligible households (the ones classified as poor) in treatment localities started to receive the benefits in July 1998, whereas the eligible households in control localities started to receive the benefits by December 1999 (Skoufias, 2005). Households in control villages were not informed that they would receive the benefits until two months before the start. Attanasio et al. (2011) explicitly test for anticipation effects and find no evidence. Todd and Wolpin (2006) report that they find no evidence of anticipation either.

Skoufias (2005), using administrative data, reports that out of the 7837 households classified as poor, 478 households did not receive any transfers. So the take-up rate was 93.90%.

Once enrolled, households received the benefits for three years, conditional on meeting the program requirements stated above. As explained by Gertler et al. (2012), after the three years, they were "recertified", that is, their living conditions were reassessed; if they were recertified as eligible, then they continued receiving the benefits for three more years, until the next recertification. If not, they were granted the benefits for six more years before being phased off the program. This means that eligible households in treated villages could expect to receive the benefits for at least nine years. This was explicitly designed to minimize disincentives to work, as stated by Schultz (2004), but also to minimize administrative costs and difficulties related with ascertaining precise income levels in data-poor environments (Banerjee et al., 2017).

Five household surveys were collected: ENCASEH[5] October 1997 (S1), ENCEL[6] March 1998 (S2), October 1998 (S3), May 1999 (S4) and November 1999 (S5). The first two were collected at baseline, before the start of PROGRESA, and the last three after the start of the program. However, the second survey does not include any data related to labor outcomes. Therefore, like Parker and Skoufias (2000) and Skoufias and di Maro (2008) I do not use that survey in my analysis. Throughout the paper t refers to time, where $t \in \{1, 3, 4, 5\}$ corresponds to the timing of each of the relevant surveys. All households, eligible and non-eligible, were surveyed. For most of the analysis, I will only use data of eligible (poor) households in treated and non-treated localities; I will only use data of non-eligible people for falsification checks.

Regarding attrition, there is information for just 4.94% of people before the start of the program (in S1) but not after. The percentage among the treated is 4.99%, among the non-treated 4.84%, the difference of 0.15% is not significant. Moreover, a joint F-test (for eleven baseline characteristics)[7] shows that attriters are not significantly different depending on whether they are treated or not. Instead, 30.95% of people cannot be followed throughout the four surveys. The percentage among the treated is 31.30%, among the non-treated 30.36%, the difference of 0.94% is not significant. Given that the percentage is big and that a joint F-test (for the same eleven characteristics) rejects equality between those who can be followed through the four surveys and those who cannot, I proceed like Schultz (2004) does and report all the results both for the "panel" (agents that can be observed four times) and the "pooled" (all observations without missing data) samples.[8]

[5]Encuesta de Características Socioeconómicas de los Hogares.

[6]Encuesta de Evaluación de los Hogares Rurales.

[7]Sex, whether the agent works, has health insurance, is or not is a household head,, marital status, education, type of work, number of people living in the household, hours worked per week, age, and means index.

[8]Attriters, as compared with people that can be followed throughout the four samples, are different in many characteristics at baseline: they are more often men, more educated, work more, more of them have a DANAE, are younger, more often do not live together as a couple, fewer of them are household heads, live in bigger households and are marginally less poor.

4 Data

4.1 Sample Selection Criteria

In October 1997, before the start of PROGRESA, 97.6% of kids between 7 and 11 years were already attending school. At the age of 12, the attendance rate sharply decreases. Fig.1 shows the school attendance rate by age before the start of PROGRESA for eligible (poor) kids living in treatment and control localities. Kids start attending elementary or primary school at the age of 5-6 years (most of them at the age of 6), therefore the age of 12 coincides for most kids with the transition from primary school (grades 1 to 6) to junior secondary school (grades 7 to 9).

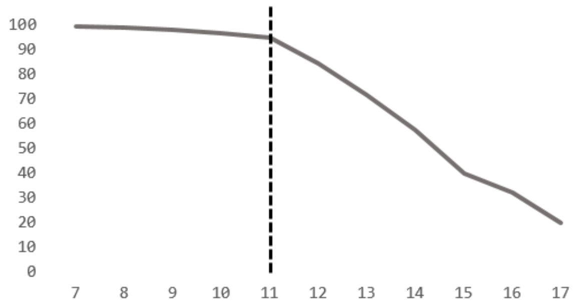

Figure 1: **School Assistance (%) by age:** The graph shows the attendance rate by age, from 7 to 17 years old, for poor people in treatment and control localities. I use information from ENCASEH 1997 survey and include only observations without missing school attendance data.

School attendance was almost universal before the start of the program for kids below 12 years, which means that in practice the conditionality imposed by PROGRESA was not a binding constraint for them: most of these kids did not change their behavior to meet the imposed conditions. This has been acknowledged by several authors. For instance, Todd and Wolpin (2006): “Because attendance, in the absence of any subsidy, is almost universal through the elementary school ages, subsidizing attendance at the lower grade levels, as under the existent program, is essentially an income transfer”. Attanasio et al. (2011): “... the grant hardly changes their behaviour in the first place because almost all children go to school below grade 6, making it an unconditional transfer for that age group”. Attanasio and Lechene (2014): “In practice, nearly all children go to primary school. (. . .) for households with children who have finished primary school, the conditions might be binding”. Finally, de Brauw and Hoddinott (2011): “For children continuing primary school (having completed grades 3, 4 or 5), there is no evidence that conditionality has a significant effect on school enrollment. We may not find an effect of conditionality at these grade levels in part because almost all children were already completing these grades.” [9]

I claim that in households without kids aged between 12 and 17 years, the effect of PROGRESA on adults’ labor outcomes is essentially an income effect. In a vast majority of these households school-aged children were already going to school before the start of the program. That is, the conditionality of PROGRESA did not induce them to modify their behavior. Therefore adults

[9] Schultz (2004) finds that the impact of PROGRESA in the school attendance rate of kids in primary school is positive but small. The magnitude of this effect is smaller than one percentage point for his panel sample (kids that can be observed throughout all the surveys) and slightly higher than one percentage point for his pooled sample (sample of all valid child observations).

were not induced themselves to modify their time allocation through a cross-substitution effect.

In order to define my sample, I create a variable called "$sec_{i,t}$" (sec means secondary school).

Definition: I define $sec_{i,t} = 0$ if agent i lives at time t in a household in which:

1. There has been no kid between 12 and 17 years since Oct 1997 ($t = 1$) and up to t, **and**
2. There has been no kid who meets the requisites to be in secondary school since Oct 1997 ($t = 1$) and up to t.[10]

If any of the two conditions is not satisfied, then $sec_{i,t} = 1$. The variable "$sec_{i,t}$" is exogenous, i.e, it is not affected by PROGRESA, because it depends only on the age of the members of the household and on information collected at baseline (before the start of the program).

Having $sec_{i,t} = 0$ means that the agent lives in a household which never, up to t, received an educational transfer for a kid aged 12 and above or who is in secondary school. That is, the conditionality of PROGRESA did not affect the behavior of the person in t nor in previous periods. One could fear that *future* conditionality may affect the present decisions of the adults of the household.[11] In order to address this concern, in a robustness check presented in Appendix C, I restrict the definition of $sec_{i,t} = 0$: I change the age range of the definition so that it reads: "there has been no kid of ages between **11** and 17 since Oct 1997 ($t = 1$) and up to t". Arguably, future conditionality is less problematic if it is far away in time. As reported in Appendix C, all the results hold qualitatively when I replicate the estimations for this restricted subsample.

Fig.2 shows graphically the design of PROGRESA and the groups that I use to identify its effects. Previous studies analyzed the effect of PROGRESA by comparing the outcomes of poor people living in treated localities with the ones of poor people living in control localities. The novelty of my analysis is to focus on a sample that, I claim, is affected by PROGRESA only through an income effect. That is, I focus on observations for which $sec_{i,t} = 0$ (highlighted in Fig.2).

[10]To determine whether a kid meets the requisites to be in secondary school, I use information about completed grades and attendance at baseline, $t = 1$, and I move it forward assuming no repetition and no dropout.

If I do not observe any member in t in a household,, I assume that in t they had a kid of ages 12-17.

[11]This would be the case, for example, if the father of an 11-year-old kid was planning to drop the kid out of school next year, but thanks to PROGRESA he changed his mind. Knowing that the kid will attend secondary school next year (instead of, say, work with him in the family business) may have implications on his labor outcomes *today*.

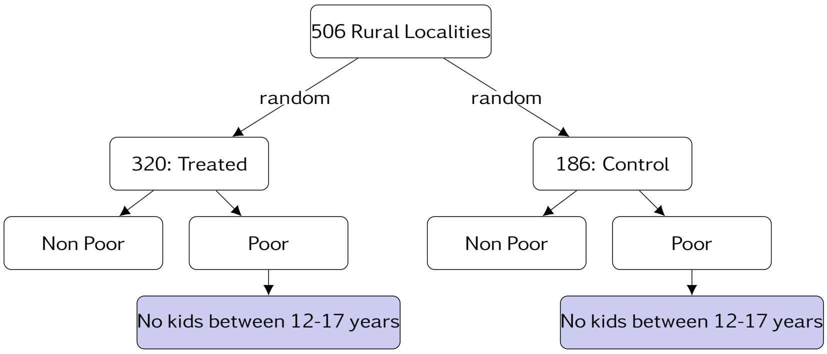

Figure 2: **Design of PROGRESA:** Out of the 506 chosen rural localities, 320 were assigned to treatment and 186 to control. Only eligible (poor) people in treated localities received the transfers during the analyzed periods of 1998-1999. In the core of this paper, I analyze the effect of PROGRESA on the labor outcomes of eligible (poor) adults for which $sec_{i,t} = 0$, that is for which (1) there have been no kid between 12 and 17 yeas since Oct 1997 ($t = 1$) and up to t, **and** (2) there have been no kids who meet the requisites to be in secondary school since Oct 1997 ($t = 1$) and up to t. Groups with $sec_{i,t} = 0$ are highlighted in the diagram.

