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, providing unconditional cash to people has detrimental effects on the probability of finding a job (Chetty, 2008). 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 theories there is a negative "income effect" on labor outcomes. For developed countries there is empirical evidence that supports these theoretical predictions (Cesarini et al., 2017, Picchio et al., 2018, Chetty, 2008, Card et al., 2007, Basten et al., 2014, Schirle, 2015, Gonzalez, 2013). 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. Cash transfers in those contexts could possibly be used by the recipients to cope with these difficulties. Therefore, they could allow them 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 standard

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properties of canonical job-search and neo-classical models of labor supply (Baird et al., 2018, Bosch and Manacorda, 2012). 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 unconditionally is or not detrimental to work.

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, as just said, PROGRESA is a *conditional* program. Thus, it is normal to expect the conditionality to play some role in preventing negative effects on labor outcomes of the adult recipients. In fact, a program offering cash subject to the condition that school age kids go to school, induces a "cross-substitution effect" on adults (Rubio-Codina, 2010, Parker and Todd, 2017). That is, as a response to the program, adults might work more to substitute for child's work. Thus, it has been argued, "for adults, the program has ambiguous effects on leisure and time spent in work activities because the income effect and the cross-substitution effect of school subsidies work in different directions" (Parker and Todd, 2017) and because of that reason "the effects of a conditional transfer are likely to differ compared with an unconditional cash transfer" (ibid).

But then, would PROGRESA still be non detrimental to adult work if the analysis was restricted to a subsample not affected by the conditionality of the program? For such a subsample the "cross-substitution effect" would no longer be present. Thus, PROGRESA would induce (only) an income effect. This article intends to answer that question by restricting the dataset of PROGRESA to a subsample (exogenously defined) of adults for whom, I claim, the conditionality was not binding. Thus, in practice, for this subsample, it is as if PROGRESA was an *un*conditional cash transfer.

I focus on the subsample of adults living in households without kids between 12-17 years old. Since school attendance for kids below 12 years was almost universal (see Fig.1), the school conditionality of PROGRESA was not binding for them. That is, it did not induce them (nor their parents) to change their behavior. Throughout the paper I focus on this subsample and 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), a measure that excludes those who are self-employed or who work without receiving any payment and (3) the number of hours worked per week. I find, using a difference-in-differences (DiD) empirical strategy, that PROGRESA did not induce this subsample of adults to work less. If anything, the results on DANAE (which is closer to salaried work) are positive.

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

This section starts by presenting empirical evidence about the effect of UCT in developed countries in contexts in which the recipients do not live in poverty. Further, it discusses additional mechanisms that could be in place in a context of poverty. Finally, it sums up empirical evidence of the effects of cash transfers in contexts of scarcity. As stated previously, the aim of the current work is to contribute to the latter literature.

As highlighted by Schirle (2015) few articles analyze the effect of demogrants (grants awarded on purely demographic principals, and thus in practice, unconditional) on labor supply, since many of the existing cash transfer programs have conditions or requirements that affect labor supply incentives, and therefore are substantially different from unconditional transfers. The findings of the articles that have successfully analyzed the effect of unconditional transfers in developed countries, for households that do not live in poverty, tend to find results that are consistent with the predictions of the standard job-search model or the canonical model of labor supply. That is, they find that people receiving unconditional money tend to work less, even if the magnitude of the reduction is not necessarily big.

Some papers have looked at this by 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. Yet, as highlighted by Gonzalez (2013), their results may not be typical responses to increases in other forms of unearned income. Moreover, these studies might not be representative of the overall population.

Other studies have looked at the effect of providing cash to people by analyzing data of severance payments (which are received on top of the unemployment compensations). Chetty (2008) does 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. Again, one should keep in mind that the sample of laid off people can be highly selective.

Still other set of articles has looked at the effects of family allowances. In fact, family allowances (which are in place in most developed countries) could act as a sort of unconditional transfers for families, at least in so far as the transfers do not induce the couple to have (more) kids. That is, in practical terms, the benefit is exogenous to the households given the presence of children (Kooreman, 2000).

For example, Schirle (2015) looks at the effect of the universal child care benefit (UCCB) introduced in 2006 by the Government of Canada. This program gives benefits of $100 (Canadian dollars) to families per each kid below the age of 6. With a DiD estimation, and data from 2003-2009, she finds a small but negative effect of the UCCB on both parents labor supply (1.3 percentage points reduction in the extensive margin for married women and 0.4 percentage points for men). Similarly, Gonzalez (2013), using a regression discontinuity design, looks at the effect of a one-time cash transfer of 2500€ in Spain that was paid to women having a baby from July 2007 onward. She finds that women who received the benefit were 4 percentage points less likely to be working when the baby had 12 months as compared to those who did not receive the transfer.

Moreover, a recent article studies the effects of a sustained unconditional cash transfer in Alaska. Since 1982 Alaskan residents (of any age) have been entitled to a yearly cash dividend from the Alaska Permanent fund which in recent years is of around $2000 per person. Using a synthetic control method (which mixes DiD estimators and elements of matching), Jones and Marinescu (2018) do not find any significant effect on employment (i.e, on the extensive margin), but the find an increase of 1.8 percentage points in the share of Alaskans who work in par-time jobs.

However, the effect that UCT have on labor outcomes might be substantially different for people living in poverty. In such a context one could expect them to have positive effects that, overall, might outweigh the typical negative effect emphasized by the neo-classical labor supply model. One reason for this to happen is the one put forward, many years ago, by Leibenstein (1957): “the amount of work that the representative laborer can be expected to perform depends on his energy level, his health, and his vitality, which in turn depend on his consumption level and most directly on the nutritive value of his food intake”. This might be particularly true in a developing rural economy in which work requires a high-energy expenditure, as emphasized by Strauss, 1986. Another possible reason is that the money could be used to lessen liquidity constraints in contexts of incomplete financial markets where the access to credit is nearly impossible (Alderman and Yemtsov, 2013, Banerjee et al., 2019). Still, another mechanism that could be in place for people living in poverty is the one analyzed by Shah et al. (2012), Mullainathan and Shafir (2013), Mani et al. (2013), Shah et al. (2015), and Schilbach et al. (2016). According to them, living in a context of scarcity taxes cognitive resources and this might be detrimental for other aspects of life. Therefore it could be argued that cash transfers, provided to people living in poverty, might lessen the cognitive capacity constraints and thus have a positive effect on labor outcomes (Mesén Vargas and Van der Linden, 2019).

