The schooling repayment hypothesis for private transfers: Evidence from the PROGRESA/Oportunidades experiment*

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Abstract

The schooling repayment hypothesis for private transfers predicts a positive relationship between the amount of parental investment in children's education and the amount that adult children transfer to their parents. This paper provides evidence on the repayment motive using data from the Mexican conditional cash transfer program PROGRESA/Oportunidades. The program pays a transfer to parents for sending their children to school. Thus, if private transfers from adult children to parents are in part repayment for parental schooling investments made in the past, then PROGRESA/Oportunidades should decrease these transfers, because parents were already exogenously compensated by the government for sending their kids to school and not to work. Exploiting the exogenous variation in the amount of cash transfers a household receives from the program for sending its children to school, we compare the private transfers received in 2007 by parental households who had children 0-16 in 1997 and started receiving the programs' benefits in 1998 with the transfers received by similar parental households who started receiving benefits in 1999. Our results suggest that (i) there exists a repayment motive and (ii) that PROGRESA/Oportunidades is causing adult children to transfer less resources to their parents.

JEL codes: D19, J18, J19.

Keywords: Parental schooling investments; schooling repayment hypothesis; intergenerational transfers; PROGRESA/Oportunidades.

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1 Introduction

Are transfers from adult children to their parents partly repayment for schooling investments made by parents in the past? In the theoretical literature, in addition to the altruistic and exchange motives for private transfers, some authors model the relationship between parents and children as an implicit intergenerational contract in which parents invest in their children's education, when children are young, and receive a repayment from them when they become adults (Becker, 1993; Cigno, 1993; Cox and Stark, 1994; Ehrlich and Lui, 1991; Guttman, 2001). These models predict a positive relationship between the amount of parental investment in children's human capital and the private transfers that adult children give to their parents. This paper provides evidence of the repayment motive for these transfers using data from Mexico's PROGRESA/Oportunidades program.

For both developed and developing countries, previous empirical work examines the determinants of the transfers that adult children give to their parents and viceversa.[1] However, the specific evidence on the schooling repayment hypothesis is scarce and mostly based on estimating the effect of the educational attainment of adult children on the transfers that parents receive from them, without controlling for the endogeneity of education. For instance, early work by Lillard and Willis (1997) finds that the number of children with higher educational attainments has a positive effect on the transfers received by parents using data from Malasya. Also using Malasyan data, Park (2003) finds no significant effect of the educational attainment of children on the monetary transfers paid to parents after controlling for children's income and other characteristics. Raut and Tran (2005) use Indonesian data and find that the positive effect of an adult child's educational attainment on the transfers made to her parents is sensitive to the empirical specification. Several limitations explain why only a few studies have tried to look at the repayment motive. First, unobserved family characteristics affecting the transfer received from children might also be correlated with the human capital investment in children. Parents who are more altruistic toward their children might invest more heavily in their education and have more altruistic children, so they might also receive more transfers from them. Second, high-ability children

[1] For surveys of this literature, see Laitner (1997) and Arrondel and Masson (2006). For developing countries, see, for instance, Cox and Jimenez (1992) and Cox, Eser, and Jimenez (1998).

might attain a higher educational level at a lower cost for parents, and then transfer less to them as adults, because they owe them less. Thus, previous estimates are likely contaminated by endogeneity bias due to the unobserved heterogeneity in parent's preferences and children's ability.

To provide evidence on the repayment motive, we exploit the features and randomized design of PROGRESA/Oportunidades, a Mexican antipoverty program that pays a cash transfer to rural parents for sending their children to school. The schooling transfer from the program, which represents the largest fraction of total program benefits for most households, is conditioned on children's enrollment and substantial attendance to school. By design, when PROGRESA/Oportunidades was first implemented in 1997, 320 rural localities were randomly chosen to participate in the evaluation sample of the program, and 186 rural localities were kept as controls. Households classified as poor by the program administration in treatment localities started receiving benefits in May 1998, whereas poor households in control localities were not incorporated into the program until December 1999. Nonpoor households did not qualify for program benefits regardless of their locality of residence. Both poor and nonpoor households in these localities have been followed over time. Thus, the conditionality of the schooling grant and the randomized design of the program provide a unique opportunity to look at the repayment motive and overcome the limitations of previous work. If private transfers from adult children to parents are in part repayment for parental schooling investments made in the past, then children exposed to PROGRESA/Oportunidades should transfer less to their parents as adults, because their parents were already exogenously compensated by the government for sending them to school and not to work.

We use data from the 1997 baseline survey and the 2007 round of the PROGRESA/Oportunidades' rural evaluation sample. We use a sample of poor parental households that had children 0-16 years old in 1997 from the baseline survey and information on the private transfers they received from children and other donors in 2007, ten years after the start of the program. Any parent with at least one child older than 16 in 1997 is dropped from our sample. By doing this, we keep only families with age-qualifying children in 1997 to reduce heterogeneity. Non-poor households are also excluded from the main analysis, but are used to perform

a falsification test.

Our identification strategy exploits the exogenous variation in the amount of cash transfers a parental household receives from PROGRESA/Oportunidades for sending its children to school. This exogenous variation is induced by the age of the child in 1997, before the start of the program, and the year in which the household was incorporated into the program. Using the child's age in 1997 and the year of treatment, we calculate the child's potential years exposure to the program by 2007 assuming that a given child enters first grade at age 6, and abstracting from any grade repetition. Thus, our exposure measure is exogenous because it does not depend on actual participation in the program or school enrollment. Given that PROGRESA/Oportunidades starts paying schooling transfers to parents when their children get enrolled and attend third grade, which is when children are about 8 years old, children younger than 6 in 1997 have the same exposure to PROGRESA/Oportunidades, regardless of where they lived. In contrast, children 6 to 16 years old in early treated communities have between 1 and 2 years more of exposure to the program by 2007, compared to same-age children in localities where the program started later. In addition to variation in total years of program exposure, the child's age in 1997 and the year of treatment induce exogenous variation in the schooling level that was financed by the extra years of PROGRESA/Oportunidades grants. So, for children who were 6-9 years old in 1997 and treated early, their extra program exposure financed part of their primary education only, whereas for children 10-16 years old in 1997, it financed mostly their secondary education.

The ideal dataset would allow us to observe the private transfers that parents receive from each child in 2007, so we can link these transfers with our measure of the individual child's exposure to the program. Our data have information on the total amount of private transfers received by the parent from her children and from other sources in the previous year, but we do not observe the transfers given by each individual child. In addition, our data has only information on private transfers from donors who do not belong to the household, so we do not observe any transfers from children who still live in the parental household in 2007. As a result, instead of estimating the effect of the individual exposure of each child who is absent from the parental household in 2007, as would be ideal in

our case, we estimate the effect of the number of children age 0-5, 6-9, 10-13 and 14-16 a parental household had in 1997, who are absent in 2007, interacted with a dummy for early treatment, on the amount of private transfers the parental household and the head receive from children in 2007. These key interactions capture differences, induced by their age in 1997 and the year of treatment, in the exposure of children absent in 2007, whose transfers are potentially reflected in our dependent variable. In all estimations, we control for the total number of children the parental household had in each age group in 1997, the interaction of these variables with the treatment dummy and the number of children in each age group in 1997 who are absent in 2007, so that our key interactions provide us with evidence of repayment. Given that a longer exposure to the program implies receiving more educational financing from it, we expect the coefficients of our key interactions to be negative for the number of children age 6-16 if transfers are motivated by repayment.

In our data, for the private transfers received from children, we also observe whether the transfers come from a child who left the household before 1997, one year before PROGRESA/Oportunidades started, or from a child who left after 1997. This distinction, together with the information on transfers from donors different from children, allows us to show that longer exposure to PROGRESA/Oportunidades affects only transfers coming from children and, in particular, from children potentially exposed to the program, and not transfers from children who left the household before the start of the program, or from other friends and relatives.

Our results show that, as expected, the interactions of the treatment dummy with the number of children age 6-16 in 1997, who are absent in 2007, have negative effects on the amount of private transfers the head of the parental household receives from children in 2007, but these effects are not statistically different from zero. However, when we focus on transfers received from children who left the parental household after the program started, these effects increase in size and become statistically different from zero for the number of treated children 14-16 in 1997 who are absent in 2007. An additional child who was 14-16 years old in 1997 and who is absent in 2007 reduces the private transfers received from children who left the household after 1997 by 185 pesos per year. If instead of a two-tailed t-test, we test the null hypothesis that a given coefficient is nonnegative versus

the alternative that it is strictly negative, we reject the null for the number of children 10-13 years old in 1997 who are absent in 2007 (-66.86 pesos) at 10 percent and the one for children age 14 to 16 at 2 percent.No negative and significant effects are found for transfers from children who left before the program or from other donors. According to the rules of the program and our assumed age-grade relationship, children age 10-16 in 1997 are those for whom the additional exposure to the program, induced by the early treatment, likely financed their secondary and high school education, which is precisely the time when the trade-off between school and work becomes stark for parents. Hence, we interpret our results as suggestive evidence in favor of the repayment hypothesis. The results for the private transfers received by the parental household as a whole are consistent with those obtained for the heads.