In my sample, I exclude all people who were younger than 18 at $t = 1$. I also exclude all women older than 68 and men older than 72 (according to the OECD, 2017 these are the effective ages of retirement in Mexico). I drop all observations with missing relevant data, and call the remaining ones "valid observations".

As stated previously, I report the results for two different samples: (1) the pooled sample: composed by all valid observations (37666 observations) with $sec_{i,t} = 0$ and (2) the panel sample: composed by valid observations of people with $sec_{i,t} = 0$ throughout the four surveys (26164 observations).

4.2 Outcome Variables

In this subsection I describe the three outcome variables that will be used in the empirical estimations of Section 5.

Labor force participation ($Work_{i,t}$) is a dummy variable equal to 1 if agent i reports in t that she/he worked during the last week and it is equal to 0 otherwise. The four surveys include a question that asks the person whether, during last week, she/he (1) worked (2) had a job but did not work (3) worked helping in a family business without receiving any payment (4) did not work. If agent i in t answered yes to (1), (2) or (3), then $work_{i,t} = 1$. If the agent reported that she/he did not work in the previous question, then a verification question asks whether she/he was involved in selling products, helping in some business, built products for sale, helped to work in agricultural activities, or ironed/washed clothes for a pay. If agent i performs any of these activities in t, then $work_{i,t} = 1$.

Labor force participation in DANAE ($DANAE_{i,t}$) is a dummy variable equal to 1 if agent i reports in t to have day agricultural employment or to be a non-agricultural employee and is equal to 0 otherwise (i.e, if the person reports that she/he does not work or has another kind of work). The four surveys include a question about the main occupation at work for those who work. This question contains eight alternatives: (1) agricultural worker (2) nonagricultural employee (3) self-employed (4) business owner (5) family worker (6) worker without payment (non including family businesses) (7) member of a cooperative (8) ejidatarios. If agent i answered (1) or (2) to this question in t, then $DANAE_{i,t} = 1$. $DANAE_{i,t} = 0$ if agent i reports in t

that she/he performs activities (3) to (8) or if she reports that she/he does not work.[12]

The number of hours worked per week ($Hours_{i,t}$) is a continuous outcome variable. The question about the number of hours worked per week was asked differently before and after the start of PROGRESA. In $S1$ it was asked to everyone who declared to work. However, in $S3$ and $S5$ it was only asked to those who declared to have a *salaried* job (in $S4$ the question was not asked). No question in $S1$ asked explicitly whether the person had a salaried job. Because of this, the empirical strategy that I follow to estimate the effect of PROGRESA on the number of hours worked per week is different to the one used for the other two outcome variables. The empirical strategy that I follow is explained in Subsection 5.2.

4.3 Descriptive Statistics

In Table 10 of Appendix B, I divide the "total sample" of PROGRESA into observations with $sec_{i,t} = 0$ (in which I focus throughout the paper) and the remainder, i.e, those with $sec_{i,t} = 1$. As can be observed, my sample differs in several characteristics from the sample with $sec_{i,t} = 1$: in general, members of my sample are younger, more educated, live in smaller households, have fewer kids, more often live together as a couple and according to the means index are marginally less poor.

Table 2 provides information of my samples (those with $sec_{i,t} = 0$) at baseline: a high percentage of people live together as a couple, they have on average between three and four years of education, the average age is 34 (36) years for men and 30 (32) for women in the pooled (panel) sample. Most men are household heads. Labor characteristics differ substantially among men and women. While 94% (94%) of men report to work in the pooled (panel) sample, only 12% (10%) of women do. 71% (72%) of men in the pooled sample have a DANAE, but just 4% (4%) of women do. On average, the number of adults working per household is 1.2, and the number of kids (people below 18) per household is a bit higher than 2.0.

[12] What I call DANAE is what Skoufias et al. (2001) and Skoufias and di Maro (2008) call "salaried work". Nevertheless, the term "salaried work" is used in the surveys with a different meaning. Therefore, to avoid confusion, I prefer to use the term DANAE instead.

Table 2: Individual and Household Characteristics at Baseline (S1)

	MEN		WOMEN	
Individual characteristics	**Pooled**	**Panel**	**Pooled**	**Panel**
Living as a couple	0.85	0.87	0.83	0.85
Years of education	3.59	3.70	3.33	3.50
Age	34.23	35.44	31.85	30.74
Household head	0.84	0.86	0.05	0.05
Work	0.94	0.94	0.12	0.10
DANAE	0.71	0.72	0.04	0.04
N. Obs	18360	12692	19306	13472

Household (hh) characteristics	**Pooled**	**Panel**
# people working per hh	1.21	1.15
# of people in the hh	4.69	4.42
# of kids per hh	2.35	2.13
# of hh	5784	2999

Note: These tables report the demographic characteristics of individuals and households in the panel and pooled sample at baseline (S1). Work (%) is the percentage of people that reported to work. DANAE is an acronym for: day agricultural or nonagricultural employment. I report the percentage of people (*among the total*) that reported to have this type of work.

In Table 11 Appendix B I report data about the amount of money that people in my sample spend on food, transportation, and clothes. I also report information about the ownership of animals, which is relevant because animals can increase home consumption. Unfortunately this information was not collected at baseline. Table 11 shows information of households in control villages in Survey 3. Since people living in treated and control villages are quite comparable (see table 9 in Appendix A) this can give an idea of the expenditures and ownership of animals of all the households (treated or not) at baseline. According to this information, the typical household has a weekly expenditure on food of around 125-130 pesos.[13] Transportation and clothing seem to be minor expenditures.

Moreover, according to a question asked at baseline (S1), more than 98% of the people report that either they own the house in which they live (which is totally paid), or someone lends it to them. This suggests that rent is not an important expenditure for them.

For those who work, the average weekly wage at baseline (in real pesos of Oct 1998) is 177 (164) pesos for men and 125 (115) pesos for women in the pooled (panel) sample. On average, both men and women of the pooled (panel) sample report to work 5.3 (5.2) days a week.

All this information facilitates the comprehension of the magnitude (and relevance) of the transfer granted by PROGRESA to the households in my sample. For instance, just the nutritional grant amounts to around 18% of the monthly expenditures in food (which is the largest expenditure of these households). These transfers are very generous, compared with other CCT in developing countries (see for instance Alzua et al., 2013 and Banerjee et al., 2017).

[13]On Appendix E, Table 20 , I provide information of the prices of some consumption goods in treated and control localities.

5 Econometric Specification and Results

This section presents the econometric specification and the impact of PROGRESA on: (1) labor force participation in all kinds of work, (2) labor force participation in DANAE and (3) the number of hours worked per week.

Since I do not know who actually received the transfers and who did not, in all the cases I report estimates of the "intention to treat" effect (Angrist et al., 1996). Nevertheless, given that the take-up of PROGRESA among eligible people in treated villages is very high (93.9% of eligible households in treated villages received the transfers), and given that no one in control villages was entitled to receive the transfers, the estimates should be close to the "treatment effect on the treated". For the first part of the section, treatment is defined as a dummy variable: a person is treated if she/he lives in a treated locality and not treated otherwise.

As I mentioned before, female heads of the household are the ones entitled to receive the transfer of PROGRESA. This means that my estimates of the effect of PROGRESA on the labor outcomes of men implicitly assume that there is income pooling in the household. This is a caveat (see for instance Duflo, 2003 and Attanasio and Lechene, 2014). But, to the best of my knowledge, this has always been assumed when estimating the effect of PROGRESA on labor outcomes (see for instance: Skoufias et al., 2001, Skoufias and di Maro, 2008, Rubio-Codina, 2010, Alzua et al., 2013, Banerjee et al., 2017). Moreover, Haushofer and Shapiro (2016) recently report they that do not find evidence against income pooling in their experiment in Kenya.

In the coming subsections I present the econometric specification and results for the three indicators mentioned before. First I analyze the effect of PROGRESA on the labor force participation and then focus on the impact of PROGRESA on the number of hours worked per week. I split the analysis because, given the design of the surveys, the empirical strategy that I follow is different. At the end of the section I change the definition of treatment to a continuous variable equal to the amount of the transfer per adult equivalent. This allows to see the effect on labor outcomes of a marginal change in the transfers.

5.1 Labor Force Participation

5.1.1 Specification

To identify the effect of PROGRESA on the labor force participation of adults I use a DiD specification, which allows me to exploit the panel structure of the data. This specification eliminates all pre-program differences between treatment and control groups under the assumption that unobserved heterogeneity between these two groups is fixed over time.

The **Baseline** specification is the following:

$$Y_{i,t} = \alpha + \beta_1 T_i + \beta_2 T_i * Expost_t + \beta_3 S3 + \beta_4 S4 + \beta_5 S5 + \sum_{j=1}^{J} \gamma_j X_{ji} + u_{it} \tag{1}$$

where:

$Y_{i,t}$ is the dummy outcome variable for individual i in time $t \in 1,3,4,5$. I do the estimation for (1) $Work_{i,t}$ and (2) $DANAE_{i,t}$ (see Subsection 4.2 for the definition of the outcome variables).

T_i is the treatment, in this case a dummy variable. It is equal to 1 if person i lives in a treated locality and it is equal to 0 otherwise.

$Expost_t$ is also a dummy variable. It is equal to 1 if the time of the survey is 3, 4 or 5 (that is, after the start of the program) and it is equal to 0 if the time of the survey is 1 (before the start of the program).

$S3, S4, S5$ are time dummies, equal to 1 if the time of the survey is, respectively, 3, 4 or 5, and zero otherwise.