In fact, there is some recent empirical evidence showing that providing unconditional cash to agents in poverty is not detrimental to their labor outcomes. 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 on 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. 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. The transfers boosted the incomes of poor households. Since the previous subsidy was regressive, poor households were more than compensated with the new policy. Transfers amounted to 29% of the median household income. The authors 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.

The current article intends to contribute to this literature by exploiting high quality RCT data and looking at the effects on adults’ labor outcomes of a long-lasting, generous and periodical transfer provided to thousands of recipients living in poverty.

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 changed its name to “Oportunidades” in 2000.[1] It targeted benefits directly to people living in extreme poverty in rural areas of Mexico. As

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

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 (typically the mother of the kids in school age, if any). They had two main components. First, the nutritional grant was received by all beneficiary households 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, and for kids in secondary school according to gender as well. 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).

[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).

[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 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.

Table 1: Monthly Amount of Transfers in Real Pesos of Oct 1998

	Oct 1998	**May 1999**	**Nov 1999**
Education Grant in Primary School per Kid			
Third Grade	70	68	69
Fourth Grade	80	81	82
Fifth Grade	100	104	108
Sixth Grade	135	135	142
Education Grant Secondary School per Kid			
Girls			
Seventh	210	212	216
Eight	235	234	242
Ninth	255	257	263
Boys			
Seventh	200	198	207
Eight	210	212	216
Ninth	220	221	229
School Materials per Kid (once a year)			
Primary (September)			181
Primary (January)		41	
Secondary (September)	170		177
Nutritional Grant (per Household)	100	104	108
Maximum Grant (per Household)	625	626	647

Note: The data to construct this table is taken from Skoufias (2005). Amounts are in real pesos of Oct 1998 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 a panel data of 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 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 in treatment localities, 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] in October 1997 (S1), ENCEL[6] in March 1998 (S2), in October 1998 (S3), in May 1999 (S4) and in 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 observa-

[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.

tions without missing data) samples.[8]

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).

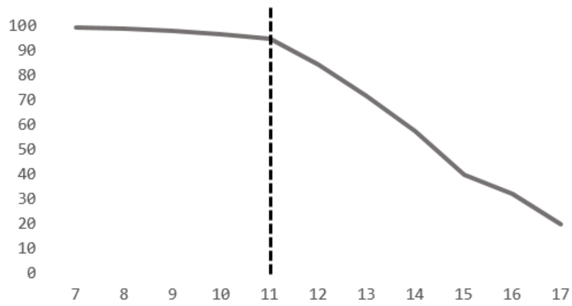

Figure 1: **School Assistance (%) by age:** The graph shows the attendance rate by age, from 7 to 17 years old, for poor people in treatment and control localities. I use information from ENCASEH 1997 survey and include only observations without missing school attendance data.

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) write “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) write “... 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) write “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) write “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]

[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.

[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).

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 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*.

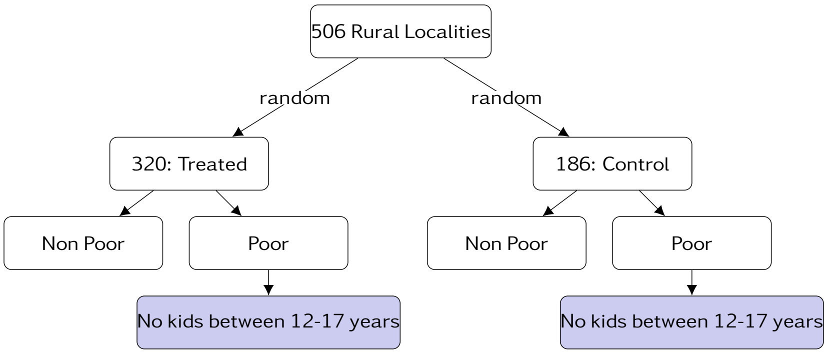

Figure 2: **Design of PROGRESA:** Out of the 506 chosen rural localities, 320 were assigned to treatment and 186 to control. Only eligible (poor) people in treated localities received the transfers during the analyzed periods of 1998-1999. In the core of this paper, I analyze the effect of PROGRESA on 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 years 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 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 for whom $work_{i,t} = 1$. This question contains eight alternatives: (1) agricultural worker (2) nonagricultural employee (3) self-employed (4) business owner (5) worker in a family business (without receiving any payment) (6) worker without payment (non including family businesses) (7) member of a cooperative (8) ejidatarios.[12] 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.[13]

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 but more kids below 6 years old, 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 12) per household is a bit higher than 2.

As reported in Tables 11 and 12 in Appendix B, which look at data in $t = 1$ (before the start of PROGRESA), women who work more often are household heads, less often live in a couple, tend to have less kids below 6 years old, and are older and less educated than women who do not work. Men who work more often are household heads than men who do not work, but contrary to women, working men often have more kids below 6 years old, more often live in a couple, and are younger and more educated than men who do not work. The same patterns are true when one compares women and men who have a DANAE vs. those who do not (see Tables 13 and 14). These descriptive statistics are compatible with a very traditional division of labor between genders, and they suggest that women work (outside the household) mostly when they *have to*, i.e, when they are household heads.[14]

[12]In Mexico an ejido is an area of communal land used for agriculture, on which community members individually farm designated parcels and collectively maintain communal holdings.

[13]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.

[14]In fact, whereas 44% (38%) of women who are household heads report to work in the pooled (panel) sample, only 12% (9%) of women who are not household heads do. The patterns for DANAE are similar, 23% (21%) of women who are household heads report to have a DANAE, instead only 4% (3%) of women who are not household heads do. Moreover, 89% (87%) of women who report to be household heads do not live in a couple, i.e, apparently women report to be household heads mostly when they do not have a partner.

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 fraction of people that reported to work. DANAE is an acronym for: day agricultural or nonagricultural employment. I report the fraction of people (*among the total*) that reported to have this type of work.

In Table 15 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 15 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 real pesos of Oct 1998.[15] 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).