Although in our regressions we control for the number-of-children variables and the interactions described above, together with parental characteristics and locality fixed effects, some confounders could still compromise our identification strategy. For example, by design, PROGRESA/Oportunidades affects the health and educational level of its beneficiaries, which could directly affect the amount of transfers adult children give to their parents, even in the absence of repayment. We discuss the previous evidence on these program effects in section 5.2. Under some assumptions, a positive effect of the program on the health and education of children, and thus on their adult earnings, would work against our results. We also perform a falsification test by re-estimating our transfer equations using the sample of non-poor households. Given that previous evidence shows that the program also increased the education of non-eligible children in treatment localities, even though these children were not financed by PROGRESA/Oportunidades (Bobonis and Finan, 2009; Lalive and Cattaneo, 2009), our falsification test provides rough evidence on whether the increase in the education of children alone can explain our main results, and we find that it cannot. The program might also affect the child's migration decision and motives which, given that we observe only transfers from donors who do not belong to the household, could contaminate our results. Using the information of poor individual children in our sample, we estimate the effect of early treatment by age on the probability and motives of migrating, and find no significant effects. So, our main results cannot be attributed to the effect of the program

on the migration of children with longer exposure.

Our paper contributes to the literature on the motives for private transfers from adult children to parents. It also contributes to the evidence on the medium-term unintended effects of PROGRESA/Oportunidades. Most of the existing studies about these effects focus on the program's impact on the children's schooling and labor market outcomes.[2] To our knowledge, this is the first paper that looks at the effect of a conditional schooling subsidy on the transfers that parents receive from their adult children who were exposed to the program. Our results suggest that this effect is negative, particularly for children whose exposure to the program financed their secondary education, and we provide crude evidence that parents did not anticipate this reduction in private transfers by accumulating assets. Thus, the first generation of PROGRESA/Oportunidades parental households might be worse-off in the future, especially because the largest part of the program transfer, which is the schooling subsidy, is temporary. From a distributional point of view, for the first generation of beneficiary children, the program could become a positive net transfer from society, because it allowed them to get more education, and to earn more and transfer less to their parents as adults. Whether these children repay the government for their schooling through taxes depends crucially on whether they get jobs in the formal sector, where tax compliace is usually higher, after graduating from the program. However, more research seems due given that our data are not ideal, and that the parents and the adult children in our sample might still be young to be receiving and giving important amounts of transfers, respectively

[2] See Parker, Ruvalcaba and Teruel (2008) for a discussion of the existing evidence about the short and medium term effects of PROGRESA/Oportunidades.

2 Background: PROGRESA/Oportunidades and its evaluation data set[3]

2.1 The program

In 1997, the Mexican government started the *Programa de Educación, Salud y Alimentación* (PROGRESA) in rural Mexico in an effort to break the intergenerational transmission of poverty.[4] The primary objective of the program is to improve the educational, health, and nutritional status of poor families, particularly of children and mothers (Skoufias, 2005).[5] For this paper, the educational component of the program is the most relevant one, because it compensates households for sending their children to school.

Beneficiary households with children enrolled from third to twelfth grade who attend at least 85 percent of the school days each month, as well as during the whole academic year, receive an education-conditional grant.[6] The grants increase with grade and, from seventh grade onwards, are slightly higher for girls than for boys. The size of the grants tries to reflect the opportunity cost of sending the children to school (Skoufias, 2005). In addition, households with children enrolled in any grade receive a grant for school supplies. The conditionality of the education grants is relevant for us as it makes it salient to children–particularly older ones–that if they get enrolled and attend school, their household receives the grant through PROGRESA/Oportunidades.

The health and nutritional components are closely linked. If the mother or most senior woman in the household attends a monthly educational talk and every family member complies with scheduled visits to health centers, the household qualifies for PROGRESA/Oportunidades' nutritional component: a small fixed monetary transfer independent of the household size.[7] In general, all transfers are received by the mother (or most senior woman in the household) and, as a matter of fact, households are free to spend the

[3] This section draws extensively on Skoufias (2005) who provides a much more detailed description of the program and its evaluation data set.

[4] During the administration of President Fox (2000-2006) the program was renamed "Oportunidades."

[5] A household is classified as poor, and hence elegible to receive the programs benefits, according to an index based on household demographics, assets and characteristics of the household members.

[6] The program started giving grants for children attending tenth to twelfth grade in 2001. In 2003 a one-time cash bonus for students finishing twelfth grade was also introduced.

[7] Households also receive nutritional supplements for children less than 24 months (and for children 24-60 months if they present stunting symptoms) and for pregnant and lactating women.

money they get as they find it fit.

Transfers-wise, the most important component of the program is the educational one. In fact, the total amount of transfers a household can receive via educational grants is capped. In 1998, beneficiary households were receiving on average about 197 pesos monthly (expressed in November 1998 pesos). At least half of that amount was coming from the educational grants. The amount households were receiving represented 19.5 percent of the mean value of consumption of eligible households in control localities (Skoufias, 2005).[8]

2.2 The data

Given that PROGRESA/Oportunidades' followed a sequential expansion, an experimental design was adopted for its evaluation. A subset of 506 eligible localities in Guerrero, Hidalgo, Michoacán, Puebla, Querétaro, San Luis Potosí, and Veracruz was randomly chosen to participate in the evaluation sample: 320 localities were randomly chosen as beneficiaries and started receiving benefits in May 1998, whereas 186 localities were used as controls and started receiving treatment in December 1999. In these control localities, none of the households were informed that PROGRESA/Oportunidades would have started to give them benefits at a later date. The data collected comprises repeated observations over eight survey rounds for 24,000 households, both poor (elegible) and nonpoor (non-elegible).

In this paper we use data from the first (ENCASEH97) and last (ENCEL 2007) survey rounds. We are particularly interested in the private transfers received by each parental household in 2007. We use the ENCASEH97 round to select a sample of elegible (parental) households who had children 0-16 years old before the start of the program, and to recover relevant household-level characteristics.[9] We drop all households with at least one child older than 16 in 1997. Such households might also have younger children elegible for PROGRESA/Oportunidades, but focusing on our sample reduces heterogeneity. We get the data on the private tranfers received by each (parental) household in 2007 from the last survey round. We drop households who do not have information on the relevant variables

[8] The calculation of this average includes households that did not receive any benefits due to nonadherence to the conditions of the program or delays in the verification of the requirements of the program or in the delivery of the monetary benefits (Skoufias, 2005). The exchange rate at the time was about MX$10.00 = US$1.00.

[9] These variables are the demographic structure of the household, characteristics of the head and its children such as age, sex and schooling, and the head's marital status.

we use in the analysis. We also drop households that report having more than one head. Finally, nonpoor, non-elegible households are excluded from the main analysis, and are used only for the falsification test in section 6.2. This leaves us with a cross-sectional sample of 2,271 eligible households.

3 Theoretical discussion

From a theoretical point of view, a number of authors have tried to rationalize the link between parental investments in their children's human capital and adult children's transfers to their parents. Becker and Murphy (1988) argue that even altruistic parents face the trade-off between their own current consumption and the investment in the human capital of their children. In their model, parents invest efficiently in their children education if they can effectively force their children to repay for this investment by reducing bequests and gifts to children as adults.

Other models explicitly consider an implicit intergenerational contract in which parents invest in their children's education and bear the cost, including the children's foregone labor income, when children are young, and receive a compensation from them when they become adults. Such models predict a positive relationship between the amount of parental investment in the child's education and the amount of transfers the child gives to her parents as an adult. For instance, Ehrlich and Lui (1991) consider the case in which the main motivations for investment in children's human capital are parental altruism combined with old-age support. In their model, parents receive an old-age transfer from their children that is proportional to their human capital investment in them. In Ehrlich and Lui (1991) and in Cigno (1993), children comply with this agreement under the threat of losing old-age benefits from their own children. Other mechanisms that lead children to honor the repayment agreement with their parents are the demonstration effect, i.e, the desire to set an example for their own children (Cox and Stark, 1994), the threat of negative reputation and social exclusion (Guttman, 2001), and guilt (Becker, 1993).

As we have explained, PROGRESA/Oportunidades compensates beneficiary parents for sending their children to school. Hence, children exposed to this program owe less to their

parents for their education as adults, compared to similar children who were not exposed to the program. The conditionality of the educational grant, together with the randomized design of the evaluation sample, give us a unique opportunity to look at the repayment motive, because beneficiary parents were exogenously compensated by the government for investing in their children's human capital when children were young. Given our data, in this paper we are not able to formally test any of the repayment models or to distinguish between the different enforcement mechanisms. However, our empirical results show that a parent whose children were exposed longer to PROGRESA/Oportunidades when young receives lowers transfers from them when they are adults, which we interpret as evidence supporting the repayment motive.

In the theoretical literature, repayment is not the only motive for transfers from children to their parents. Other motives explored by the literature are pure altruism (Becker, 1974) and exchange (Bernheim et al., 1985; Cox, 1987). Both of these motives imply that transfers from children to parents depend on the relative incomes of donor and recipient, and not directly on parental investments in children. For instance, if the program increases the income of parents, then the effect on the transfers received from their children would be negative if these transfers are motivated by altruism, and could be positive if they are motivated by exchange.[10] If the program increases the child's educational attainment, and as a consequence, her earnings as an adult increase too, transfers would also be affected in the altruistic and exchange framework, even without any repayment motive.

We account separately for the effect of PROGRESA/Oportunidades on parental income by controlling for the total number of children of different ages in 1997, which, according to the rules of the program, determine the total program transfer received by the parent. Controlling for these variables, only the exposure of children who are absent in 2007 would provide evidence of repayment, because our data only record transfers received from donors that do not belong to the parental household. In addition, we report results for transfers made by other children in the same family, whose age or migration decisions precluded them from participating in the program, and from donors different from children. Any effect of

[10] Under exchange, an increase in the income of the parent would decrease her supply of services to the child, leading to an increase in the implicit price of services and a decrease in the quantity. If the demand for services is inelastic, private transfers from the child to the parent would increase (Cox, 1987).

parental income on transfers caused by PROGRESA/Oportunidades would also affect these types of transfers, and not just those received from children exposed to the program. We address the possible confounding effect of the program on the child's education in more detail in section 5.2.1, and conclude that our main results cannot be attributed to the effect of PROGRESA/Oportunidades on education.