Finally, X_{ij} is a set of j characteristics for individual i measured at $t = 1$. These are control variables that are included to increase precision of the estimates (Duflo et al., 2007). I include the following controls: age, age squared, locality of residence, whether the person lives together as a couple, number of people in the household, whether the person is the household head, and number of years of education.[14]

The coefficient of interest is β_2, it provides the difference in the dependent variable across the treated and control individuals relative to their baseline values, conditional on the control variables.

I run the regression using OLS (about the good performance of OLS with limited dependent variables, see for instance Angrist and Pischke, 2009, Ch. 3). Nevertheless, qualitative results do not change if I run a Probit regression instead (results are available upon request). Because of the experimental design, localities rather than individuals, were assigned to treatment. Therefore I cluster the errors at the locality level (Abadie et al., 2017, Bertrand et al., 2004). Clustering allows any kind of autocorrelation of the errors within the cluster, in this case the localities (Cameron and Miller, 2015). I estimate this regression separately for men and women.

I also estimate a specification with **Dynamic Effects**. This allows to estimate the effect of PROGRESA, separately, at each survey time: S3, S4, S5. To do that I estimate:

$$Y_{i,t} = \alpha + \beta_1 T_i + \beta_2 T_i * S3 + \beta_3 T_i * S4 + \beta_4 T_i * S5 + \beta_5 S3 + \beta_6 S4 + \beta_7 S5 + \sum_{j=1}^{J} \gamma_j X_{ji} + u_{it} \quad (2)$$

where everything is the same as before, except for the fact that now I disentangle the effect of the treatment for each survey time. The coefficients of interest are: β_2, β_3 and β_4. Each of these coefficients provide the effect of PROGRESA on $Y_{i,t}$ from $t = 1$ up to $t \in 3, 4, 5$, respectively.

My final specification explores the presence of **Heterogeneous Effects**. I want to know whether the effect of PROGRESA on people who were intended to receive *only* the nutritional grant (fully unconditional, except for the annual medical check-ups) is different from the effect of PROGRESA on the rest of the people, i.e, those who were intended to receive the nutritional grant but also, in some t, the educational grant coming from a kid in primary school.

In order to do this, I created a variable called "GA_i".

Definition: I define GA_i=0 for an agent i if $sec_{i,t} = 0$ for all t in which i appears, and moreover the person lives in a household in which:

(1) There has been no kid between 8 and 11 years through all the surveys in which the household appears, **and**

[14] I include the same controls as Banerjee et al. (2017) plus the locality of residence and whether the person is the household head.

(2) There has been no kid who meets the requisites to be in grades 3 to 6 of primary school through all the surveys in which the household appears.[15]

GA_i=1 for the rest of the sample, i.e, for those who live in a household which in some t was intended to receive an educational transfer for a kid in primary school. I estimate:

$$Y_{i,t} = \alpha + \beta_1 GA_i + \beta_2 T_i + \beta_3 T_i * GA_i + \beta_4 GA_i * Expost_t + \beta_5 T_i * Expost_t + \beta_6 T_i * GA_i * Expost_t$$
$$+ \beta_7 S3 + \beta_8 S4 + \beta_9 S5 + \sum_{j=1}^{J} \gamma_j X_{ji} + u_{it} \quad (3)$$

where GA_i is the dummy variable defined above and the rest is the same as before. The coefficients of interest are β_5 for the group with $GA_i = 0$ and $\beta_5 + \beta_6$ for the group with $GA_i = 1$.

For all the estimations (1), (2), and (3) of the panel sample, I also report the results using individual fixed effects (IFE). IFE are useful if one fears that individual unobserved factors are correlated in some way with the treatment (Wooldridge, 2016, Ch 13). It does not seem to be the case here, since treatment only depends on the locality of residence, and localities were randomly assigned into treatment. Nevertheless, I report the results using IFE as a robustness check.

Appendix D reports the results of the estimations (1), (2), and (3) for the whole sample (i.e whatever value of $sec_{i,t}$ and not only for observations with $sec_{i,t} = 0$). These results are consistent with what has been found in previous studies: the effect of PROGRESA on labor outcomes is in generally small and not significantly different from zero.

5.1.2 Results

Table 3 reports the results of regressions: (1) baseline, (2) dynamic effects, and (3) heterogeneous effects, where the dependent variable is "work". Recall from the definition of work that people who work (work=1) do not necessarily receive an income in exchange. The first column of Table 3 reports the results for the pooled sample of men, the second for the panel sample of men, and the third for the panel also, but with individual fixed effects. Columns four to six report the same results for women. Coefficients are overall negative, but none of them is significant at conventional levels. The magnitude of the coefficients for men is relatively small. In the baseline estimation, for instance, no effect is bigger than 1.4 percentage points in absolute value. For women the coefficients are bigger, mostly at the start of the program ($S3$). Nevertheless, by the time of $S5$ the coefficient is already very small. The effect of PROGRESA is not significantly different for agents living in households which never received the educational grant (that is, observations with $GA_i = 0$) compared to those which in some point received the educational grant from a kid in primary school (observations with $GA_i = 1$).

Table 4 replicates Table 3 for the outcome DANAE. DANAE=1 implies that the agent works in a day agricultural or nonagricultural work and gets paid in exchange. In this case, coefficients are mostly positive. PROGRESA seems to have a positive effect of 3.9 percentage points on men (pooled sample) when one looks at the three ex-post surveys altogether. The effect was large in Oct 98 (S3): of 5.7 percentage points. This impact, however, appears to decrease over time. Effects on women are very close to zero.

In Appendix F, I explore heterogeneity of the impact in terms of poverty. I run (3) using the means index at $t = 1$ to split the sample in two: the poorest and the less poor. I only find

[15] Again, to determine whether a kid meets the requisites to be in grades 3 to 6, I use information about completed grades and attendance at baseline, $t = 1$, and I move it forward assuming no repetition and no drop out.

a significant difference among the pooled sample of women when the dependent variable is work: the effect on the poorest women is positive (but not significant and very close to zero), while the effect on less poor women is negative (and significant at 10%).

All these regressions were carried on a sample (observations with $sec_{i,t} = 0$) which should not be affected by the substitution effect that PROGRESA may induce on labor decisions inside the household. As I argued before, the only effect that PROGRESA should have in this sample is an income effect. I do not find evidence that PROGRESA had a negative income effect on this sample. The results are more coherent with a zero income effect on the extensive margin.

Table 3: Impact of PROGRESA on the Probability of Working

	MEN			WOMEN		
	Pooled	**Panel**	**Panel (FE)**	**Pooled**	**Panel**	**Panel (FE)**
Baseline						
	-0.014	-0.006	-0.006	-0.019	-0.016	-0.016
	(.009)	(.011)	(.011)	(.018)	(.017)	(.017)
Dynamic Effects						
S3	-0.009	-0.001	-0.001	-0.020	-0.023	-0.023
	(.011)	(.022)	(.013)	(.020)	(.021)	(.021)
S4	-0.019	-0.013	-0.013	-0.027	-0.021	-0.021
	(.011)	(.013)	(.013)	(.019)	(.018)	(.018)
S5	-0.015	-0.004	-0.004	-0.009	-0.005	-0.005
	(.009)	(.012)	(.013)	(.022)	(.021)	(.021
Heterogeneous Effects						
GA_i=0	-0.020	-0.013	-0.014	-0.025	-0.021	-0.021
	(.016)	(.017)	(.017)	(.020)	(.018)	(.018)
GA_i=1	-0.013	0.001	0.002	-0.017	-0.012	-0.012
	(.009)	(.009)	(.009)	(.021)	(.022)	(.022)
Pre-Program Level	0.937	0.942	0.942	0.118	0.104	0.104
N. Obs	18360	12692	12692	19306	13472	13472

Note: This table reports the effect of PROGRESA on the probability of working. Treated individuals are those who live in treated localities. In the first column I report the results of an OLS regression for a sample that includes all valid observations of men for which $sec_{i,t} = 0$. In the second column the results of an OLS regression for all valid observations of men who have $sec_{i,t} = 0$ in all the surveys. In the third one, I use the same sample as before but I include individual fixed effects. In columns four to six I report the results of the same estimations for women. Errors are clustered at the locality level and reported in parenthesis. See the main text for the definition of GA_i.
*** significant at 1% level; ** significant at 5% level; * significant at 10% level

Table 4: Impact of PROGRESA on the probability of having a DANAE

	MEN			WOMEN		
	Pooled	**Panel**	**Panel (FE)**	**Pooled**	**Panel**	**Panel (FE)**
Baseline						
	0.039*	0.030	0.031	0.001	-0.001	-0.001
	(.022)	(.025)	(.025)	(.007)	(.007)	(.007)
Dynamic Effects						
S3	0.057*	0.033	0.034	0.000	-0.005	-0.006
	(.029)	(.030)	(.030)	(.008)	(.009)	(.009)
S4	0.027	0.028	0.029	-0.001	0.001	0.001
	(.025)	(.028)	(.028)	(.009)	(.010)	(.010)
S5	0.025	0.030	0.030	0.004	0.002	0.003
	(.028)	(.032)	(.032)	(.009)	(.008)	(.008)
Heterogeneous Effects						
GA_i=0	0.044	0.028	0.029	0.004	0.004	0.004
	(.028)	(.032)	(.032)	(.010)	(.010)	(.010)
GA_i=1	0.035	0.033	0.033	-0.002	-0.005	-0.005
	(.025)	(.029)	(.029)	(.009)	(.010)	(.010)
Pre-Program Level	0.714	0.723	0.723	0.045	0.038	0.038
N. Obs	18323	12671	12671	19231	13410	13411

Note: This table replicates Table 3, with day agricultural or nonagricultural employment (DANAE) as dependent variable. See Table 3 for details.