[15] In Appendix E, Table 24 , 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, it is women who are entitled to receive the transfers 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 amount of 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:

$$Y_{i,t} = \alpha + \beta_1 T_i + \beta_T T_i * Expost_t + \lambda_3 S3 + \lambda_4 S4 + \lambda_5 S5 + \sum_{j=1}^{J} \gamma_j X_{ji} + u_{i,t} \tag{1}$$

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 (among the 506 possible ones), 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.[16]

The coefficient of interest is β_T, 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:

$$Y_{i,t} = \alpha + \beta_1 T_i + \beta_{T3} T_i * S3 + \beta_{T4} T_i * S4 + \beta_{T5} T_i * S5 + \lambda_3 S3 + \lambda_4 S4 + \lambda_5 S5 + \sum_{j=1}^{J} \gamma_j X_{ji} + u_{i,t} \quad (2)$$

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_{T3}, β_{T4} and β_{T5}. 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**

(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.[17]

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

[17] 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.

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:

$$Y_{i,t} = \alpha + \beta_1 GA_i + \beta_2 T_i + \beta_3 T_i * GA_i + \beta_4 GA_i * Expost_t + \beta_T T_i * Expost_t + \beta_{TG} T_i * GA_i * Expost_t$$
$$+ \lambda_3 S3 + \lambda_4 S4 + \lambda_5 S5 + \sum_{j=1}^{J} \gamma_j X_{ji} + u_{i,t} \tag{3}$$

where GA_i is the dummy variable defined above and the rest is the same as before. The coefficients of interest are β_T for the group with $GA_i = 0$ and $\beta_T + \beta_{TG}$ 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 general 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". The first column of 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. Recall from the definition of work that people who work (work=1) do not necessarily receive an income in exchange. Table 4 replicates Table 3 for the outcome DANAE. DANAE=1 implies that the person works in a day agricultural or nonagricultural work and gets paid in exchange. Thus, DANAE is closer to the idea of remunerated employment. In the coming paragraphs I comment first the results for men, and then for women.

For men, when work is the outcome variable, coefficients are overall negative, but none of them is significant at conventional levels. Moreover, the magnitude of the coefficients is relatively small. In the baseline estimation, for instance, no effect is bigger than 1.4 percentage points in absolute value. Instead, when DANAE is the outcome variable, coefficients are mostly positive. PROGRESA seems to have a positive effect of 3.9 percentage points (pooled sample) when one looks at the three ex-post surveys altogether. The effect in Oct 98 ($t = 3$) was large, of 5.7 percentage points, and significant at 10%.

Appendix F explores, using a specification similar to (3), whether the effects are heterogeneous according to some relevant characteristics at baseline. Tables 30-31 look at whether the effect of PROGRESA is different among those who are poor as compared to those who are less poor (I used the means index at $t = 1$ to split the sample in two). For men, we find that the differences between the two groups are small and non significantly different from zero. Tables 32-33, in turn, look at whether the effect of PROGRESA differs for those who were household heads at baseline as compared to those who were not. The differences are not statistically different, however, they are sizeable. For instance, for both groups PROGRESA has a negative effect on work, but the effect is more negative among those who are not household heads. Instead, the effect on DANAE even if positive for both groups, is mostly driven by men who are household heads.

Overall, one could say that for men PROGRESA had negative but small effects on work and positive, bigger, effects on DANAE, even if these effects are not always significant. This suggests that PROGRESA made some men move from less formal activities (comprised in "work") into some other activities for which they received a fixed payment, that is, DANAE. Moreover, the positive effect on DANAE is much stronger among men who were household heads at baseline. With time, the positive effects on DANAE remain positive and larger in absolute value than the negative effects on work, however, they seem to lose strength. If anything, one could say that PROGRESA had a positive effect on DANAE for men.

As highlighted by Banerjee et al. (2017), the expected effect of cash transfers on women in a context like the present one is not obvious at all. The additional income might allow a woman who previously had a job to stay home with the children if she prefers, but at the same time, additional income might allow her to afford childcare and actually, to be able to work.

In Table 3 we observe a considerable reduction (even if not significantly different from zero) of work performed by women when the expost surveys are considered altogether. However, it seems that this reduction was mostly present at the beginning of the program (at $t = 3$), and did not last much, since by November 1999 ($t = 5$) the negative effect of PROGRESA on work for women decreased substantially. The effects of PROGRESA on DANAE, for women, are very small in absolute terms and statistically insignificant.

Recall that only a minority of women (no bigger than 12% at baseline for any of the samples) performed *any* kind of work outside the household at baseline, and that the biggest share of those who did, were household heads. In fact, looking at Tables 32-33 in Appendix F one can see that the reduction of work for women seems to be mostly driven by women who were household heads. Even if the difference in the effect on women who were household heads as compared with those who were not is not statistically significant, the negative effect for household heads is substantial and more negative than the one for those who were not household heads. The effects on DANAE, instead, are almost equal to zero in absolute terms for both subgroups.[18]

Tables 30-31, which analyze the heterogeneity of effects among poorer and less poor, show that the effect on work is significantly different (at 10%) among poorer and less poor women. The negative effect is almost totally driven by women who were less poor, since the effect on the poorest women is virtually equal to zero in absolute terms. Instead, the level of poverty does not significantly change the effect of PROGRESA on DANAE for women, which again seems to be close to zero in absolute terms both for the poorer and for the less poor women.

Overall, for women we observe a reduction of work which is considerable in size, even if not statistically different from zero. This negative effect is not accompanied by an increase in their participation in paid employment, as it was the case for men (in fact, the effect of PROGRESA on DANAE seems to be pretty close to zero in absolute terms for women in every considered specification). Possibly cultural norms might help explain that women did not perform more paid employment outside the household, since, for instance, as reported by Adato et al. (2000) before the start of PROGRESA more than 90% of women reported that they needed their husband's permission to visit relatives or neighbors. Strong social norms together with the fact that the activities comprised in "work" (mostly self-employment and unpaid family work) are probably badly remunerated, might help to explain that women, at the beginning, were more likely to stop performing these activities and possibly to substitute

[18]However, given the very low pre-program level, these small absolute effects become much more sizeable in relative terms (for instance, the relative effect of PROGRESA on DANAE for women who are household heads in the pooled sample is equal to 0.008/0.045=18%).

them by more activities at home. However, as remarked before, the negative effect on work for women was almost equal to zero by the time of the last survey. Therefore, one could conclude that PROGRESA had a (non-significant) negative but non long-lasting effect on women's work that was mostly driven by those who were less poor (among the poor) and household heads, and a negligible (absolute) effect on paid employment (DANAE).