Finally, if social norms punish children merely for not supporting their parents, regardless of the cost of schooling investments borne by the parents in the past, then the exposure to PROGRESA/Oportunidades would not have an effect on the transfers that adult children give to their parents, unless the increase in the child's educational attainment, due to the program, makes her less reliant on social networks in her locality, and so less concerned about any social punishment for not supporting her parents. We also address this concern in section 5.2.1. and in our falsification test in section 6.2. Our results suggest that the increase in the educational attainment induced by PROGRESA/Oportunidades on the sample of non-eligible children does not decrease the transfers these children give to their parents. So, our results for the sample of eligible households cannot be explained without the reduction in the schooling cost borne by beneficiary parents.

4 Empirical strategy and descriptive statistics

Our identification strategy exploits the exogenous variation in the amount of cash transfers a household receives from PROGRESA/Oportunidades for sending its children to school. This exogenous variation is induced by the age of children within a household in 1997 and the starting date of treatment of each household. According to the program rules, PROGRESA/Oportunidades starts paying schooling transfers to parents when their children get enrolled and attend third grade, which is when children are about 8 years old. In addition, as explained before, due to the program's experimental design, households in 320 localities started receiving program benefits in May 1998, whereas households in 186 localities were delayed benefits till December 1999. Table 1A shows the potential years of exposure to PROGRESA/Oportunidades by 2007 for a given child, depending on her age in 1997 and the year her locality was incorporated to the program. For calculating the

years of exposure, we assume the age-grade relationship shown in columns 1 and 2, so that a child who is 6 years old is enrolled in first grade, a child 12 years old is in seventh grade (first year of secondary education in Mexico) and a child 15 years old is in tenth grade (first year of high school).[11] In these calculations, we also take into account that PROGRESA/Oportunidades started providing schooling grants for high school in 2001. The actual transfers from PROGRESA/Oportunidades are conditioned on the school grade and not on the age of the child, thus in Table 1A we are abstracting from any grade repetition or from re-entry of older children to school after the program was implemented in their localities.[12] Our measure intends to be a proxy for the schooling costs that parents were compensated for by PROGRESA/Oportunidades and, given that our proxy is based on the age of the child before the start of the program and the year of treatment, it is not correlated with unobserved characteristics of the household or children that affect schooling choices or the actual years of exposure to the program.

Table 1A shows that for children who were 0-5 in 1997, the total years of exposure to the program are the same by 2007, regardless of the year their localities started treatment. If children were 6 years old in 1997 and their household was incorporated into PROGRESA/Oportunidades by 1998, the program paid for their education for an additional year by 2007 compared to same-aged children whose households were incorporated into the program at the later stage. Similarly, children between the ages of 7 and 12 in 1997 who started treatment in 1998 received two more years of PROGRESA/Oportunidades' educational grants by 2007. Children who were 13 in 1997 and started treatment in 1998 enjoyed one more year of educational grants. Finally, given our assumptions about the age-grade relationship and the grade progression, children who were 14-16 years old in 1997 had no exposure to PROGRESA/Oportunidades' educational grants, regardless of the community they lived in. However, since about 12 percent of youngsters had failed at least one grade by 2007 and Behrman, Sengupta and Todd (2005) suggest that some children re-entered school after the program was implemented in their localities, it is likely that some of the children who were 14-16 years old in 1997 did actually receive the benefits of the program.

[11] These are the standard entry ages to each schooling level in Mexico.

[12] Nevertheless, by 2007, 12 percent of youngsters age 14-25 reported that they had repeated a grade. Of these, almost 80 percent had failed a grade by age 12.

Hence, we will consider this to be the case from now on.

Table 1B shows years of exposure by schooling level. For children 6-10 years old in 1997 who started treatment in 1998, PROGRESA/Oportunidades financed between one and two years more of their primary education compared to children of the same age living in households incorporated later into the program. For these children, no difference is observed at other education levels. Children older than 10 years old in 1997 did not receive any grants during their primary education, regardless of the locality they lived in. Among these children, those 11-13 years old who started treatment in 1998, received PROGRESA/Oportunidades educational grants for one or two more years during their secondary education, compared to same-age children whose households started treatment later. Note that treated children who were 10 years old in 1997 received PROGRESA/Oportunidades grants for one more year during their primary education and for one more year during their secondary education. Given our assumptions for calculating exposure, children 14-16 years old in 1997 appear as not having received any secondary school grants from the program. However, as we argued above, some of them may have received them. Finally, Table 1B also shows that, for all children considered (those younger than 15), the starting date of treatment did not induce any difference in the grants for high school education paid by PROGRESA/Oportunidades.

The data allows us to create parent-child pairs for each child the head of the parental household had in 1997, and we observe the sociodemographic characteristics of both heads and children. Ideally, we would like to observe the private transfers that each individual child gave to the head and link this information with the individual characteristics of the head and child. However, the data on the private transfers received by the head–and by the parental household as a whole–cannot be disaggregated by child. We observe whether the parental household and who within the household, received a private transfer from *another* household and the amount. So, we only observe private transfers received from donors living in households different than the parental households we analize. The survey asks whether the donor was a child who left the parental household before 1997, a child who left the parental household after 1997, or someone else (a relative, friend, neighbor or other). We do not observe which individual child gave the transfer. We refer to children who left

the parental household by 2007 as absent children. Due to these data limitations, our unit of observation is the parental household head and our outcome variable is the total private transfers the head receives from her children and other types of donors.

If private transfers from adult children to their parents are partly repayment for the schooling investments made in the past by parents, adult children with greater exposure to PROGRESA/Oportunidades–who are less indebted to their parents–and who are absent from the household in 2007 (so we are able to observe their transfer) should transfers less. To see whether this hypothesis is supported by the data, and given that we cannot observe the individual transfers that each child gives to her parent, we estimate the following equation for each head in eligible parental households:

$$\begin{aligned} T_{hl} &= \alpha + \beta_1 X_{hl} + \beta_2 D98_l + \sum_g \gamma_g C_{ghl} + \sum_g \delta_g (D98_l \times C_{ghl}) \\ &\quad + \sum_g \rho_g A_{ghl} + \sum_g \pi_g (D98_l \times A_{ghl}) + \phi_l + \varepsilon_{hl} \end{aligned}$$

where T_{hl} are the private transfers received by the head of parental household h in locality l; X_{hl} are characteristics of the head like age, gender, years of schooling, a dummy for married and the number of male children she had in 1997; $D98_l$ is a dummy variable equal to 1 if the parental household is located in a PROGRESA/Oportunidades locality that started treatment in 1998, and 0 otherwise; C_{ghl} is the number of children in age group g the head of parental household h had in 1997; A_{ghl} is the number of children in age group g the head of parental household h had in 1997 who are absent from the parental household in 2007; ϕ_l is a locality fixed effect intended to capture any shock at the locality level that could affect the amount of transfers sent to the parental household; and ε_{hl} is an idiosyncratic error term. Following the exposure differentials shown in Table 1B, the four age groups we consider are: 0-5, 6-9, 10-13 and 14-16 years old in 1997, before the start of PROGRESA/Oportunidades.[13]

The coefficients of interest are π_g, because they measure the effect of having an addi-

[13] Children who were 10 years old in 1997 had two additional years of program exposure if their household was incorporated into PROGRESA in 1998, compared to same-aged children incorporated into PROGRESA later on: one year in primary school, and the other in secondary school. We group these children together with children who had additional program exposure during secondary school only in order to cleanly separate them from children who were differentlially exposed to PROGRESA only during their primary school years.

tional child in age group g in 1997, who is absent from the parental household in 2007, and who potentially had more exposure to the program because it started in 1998 in her locality. We interpret these coefficients as the effect of PROGRESA/Oportunidades on the private transfers due to a repayment motive, because we are controlling for the total number of children of different ages the head had in 1997 (C_{ghl}), the interactions of these variables with the treatment dummy ($D98_l \times C_{ghl}$), and the number of children of different ages the head had in 1997 who are absent in 2007 (A_{ghl}). If the repayment hypothesis holds, we expect an insignificant coefficient for our key interaction ($D98_l \times A_{ghl}$) of the 0-5 age group, and negative and significant coefficients for the older age groups because, as shown in Table 1A, only children age 6 and older in 1997 in treated localities had a longer exposure to the program schooling grants. The income effect of receiving the PROGRESA/Oportunidades' cash benefits for longer on private transfers is appropriately controlled for with the interaction of the number of children in different age groups in 1997 and the treatment dummy ($D98_l \times C_{ghl}$), because C_{ghl} proxies the total PROGRESA/Oportunidades transfer received by the household.