5.2 Hours Worked per Week

Previous subsections were interested in the effect of PROGRESA on the extensive margin (whether people work or not). In this one I am interested in its effect on the intensive margin (the number of hours worked).

Skoufias et al. (2001) and Skoufias and di Maro (2008) do not include an estimation of the effect of PROGRESA on the number of hours worked. Alzua et al. (2013) and Banerjee et al. (2017) do, using a DiD empirical strategy, but to the best of my knowledge, they do not acknowledge that the question was asked to a different set of people before and after the start of PROGRESA (see Subsection 4.2).

To avoid this problem one could rely on randomization for the identification by using only the ex-post surveys. One could use $S3$ and $S5$ data, keep all the observations (assigning zero hours worked to those who do not have a salaried job) and estimate the effect of PROGRESA on the number of hours worked in a "salaried" job. The problem of doing so is that the results would be difficult to interpret, since the estimation would mix the effect of PROGRESA on the intensive and extensive margins (Rothstein and von Watcher, 2017). To avoid this problem, and to be able to focus on the intensive margin, I proceed in a different way.

I look only at men who declared to have a DANAE in $S1$ and still declare to have a DANAE in $S3$ and $S5$, respectively.[16] Among these people, the effect of PROGRESA on the number of hours worked, if any, is on the intensive margin. Two problems arise: First, in $S3$ and $S5$ I do not have data of the number of hours worked for all the people who declared to have a DANAE (since the question was only asked to those who declared to have a "salaried" job). I

[16] I focus, for this part of the analysis, on men because as stated before, the percentage of women who had a DANAE at baseline is very small, smaller than 5%.

have data for 92.43% of them (92.44% of the control and 92.43% of the treated; the difference is not statistically significant), therefore I look at those.

Second, one could fear that this is a selected sample, i.e. that the probability of being part of the sample is affected by PROGRESA (Lee, 2009). Because of this, I first look at whether the probability of having a DANAE in $S3$ (respectively, $S5$) for those who had a DANAE in $S1$ is different for treated and control observations. To do that, I run a regression like (1), but only among men who had a DANAE in $S1$. As reported on Table 5, I find that PROGRESA did not have any significant effect at any conventional level on this group. This suggests that the sample is not selected.

Table 5: Effect of PROGRESA on DANAE for those with a DANAE at Baseline

	Pooled	**Panel**	**Panel (FE)**
Baseline	-0.010	-0.021	-0.021
	(.015)	(.014)	(.014)
Heterogeneous effects			
S3	0.003	-0.019	-0.018
	(.022)	(.022)	(.022)
S5	-0.029	-0.024	-0.024
	(.017)	(.018)	(.018)
N. Obs	10266	6864	6864

Note: This table reports the effect of PROGRESA on the probability of having a day agricultural or a nonagricultural employment (DANAE) in S3/S5 for those who had a DANAE in S1 (no data about the number of hours worked is reported in S4). Treated individuals are those who live in treated localities. In the first column I report the results of an OLS regression for a sample that includes all valid observations of men for which $sec_{i,t} = 0$ and who had a DANAE in S1. The second column shows the results of an OLS regression for all valid observations of men who have $sec_{i,t} = 0$ in all the surveys and who had a DANAE in S1. In the third column I use the same sample as before, but I include individual fixed effects. Errors are clustered at the locality level and reported in parenthesis. *** significant at 1% level; ** significant at 5% level; * significant at 10% level.

Given this, I use the following DiD specification for the sample of men having a DANAE in $S1$ and $S3$ or in $S1$ and $S5$, respectively:

$$Hours_{i,t} = \alpha + \beta_1 T_i + \beta_2 T_i * SX + \beta_3 SX + \sum_{j=1}^{J} \gamma_j X_{ji} + u_{it} \tag{4}$$

where $SX \in \{S3, S5\}$. I run the regressions separately for $S3$ and $S5$. $Hours_{i,t}$ is the number of hours worked per week. T_i and control variables are the same as before.

β_2 is the coefficient of interest. It provides the effect of PROGRESA on the number of hours worked per week for men who report to have a DANAE in $S1$ and $S3$ (or $S5$, respectively).

As reported in Table 6, PROGRESA did not have any significant effect on the intensive margin for this sample. Coefficients for $S3$ are all positive, and coefficients in $S5$ are all negative. However they are small, all of them smaller than one hour per week in absolute value. Again, these effects are coherent with a zero income effect on the intensive margin.

Table 6: Effect of PROGRESA on the Number of Hours Worked per Week

	Pooled	**Panel**	**Panel (FE)**
S3	0.376	0.680	0.689
	(.952)	(1.091)	(1.08)
Pre-Program Level	43.43	43.17	43.17
N. Obs	5535	3454	3454
S5	-0.543	-0.465	-0.449
	(.968)	(1.07)	(1.06)
Pre-Program Level	43.69	43.49	43.49
N. Obs	4967	3838	3838

Note: This table reports the effect of PROGRESA on the number of hours worked per week for men who had a day agricultural or nonagricultural employment (DANAE) in S1 and who also had it in S3/S5, respectively. Treated individuals are those who live in treated localities. In the first column I report the results of an OLS regression for a sample that includes all valid observations of men for which $sec_{i,t} = 0$ and who had a DANAE in S1 and also have it in S3/S5, respectively. In the second column the results of an OLS regression for all valid observations of men who have $sec_{i,t} = 0$ in all the surveys and who had a DANAE in S1 and who also have it in S3/S5, respectively. In the third column I use the same sample as before but I include individual fixed effects. Errors are clustered at the locality level and reported in parenthesis. *** significant at 1% level; ** significant at 5% level; * significant at 10% level.

5.3 Extension: Intensity of the Treatment

5.3.1 Specification

In this subsection I analyze the effect of the level of the transfer on the labor supply. Given that the number of people in the households is very variable (ranging from 1 to 14 members), the proportion of the transfer available for each person varies substantially from one household to another, even for households receiving the same total amount. Because of this reason, I prefer to measure the treatment as the "transfer per adult equivalent".

To compute the total transfer of each household, as Bianchi and Bobba (2013), I use the information of enrollment and education level of kids reported at baseline plus the information of Table 1 and I assume that all kids progressed by one grade in each year. I further adjust the transfer to have it in real pesos of Oct 1998. To compute the adult equivalent, I use the OECD definition (Haughton and Khandker, 2009): AE=1-0.7*(number of adults-1)+0.5*(number of kids). Given that I am using only pre-program information and the rules of PROGRESA, this definition of treatment is exogenous.

On average (for periods different from $t = 1$), the total transfer per household in treated localities is of 150 (145) pesos, the adult equivalent is equal to 3.5 (3.4) and the transfer per adult equivalent is of 45.2 (44.1) pesos for the pooled (panel) sample.

Since the amount of the transfer depends on the household composition (determined by personal decisions), it is reasonable to think that treatment is, to some extent, correlated with unobserved individual factors. To take this into account I introduce IFE in all these estimations. Given this, the **Baseline** specification, in this case, becomes:

$$Y_{i,t} = a_i + \beta_1 T_{i,t} + \beta_2 S3 + \beta_3 S4 + \beta_4 S5 + u_{it} \tag{5}$$

where now $T_{i,t}$ is the transfer per adult equivalent of agent i in time t. At time $t = 1$ it is equal to zero for all agents. For the other time periods, i.e $t \in \{3,4,5\}$, $T_{i,t}$ is bigger than zero for agents living in treated localities and equal to zero for agents living in control localities.

a_i summarizes the unobserved individual factors, that are assumed to be constant through time. Given that all controls are at baseline, they are not included in this specification as they would disappear when one computes the fixed effects estimations.

$S3, S4$ and $S5$ are time dummies, equal to 1 if the time of the survey is, respectively, 3, 4 or 5. The coefficient of interest, the DiD estimator, is β_1.

As before, I also estimate a specification with **Dynamic Effects**:

$$Y_{i,t} = a_i + \beta_1 T_{i,t} * S3 + \beta_2 T_{i,t} * S4 + \beta_3 T_{i,t} * S5 + \beta_4 S3 + \beta_5 S4 + \beta_6 S5 + u_{it} \quad (6)$$

where the coefficients of interest are β_1, β_2 and β_3.

Finally, the specification for the **Heterogeneous Effects**, in this case, is the following:

$$Y_{it} = a_i + \beta_1 GA_i * Expost + \beta_2 T_{i,t}^{Ⓝ} + \beta_3 T_{i,t}^{Ⓝ} * GA_i + \beta_4 T_{i,t}^{Ⓔ} + \beta_5 S3 + \beta_6 S4 + \beta_7 S5 + u_{i,t} \quad (7)$$

Where I split the total transfer in its two components: $T_{i,t}^{Ⓝ}$ is the nutritional transfer per adult equivalent in time t and $T_{i,t}^{Ⓔ}$ is the educational transfer per adult equivalent in time t. Notice that $T_{i,t}^{Ⓔ}$ is only different from zero for people with GA_i=1, i.e, those who live in a household that in some t received the educational grant. β_2 provides the effect of increasing the transfer for people who never received the educational grant, those with $GA_i = 0$. The effect of increasing the nutritional grant for people with GA_i=1 is given by $\beta_2 + \beta_3$ and the effect of increasing the educational grant by β_4.

5.3.2 Results

For the sake of exposition, I rescale the treatment, so that it is the transfer *in tens* per adult equivalent. This means that, β_1 in (5) for example, provides the average effect of increasing the transfer by 10 pesos per adult equivalent (on average, this implies an increase of around 35 pesos in the total transfer of the household).

The first line of Table 7 reports the β_1 coefficient of specification (5) where the dependent variable is work. Effects are negative but small, ranging from -0.30 to -0.04 percentage points, and nonsignificant. The corresponding line of Table 8 reports β_1 where the dependent variable is DANAE. In this case, the effect on men of the pooled sample is of 0.71 percentage points, significant at 5% level. The effect on the other samples is smaller and non significant at conventional levels.