Tables 3-4 also report the effect of PROGRESA for two different groups of people, those who never received an educational grant coming from a kid in primary school (those with $GA_i = 0$) and those who, in some point, received an educational grant coming from a kid in primary school (those with $GA_i = 1$). As explained before, since those with $GA_i = 0$ only received the nutritional transfer, which is fully unconditional (except for the free annual medical check-ups), the effect of PROGRESA on this subsample is a pure income effect by definition. The effects for this group, however, are similar (and the difference, i.e, β_{TG}, non-statistically different) from the effects that we observe for those with $GA_i = 1$ for which, I have claimed throughout the paper, the transfers (even if they contain an educational component) are also, in practice, unconditional.

Let me just recall at this stage that, as reported in Appendix C, these results are robust to a slight change in the definition of "$sec_{i,t}$" (see Section 4.1) according to which the sample is (further) restricted to adults living in households in which never, up to t, lived kids aged **11** and above. In fact, the results are qualitatively the same and very similar in magnitude.

Finally, it is worth to highlight that despite the existing differences between my sample (i.e, those with $\text{sec}_{i,t} = 0$) and the rest of the sample of PROGRESA (i.e, those with $\text{sec}_{i,t} = 0$), the effects of the program when one considers only those with $\text{sec}_{i,t} = 0$ are quite similar to the effects observed when the whole sample of PROGRESA is analyzed.[19] In the latter case, for men one observes negative (but not significant) effects on work (a bit smaller in absolute value when compared to the ones of my sample), and positive and significant effects on DANAE, which lose strength through time but remain positive. For women one observes negative (but not significant) effects on work and an effect on DANAE very close to zero in absolute terms. All this suggests that the effects of PROGRESA on adult labor outcomes are robust, and in particular that the conditionality of the program (which was a binding constraint for adults in households with kids in secondary school) did not induce important differences on these indicators.

[19] As stated before, the differences between the two groups could be seen in Table 10 in Appendix B and the results of the estimations for the whole sample are reported in Appendix D.

Table 3: Impact of PROGRESA on the Probability of Working

	MEN			WOMEN		
	Pooled	**Panel**	**Panel (FE)**	**Pooled**	**Panel**	**Panel (FE)**
Baseline						
β_T	-0.014	-0.006	-0.006	-0.019	-0.016	-0.016
	(.009)	(.011)	(.011)	(.018)	(.017)	(.017)
Dynamic Effects						
β_{T3} (in $t = 3$)	-0.009	-0.001	-0.001	-0.020	-0.023	-0.023
	(.011)	(.022)	(.013)	(.020)	(.021)	(.021)
β_{T4} (in $t = 4$)	-0.019	-0.013	-0.013	-0.027	-0.021	-0.021
	(.011)	(.013)	(.013)	(.019)	(.018)	(.018)
β_{T5} (in $t = 5$)	-0.015	-0.004	-0.004	-0.009	-0.005	-0.005
	(.009)	(.012)	(.013)	(.022)	(.021)	(.021)
Heterogeneous Effects						
β_T (for GA_i=0)	-0.020	-0.013	-0.014	-0.025	-0.021	-0.021
	(.016)	(.017)	(.017)	(.020)	(.018)	(.018)
$\beta_T + \beta_{TG}$ (for 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						
β_T	0.039*	0.030	0.031	0.001	-0.001	-0.001
	(.022)	(.025)	(.025)	(.007)	(.007)	(.007)
Dynamic Effects						
β_{T3} (in $t = 3$)	0.057*	0.033	0.034	0.000	-0.005	-0.006
	(.029)	(.030)	(.030)	(.008)	(.009)	(.009)
β_{T4} (in $t = 4$)	0.027	0.028	0.029	-0.001	0.001	0.001
	(.025)	(.028)	(.028)	(.009)	(.010)	(.010)
β_{T5} (in $t = 5$)	0.025	0.030	0.030	0.004	0.002	0.003
	(.028)	(.032)	(.032)	(.009)	(.008)	(.008)
Heterogeneous Effects						
β_T (for GA_i=0)	0.044	0.028	0.029	0.004	0.004	0.004
	(.028)	(.032)	(.032)	(.010)	(.010)	(.010)
$\beta_T + \beta_{TG}$ (for 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 looked at the effect of PROGRESA on the extensive margin (whether people work or not). This one, instead, looks at 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.[20] 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

[20] 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%.

DANAE (since the question was only asked to those who declared to have a "salaried" job). I 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 regressions like (1) and (2) but only among men who had a DANAE in $S1$. As reported in 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
β_T	(.015)	(.014)	(.014)
Dynamic Effects			
β_{T3} (in $t=3$)	0.003	-0.019	-0.018
	(.022)	(.022)	(.022)
β_{T5} (in $t=5$)	-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:

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

where $X \in \{3,5\}$. 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_{TX} 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 $t=3$ are positive, and coefficients in $t=5$ are negative. However they are small, all of them smaller than one hour per week in absolute value. These effects are compatible 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)**
β_{T3} (in $t = 3$)	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
β_{T5} (in $t = 5$)	-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. For 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. 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), and the number of kids and adults at baseline. 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:

$$Y_{i,t} = a_i + \beta_T T_{i,t} + \lambda_3 S3 + \lambda_4 S4 + \lambda_5 S5 + u_{i,t} \quad (5)$$

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_T.

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

$$Y_{i,t} = a_i + \beta_{T3} T_{i,t} * S3 + \beta_{T4} T_{i,t} * S4 + \beta_{T5} T_{i,t} * S5 + \lambda_3 S3 + \lambda_4 S4 + \lambda_5 S5 + u_{i,t} \quad (6)$$

where the coefficients of interest are β_{T3}, β_{T4} and β_{T5}.

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

$$Y_{i,t} = a_i + \beta_1 GA_i * Expost + \beta_N T_{i,t}^{Ⓝ} + \beta_{NG} T_{i,t}^{Ⓝ} * GA_i + \beta_E T_{i,t}^{Ⓔ} + \lambda_3 S3 + \lambda_4 S4 + \lambda_5 S5 + u_{i,t} \quad (7)$$

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_N provides the effect of increasing the transfer for people living in households which never received the educational grant (i.e, those with $GA_i = 0$). People living in households which *in some t* received the educational grant (i.e, those with $GA_i = 1$) can be further split in two groups: those who in t did not receive the educational grant; for those the total effect of increasing the transfer would be given by $\beta_N + \beta_{NG}$. And those who in t received the nutritional and the educational grant, for those the total effect of increasing the transfer (any of them, since money is fungible) is given by $\beta_N + \beta_{NG} + \beta_E$.