Table 2 presents the descriptive statistics of parental household heads and their households. We divide them in two groups: those that started receiving PROGRESA/Oportunidades' benefits early (in May 1998) and those that started receiving benefits later on (December 1999). The last column shows the difference in means between these two groups. The mean private transfers received individually by the parental household head during the previous year to the 2007 survey are 92 pesos for those receiving treatment early and 58 pesos for those receiving treatment later. These amounts are very small. About 84 percent of the private transfers received by the head come from her children and, of those, 74 percent come from children who left the household after 1997. On average, the heads of parental households receiving treatment early receive higher private transfers than those receiving treatment later, but these differences are not statistically significant. So, in the overall means we find no evidence of a decrease in private transfers received for those heads whose children had more years of exposure to PROGRESA/Oportunidades. However, simple means do not allow us to observe the variation caused by the ages of children and absent children, neither do they allow us to separate the effect of the program on the income of

the parental household.

The private transfers received by the parental household in 2007 are larger for both groups and the mean differences between those receiving treatment early and those receiving treatment later are much smaller in magnitude and not statistically significant. For both groups, about 54 percent of the private transfers come from the head's children and, of these, 77 percent come from children who left the parental household after 1997. For both groups, about 46 percent of private transfers come from other donors, whereas for heads alone only 4-16 percent do.

The mean differences between those receiving treatment early and later are very small in magnitude and never statistically significant for the number of children in different age groups in 1997, the characteristics of the parental household head in 1997 and the parental household in 2007. Particularly relevant is the fact that the years of schooling of the head and the number of children he had in 1997 are balanced, since these variables can be taken as proxies of the relative (lifetime) resources available to parents in the future. So, the PROGRESA/Oportunidades assignment still looks random, even if we are selecting a particular subsample of the evaluation data, which is reassuring. The only statistically significant mean differences between parental households receiving treatment early and those receiving treatment later are those in the children's average years of PROGRESA/Oportunidades exposure. For the average parental household treated early, PROGRESA/Oportunidades financed between 1.6-2 years more the education of its children.

In our reported estimations, we only control for the individual characteristics of the head of the parental household, because those are more likely unaffected by the program. Even though we find no statistical differences between the treatment and control groups in parental household head's mean characteristics in 2007, PROGRESA/Oportunidades might have an effect on the parental household size and its composition, the total value of its assets and the number of members who are absent in 2007. For our main estimations at the parental household head level, we check whether including these potentially bad regressors changes our results and find no evidence of this (see Table A1 in the appendix). Having said this, PROGRESA/Oportunidades might have at least three important confounding effects on the private transfers received by the parents. The first two arise due to the program's

effect on education and health. The third confounding effect may arise if the program affects the migration pattern of the members of the parental household. We are aware that, if present, these confounding effects may invalidate our identification strategy. For this reason, in the next section we discuss how each of these three confounders may affect our results, and, when possible, how we address the problems they pose to our identification strategy.

5 Results

5.1 Main results

Table 3 shows the results from OLS regressions on the amount of private transfers received by the parental household head in 2007. Only the coefficients on the treatment dummy and the number of children in different age groups in 1997, and the relevant interactions are shown. As mentioned before, these regressions control only for the individual characteristics of the parent and locality fixed effects. In all estimations, the standard errors are clustered at the locality level.

Column 1 shows the results for the total transfers that the head received from his children. The coefficient on the early treatment dummy is positive, but not statistically significant. We argue that the income effect of receiving the PROGRESA/Oportunidades transfer is captured by the number of children of different ages in 1997 and the interaction of these variables with the treatment dummy. The effects of the age of children variables alone are all negative and decreasing in value by age category. Heads of parental households that had older children in 1997 potentially received less educational transfers from PROGRESA/Oportunidades, and according to these negative coefficients, they also receive lower private transfers in 2007. However, the coefficients are statistically significant in only two cases. The total number of children age 10-13 has a negative effect of 63.7 pesos, significant at the 5 percent level, while the total number of children 14-16 has a negative effect of 93.5 pesos, significant at 1 percent. The interaction of these variables with the treatment dummy are never significant, so starting treatment earlier seems not to have an additional income effect on private transfers received from children.

As mentioned before, the survey only counts private transfers received from donors that do not belong to the parental household in 2007. In column 1, we see that the number of children 6-16 in 1997 who are absent from the parental household in 2007 have a positive effect on the transfers that a head received from his children in 2007. Only the effects for children age 10-16 are significant at 5 percent. An additional child 10-13 years old in 1997 who is absent in 2007 increases the transfers received by the parent from his children by about 71 pesos, and an additional child 14-16 years old in 1997 who is absent in 2007 increases these transfers by about 106 pesos. These results are consistent with older children leaving the parental household–some for work–and transferring resources back to their parents.

The effect of lower repayment due to longer exposure to the program, and thus of additional government school-related compensation paid to the parental households is captured by the interaction of the treatment dummy with the number of children in different age groups in 1997 who are absent in 2007. As argued before, after controlling for the total number of children in different age groups in 1997, the interactions of these variables with the treatment dummy, and the total number of children in different age groups in 1997 who are absent in 2007, the interactions of the treatment dummy with the number of children in different age groups in 1997 who are absent in 2007 should have a negative effect on the private transfers received from children if these transfers are motivated by repayment.

In column 1, the interaction effect for children age 0-5 in 1997, who had exactly the same exposure to the program regardless of the community they lived in is positive (10.8 pesos), but not statistically significant. On the other hand, the same interactions for children age 6-16 years old in 1997–where we argue there has been a difference in government financing–are negative, although not statistically significant either.

Nevertheless, we interpret these results as suggestive evidence of the repayment hypothesis, especially because the effect increases in absolute value as we consider older children. In these localities, before PROGRESA/Oportunidades began, boys and girls were dropping out of school and increasing their labor force participation at age 10 and 11, respectively, as shown in Figures 1 and 2. The patterns in Figures 1 and 2 suggest that when children turn 10 years old, the school-work tradeoff becomes important for parents. Hence, children of

that age who continue to go to school are more likely to feel more indebted to their parents in the absence of the program. The tradeoff would be even more important for children age 14-16 in 1997, who have the largest negative effect on the private transfers received by treated heads, because they have even better labor market opportunities and the largest probability of being absent from the parental household.

Columns 2 and 3 in Table 3 report separate estimations for the transfers received by the parental household head from children who left the household after and before 1997. Note that children who left the household before 1997 were not exposed to the program, whereas children who left after 1997 might have been. In column 2, for the transfers received from children who left the household after 1997, all the effects are similar to those in column 1, but some become larger and statistically significant. Of particular interest for us are the interactions of the number of absent children of different ages with the treatment dummy. Now, all these interactions are negative, but for children 6-16 years old in 1997 the coefficients become larger in absolute value than those in column 1. Furthermore, the one for the oldest group becomes significantly different from zero at 5 percent. If instead of a two-tailed t-test, for each of these coefficients we test the null hypothesis that they are nonnegative versus the alternative that they are strictly negative, we reject the null for the estimate for children 10-13 years old in 1997 (-66.86 pesos) at 10 percent and the one for children age 14 to 16 (-185.2 pesos) at 2 percent, which reinforces our interpretation of these coefficients as evidence of repayment, because these transfers are coming precisely from children potentially exposed to the program.

In column 3, the same key interactions for the transfers received from children who left the household before the start of the program are positive and mostly smaller in magnitude than those in column 2. The only significant one is the the increase of 48.1 pesos with an additional child age 14-16 who received early treatment. So, the negative effects of the number of absent children exposed longer to PROGRESA/Oportunidades on the transfers received from children are mostly due to the negative effects on the transfers from children who left the household after the program (column 2), and not to the effects on the transfers received from other children in the same family, who left before the program.

Finally, column 4 shows the results for the private transfers received by the parental

household head from other donors, like friends, neighbors and relatives other than children. The key interactions are relatively small for these transfers and are not statistically significant as would be expected if the differential exposure of absent children to PROGRESA/Oportunidades affected only the transfers from children, due to the repayment hypothesis, and not those from other donors. We check next whether our results for the sample of individual parental household heads hold for the parental households as a whole.

Table 4 reports the results for the total private transfers received by the parental household from different sources. In general, results are consistent with the ones shown in Table 3. In columns 1 and 2 of Table 4, the interactions of the number of children age 6-16 who are absent in 2007 with the treatment dummy are negative and increasing in magnitude in absolute value. As in Table 3, none of these coefficients is statistically significant in column 1. In column 2, for transfers received by the parental household from children who left after PROGRESA/Oportunidades began, the effect of the treatment interaction with the number of children age 14-16 becomes larger and statistically different from zero at 10 percent. An additional child age 14-16 years old decreases the transfers received by the household by 215 pesos per year. For a one-sided test, we are able to reject the null that this coefficient is nonnegative versus the alternative that it is strictly negative at 5 percent.

In column 3 the key interaction of the number of children age 14-16 who are absent in 2007 is positive and statistically significant at 10 percent. Finally in column 4, the key interactions are not significantly different from zero for the transfers received by the parental household from other donors, which as shown in Table 2, are considerable. For a one-sided test, we are not able to reject the null that these key interactions in column 4 are nonnegative at any conventional level. This reinforces the idea that our key interactions are not capturing factors that would affect all private transfers from different sources, but something that affects only the transfers from adult children to their parental households, in particular, only transfers received from children potentially exposed to the program, and that is related to the exposure of each individual absent child to PROGRESA/Oportunidades.

In summary, our reduced-form estimates so far suggest that controlling for the number of children of different ages in 1997, the interactions of these variables with the treatment dummy, and the number of those children who are absent in 2007, the number of treated

children age 14-16 in 1997, who are absent in 2007, has negative effects on the amount of private transfers received from children by the parental household and its head. These negative effects are not found for transfers from other sources. Moreover, children in these age groups are those for whom the additional exposure to PROGRESA/Oportunidades likely financed their secondary and high school education. According to the drop-out patterns observed before the program in Figures 1 and 2, this is precisely the time when the trade-off between school and work becomes stark for parents. We interpret our estimates in Tables 3 and 4 as suggestive evidence in favor of the repayment hypothesis for private transfers of adult children to their parents.