The second set of lines of Table 7 reports, respectively, coefficients β_1, β_2 and β_3 of specification (6) where the dependent variable is work. The interpretation is the same as before, but now one can see the effect at the moment of each ex-post survey. The effect is non significant in most of the cases, except for men of the pooled sample in May 1999 ($S4$) for which the effect is equal to -0.34 percentage points and is significant at 10% level; however, this impact seems to decrease up to almost zero (to -0.03 percentage points) six months after. The corresponding lines of Table 8 show that the effect on DANAE is also non significant in most of the cases, except for men in the pooled sample in October 1998 ($S3$), for which the effect is positive, equal to 0.98 percentage points and significant at 5% level. This effect also decreases through time.

Finally, the third set of lines of Table 7 reports coefficient β_2 of specification (7) for GA_i=0. And for GA_i=1, it reports the effect of increasing the nutritional grant ($\beta_2 + \beta_3$) and the educational grant (β_4). This table shows that the effect on the probability of working for men living in households which *only* received the nutritional grant through all the surveys (men with GA_i=0) is significantly negative: increasing the transfer per adult equivalent by 10 pesos reduces the probability of working by 0.71 (0.60) percentage points for the pooled (panel) sample. Recall that a person might be working without receiving any payment. Table 8, corresponding line, reports instead that the effect on DANAE for men is positive and non significantly different from zero. This suggests that for men living in households who only received the nutritional grant, higher transfers might reduce their participation in self-employment or non payed activities but not in activities that generate a fixed income. Table 7 also shows that the effect on the working probability of increasing the educational grant for men with GA_i=1 is positive (0.38 percentage points for the pooled sample and 0.63 percentage points for the panel sample). The effect on DANAE (Table 8) for men of the pooled sample is also positive and equal to 0.95 percentage points.

Table 7: Impact of PROGRESA on the Probability of Working [treatment= transfer (in tens) per adult equivalent]

	MEN		WOMEN	
	Pooled	**Panel**	**Pooled**	**Panel**
Baseline				
	-0.0015	-0.0004	-0.0015	-0.0030
	(.0014)	(.0018)	(.0029)	(.0029)
Dynamic Effects				
S3	-0.0016	-0.0003	-0.0016	-0.0041
	(.0019)	(.0025)	(.0035)	(.0039)
S4	-0.0034*	-0.0025	-0.0031	-0.0047
	(.0020)	(.0025)	(.0031)	(.0033)
S5	-0.0003	0.0004	-0.0006	-0.0019
	(.0015)	(.0017)	(.0030)	(.0029)
Heterogeneous Effects				
GA=0	-0.0071**	-0.0060*	-0.0027	-0.0049
	(.0030)	(.0034)	(.0047)	(.0047)
GA=1				
Nutritional Transfer	-0.0043	-0.0025	-0.0014	-0.0043
	(.0034)	(.0036)	(.0068)	(.0073)
Educational Transfer	0.0038*	0.0063**	-0.0007	-0.0006
	(.0021)	(.0028)	(.0033)	(.0039)
Pre-Program Level	0.9386	0.9423	0.1128	0.1041
N Obs	16995	12692	17751	13472

Note: This table reports the effect of PROGRESA on the probability of working. Treatment is defined as the transfer *in tens* per adult equivalent. I compute the transfer using information at baseline and the program rules. In the first column I report the results of an OLS regression, with individual fixed effects, for a sample that includes all valid observations of men who have $sec_{i,t} = 0$ for at least two different time periods. In the second column the results of an OLS regression, with individual fixed effects, for all valid observations of men who have $sec_{i,t} = 0$ in all the surveys. In the third and fourth columns I report the results of the same estimations for women. Errors are clustered at the locality level and reported in parenthesis. See the main text for the definition of GA_i. *** significant at 1% level; ** significant at 5% level; * significant at 10% level.

Table 8: Impact of PROGRESA on the probability of having a DANAE [Treatment= Transfer (in tens) per Adult Equivalent]

	MEN		WOMEN	
	Pooled	**Panel**	**Pooled**	**Panel**
Baseline				
	0.0071**	0.0062	0.0008	-0.0006
	(.0034)	(.0040)	(.0012)	(.0014)
Dynamic Effects				
S3	0.0098**	0.0071	0.0011	-0.0013
	(.0045)	(.0055)	(.0015)	(.0019)
S4	0.0056	0.0076	-0.0010	-0.0022
	(.0039)	(.0046)	(.0018)	(.0022)
S5	0.0054	0.0054	0.0014	0.0002
	(.0042)	(.0045)	(.0014)	(.0015)
Heterogeneous Effects				
GA=0	0.0059	0.0051	0.0025	0.0012
	(.0061)	(.0072)	(.0026)	(.0031)
GA=1				
Nutritional Transfer	0.0050	0.0030	-0.0025	-0.0033
	(.0089)	(.0101)	(.0036)	(.0039)
Educational Transfer	0.0095*	0.0089	0.0024	0.0004
	(.0052)	(.0065)	(.0023)	(.0028)
Pre-Program Level	0.7154	0.7229	0.0413	0.0375
N Obs	16961	12671	17676	13410

Note: This table replicates Table 7, with day agricultural or nonagricultural employment (DANAE) as dependent variable. See Table 7 for details.

6 Falsification Tests

In this section I discuss some threats for identification and also perform falsification tests to try to rule out these threats; all the tables of this section are reported on Appendix E.

One may fear that the absence of negative effects on labor outcomes is driven by general equilibrium effects unleashed by PROGRESA. A common concern is that PROGRESA caused prices (of goods and land) in treated localities to increase or wages to decrease and therefore people maintained their previous labor choices (even in the presence of the subsidy) to cope with this.

The surveys at the locality level ($S3$, $S4$ and $S5$) collected information about prices. I report an extract of this information in Table 20. Out of fifteen consumption goods just one good has a price that is significantly different (although the difference very small) among treated and control localities. The rest of the prices are very similar.

I have no information about the price of rents in the different localities. Nevertheless, as reported in Table 21, more than 98% of people report that they do not pay any rent: either because they own their house, or because it is lent to them by someone. Therefore this does not seem to be a source of concern.

The surveys at the locality level also contain information about average wages. I report this information in Table 22. According to this information, wages are not significantly different in treated and control localities. Moreover, I estimate the effect on hourly labor earnings using the same methodology as the one used to estimate the effect of PROGRESA on the number of hours worked per week. I report these results in Table 23. I find no significant effect at 5% level, and the point estimates are small but positive.

Finally, I try to rule out the presence of general equilibrium effects by testing whether other time varying factors in the locality characteristics affected labor outcomes. I do this by using the data on non-eligibles:[17] people living in control and treated localities who were not classified as "poor", and therefore did not receive any transfer. In Appendix E I replicate the baseline estimations for non-eligibles with $sec_{i,t} = 0$ and I find no significant effects at 5% level; this is consistent with previous findings of Skoufias and di Maro (2008) and Alzua et al. (2013). I obtain the same if I focus on the poorest half of the non-eligibles with $sec_{i,t} = 0$.

7 Conclusion

In the context of PROGRESA, adults living in households without kids in secondary school should only be affected by the income effect (and not by the cross-substitution effect) of the cash transfer. I have shown that the income effect is not detrimental to the labor outcomes (both the extensive and intensive margin) of the adult recipients. Adults do not seem to have used the additional money to "buy leisure". Given that the cross-substitution effect cannot explain the absence of a response in the labor outcomes, how can this be explained? Two possibilities are: the presence of incomplete financial markets and health and consumption improvements.

At baseline, less than 1% of the analyzed sample reported to have savings. After the start of the program only 4% of households living in control localities reported to have a loan, the majority coming from an informal source (friends or family). The lack of savings, the limited possibilities to get indebted, together with the extreme poverty, may explain that people prefer to use the money provided by PROGRESA to face urgent expenditures or to make investments instead of working less. Gertler et al. (2012) find that PROGRESA beneficiaries invested part of the transfers in productive assets, which allowed them to increase agricultural income by almost 10% after 18 months.

Hoddinott and Skoufias (2004) show that eligible households in treated localities increased their caloric acquisition by 6.4%, and that this higher intake is mostly driven by calories coming from vegetables and animal products. The better food intake can translate in better health outcomes (Gertler, 2000) which in turn may increase productivity and availability to work.

All this suggests that providing cash to people in poor and liquidity constrained environments is not detrimental to adult work.

[17] I stick to the original criteria of eligibility. I do not consider the "densified" as non-eligible (nor as eligible). Like Angelucci and de Giorgi (2009), I drop these observations.

Appendix

A Balance Check

The following table presents a balance check for the observations that I use throughout the paper, i.e those with $sec_{i,t} = 0$. The first four sets of columns are for observations in the "pooled sample" (present in $S1$,$S3$, $S4$ and $S5$ respectively) and the last set of columns for the "panel sample" (those who are present in the four surveys).