5.3.2 Results

Table 7 reports the results of regressions (5) baseline, (6) dynamic effects, and (7) heterogeneous effects, where the dependent variable is "work". The first column of 3 reports the results for the pooled sample of men and the second column for the panel sample of men, in both cases I report the results of the OLS estimations with individual fixed effects. The third and fourth columns report the same results for women. Table 8 replicates Table 7 for the outcome DANAE.

For the sake of exposition, I rescale the treatment, so that it is the transfer *in tens* per adult equivalent. This means that, β_T 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).

Patterns are similar to those seen in Subsection 5.1.2. For men coefficients are overall negative when work is the dependent variable. The biggest negative effect shows up in $t = 4$, this effect is significant at 10% level, it tells us that an increase of 10 pesos in the transfer per adult equivalent decreases work in 0.34 percentage points. The effects on DANAE instead, are positive and significant, and the biggest effect, significant at 5% level, is at the beginning of the program, i.e, at $t = 3$, which is equal to 0.98 percentage points.

When looking at the heterogeneous effects for men, we see that the effect on work of increasing the transfer for those who only received the nutritional grant throughout the analysis (i.e, those with $GA_i = 0$) is negative and significant at 5% level, equal to -0.71 percentage points. For those which in some moment (different from t) received the nutritional transfer, the effect is equal to -0.43 percentage points. The difference among these two groups is not statistically significant (i.e, β_{NG} is not statistically different from 0). The effect for those who in t received the nutritional and the educational grant is still negative but much smaller, equal to -0.05 percentage points.[21] Instead, the effects on DANAE are positive for the three sets of men. The biggest effect is for those with $GA_i = 1$ who in t received the educational grant, for those, the effect is equal to 1.4 percentage points, and is significant at 10% level.

For women as well the patterns are similar to those observed and commented in Subsection 5.1.2. All the effects are statistically insignificant, the effects on work are however, negative, but tend to phase out with time. The effects on DANAE, if anything, are positive, but typically small.

When looking at the heterogeneous effects for women, we see that the effects on work of increasing the transfers are similar for the three groups, i.e, those with $GA_i = 0$, those with $GA_i = 1$ without educational grant in t, and those with $GA_i = 1$ with an educational grant in t; in the three cases the effects are negative and non-significantly different from zero. On DANAE, however, the effects even if non significant, go in opposite directions: for those with $GA_i = 0$ the effect of increasing the transfer per adult equivalent by 10 units has a positive effect of 0.25 percentage points, instead, for the group of women with $GA_i = 0$ the effect has the same magnitude but the opposite sign (however, the difference among the two, i.e β_{NG} is not significantly different from zero), finally the effect for those which currently receive an educational transfer from a kid in primary school is exactly equal to 0.

[21] The average transfer per adult equivalent for people with $GA_i = 0$ in the pooled (panel) sample living in treated villages is 38 (38) pesos. For those with $GA_i = 1$ but who did not receive the educational grant in t, it is equal to 30 (30) pesos. The difference between these two groups is due to the fact that on average, households of the second group tend to have more members. Finally, for those with $GA_i = 1$ who in t received also the educational grant, it is equal to 59 (59) pesos.

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

	MEN		WOMEN	
	Pooled	**Panel**	**Pooled**	**Panel**
Baseline				
β_T	-0.0015	-0.0004	-0.0015	-0.0030
	(.0014)	(.0018)	(.0029)	(.0029)
Dynamic Effects				
β_{T3} (in $t = 3$)	-0.0016	-0.0003	-0.0016	-0.0041
	(.0019)	(.0025)	(.0035)	(.0039)
β_{T4} (in $t = 4$)	-0.0034*	-0.0025	-0.0031	-0.0047
	(.0020)	(.0025)	(.0031)	(.0033)
β_{T5} (in $t = 5$)	-0.0003	0.0004	-0.0006	-0.0019
	(.0015)	(.0017)	(.0030)	(.0029)
Heterogeneous Effects				
$GA_i = 0$				
β_N *(only received the nutritional grant at every t)*	-0.0071**	-0.0060*	-0.0027	-0.0049
	(.0030)	(.0034)	(.0047)	(.0047)
$GA_i = 1$				
$\beta_N + \beta_{NG}$ *(in t only received the nutritional grant)*	-0.0043	-0.0025	-0.0014	-0.0043
	(.0034)	(.0036)	(.0068)	(.0073)
$\beta_N + \beta_{NG} + \beta_E$ *(in t received also the educational grant)*	-0.0005	.00383	-0.0022	-0.0049
	(.0032)	(.0035)	(.0069)	(.0069)
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				
β_T	0.0071**	0.0062	0.0008	-0.0006
	(.0034)	(.0040)	(.0012)	(.0014)
Dynamic Effects				
β_{T3} (in $t = 3$)	0.0098**	0.0071	0.0011	-0.0013
	(.0045)	(.0055)	(.0015)	(.0019)
β_{T4} (in $t = 4$)	0.0056	0.0076	-0.0010	-0.0022
	(.0039)	(.0046)	(.0018)	(.0022)
β_{T5} (in $t = 5$)	0.0054	0.0054	0.0014	0.0002
	(.0042)	(.0045)	(.0014)	(.0015)
Heterogeneous Effects				
$GA_i = 0$				
β_N *(only received the nutritional grant at every t)*	0.0059	0.0051	0.0025	0.0012
	(.0061)	(.0072)	(.0026)	(.0031)
$GA_i = 1$				
$\beta_N + \beta_{NG}$ *(in t only received the nutritional grant)*	0.0050	0.0030	-0.0025	-0.0033
	(.0089)	(.0101)	(.0036)	(.0039)
$\beta_N + \beta_{NG} + \beta_E$ *(in t received also the educational grant)*	0.0145*	0.0118	0.0000	-0.0029
	(.0078)	(.0091)	(.0040)	(.0034)
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 in Appendix E.

One may fear that the absence of negative effects on labor outcomes is driven by general equilibrium effects unleashed by PROGRESA. This might be the case since the proportion of households who received the transfers in treated villages is substantial.[22] 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 24. Out of fifteen consumption goods just one good has a price that is significantly different (although the difference is 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 25, 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

[22]As mentioned before, the original classification scheme classified around 52% of the households of the selected localities as poor, and the take-up rate among eligibles in treated localities was of 93.90% (Skoufias, 2005).