5.2 Confounders

5.2.1 Education

PROGRESA/Oportunidades intends, and has been shown, to increase children's schooling attainment both in the short (Behrman, Sengupta and Todd, 2005; Schultz, 2004) and in the medium run (Behrman, Parker and Todd, 2011). For this reason, we are not explicitly controlling for the adult children's education in our estimations. Higher educational attainment could have a direct positive income effect on the amount that adult children transfer to their parents if it increases the child's earnings and transfers are motivated by altruism. This positive income effect works in the opposite direction of finding a negative effect due to a decrease in the repayment owed to parents, which we nevertheless do find.

On the other hand, education could have a negative effect on the transfers given to the parents for at least two reasons. First, in order to acquire even more education, adult children may leave the parental household, but delay their entry into the labor market (Behrman, Parker and Todd, 2011; Parker and Skoufias, 2001) which may cause them to have less resources in their early adult lives to transfer. Second, education may make the adult children less reliant on social networks in their localities of origin, and so less concerned about any social punishment for decreasing their support to their parents.

In section 6.1, we provide evidence on the motives for migrating for the small subsample of eligible children for whom this information is available. We find no significant effects of longer exposure to the program on the probability of migrating for studying ver-

sus work. Unfortunately, this is the best we can do because the ENCEL 2007 does not contain this type of information for all children who are absent in 2007. With our data, neither can we assess whether children exposed to PROGRESA/Oportunidades are less reliant or concerned about the social networks in their localities of origin. However, we can use the sample of non-poor children in all localitites, who did not qualify to receive the schooling grants from the program, as a rough counterfactual, because some studies show that PROGRESA/Oportunidades has had positive educational spillover effects on non-poor children living in treated localities.[14] So, estimating similar transfer equations for non-eligible parental households allows us to crudely separate the effect of children's higher educational attainment from the effect of receiving the schooling grants.

So, in section 6.2 we run a falsification test using the sample of non-poor households. For this sample of non-eligible households, none of our key interactions are statistically significant. Moreover, the coefficient of our key interaction for the number of non-poor children who were 14-16 years old in 1997, and are absent in 2007, on the transfers received by their parental households is positive, although not statistically significant. This evidence is not conclusive, but it suggests that acquiring more education per se does not have a negative effect on the transfers given to parents.

5.2.2 Health

Health is another aspect of human capital related to adult productivity and earnings. PROGRESA/Oportunidades has been found to increase the use of health services for all household members and their health levels (Gertler, 2000; Behrman and Hoddinott, 2000). Because of this, we do not control for the health levels of both parental household members and adult children in 2007. Hence, we are unable to directly check whether our results are affected due to a better health level of the adult children, but as argued in the theoretical section, a higher productivity and earnings due to better health would tend to attenuate our results.

Also, adult children may transfer money to their parents not only due to a repayment motive, but as a response to an idiosincratic shock such as illness of a member of the parental

[14] See Bobonis and Finan (2009) and Lalive and Cattaneo (2009).

household. If early treated parental households are, on average, healthier than later treated households, this could be explaining our results. However, Bautista Arredondo et al. (2008) find that seniority as a beneficiary of the program, measured by the year of enrollment, is not correlated with differences in the health level of beneficiaries or their utilization of medical services in 2007.

5.2.3 Migration

Finally, any effect of PROGRESA/Oportunidades on the migration of children would affect our results because our data have only information on transfers received from donors that do not belong to the parental household in 2007. A positive effect of PROGRESA/Oportunidades on the probability that treated children leave the parental household would increase the likelihood that we observe transfers from these children to their parents. This behavior would work against our results. However, if the effect of PROGRESA/Oportunidades on the probability of migration is negative, a decrease in private transfers from treated children could not be entirely attributed to the effect of lower repayment. Angelucci (2005) finds no effect of PROGRESA/Oportunidades on migration for those children of secondary and high school age who were exposed to the program. In the next section we show that, for the relevant age group in our sample (14-16), neither do we.

6 Robustness and additional empirical checks

6.1 Migration

To check whether our results are due to the effect of exposure to PROGRESA/Oportunidades, as proxied by age and date of treatment, on the adult child's migration decisions, we use individual level data for the 9,576 children of the parental household heads in our sample. We observe whether the adult children are absent or not from the parental household in 2007. Of those who are absent in 2007, the survey provides further information on the motives for migrating and the date of departure for only 1,669 adult children through a migrant questionnaire. For our sample of all children, about 35 percent are absent in 2007. Of those with information from the migrant questionnaire 33 percent left the household due

to marriage, 6 percent for studying, 56 percent for work and 4 percent for other reasons.[15]

Table 5 presents the results from OLS regressions on the probability that the adult child is absent in 2007, and–conditional on being absent and having completed the migrant questionnaire–the motive for migrating. All estimations control for a treatment dummy; characteristics of the child such as dummies for age in 1997 (6-9, 10-13 and 14-16, to be consistent with main estimations in section 5.1); a female dummy; the number of siblings, and the number of male siblings; characteristics of the parent such as age, education, dummies for male and married; and locality fixed effects. The key independent variables in these regressions, i.e. those measuring the effect of additional exposure to PROGRESA/Oportunidades are the interactions between the treatment dummy and the dummies for the 1997 age of the child. In all estimations, the standard errors are clustered at the locality level.

Column 1 shows that the effect of the treatment dummy on the probability of being absent in 2007 is close to zero and not statistically significant. So, living in a treated locality has no significant effect on such probability. The age dummies are all positive and significant at 1 percent. Relative to the omitted 0-5 age category, being 6-9 years old in 1997 increases the probability of being absent in 2007 by 43 percentage points, being 10-13 in 1997 increases this probability by 70 percentage points and being 14-16 in 1997 increases it by 77 percentage points. These effects capture the fact that the probability of leaving the parental household by 2007 increases with the age of the child in 1997. Controlling for these age effects, the interactions of the treatment and the age dummies are all close to zero and not statistically significant. Therefore, the negative effects of additional program exposure on the transfers that parental households and their heads receive from their absent adult children in Tables 3 and 4 are not explained by the effect of PROGRESA/Oportunidades on the children's decision to leave the parental household.

Columns 2 to 5 show the results of OLS regressions on the motive for migrating, for the sample that did migrate and has information from the PROGRESA/Oportunidades migrant questionnaire. The treatment dummy by itself has no statistically significant effect on any of the different motives for migrating, except for the negative effect of 17 percentage points

[15] Other reasons include "had problems," "her parents left and she left with them" and "other reasons."

on the probability of migrating for marriage. Most of the age dummies are statistically significant at conventional levels. Relative to the ommitted age category, the child's age in 1997 has a positive and significant effect on the probability of migrating for marriage and work, as would be expected, and a negative effect on the probability of migrating for studies. For studies and work, none of the interactions of the treatment dummy with the age dummies are statistically significant after controlling for the main age effects. This confirms that children with longer PROGRESA/Oportunidades exposure in our sample are not decreasing their transfers to their parents because they are more likely to leave the parental household to continue studying rather than for work. For marriage, the interactions of the treatment dummies are positive and significant, especially those for children who were 10-16 years old in 1997. However, given the magnitude of the negative effect of the treatment dummy alone (-0.17), the positive interactions suggest that the effect of being in a treated locality on the probability of migrating for marriage for children age 10-16 years old, compared to children who were their same age in 1997 in control localities, is close to zero. Overall, the results in Table 5 favor our interpretation of the results in Tables 3 and 4 as evidence of the schooling repayment hypothesis for private transfers.

6.2 Falsification test

To further check the validity of our results, we perform a falsification test. We run the same regressions presented in Tables 3 and 4 but using data from non-poor and, hence, non-eligible parental households.[16] That is, we check whether the interactions of the treatment dummy with the number of children in different age groups in 1997, who are absent in 2007, have any effects on the private transfers received by non-poor parental households and their heads. As these households are not eligible to receive the benefits of PROGRESA/Oportunidades, they have not been compensated by the government for sending their children to school, regardless of their locality of residence. However, as mentioned before, some studies show that the program has had a positive effect on the education of noneligible children in treatment localities (Bobonis and Finan, 2009; Lalive and Cattaneo,

[16] Within every locality where the program has been implemented, households are non-eligible to receive PROGRESA's benefits if they are above the poverty level as determined by discriminant analysis on census data.

2009). Hence, if children transfer money to their parents partly because of repayment, PROGRESA/Oportunidades has changed the education of noneligible children, but not this motive.

Tables 6 and 7 show that, as expected, the effect of early exposure to the program captured by the interaction of the number of children in different age groups in 1997 who are absent in 2007 with the treatment dummy is never statistically different from zero. If we perform one-sided tests for the null that each of these key interactions is nonnegative versus the alternative that it is strictly negative, we are not able to reject the null at any conventional level for any of them. Furthermore, when we look at the transfers received by non-poor parental households (Table 7) the effect of longer exposure to PROGRESA/Oportunidades for children 10-16 in 1997, who are absent in 2007, is even positive, but not statistically different from zero (columns 1 and 2). These results further suggest that our findings are a consequence of the additional exposure to PROGRESA/Oportunidades, and not of some other circumstance that occurred in the localities treated early.