Table 9: Balance Check of Characteristics at $t = 1$

	S1				S3				S4				S5				Panel			
Variables at Baseline	**C**	**T**	**PV**	**ND**	**C**	**T**	**PV**	**ND**	**C**	**T**	**PV**	**ND**	**C**	**T**	**PV**	**ND**	**C**	**T**	**PV**	**ND**
Sex (men)	0.49	0.49	0.32	-0.01	0.49	0.49	0.43	-0.01	0.48	0.49	0.34	-0.01	0.48	0.49	0.18	-0.01	0.48	0.49	0.34	-0.01
Work	0.51	0.53	0.02**	-0.06	0.51	0.53	0.09*	-0.04	0.50	0.52	0.09*	-0.04	0.50	0.52	0.10	-0.04	0.50	0.52	0.08*	-0.04
Health insurance	0.02	0.01	0.23	0.03	0.02	0.01	0.14	0.04	0.02	0.01	0.33	0.03	0.02	0.01	0.17	0.04	0.02	0.01	0.28	0.03
Household (hh) head	0.43	0.43	0.93	0.00	0.44	0.43	0.45	0.01	0.44	0.43	0.74	0.00	0.44	0.43	0.43	0.01	0.44	0.44	0.33	0.01
Living in a couple	0.84	0.83	0.40	0.03	0.85	0.84	0.23	0.04	0.86	0.85	0.20	0.05	0.86	0.85	0.31	0.04	0.88	0.86	0.11	0.06
DANAE=1	0.39	0.36	0.11	0.05	0.39	0.36	0.06*	0.06	0.39	0.36	0.10	0.05	0.39	0.36	0.14	0.05	0.39	0.36	0.14	0.05
Years of education	3.33	3.40	0.69	-0.02	3.41	3.49	0.64	-0.03	3.49	3.52	0.87	-0.01	3.51	3.61	0.55	-0.04	3.52	3.65	0.46	-0.04
Num. of people in the hh	4.99	5.02	0.68	-0.02	5.26	5.29	0.72	-0.01	5.30	5.28	0.79	0.01	4.64	4.70	0.41	-0.04	4.63	4.67	0.59	-0.02
Hours worked p/week	21.74	22.65	0.19	-0.04	21.65	22.20	0.46	-0.02	21.35	21.74	0.57	-0.02	21.55	21.91	0.63	-0.02	21.41	21.77	0.62	-0.02
Age	33.52	33.64	0.75	-0.01	32.88	33.03	0.72	-0.01	32.61	32.83	0.62	-0.02	32.25	32.34	0.84	-0.01	32.06	32.13	0.88	-0.01
Means Index	648.52	646.59	0.68	0.03	649.91	648.85	0.83	0.01	652.06	650.14	0.70	0.03	654.02	653.59	0.84	0.01	653.31	652.89	0.93	0.01
Joint F-test			0.184				0.107				0.169				0.112				0.119	

Note: This table reports the results of a balance check, for each column "C" is the mean in control localities, and "T" the mean in treated localities. Errors are clustered at the locality level and reported in parenthesis. ND: normalized difference $\frac{\mu_1-\mu_2}{\sqrt{(\sigma_1^2+\sigma_2^2)/2}}$ (Imbens and Rubin, 2015 pg. 310). All the variables are at baseline. DANAE: day agricultural or nonagricultural employment.

The table is organized in five sets of columns. The first set provides the balance check for all the observations (12773) present in $S1$. The second one, the balance check for all the observations (9978) present in $S3$, etc. The last set presents the balance check for observations of people who were present through all the four surveys (and therefore belong to the panel sample). Number of observations: S1:12 773, S3: 9978, S4: 8003, S5: 6912, Panel: 6541. *** significant at 1% level; ** significant at 5% level; * significant at 10% level

B Descriptive Statistics

This Appendix presents two tables. The first table shows the characteristics of agents with $sec_{i,t} = 0$ vs. those with $sec_{i,t} = 1$ (see Section 4 for the definition of $sec_{i,t}$). The second one summarizes the expenditures of the households that belong to my samples (agents with $sec_{i,t} = 0$). It also includes the percentage of households who own domestic animals.

Table 10: Characteristics of observations with "$sec_{i,t} = 0$" vs. "$sec_{i,t} = 1$"

Characteristics at Baseline ($t = 1$)	$sec_{i,t}$**=0**	$sec_{i,t}$**=1**	**P-value**	**ND**
Sex (men)	0.49	0.50	0.001***	-0.02
Work	0.52	0.54	0.003***	-0.03
Health insurance at work	0.02	0.01	0.422	0.01
Household head	0.43	0.38	0.000***	0.11
Living in a couple	0.83	0.74	0.000***	0.21
DANAE=1	0.37	0.36	0.003***	0.03
Years of education	3.37	2.94	0.000***	0.15
Num. of people in the hh	5.01	7.66	0.000***	-1.21
Num of kids	2.40	4.51	0.000***	-1.20
Hours worked per week	22.32	23.33	0.000***	-0.04
Age	33.59	37.17	0.000***	-0.28
Means Index	647.30	635.26	0.000***	0.15
N. Obs	12773	16714		

Note: This table splits the observations in S1 (at baseline) among those with $sec_{i,t} = 0$ and those with $sec_{i,t} = 1$, and reports the differences among the two. Errors are clustered at the locality level and reported in parenthesis. DANAE: day agricultural or nonagricultural employment. ND: normalized difference: $\frac{\mu_1 - \mu_2}{\sqrt{(\sigma_1^2 + \sigma_2^2)/2}}$. *** significant at 1% level; ** significant at 5% level; * significant at 10% level.

Table 11: Expenditures of Households in Control Localities (S3)

	Pooled	**Panel**
Weekly Food Expenditure		
Fruits	20.6	19.3
Grains	54.6	53.7
Animal origin	24.2	24.1
Industrialized	28.8	28.0
Weekly Transportation Expenditure		
To school	0.6	0.7
Other transportation	6.8	5.8
Expenditure on clothes (6 months)	142.8	124.7
Do you have at home?		
Goats	0.4	0.4
Cows	0.4	0.3
Hens	3.6	3.6
Rabbits	0.0	0.4
Horses	0.1	0.1
Donkeys	0.2	0.2
Oxen	0.0	0.2
N. Households	1720	1141

Note: This table reports data about expenditure and animal ownership for households in the pooled and panel samples in control localities in Oct 1998 (S3). Amounts are in real pesos of Oct 1998.

C Results with "$sec_{i,t} = 0$" restricted

This Appendix includes the results for a robustness check, where I restrict the definition of $sec_{i,t}$ (see Subsection 4.1 for details). Table 12 shows the effect on work, Table 13 on DANAE and Tables 14 & 15 the effect on the number of hours worked per week in a DANAE. Results are qualitatively equal to those obtained in the main text for the original definition of $sec_{i,t}$.

There is, nevertheless, a difficulty with the measurement of the effect of PROGRESA on the number of hours worked per week in a DANAE (see Subsection 5.2 for the empirical strategy). Table 14 shows the impact of PROGRESA on DANAE for those who had a DANAE in $S1$ (as Table 5 does for the original definition of $sec_{i,t} = 0$ in the main text). The effect of PROGRESA on the probability to have a DANAE in $S3$ for those who had a DANAE in $S1$ is small and non significant. Nevertheless, the effect is significantly negative in $S5$, that is, PROGRESA had a negative effect (of 3.2-3.7 percentage points) on the probability of having a DANAE in $S5$ for those who had a DANAE in $S1$. This implies that the sample used to estimate the effect of PROGRESA on the number of hours worked in $S5$ (second set of lines of Table 15) is selected: the probability to belong to the sample is higher for people from control localities than from treated localities, and therefore the estimates for $S5$ may be biased.

Table 12: Impact of PROGRESA on the Probability of Working [$sec_{i,t} = 0$ restricted]

	MEN			WOMEN		
	Pooled	**Panel**	**Panel (FE)**	**Pooled**	**Panel**	**Panel (FE)**
Baseline						
	-0.014	-0.007	-0.007	-0.022	-0.018	-0.018
	(.010)	(.012)	(.012)	(.019)	(.015)	(.015)
Dynamic Effects						
S3	-0.009	-0.002	-0.002	-0.027	-0.023	-0.023
	(.012)	(.014)	(.014)	(.022)	(.018)	(.018)
S4	-0.020	-0.014	-0.014	-0.026	-0.024	-0.024
	(.012)	(.014)	(.014)	(.019)	(.017)	(.017)
S5	-0.013	-0.006	-0.006	-0.010	-0.007	-0.007
	(.010)	(.013)	(.013)	(.023)	(.019)	(.019)
Heterogeneous Effects						
GA_i=0	-0.020	-0.013	-0.014	-0.025	-0.021	-0.021
	(.016)	(.017)	(.017)	(.020)	(.018)	(.018)
GA_i=1	-0.012	0.000	0.001	-0.021	-0.015	-0.015
	(.010)	(.011)	(.011)	(.023)	(.020)	(.020)
Pre-Program Level	0.936	0.940	0.940	0.116	0.102	0.102
N. Obs	16545	11336	11336	17337	12024	12024

Note: This table reports the effect of PROGRESA on the probability of working. Treated individuals are those who live in treated localities. I modify the definition of $sec_{i,t} = 0$, I change the first point of the definition so that it reads: "there have been no kids of ages between **11** to 17 since Oct 1997 ($t = 1$) and up to t". In the first column I report the results of an OLS regression for a sample that includes all valid observations of men for which $sec_{i,t} = 0$. In the second column the results of an OLS regression for all valid observations of men who have $sec_{i,t} = 0$ in all the surveys. In the third one, I use the same sample as before but I include individual fixed effects. In the fourth-sixth columns I report the results of the same estimations for women. Errors are clustered at the locality level and reported in parenthesis. See the main text for the definition of GA_i. *** significant at 1% level; ** significant at 5% level; * significant at 10% level.