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 26. According to this information, wages are not significantly different in treated and control localities. Using my data I further verify this by estimating the effect on wages using the same methodology to the one used to estimate the effect of PROGRESA on the number of hours worked per week. I report these results in Table 27. 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:[23] 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 induced by the conditionality) of the cash transfer. This implies that for this exogenously selected subsample, PROGRESA acts essentially as an UCT. I find that, contrary to the predictions of the neo-classical theory of labor supply, these adults did not use the additional money to "buy leisure". To the contrary, if anything, PROGRESA had a positive effect on remunerated employment.

The studies cited in the literature review could shed some light on the reasons of why this is the case. For example, as explained by Alderman and Yemtsov (2013), the cash transfer might have been used to lessen liquidity constraints. In fact, less than 1% of the analyzed sample reported to have savings, and 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). Thus, the lack of savings and 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. This is consistent with the findings of Gertler et al. (2012) who show that PROGRESA beneficiaries invested part of the transfers in productive assets, which allowed them to increase agricultural income by almost 10% after 18 months. Further, 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 (Leibenstein, 1957).

It could be further argued that the absence of negative effect on labor outcomes of adults is explained by the fact that even if the transfers are generous, they are not high enough to induce exits from employment.[24] This could be true, however, what is also true is that these substantial transfers do have an immediate effect on the reduction of extreme poverty which is

[23] 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.

[24] Even if the transfer is not high enough to persuade the primary earner (household head) of the household to leave his/her remunerated employment, one could have possibly expected other members of the household to withdraw from DANAE. However recall that, as shown in Table 31 in Appendix F, this was not the case.

valuable by itself (Fiszbein and Schady, 2009), and as emphasized by Alderman and Yemtsov (2013) allow to better distribute among *all* the gains of growth.

I agree with Fiszbein and Schady, 2009 on the fact that the conditionality of CCT programs (which, after PROGRESA, became widely extended in developing countries) might play an important role.[25] However, as the current work pretends to contribute to show, this does not imply that it is necessarily the conditionality of the programs that prevents long-term dependency or an irresponsible attitude of the recipients. In fact, people living in poverty have many good reasons to use the money (even if unconditionally provided) in a responsible way, consistent with their present and future well-being. Thus, the role that the conditionality of this kind of programs play in preventing negative effects on adult labor outcomes, in my opinion, should not be overemphasized. Because doing so shadows other important mechanisms that might be at play and that should be explored to better understand the needs and requirements of people living in poverty.

[25]Two reasons are: First, it might facilitate the implementation of these kind of programs, since voters are more willing to "help" the "deserving" poor. Second, it could be useful to emphasize and strength the importance of education, and could help to reduce its private costs, which is desirable given the high social benefits that it provides.

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**	**ND**	**PV**	**C**	**T**	**ND**	**PV**	**C**	**T**	**ND**	**PV**	**C**	**T**	**ND**	**PV**	**C**	**T**	**ND**	**PV**
Sex (men)	0.49	0.49	-0.01	0.32	0.49	0.49	-0.01	0.43	0.48	0.49	-0.01	0.34	0.48	0.49	-0.01	0.18	0.48	0.49	-0.01	0.34
Work	0.51	0.53	-0.06	0.02**	0.51	0.53	-0.04	0.09*	0.50	0.52	-0.04	0.09*	0.50	0.52	-0.04	0.10	0.50	0.52	-0.04	0.08*
Health insurance	0.02	0.01	0.03	0.23	0.02	0.01	0.04	0.14	0.02	0.01	0.03	0.33	0.02	0.01	0.04	0.17	0.02	0.01	0.03	0.28
Household (hh) head	0.43	0.43	0.00	0.93	0.44	0.43	0.01	0.45	0.44	0.43	0.00	0.74	0.44	0.43	0.01	0.43	0.44	0.44	0.01	0.33
Living in a couple	0.84	0.83	0.03	0.40	0.85	0.84	0.04	0.23	0.86	0.85	0.05	0.20	0.86	0.85	0.04	0.31	0.88	0.86	0.06	0.11
DANAE=1	0.39	0.36	0.05	0.11	0.39	0.36	0.06	0.06*	0.39	0.36	0.05	0.10	0.39	0.36	0.05	0.14	0.39	0.36	0.05	0.14
Years of education	3.33	3.40	-0.02	0.69	3.41	3.49	-0.03	0.64	3.49	3.52	-0.01	0.87	3.51	3.61	-0.04	0.55	3.52	3.65	-0.04	0.46
# of people in the hh	4.99	5.02	-0.02	0.68	5.26	5.29	-0.01	0.72	5.30	5.28	0.01	0.79	4.64	4.70	-0.04	0.41	4.63	4.67	-0.02	0.59
Hours worked p/week	21.74	22.65	-0.04	0.19	21.65	22.20	-0.02	0.46	21.35	21.74	-0.02	0.57	21.55	21.91	-0.02	0.63	21.41	21.77	-0.02	0.62
Age	33.52	33.64	-0.01	0.75	32.88	33.03	-0.01	0.72	32.61	32.83	-0.02	0.62	32.25	32.34	-0.01	0.84	32.06	32.13	-0.01	0.88
Means Index	648.52	646.59	0.03	0.68	649.91	648.85	0.01	0.83	652.06	650.14	0.03	0.70	654.02	653.59	0.01	0.84	653.31	652.89	0.01	0.93
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. ND: normalized difference $\frac{\mu_1 - \mu_2}{\sqrt{(\sigma_1^2 + \sigma_2^2)/2}}$ (Imbens and Rubin, 2015 pg. 310), PV: P-Value. 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 several tables. The first table, Table 10, shows the characteristics of agents with $sec_{i,t} = 0$ vs. those with $sec_{i,t} = 1$ (see Section 4 for the definition of $sec_{i,t}$). Table 11, shows the characteristics for men who worked at baseline vs. those who did not and Table 13 shows the characteristics of men who had a DANAE at baseline vs. those who did not. Respectively, Tables 11 and 12 show the same information for women. Finally, Table 15 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 $t = 1$	$sec_{i,t}$**=0**	$sec_{i,t}$**=1**	**ND**	**P-value**
Sex (men)	0.49	0.50	-0.02	0.001***
Work	0.52	0.54	-0.03	0.003***
Health insurance at work	0.02	0.01	0.01	0.422
Household head	0.43	0.38	0.11	0.000***
Living in a couple	0.83	0.74	0.21	0.000***
DANAE=1	0.37	0.36	0.03	0.003***
Years of education	3.37	2.94	0.15	0.000***
Num. of people in the hh	5.01	7.66	-1.21	0.000***
Num of kids (below 18)	2.40	4.51	-1.20	0.000***
Num of small kids (below 6)	1.46	1.11	0.32	0.000***
Hours worked per week	22.32	23.33	-0.04	0.000***
Age	33.59	37.17	-0.28	0.000***
Means Index	647.30	635.26	0.15	0.000***
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. 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: Characteristics of Men who Work and who did not Work at Baseline