6.3 Additional Empirical Checks

Finally, we check for any effects on parental assets and current per capita consumption in the parental household in 2007 to provide some indirect evidence on whether parents of children treated in 1998 anticipated lower transfers from them as adults. Table 8 presents OLS regressions for the logarithms of the value of parental household assets and consumption per capita in 2007. Household assets include properties (except agricultural plots), vehicles, agricultural and non-agricultural machinery, electronics, household appliances, jewelry, animals and other assets. The survey asks how many of these assets are owned by the household and also how much would the family sell the asset for. We calculate the value of assets multiplying the number of particular assets by the median price reported by households in each locality. We are aware of the measurement error issues that arise by doing this, so we are presenting the results in Table 8 only as additional evidence. Expenditure per capita is calculated as total household expenditure divided by the total number of household members in 2007, without adjusting for the number of children versus adults in the household. This is a very crude measure, but once again, we use it just as

additional evidence. In both estimations, we include the treatment dummy and the number of children of different ages in 1997. We do not include variables for the number of children who are absent in 2007. The interactions of interest are those of the number of children in different age groups in 1997 with the treatment dummy, because we want to provide evidence on whether the parent anticipated that the PROGRESA/Oportunidades schooling subsidy could lower the transfers he would receive from his children in the future, before any of them actually decided to leave the household. If he did, we might observe a higher asset accumulation and no effect on current consumption. Note that these key interactions might capture the effect of these parental expectations, but also the effect of the total amount received from PROGRESA/Oportunidades, which might have a positive effect on both dependent variables. In both estimations in Table 8, we control for the same characteristics of the parent as in Tables 3 and 4.

In column 1 of Table 8, the effect of the treatment dummy on the log of household assets is positive (0.25), but not statistically significant. The age effects are positive, but not significant, except for the effect of an additional child age 10-13 in 1997, which increases parental households' assets by 14 percent and is significant at 10 percent. Even though we control for the age of the head, this effect might reflect differences in the particular life-cycle stage the family is at. The coefficients for having children in any other age group in 1997 are not significant.

The interactions of the number of children of different ages with the treatment dummy are negative in all but one case (children age 14-16), but not statistically significant at any conventional levels. We take this as rough evidence of parents not increasing their asset accumulation, because they expected lower repayment transfers in the future. Note that the effect of the total PROGRESA/Oportunidades subsidy received on asset accumulation would reinforce the positive effects we find for our key regressors, and still we find they are not significant.

In column 2, the treatment dummy has a small and not statistically significant effect on the log of total expenditure per capita in 2007. The dummies for having children of different ages in 1997 are all negative. They are significant for the 0-5, 6-9, and 10-13 age groups, which again might capture the effect of being in different stages of the family cycle.

The interactions of the age dummies with the treatment dummy are small and mostly not significant. The interaction for the number of children age 14-16 in 1997 is negative, the largest in absolute value and signficant at 10 percent only. So, having an additional child age 14-16 in 1997 who started treatment early decreases consumption per capita in the parental household in 2007 by 9.1 percent. Recall that the largest reductions in transfers, and the only ones that are significant, are due to the number of treated children in this same age group. Together with the insignificant effects found for household assets, this effect suggests that parents did not expect the reduction in transfers potentially due to lower repayment. However, this evidence is not conclusive.

7 Conclusions

In this paper, we provide suggestive evidence of a repayment motive for the private transfers that adult children give to their parents. We overcome the endogeneity bias that contaminates previous estimates by exploiting the features and experimental design of PROGRESA/Oportunidades, a Mexican antipoverty program that pays a cash transfer to rural parents for sending their children to school. Our results show that the number of absent children with longer exposure to the program have negative, but not significant, effects on the total amount of private transfers that the head of the parental household receives from children in 2007, ten years after the start of the program. For the transfers received from children who left after the program started, these negative effects increase in size and become statistically different from zero for the number of treated absent children who were 14-16 years old in 1997. Performing one-sided tests, we are able to reject the null that these effects are nonnegative, in favor of the alternative that they are strictly negative, at conventional levels.

Children in this age group are precisely those for whom the additional exposure to the program, induced by the early treatment, likely financed their secondary and high school education, which is when the trade-off between school and work becomes stark for parents. So, we interpret our findings as evidence of a repayment motive. Furthermore, our key independent variables have no significant negative effects on the transfers received from

children who left the parental household before the program or from other donors, which confirms that these variables are capturing something that affects only the transfers from children who were exposed to the program. In addition, our results for the private transfers received by the parental household as a whole are consistent with those obtained for the heads. Finally, after discussing the previous evidence on the effects of the program on relevant confounders, like education, health and migration, and performing some robustness checks, we reasonably conclude that the reduction in the amount of private transfers that we find in our main analysis cannot be attributed to any of these potential confounders.

To our knowledge, this is the first paper that looks at the unintended effects of a conditional schooling subsidy on the transfers that parents receive from adult children who were exposed to the program. This is an important contribution because households in low-income countries typically depend on family transfers to achieve optimal consumption patterns. In this paper, we show that PROGRESA/Oportunidades reduces the transfers that adult children, who were exposed to the program, give to their parental households. Furthermore, we provide crude evidence on whether parental households anticipated this reduction by accumulating more assets and find that this is not the case. As a result, the number of children age 14-16 the household head had in1997 has a negative effect on the consumption per capita in the parental household in 2007. Thus, our findings suggest that the first generation of parental households who were beneficiaries of PROGRESA/Oportunidades might be worse-off in the future, especially because the largest part of the program transfer, which is the schooling subsidy, is temporary. To the extent that our findings can be extrapolated to other conditional cash transfer programs, a large number of parental households might be at risk, given that this type of programs have expanded to over 30 countries, which only in Latin America cover more than 27 million households (Hoddinott and Basset, 2009; Maldonado et al., 2011).

On the other hand, the first generation of beneficiary children might be the big winners of this type of programs, because they end up with more education and potentially higher earnings in their adult life. As our results suggest, these children also transfer less resources to their parents, because they owe them less. From a distributional point of view, the program could become a positive net transfer from society to these children, unless they

are forced to repay the government for their schooling through taxes. The latter depends crucially on whether PROGRESA/Oportunidades children get jobs in the formal sector, where tax compliace is usually higher, after graduating from the program.

A final caveat is that our data are not ideal. Both the parents and the adult children in our sample might still be young to be receiving and giving important amounts of transfers, respectively. Thus, future research with better and later data from PROGRESA/Oportunidades and other conditional cash transfer programs seems due.

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Table 1A: Children's exposure to PROGRESA/Oportunidades based on age and year of treatment

Age in 1997	School grade in 1997	Age in 2007	Years of exposure to PROGRESA-/Oportunidades in 2007: Treatment started in May 1998	Years of exposure to PROGRESA-/Oportunidades in 2007: Treatment started in December 1999	Difference in exposure
0+	-	10	3	3	0
1	-	11	4	4	0
2	-	12	5	5	0
3	-	13	6	6	0
4	-	14	7	7	0
5	-	15	8	8	0
6	1 pri	16	9	8	1
7	2 pri	17	10	8	2
8	3 pri	18	9	7	2
9	4 pri	19	8	6	2
10	5 pri	20	7	5	2
11	6 pri	21	6	4	2
12	1 sec	22	4	2	2
13	2 sec	23	2	1	1
14	3 sec	24	0	0	0
15	1 high sch	25	0	0	0
16	2 high sch	26	0	0	0

Source: Authors' calculations based on the age-grade relationship in the first two columns.

Table 1B: Children's exposure to PROGRESA/Oportunidades by school grade based on age and year of treatment

			Primary years of exposure to PROGRESA/Oportunidades in 2007			Secondary years of exposure to PROGRESA/Oportunidades in 2007			High school years of exposure to PROGRESA/Oportunidades in 2007		
Age in 1997	School grade in 1997	Age in 2007	Treatment started in May 1998	Treatment started in December 1999	Difference	Treatment started in May 1998	Treatment started in December 1999	Difference	Treatment started in May 1998	Treatment started in December 1999	Difference
0+	-	10	3	3	0	0	0	0	0	0	0
1	-	11	4	4	0	0	0	0	0	0	0
2	-	12	4	4	0	1	1	0	0	0	0
3	-	13	4	4	0	2	2	0	0	0	0
4	-	14	4	4	0	3	3	0	0	0	0
5	-	15	4	4	0	3	3	0	1	1	0
6	1 pri	16	4	3	1	3	3	0	2	2	0
7	2 pri	17	4	2	2	3	3	0	3	3	0
8	3 pri	18	3	1	2	3	3	0	3	3	0
9	4 pri	19	2	0	2	3	3	0	3	3	0
10	5 pri	20	1	0	1	3	2	1	3	3	0
11	6 pri	21	0	0	0	3	1	2	3	3	0
12	1 sec	22	0	0	0	2	0	2	2	2	0
13	2 sec	23	0	0	0	1	0	1	1	1	0
14	3 sec	24	0	0	0	0	0	0	0	0	0
15	1 high sch	25	0	0	0	0	0	0	0	0	0
16	2 high sch	26	0	0	0	0	0	0	0	0	0

Source: Authors' calculations based on the age-grade relationship assumed in the first two columns.