Table 13: Impact of PROGRESA on the probability of having a DANAE [$sec_{i,t}$ = 0 restricted]

	MEN			WOMEN		
	Pooled	**Panel**	**Panel (FE)**	**Pooled**	**Panel**	**Panel (FE)**
Baseline						
	0.035	0.016	0.016	-0.002	-0.004	-0.004
	(.0230)	(.027)	(.027)	(.007)	(.007)	(.007)
Dynamic Effects						
S3	0.052*	0.020	0.021	-0.004	-0.007	-0.008
	(.029)	(.032)	(.032)	(.008)	(.009)	(.009)
S4	0.027	0.016	0.017	-0.002	-0.004	-0.004
	(.025)	(.029)	(.029)	(.009)	(.010)	(.010)
S5	0.020	0.011	0.012	0.001	-0.002	-0.001
	(.028)	(.033)	(.033)	(.009)	(.009)	(.009)
Heterogeneous Effects						
GA_i=0	0.044	0.028	0.029	0.004	0.004	0.004
	(.028)	(.032)	(.032)	(.010)	(.010)	(.010)
GA_i=1	0.027	0.001	0.001	-0.007	-0.014	-0.014
	(.026)	(.033)	(.033)	(.009)	(.010)	(.010)
Pre-Program Level	0.674	0.466	0.466	0.059	0.049	0.049
N. Obs	16510	11317	11317	17219	11973	11973

Note: This table replicates Table 12, with day agricultural or nonagricultural employment (DANAE) as dependent variable. See Table 12 for details.

Table 14: Effect of PROGRESA on DANAE for those with a DANAE at baseline [$sec_{i,t}$ = 0 restricted]

	Pooled	**Panel**	**Panel(FE)**
Baseline	-0.014	-0.025	-0.025
	(.015)	(.015)	(.015)
Heterogeneous effects			
S3	0.002	-0.018	-0.017
	(.022)	(.024)	(.023)
S5	-0.037**	-0.033*	-0.032*
	(.018)	(.019)	(.019)
N. Obs	9256	6091	6091

Note: This table reports the effect of PROGRESA on the probability of having a day agricultural or a nonagricultural employment (DANAE) in S3/S5 for those who had a DANAE in S1 (no data about the number of hours worked is reported in S4). Treated individuals are those who live in treated localities. I modify the definition of $sec_{i,t} = 0$, I change the first point of the definition so that it reads: "there have been no kids of ages between **11** to 17 since Oct 1997 ($t = 1$) and up to t". In the first column I report the results of an OLS regression for a sample that includes all valid observations of men for which $sec_{i,t} = 0$ and who had a DANAE in S1. In the second column the results of an OLS regression for all valid observations of men who have $sec_{i,t} = 0$ in all the surveys and who had a DANAE in S1. In the third column, I use the same sample as before but I include individual fixed effects. Errors are clustered at the locality level and reported in parenthesis. *** significant at 1% level; ** significant at 5% level; * significant at 10% level.

Table 15: Effect of PROGRESA on the number of hours worked [$sec_{i,t} = 0$ restricted]

	Pooled	Panel	Panel (FE)
S3	0.262	0.604	0.613
	(.964)	(1.132)	(1.130)
Pre-Program Level	43.48	43.25	43.25
N. Obs	5037	3066	3066
S5	-0.607	-0.482	-0.470
	(1.001)	(1.100)	(1.098)
Pre-Program Level	43.78	43.61	43.61
N. Obs	4465	3404	3404

Note: This table reports the effect of PROGRESA on the number of hours worked per week for men who had a day agricultural or nonagricultural employment (DANAE) in S1 and who also had it in S3/S5, respectively. Treated individuals are those who live in treated localities. I modify the definition of $sec_{i,t} = 0$, I change the first point of the definition so that it reads: "there have been no kids of ages between **11** to 17 since Oct 1997 ($t = 1$) and up to t". In the first column I report the results of an OLS regression for a sample that includes all valid observations of men for which $sec_{i,t} = 0$ and who had a DANAE in S1 and also have it in S3/S5, respectively. In the second column the results of an OLS regression for all valid observations of men who have $sec_{i,t} = 0$ in all the surveys and who had a DANAE in S1 and who also have it in S3/S5 respectively. In the third column, I use the same sample as before but I include individual fixed effects. Errors are clustered at the locality level and reported in parenthesis. *** significant at 1% level; ** significant at 5% level; * significant at 10% level.

D Results for the Entire Sample

This Appendix replicates Tables 3, 4, 5 and 6 of the main text for the *whole* sample of PROGRESA. That is, for all adult agents regardless of their value of $sec_{i,t}$ (see Section 4 for the definition of $sec_{i,t}$). Results of Tables 16 and 17 are similar to what was previously found by Skoufias et al. (2001) and Skoufias and di Maro (2008). They are, all in all, coherent with no negative effect on the extensive margin. If something, PROGRESA had a positive effect on DANAE for men, however this effect seems to be important at the beginning (S3) and to decrease afterwards (S4 and S5).

There is, nevertheless, a difficulty with the measurement of the effect of PROGRESA on the number of hours worked per week in a DANAE (see Subsection 5.2 for the empirical strategy). Table 18 shows the impact of PROGRESA on DANAE for those who had a DANAE in $S1$ (as Table 5 does for people with $sec_{i,t} = 0$ in the main text). The effect of PROGRESA on the probability to have a DANAE in $S3$ for those who had a DANAE in $S1$ is small and non significant. Nevertheless, the effect is significantly negative (at 10% level) in $S5$ for the pooled sample. PROGRESA had a negative effect of 3 percentage points on the probability of having a DANAE in $S5$ for people in the pooled sample who had a DANAE in $S1$. This implies that the pooled sample used to estimate the effect of PROGRESA on the number of hours worked in $S5$ (second set of lines of Table 19) is selected: the probability to belong to the sample is higher for people from control localities than from treated localities, and therefore the estimates for $S5$ may be biased.

Table 16: Impact of PROGRESA on the Probability of Working [full sample]

	MEN			WOMEN		
	Pooled	**Panel**	**Panel (FE)**	**Pooled**	**Panel**	**Panel (FE)**
Baseline						
	-0.003	0.003	0.003	-0.013	-0.014	-0.014
	(.007)	(.007)	(.007)	(.015)	(.014)	(.014)
Dynamic Effects						
S3	0.003	0.008	0.008	-0.014	-0.017	-0.017
	(.008)	(.008)	(.008)	(.016)	(.016)	(.016)
S4	-0.011	-0.005	-0.004	-0.013	-0.011	-0.011
	(.008)	(.009)	(.009)	(.015)	(.015)	(.015)
S5	-0.001	0.006	0.006	-0.013	-0.013	-0.013
	(.008)	(.008)	(.008)	(.018)	(.017)	(.017)
Pre-program Level	0.927	0.936	0.936	0.143	0.130	0.130
N. Obs	51158	39784	39784	52871	41652	41652

Note: This table reports the effect of PROGRESA on the probability of working. Treated individuals are those who live in treated localities. In the first column I report the results of an OLS regression for a sample that includes all valid observations of men (not only those with $sec_{i,t} = 0$) . In the second column the results of an OLS regression for all valid observations of men who can be observed throughout the four surveys. In the third column, I use the same sample as before but I include individual fixed effects. In the fourth-sixth columns I report the results of the same estimations for women. Errors are clustered at the locality level and reported in parenthesis. *** significant at 1% level; ** significant at 5% level; * significant at 10% level.

Table 17: Impact of PROGRESA on the probability of having a DANAE [full sample]

	MEN			WOMEN		
	Pooled	**Panel**	**Panel (FE)**	**Pooled**	**Panel**	**Panel (FE)**
Baseline						
	0.045**	0.037	0.038	0.005	0.005	0.005
	(.021)	(.023)	(.023)	(.006)	(.005)	(.005)
Dynamic Effects						
S3	0.066**	0.056**	0.056**	0.008	0.006	0.006
	(.027)	(.028)	(.028)	(.007)	(.006)	(.006)
S4	0.037	0.030	0.031	0.003	0.004	0.004
	(.024)	(.025)	(.025)	(.007)	(.006)	(.006)
S5	0.028	0.026	0.026	0.005	0.005	0.005
	(.026)	(.028)	(.028)	(.007)	(.007)	(.007)
Pre-program Level	0.674	0.466	0.466	0.059	0.049	0.049
N. Obs	51015	39688	39688	52676	41491	41491

Note: This table replicates Table 16, with day agricultural or nonagricultural employment (DANAE) as dependent variable. See Table 16 for details.

Table 18 shows the impact of PROGRESA on DANAE for those who had a DANAE in $S1$. The effect of PROGRESA on the probability to have a DANAE in $S3$ for those who had a DANAE in $S1$ is small and non significant. But, the effect is significantly negative in $S5$ for the pooled sample. PROGRESA had a negative effect of 3 percentage points on the probability of having a DANAE in $S5$ for those who had a DANAE in $S1$, this effect is significant at 10% level. This implies that the sample used to estimate the effect of PROGRESA on the number of hours worked in $S5$ (second set of lines of Table 19) could be slightly selected: the probability to belong to the sample is higher for people from control localities than from trated localities, and therefore the estimates for $S5$ may be biased.

Table 18: Effect of PROGRESA on DANAE for those with a DANAE at Baseline [full sample]

	Pooled	**Panel**	**Panel(FE)**
Baseline	-0.008	-0.013	-0.013
	(.015)	(.015)	(.015)
Heterogeneous effects			
S3	0.011	-0.002	-0.002
	(.023)	(.022)	(.022)
S5	-0.030*	-0.024	-0.024
	(.016)	(.017)	(.017)
N. Obs	26394	20220	20220

Note: This table reports the effect of PROGRESA on the probability of having a day agricultural or a nonagricultural employment (DANAE) in S3/S5 for those who had a DANAE in S1 (no data about the number of hours worked is reported in S4). Treated individuals are those who live in treated localities. In the first column I report the results of an OLS regression for a sample that includes all valid observations of men (not only those with $sec_{i,t} = 0$) who had a DANAE in S1. In the second column the results of an OLS regression for all valid observations of men who can be observed throughout the four surveys and who had a DANAE in S1. In the third column, I use the same sample as before but I include individual fixed effects. Errors are clustered at the locality level and reported in parenthesis. *** significant at 1% level; ** significant at 5% level; * significant at 10% level.