	Pooled				Panel			
	work=1	**work=0**	**ND**	**P-value**	**work=1**	**work=0**	**ND**	**P-value**
# of kids under 6 years	1.46	0.98	-0.46	0.000***	1.52	0.79	-0.74	0.000***
Living in a couple	0.86	0.57	-0.70	0.000***	0.89	0.60	-0.70	0.000***
# of people in the hh	4.94	4.99	0.02	0.697	4.60	4.48	-0.08	0.375
Hh head	0.84	0.54	-0.70	0.000***	0.87	0.60	-0.64	0.000***
Education	3.55	2.99	-0.18	0.002***	3.74	3.16	-0.19	0.014**
Age	34.26	41.18	0.45	0.000***	33.18	39.55	0.44	0.000***
N. Observations	5830	407			2990	183		

Note: This table splits the observations of men in S1 (at baseline) among those who work and those who did not work, and reports the differences among the two. Hh: Household, 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 12: Characteristics of Women who Work and who did not Work at Baseline

	Pooled				Panel			
	work=1	**work=0**	**ND**	**P-value**	**work=1**	**work=0**	**ND**	**P-value**
# of kids under 6 years	1.35	1.50	0.14	0.001***	1.34	1.55	0.20	0.000***
Living in a couple	0.62	0.85	0.54	0.000***	0.68	0.87	0.48	0.000***
# of people in the hh	5.03	5.08	0.03	0.626	4.47	4.74	0.17	0.013**
Hh head	0.18	0.03	-0.47	0.000***	0.17	0.03	-0.46	0.000***
Education	2.68	3.33	0.23	0.000***	2.81	3.59	0.26	0.000***
Age	34.32	32.25	-0.15	0.000***	33.26	30.44	-0.22	0.001***
N. Observations	863	5673			353	3015		

Note: This table splits the observations of women in S1 (at baseline) among those who work and those who did not work, and reports the differences among the two. Hh: Household, 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 13: Characteristics of Men with DANAE=1 vs. DANAE=0 at Baseline

	Pooled				Panel			
	DANAE=1	**DANAE=0**	**ND**	**P-value**	**DANAE=1**	**DANAE=0**	**ND**	**P-value**
# of kids under 6 years	1.46	1.35	-0.11	0.002***	1.52	1.37	-0.15	0.004***
Living in a couple	0.87	0.78	-0.25	0.000***	0.84	0.82	-0.04	0.000***
# of people in the hh	4.87	5.13	0.13	0.000***	4.56	4.70	0.09	0.101
Hh head	0.86	0.75	-0.27	0.000***	0.88	0.80	-0.23	0.000***
Education	3.65	3.17	-0.17	0.000***	3.83	3.38	-0.16	0.002***
Age	33.58	37.47	0.29	0.000***	32.61	36.01	0.27	0.000***
N. Observations	4415	1812			2291	878		

Note: This table splits the observations of men in S1 (at baseline) among those who had a DANAE and those who did not have a DANAE, and reports the differences among the two. DANAE: day agricultural and non agricultural employment, Hh: Household, 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 14: Characteristics of Women with DANAE=1 vs. DANAE=0 at Baseline

	Pooled				Panel			
	DANAE=1	**DANAE=0**	**ND**	**P-value**	**DANAE=1**	**DANAE=0**	**ND**	**P-value**
# of kids under 6 years	1.24	1.50	0.23	0.001***	1.27	1.54	0.26	0.004***
Living in a couple	0.43	0.84	0.93	0.000***	0.38	0.82	-0.04	0.000***
# of people in the hh	5.05	5.08	0.01	0.876	4.41	4.73	0.19	0.109
Hh head	0.23	0.04	-0.57	0.000***	0.26	0.04	-0.66	0.000***
Education	2.97	3.26	0.09	0.192	3.06	3.53	0.16	0.118
Age	33.64	32.47	-0.09	0.147	33.37	30.64	-0.22	0.022**
N. Observations	340	6184			126	3234		

Note: This table splits the observations of women in S1 (at baseline) among those who had a DANAE and those who did not have a DANAE, and reports the differences among the two. DANAE: day agricultural and non agricultural employment, Hh: Household, 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 15: 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 16 shows the effect on work, Table 17 on DANAE and Tables 18 & 19 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 18 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 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 [$sec_{i,t} = 0$ restricted]

	MEN			WOMEN		
	Pooled	**Panel**	**Panel (FE)**	**Pooled**	**Panel**	**Panel (FE)**
Baseline						
β_T	-0.014	-0.007	-0.007	-0.022	-0.018	-0.018
	(.010)	(.012)	(.012)	(.019)	(.015)	(.015)
Dynamic Effects						
β_{T3} (in $t = 3$)	-0.009	-0.002	-0.002	-0.027	-0.023	-0.023
	(.012)	(.014)	(.014)	(.022)	(.018)	(.018)
β_{T4} (in $t = 4$)	-0.020	-0.014	-0.014	-0.026	-0.024	-0.024
	(.012)	(.014)	(.014)	(.019)	(.017)	(.017)
β_{T5} (in $t = 5$)	-0.013	-0.006	-0.006	-0.010	-0.007	-0.007
	(.010)	(.013)	(.013)	(.023)	(.019)	(.019)
Heterogeneous Effects						
β_T (for GA_i=0)	-0.020	-0.013	-0.014	-0.025	-0.021	-0.021
	(.016)	(.017)	(.017)	(.020)	(.018)	(.018)
$\beta_T + \beta_{TG}$ (for 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 17: 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						
β_T	0.035	0.016	0.016	-0.002	-0.004	-0.004
	(.0230)	(.027)	(.027)	(.007)	(.007)	(.007)
Dynamic Effects						
β_{T3} (in $t = 3$)	0.052*	0.020	0.021	-0.004	-0.007	-0.008
	(.029)	(.032)	(.032)	(.008)	(.009)	(.009)
β_{T4} (in $t = 4$)	0.027	0.016	0.017	-0.002	-0.004	-0.004
	(.025)	(.029)	(.029)	(.009)	(.010)	(.010)
β_{T5} (in $t = 5$)	0.020	0.011	0.012	0.001	-0.002	-0.001
	(.028)	(.033)	(.033)	(.009)	(.009)	(.009)
Heterogeneous Effects						
β_T (for GA_i=0)	0.044	0.028	0.029	0.004	0.004	0.004
	(.028)	(.032)	(.032)	(.010)	(.010)	(.010)
$\beta_T + \beta_{TG}$ (for 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 16, with day agricultural or nonagricultural employment (DANAE) as dependent variable. See Table 16 for details.