Table 2: Descriptive statistics by year of treatment start

	Treatment in May 1998		Treatment in December 1999		Difference	
	Mean	SE	Mean	SE	T98-T99	SE
Private transfers received by the parent during the previous year						
Total	91.97	19.01	58.39	18.13	33.58	26.26
From children	77.04	17.19	56.10	18.06	20.94	24.94
From children who left before 1997	19.87	7.27	9.24	4.47	10.63	8.54
From children who left after 1997	57.17	15.63	46.87	17.53	10.30	23.49
From other donors	14.92	7.99	2.28	1.61	12.64	8.15
Private transfers received by the parental household during the previous year						
Total	282.57	41.70	280.48	52.29	2.09	66.88
From children	152.08	27.15	151.66	33.68	0.42	43.26
From children who left before 1997	34.65	10.88	22.92	9.30	11.73	14.32
From children who left after 1997	117.43	24.83	128.74	32.47	-11.31	40.87
From other donors	130.49	30.40	128.83	38.61	1.67	49.14
Number of children by age in 1997						
Age 0-5	1.37	0.03	1.31	0.04	0.06	0.04
Age 6-9	1.28	0.02	1.24	0.03	0.04	0.04
Age 10-13	1.17	0.02	1.24	0.03	-0.07*	0.04
Age 14-16	0.55	0.02	0.57	0.02	-0.02	0.03
Number of children by age in 1997 who are absent in 2007						
Age 0-5	0.13	0.01	0.11	0.01	0.02	0.02
Age 6-9	0.63	0.02	0.61	0.02	0.01	0.03
Age 10-13	0.89	0.02	0.92	0.03	-0.03	0.03
Age 14-16	0.45	0.02	0.44	0.02	0.01	0.03
Average years of children's exposure to Progresa in 2007	5.02	0.04	3.36	0.05	1.65***	0.06
Average years of absent children's exposure to Progresa in 2007	4.24	0.06	2.29	0.07	1.95***	0.09
Characteristics of the parent						
Age	48.22	0.22	48.31	0.27	-0.09	0.35
Male	0.95	0.01	0.96	0.01	0.01	0.01
Years of schooling	3.24	0.07	3.13	0.09	0.11	0.11
Married	0.81	0.01	0.83	0.01	-0.01	0.02
Number of children in 1997	4.37	0.04	4.35	0.06	0.01	0.07
Number of male children in 1997	2.21	0.04	2.18	0.05	0.02	0.06
Parental household characteristics in 2007						
Household size	8.03	0.07	7.96	0.09	0.07	0.11
Number of children age 0-5	0.59	0.02	0.54	0.03	0.05	0.04
Number of children age 6-17	2.80	0.05	2.69	0.06	0.11	0.07
Total value of hh assets	22371	2066	19722	1679	2649	2662
Number of absent members of the household	2.10	0.03	2.09	0.04	0.009	0.05
Number of observations	1394		877		2271	

Sample: Poor heads of household who had children age 0-16 years old in 1997 from the PROGRESA/Oportunidades evaluation sample. Only heads of households with at least one member absent in 2007 are included.

Table 3: OLS regressions for private transfers received in 2007 by poor parental household heads

	From children	From children who left after 1997	From children who left before 1997	From other donors
	(1)	(2)	(3)	(4)
Treatment May 1998 dummy	97.41	80.80	16.62	-23.92
	(87.53)	(86.63)	(24.11)	(17.49)
Number of children in the hh by age in 1997				
Age 0-5	-12.64	-16.35	3.716	-1.466
	(30.78)	(29.80)	(5.936)	(1.666)
Age 6-9	-37.16	-45.40*	8.239*	-3.047
	(25.44)	(25.14)	(4.928)	(2.674)
Age 10-13	-63.65**	-58.18**	-5.466	-0.634
	(27.43)	(25.93)	(3.623)	(2.988)
Age 14-16	-93.47***	-81.66**	-11.81	-2.655
	(35.07)	(33.90)	(7.191)	(2.334)
Treatment May 1998 x number of children in the hh by age in 1997				
Age 0-5	-2.954	-1.005	-1.949	6.201
	(31.10)	(29.94)	(7.848)	(6.029)
Age 6-9	0.956	24.32	-23.36*	-19.16
	(37.24)	(34.61)	(13.28)	(13.17)
Age 10-13	23.96	37.35	-13.39	17.91
	(37.60)	(35.07)	(11.16)	(15.13)
Age 14-16	111.5	136.2*	-24.63	8.810
	(75.26)	(69.89)	(24.16)	(14.94)
Number of children in the hh by age in 1997 absent in 2007				
Age 0-5	-35.54	-30.07	-5.462	7.573
	(41.79)	(43.11)	(5.808)	(4.914)
Age 6-9	43.88	44.62	-0.735	7.656
	(35.70)	(36.68)	(5.936)	(7.068)
Age 10-13	71.31**	56.88*	14.43**	4.391
	(33.78)	(32.03)	(6.971)	(3.215)
Age 14-16	105.9**	107.9**	-2.069	2.102
	(53.39)	(54.41)	(4.866)	(2.505)
Treatment May 1998 x number of children in the hh by in 1997 absent in 2007				
Age 0-5	10.79	-1.197	11.99	-18.24
	(64.24)	(56.16)	(32.76)	(12.64)
Age 6-9	-7.579	-15.32	7.738	18.43
	(48.52)	(47.47)	(14.84)	(15.96)
Age 10-13	-52.15	-66.86	14.70	-8.750
	(46.93)	(42.29)	(18.10)	(21.73)
Age 14-16	-137.0	-185.2**	48.11**	7.845
	(91.69)	(89.60)	(21.92)	(19.08)
Constant	-82.48	28.16	-109.6**	25.23
	(121.1)	(110.7)	(48.27)	(27.19)
Observations	2,271	2,271	2,271	2,271
Adjusted R-squared	0.0386	0.0410	0.0162	0.023

Sample: Poor heads of household who had children 0-16 in 1997. Only heads of households with at least one child absent in 2007 are included. All estimations include the head's age and years of education, dummies for whether the head is male or married, the number of male children a head had in 1997, and locality dummies. Standard errors clustered at the locality level are reported in parentheses.

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

Table 4: OLS regressions for the private transfers received in 2007 by poor parental households

	From children	From children who left after 1997	From children who left before 1997	From other donors
	(1)	(2)	(3)	(4)
Treatment May 1998 dummy	73.11	69.33	3.777	2.424
	(135.3)	(134.2)	(33.19)	(117.5)
Number of children in the hh by age in 1997				
Age 0-5	-15.07	-8.516	-6.550	37.14
	(60.03)	(58.78)	(11.13)	(41.91)
Age 6-9	-88.07**	-96.30**	8.224	-88.94
	(42.91)	(42.71)	(10.59)	(80.62)
Age 10-13	-96.28*	-82.26	-14.02	119.4
	(57.32)	(61.44)	(10.73)	(115.7)
Age 14-16	-176.0**	-156.6*	-19.34*	-119.4
	(83.56)	(83.25)	(10.46)	(135.1)
Treatment May 1998 x number of children in the hh by age in 1997				
Age 0-5	-22.28	-36.21	13.93	-59.07
	(60.83)	(58.23)	(16.92)	(50.78)
Age 6-9	43.99	76.71	-32.72	89.77
	(56.02)	(52.91)	(20.17)	(101.4)
Age 10-13	62.45	49.51	12.94	-150.4
	(70.25)	(70.09)	(21.32)	(141.2)
Age 14-16	118.8	155.3	-36.46	362.0
	(109.4)	(104.2)	(31.28)	(280.8)
Number of children in the hh by age in 1997 absent in 2007				
Age 0-5	-152.4*	-142.5	-9.968	31.89
	(90.99)	(93.73)	(11.14)	(218.1)
Age 6-9	44.13	39.48	4.643	95.67
	(53.14)	(51.96)	(11.75)	(105.7)
Age 10-13	76.18	56.20	19.98	-100.2
	(56.25)	(54.18)	(15.22)	(127.1)
Age 14-16	178.1*	172.1*	5.991	56.38
	(91.47)	(92.36)	(12.37)	(125.3)
Treatment May 1998 x number of children in the hh by in 1997 absent in 2007				
Age 0-5	57.74	49.59	8.156	110.3
	(118.1)	(115.2)	(34.59)	(259.2)
Age 6-9	-1.990	-4.286	2.296	-54.80
	(74.59)	(72.10)	(21.32)	(127.3)
Age 10-13	-72.71	-57.39	-15.32	93.69
	(76.29)	(69.54)	(29.74)	(137.4)
Age 14-16	-138.4	-215.4*	77.03*	-300.2
	(124.9)	(119.6)	(39.22)	(276.0)
Constant	-106.1	108.3	-213.4**	549.8**
	(194.7)	(175.3)	(82.36)	(228.0)
Observations	2,271	2,271	2,271	2,271
Adjusted R squared	0.0479	0.0666	0.0428	0.0526

Sample: Poor households whose head had children 0-16 in 1997. Only households with at least one child absent in 2007 are included. All estimations include the head's age and years of education, dummies for whether the head is male or married, the number of male children a head had in 1997, and locality dummies. Standard errors clustered at the locality level are reported in parentheses.