Table 19: Effect of PROGRESA on the Number of Hours Worked per Week [full sample]

	Pooled	**Panel**	**Panel (FE)**
S3	0.149	0.521	0.520
	(.808)	(.852)	(.851)
Pre-Program Level	43.60	43.37	43.37
N. Obs	12670	9750	9750
S5	0.405	0.418	0.428
	(.770)	(.779)	(.779)
Pre-Program Level	43.86	43.65	43.65
N. Obs	12492	10736	10736

Note: This table reports the effect of PROGRESA on the number of hours worked per week for men who had a day agricultural or nonagricultural employment (DANAE) in S1 and who also had it in S3/S5, respectively. Treated individuals are those who live in treated localities. In the first column I report the results of an OLS regression for a sample that includes all valid observations of men (not only those with $sec_{i,t} = 0$) who had a DANAE in S1 and also have it in S3/S5, respectively. In the second column the results of an OLS regression for all valid observations of men who can be observed throughout the four surveys, had a DANAE in S1 and who also have it in S3/S5 respectively. In the third column, I use the same sample as before but I include individual fixed effects. Errors are clustered at the locality level and reported in parenthesis. *** significant at 1% level; ** significant at 5% level; * significant at 10% level.

E Threats to Identification and Falsification Tests

In this Appendix I try to rule out some threats to identification (see Section 6 for details).

Table 20: Prices of Consumption Goods

	Obs.	Villages	Control	Treat	t-stat	P-value
Kg tomatoe	757	413	6.7	6.9	1.11	0.269
Kg onion	751	406	5.1	5.6	1.56	0.120
Kg potatoe	668	382	6.0	6.6	2.45	0.015**
Kg carrot	229	191	4.0	4.2	0.62	0.538
Kg orange	383	276	3.2	3.0	-0.82	0.415
Kg banana	542	350	3.6	3.7	0.93	0.352
Kg apple	322	250	9.3	9.8	1.52	0.130
Kg tortillas	239	198	3.6	3.6	-0.42	0.678
Kg rice	1065	473	6.5	6.5	0.03	0.975
Kg meat of chicken	376	278	18.6	19.4	1.37	0.171
Kg meat of cow	208	185	26.8	26.3	-0.36	0.719
Kg beans	938	459	9.6	9.7	0.38	0.703
Kg eggs	968	463	9.1	9.1	0.05	0.960
L of milk	682	398	5.9	6.0	0.45	0.653
Kg sugar	1092	479	5.6	5.6	0.39	0.698

Note: Errors are clustered at the locality level. All prices are expressed in Oct 1998 (S3) pesos. Information is taken from the ENCEL surveys: Cuestionario de la localidad. *** significant at 1% level; ** significant at 5% level; * significant at 10% level.

Table 21: Ownership Status of the House

	Pooled	**Panel**
Own house (fully paid)	90.71	90.69
Own house (paying)	0.28	0.38
Rented	0.5	0.41
Lent	8.23	8.35
Received in exchange of something	0.16	0.09
Others	0.09	0.06
Does not know/No answer	0.03	0.02
Total	12773	6541

Note: Data taken from $S1$ for observations with "$sec_{i,t} = 0$".

Table 22: Wages Reported at the Locality Level

	Obs.	**Localities**	**Control**	**Treat**	**t-statistic**
Legal minimum daily agricultural w.	1497	505	30.8	30.6	-0.17
Real daily agricultural w (men)	1449	504	29.4	29.2	-0.17
Real daily agricultural w (women)	619	349	26.5	26.9	0.29

Note: w: wage. Errors are clustered at the locality level. Data on wages is available for S3, S4 and S5. All prices are expressed in Oct 1998 (S3) pesos. This information is taken from the ENCEL surveys: Cuestionario de la localidad.

Table 23: Effect of PROGRESA on Wages per Hour

	Pooled	**Panel**	**Panel (FE)**
S3	0.206	0.234	0.261*
	(.131)	(.154)	(.154)
Pre-Program Level	4.39	4.07	4.07
N. Obs	4927	3010	3010
S5	0.180	0.224	0.216
	(.142)	(.148)	(.149)
Pre-Program Level	4.31	4.11	4.11
N. Obs	4363	3348	3348

Note: This table reports the effect of PROGRESA on the wage per hour for men who had a DANAE in S1 and also have it in S3/S5, respectively. I restrict the analysis for men within the 99% of hourly wage range, that is, for all who had a real hourly wage smaller than 26 pesos per hour. All figures are in real pesos of Oct. 1998. To compute the wages I divide the earnings per week by the total number of hours worked per week. *** significant at 1% level; ** significant at 5% level; * significant at 10% level.

Table 24: Effect of PROGRESA on Non-Eligibles

	MEN			WOMEN		
Baseline	**Pooled**	**Panel**	**Panel (FE)**	**Pooled**	**Panel**	**Panel (FE)**
Work	0.000	0.009	0.009	-0.030*	-0.001	-0.002
	(.009)	(.012)	(.012)	(.017)	(.026)	(.026)
Pre-Program Level	0.939	0.948	0.948	0.249	0.269	0.269
N. Obs	17804	7436	7436	16841	6996	6996
DANAE	0.018	0.008	0.008	-0.009	-0.001	0.003
	(.023)	(.032)	(.032)	(.010)	(.017)	(.010)
Pre-Program Level	0.575	0.520	0.520	0.103	0.099	0.099
N. Obs	17754	7418	7418	16762	6969	6969

Note: This table reports the effect of PROGRESA on the probability of working and on the probability of having a day agricultural or a nonagricultural employment (DANAE). I restrict the analysis to people who were classified as non poor, and therefore who were not eligible to receive the cash transfers of PROGRESA. Treated individuals are those who live in treated localities. In the first column I report the results of an OLS regression for a sample that includes all valid observations of men for which $sec_{i,t} = 0$. In the second column the results of an OLS regression for all valid observations of men who have $sec_{i,t} = 0$ in all the surveys. In the third column, I use the same sample as before but I include individual fixed effects. In the fourth-sixth columns I report the results of the same estimations for women. Errors are clustered at the locality level and reported in parenthesis. *** significant at 1% level; ** significant at 5% level; * significant at 10% level.

Table 25: Effect of PROGRESA on the Poorest Half of Non-Eligibles

	MEN			WOMEN		
Baseline	**Pooled**	**Panel**	**Panel (FE)**	**Pooled**	**Panel**	**Panel (FE)**
Work	0.002	0.005	0.007	-0.034	-0.023	-0.023
	(.012)	(.018)	(.018)	(.020)	(.035)	(.035)
Pre-Program Level	0.937	0.944	0.944	0.230	0.258	0.258
N. Obs	8773	3116	3116	8407	2940	2940
DANAE	0.019	0.022	0.025	-0.017	0.002	0.002
	(.027)	(.041)	(.040)	(.014)	(.029)	(.029)
Pre-Program Level	0.625	0.580	0.580	0.097	0.090	0.090
N. Obs	8753	3112	3112	8369	2932	2932

Note: This table replicates Table 24, but here I restrict the analysis to the poorest half (using the means index) of people who were classified as non poor, and therefore who were not eligible to receive the cash transfers of PROGRESA. See Table 24 for details.

F Heterogeneity with respect to the Means Index

In this Appendix I report the results of running specification (3) using the means index to split the sample in two: the poorest and the less poor. As reported in the main text, I only find a significant difference among the pooled sample of women when the dependent variable is work: the effect on the poorest women is positive (but not significant and very close to zero) while the effect on less poor women is negative (and significant at 10%).

Table 26: Effect of PROGRESA on the probability of working; Heterogeneity with respect to the Means Index

	MEN			WOMEN		
	Pooled	**Panel**	**Panel (FE)**	**Pooled**	**Panel**	**Panel (FE)**
Less Poor	-0.012	-0.001	-0.001	-0.038*	-0.029	-0.029
	(.011)	(.012)	(.012)	(.021)	(.021)	(.021)
Poorest	-0.019	-0.012	-0.012	0.003	0.000	0.000
	(.013)	(.017)	(.017)	(.023)	(.020)	(.020)
N. Obs	18360	12692	12692	19306	13472	13472

Note: This table reports the effect of PROGRESA on the probability of working for the poorest and for the less poor, separately. I run a regression similar to (3), but in this case I exploit the Means Index to see whether PROGRESA has a different effect on the poorest and the less poor . Both the poorest and the less poor were classified as "poor" and therefore are eligible to receive the benefits. Treated individuals are those who live in treated localities. In the first column I report the results of an OLS regression for a sample that includes all valid observations of men for which $sec_{i,t} = 0$. In the second column the results of an OLS regression for all valid observations of men who have $sec_{i,t} = 0$ in all the surveys. In the third column, I use the same sample as before but I include individual fixed effects. In the fourth-sixth columns I report the results of the same estimations for women. Errors are clustered at the locality level and reported in parenthesis. *** significant at 1% level; ** significant at 5% level; * significant at 10% level.

Table 27: Effect of PROGRESA on the probability of having a DANAE; Heterogeneity with respect to the Means Index

	MEN			WOMEN		
	Pooled	**Panel**	**Panel (FE)**	**Pooled**	**Panel**	**Panel (FE)**
Less Poor	0.039	0.039	0.040	-0.002	-0.003	-0.003
	(.024)	(.030)	(.030)	(.009)	(.010)	(.010)
Poorest	0.041	0.019	0.019	0.004	0.002	0.002
	(.031)	(.035)	(.035)	(.009)	(.009)	(.009)
N. Obs	18323	12671	12671	19231	13410	13411

Note: This table replicates Table 26, with day agricultural or nonagricultural employment (DANAE) as dependent variable. See Table 26 for details.

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