Table 18: 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
β_T	(.015)	(.015)	(.015)
Dynamic effects			
β_{T3} (in $t = 3$)	0.002	-0.018	-0.017
	(.022)	(.024)	(.023)
β_{T5} (in $t = 5$)	-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 19: Effect of PROGRESA on the number of hours worked [$sec_{i,t} = 0$ restricted]

	Pooled	**Panel**	**Panel (FE)**
β_{T3} (in $t = 3$)	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
β_{T5} (in $t = 5$)	-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 20 and 21 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 22 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 23) 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 20: Impact of PROGRESA on the Probability of Working [full sample]

	MEN			WOMEN		
	Pooled	**Panel**	**Panel (FE)**	**Pooled**	**Panel**	**Panel (FE)**
Baseline						
β_T	-0.003	0.003	0.003	-0.013	-0.014	-0.014
	(.007)	(.007)	(.007)	(.015)	(.014)	(.014)
Dynamic Effects						
β_{T3} (in $t = 3$)	0.003	0.008	0.008	-0.014	-0.017	-0.017
	(.008)	(.008)	(.008)	(.016)	(.016)	(.016)
β_{T4} (in $t = 4$)	-0.011	-0.005	-0.004	-0.013	-0.011	-0.011
	(.008)	(.009)	(.009)	(.015)	(.015)	(.015)
β_{T5} (in $t = 5$)	-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 21: Impact of PROGRESA on the probability of having a DANAE [full sample]

	MEN			WOMEN		
	Pooled	**Panel**	**Panel (FE)**	**Pooled**	**Panel**	**Panel (FE)**
Baseline						
β_T	0.045**	0.037	0.038	0.005	0.005	0.005
	(.021)	(.023)	(.023)	(.006)	(.005)	(.005)
Dynamic Effects						
β_{T3} (in $t = 3$)	0.066**	0.056**	0.056**	0.008	0.006	0.006
	(.027)	(.028)	(.028)	(.007)	(.006)	(.006)
β_{T4} (in $t = 4$)	0.037	0.030	0.031	0.003	0.004	0.004
	(.024)	(.025)	(.025)	(.007)	(.006)	(.006)
β_{T5} (in $t = 5$)	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 20, with day agricultural or nonagricultural employment (DANAE) as dependent variable. See Table 20 for details.

Table 22 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 23) 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 22: 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
β_T	(.015)	(.015)	(.015)
Dynamic effects			
β_{T3} (in $t = 3$)	0.011	-0.002	-0.002
	(.023)	(.022)	(.022)
β_{T5} (in $t = 5$)	-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 23: Effect of PROGRESA on the Number of Hours Worked per Week [full sample]

	Pooled	**Panel**	**Panel (FE)**
β_{T3} (in $t = 3$)	0.149	0.521	0.520
	(.808)	(.852)	(.851)
Pre-Program Level	43.60	43.37	43.37
N. Obs	12670	9750	9750
β_{T5} (in $t = 5$)	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 24: 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) real 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 25: 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 26: 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 27: 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 28: 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 29: 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 28, 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 28 for details.

F Heterogeneity

In this Appendix I report two sets of results, to see whether the effects of PROGRESA are different for different subgroups of the sample. For this purpose, I use specification (3).

In the first two tables I use the means index to split the sample in two: the poorest and the less poor. In the second set of tables I I explore whether the effect of PROGRESA is different among persons who were household heads at baseline and those who were not. See Subsection 5.1.2 for comments about these four tables.

Table 30: 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)
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 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 31: 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)
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 30, with day agricultural or nonagricultural employment (DANAE) as dependent variable. See Table 30 for details.

Table 32: Effect of PROGRESA on the probability of working; Heterogeneity with respect to whether the person was household head at baseline (S1)

	MEN			WOMEN		
	Pooled	**Panel**	**Panel (FE)**	**Pooled**	**Panel**	**Panel (FE)**
Household Heads	-0.009	-0.002	-0.002	-0.047	-0.057	-0.057
	(.0081)	(.0106)	(.0106)	(.0561)	(.0730)	(.0730)
Non Household Heads	-0.037	-0.034	-0.035	-0.017	-0.014	-0.014
	(.0299)	(.0373)	(.0373)	(.0192)	(.0183)	(.0183)
Pre-Program level	0.937	0.942	0.942	0.118	0.104	0.104
N. Observations	18360	12692	12692	19306	13472	12692

Note: This table reports the effect of PROGRESA on the probability of working for the those who were household heads at baseline and those who were not, separately. 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 33: Effect of PROGRESA on the probability of having a DANAE; Heterogeneity with respect to whether the person was household head at baseline (S1)

	MEN			WOMEN		
	Pooled	**Panel**	**Panel (FE)**	**Pooled**	**Panel**	**Panel (FE)**
Household Heads	0.0437*	0.032	0.032	0.008	-0.009	-0.008
	(.0231)	(.0262)	(.0262)	(.0524)	(.0694)	(.0694)
Non Household Heads	0.007	0.017	0.018	0.001	0.000	0.000
	(.0415)	(.0516)	(.0515)	(.0070)	(.0069)	(.0069)
Pre-Program level	0.714	0.723	0.723	0.045	0.038	0.038
N. Observations	18823	12671	12671	19231	13410	13410

Note: This table replicates Table 32, with day agricultural or nonagricultural employment (DANAE) as dependent variable. See Table 32 for details.

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