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

Table 5: OLS regressions for the child's migration probability and motives

		Motive for migrating			
	Child is absent in 2007	Marriage	Studies	Work	Other
	(1)	(2)	(3)	(4)	(5)
Treatment May 1998 dummy	-0.004	-0.172**	-0.118	0.093	0.150
	(0.042)	(0.081)	(0.122)	(0.152)	(0.107)
Dummies for age in 1997 (Omitted category: Age 0-5)					
Age 6-9	0.427***	0.151***	-0.230**	0.227***	-0.165**
	(0.021)	(0.054)	(0.104)	(0.082)	(0.069)
Age 10-13	0.697***	0.244***	-0.281***	0.196**	-0.178**
	(0.022)	(0.054)	(0.104)	(0.081)	(0.069)
Age 14-16	0.770***	0.251***	-0.288***	0.151	-0.166**
	(0.024)	(0.072)	(0.109)	(0.094)	(0.073)
Treatment May 1998 x Age 6-9	-0.020	0.119*	0.083	-0.066	-0.100
	(0.026)	(0.070)	(0.132)	(0.114)	(0.111)
Treatment May 1998 x Age 10-13	0.011	0.156**	0.072	-0.099	-0.086
	(0.026)	(0.071)	(0.127)	(0.116)	(0.111)
Treatment May 1998 x Age 14-16	0.015	0.182**	0.089	-0.102	-0.108
	(0.028)	(0.084)	(0.137)	(0.130)	(0.114)
Female dummy	0.041***	0.423***	0.007	-0.426***	-0.004
	(0.009)	(0.026)	(0.015)	(0.029)	(0.010)
Number of siblings	-0.026***	-0.009	-0.006	0.021**	-0.005
	(0.004)	(0.007)	(0.006)	(0.009)	(0.005)
Number of male siblings	0.010**	0.011	0.006	-0.014	-0.006
	(0.004)	(0.010)	(0.006)	(0.012)	(0.006)
Characteristics of the parent					
Age	-0.003***	-0.003	0.000	0.003*	-0.001
	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)
Male dummy	0.040	0.080*	-0.003	-0.065	-0.021
	(0.025)	(0.041)	(0.038)	(0.062)	(0.035)
Years of schooling	-0.003	-0.011*	0.007**	0.008	-0.003
	(0.002)	(0.006)	(0.003)	(0.006)	(0.004)
Married dummy	-0.048***	0.020	-0.021	0.041	-0.043**
	(0.013)	(0.033)	(0.022)	(0.040)	(0.022)
Constant	0.238***	0.195*	0.326**	0.253	0.253***
	(0.059)	(0.114)	(0.125)	(0.190)	(0.082)
Observations	9,576	1,669	1,669	1,669	1,669
Adjusted R-squared	0.388	0.390	0.190	0.335	0.184

Sample: In column 1, all children from the poor heads of household included in our estimation samples, who were 0-16 years old in 1997. In columns 2-5, a subsample of children who are absent in 2007 and for whom we have some information from the migrant questionnaire in that same year. All estimations include locality fixed effects. Standard errors clustered at the locality level are reported in parentheses.

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

Table 6: OLS regressions for private transfers received in 2007 by non-poor parental household heads

	From children	From children who left after 1997	From children who left before 1997	From other donors
	(1)	(2)	(3)	(4)
Treatment May 1998 dummy	-118.365	-111.974	-6.391	-6.394
	(74.555)	(73.456)	(8.729)	(8.162)
Number of children in the hh by age in 1997				
Age 0-5	-30.986	-29.013	-1.972	6.073
	(19.104)	(18.729)	(3.200)	(6.442)
Age 6-9	-26.303	-28.358	2.055	3.621
	(30.389)	(30.014)	(5.304)	(4.492)
Age 10-13	-12.345	-11.523	-0.823	1.401
	(12.903)	(13.508)	(5.537)	(4.332)
Age 14-16	-26.161	-32.121	5.960	0.786
	(26.888)	(25.250)	(11.813)	(5.604)
Treatment May 1998 x number of children in the hh by age in 1997				
Age 0-5	18.417	19.425	-1.008	-0.667
	(37.425)	(36.995)	(4.081)	(2.646)
Age 6-9	-37.421	-33.527	-3.894	-4.577
	(55.302)	(54.816)	(6.843)	(5.249)
Age 10-13	32.279	31.338	0.941	-3.752
	(57.056)	(57.011)	(5.643)	(5.814)
Age 14-16	130.127	137.549	-7.422	0.053
	(136.060)	(135.782)	(12.409)	(7.681)
Number of children in the hh by age in 1997 absent in 2007				
Age 0-5	21.126	33.099*	-11.973	1.925
	(24.668)	(19.823)	(15.107)	(5.468)
Age 6-9	26.228	5.472	20.756	-0.543
	(38.121)	(21.150)	(29.955)	(4.127)
Age 10-13	-1.089	16.973	-18.062	0.576
	(38.028)	(31.502)	(22.456)	(4.040)
Age 14-16	-21.667	-11.344	-10.324	-1.598
	(24.829)	(22.279)	(12.668)	(6.537)
Treatment May 1998 x number of children in the hh by in 1997 absent in 2007				
Age 0-5	3.383	-10.268	13.650	-4.190
	(39.969)	(36.650)	(15.954)	(8.383)
Age 6-9	81.609	103.527	-21.918	6.718
	(93.427)	(87.339)	(31.990)	(6.934)
Age 10-13	43.148	26.997	16.150	6.658
	(80.281)	(77.926)	(20.286)	(7.511)
Age 14-16	-99.068	-111.282	12.214	3.543
	(156.749)	(156.414)	(13.659)	(9.236)
Constant	277.675*	227.784	50.891	-42.164
	(145.378)	(139.844)	(42.266)	(101.390)
Observations	670	670	670	670
Adjusted R squared	0.0736	0.0401	0.392	0.238

Sample: Non-poor heads of household who had children 0-16 in 1997. Only heads of households with at least one child absent in 2007 are included. All estimations include the head's age and years of education, dummies for whether the head is male or married, the number of male children a head had in 1997 and locality dummies. Standard errors clustered at the locality level are reported in parentheses.

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

Table 7: OLS regressions for private transfers received in 2007 by non-poor parental households

	From children	From children who left after 1997	From children who left before 1997	From other donors
	(1)	(2)	(3)	(4)
Treatment May 1998 dummy	314.467	248.202	66.265	74.661
	(386.084)	(384.615)	(80.158)	(166.053)
Number of children in the hh by age in 1997				
Age 0-5	-167.143	-163.641	-3.503	-69.403
	(127.903)	(128.626)	(3.461)	(69.960)
Age 6-9	79.225	73.918	5.307	62.928
	(121.239)	(119.875)	(6.405)	(66.346)
Age 10-13	392.145	387.973	4.173	225.179
	(504.079)	(504.003)	(9.867)	(228.068)
Age 14-16	-30.715	-34.788	4.073	-41.486
	(118.203)	(114.818)	(12.232)	(57.409)
Treatment May 1998 x number of children in the hh by age in 1997				
Age 0-5	96.532	98.871	-2.340	64.134
	(137.200)	(138.145)	(5.769)	(61.291)
Age 6-9	-56.667	-46.000	-10.667	-68.957
	(193.995)	(192.972)	(9.011)	(82.425)
Age 10-13	-457.650	-448.220	-9.430	-290.286
	(526.918)	(527.237)	(13.562)	(241.791)
Age 14-16	-10.225	-0.471	-9.754	177.587
	(195.434)	(194.036)	(13.489)	(157.746)
Number of children in the hh by age in 1997 absent in 2007				
Age 0-5	-19.360	-5.638	-13.722	-108.257
	(166.130)	(159.287)	(15.891)	(99.189)
Age 6-9	339.565	322.734	16.831	-20.513
	(233.587)	(232.910)	(30.251)	(143.136)
Age 10-13	-231.600	-205.556	-26.044	-176.937
	(400.264)	(396.722)	(24.362)	(181.932)
Age 14-16	-103.612	-93.203	-10.409	-33.752
	(166.657)	(164.511)	(12.820)	(81.835)
Treatment May 1998 x number of children in the hh by in 1997 absent in 2007				
Age 0-5	-143.843	-160.355	16.512	67.794
	(235.406)	(231.198)	(17.200)	(104.750)
Age 6-9	-282.207	-263.208	-18.999	83.132
	(317.154)	(317.453)	(32.246)	(168.702)
Age 10-13	411.187	386.777	24.409	196.283
	(417.461)	(414.240)	(22.360)	(180.184)
Age 14-16	200.383	182.457	17.925	-66.667
	(256.403)	(255.036)	(15.195)	(165.307)
Constant	-176.036	-162.501	-12.535	-305.651
	(610.347)	(606.825)	(87.563)	(285.477)
Observations	670	670	670	670
Adjusted R squared	0.111	0.093	0.945	0.0659

Sample: Nonpoor households whose head had children 0-16 in 1997. Only households with at least one child absent in 2007 are included. All estimations include the head's age and years of education, dummies for whether the head is male or married, the number of male children a head had in 1997, and locality dummies. Standard errors clustered at the locality level are reported in parentheses.

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

Table 8: OLS regressions for parental household assets and consumption in 2007

	Log (hh assets)	Log(expcapita)
	(1)	(2)
Treatment May 1998 dummy	0.253	0.065
	(0.291)	(0.143)
Number of children by age in 1997		
Age 0-5	-0.108	-0.062**
	(0.067)	(0.025)
Age 6-9	0.106	-0.063**
	(0.072)	(0.029)
Age 10-13	0.139*	-0.094**
	(0.071)	(0.041)
Age 14-16	0.081	-0.025
	(0.092)	(0.044)
Treatment May 1998 x number of children by age in 1997		
Age 0-5	-0.025	0.002
	(0.084)	(0.032)
Age 6-9	-0.129	-0.040
	(0.098)	(0.038)
Age 10-13	-0.084	0.015
	(0.091)	(0.049)
Age 14-16	0.059	-0.091*
	(0.121)	(0.050)
Constant	8.022***	7.677***
	(0.437)	(0.157)
Observations	2271	2271
Adjusted R squared	0.145	0.204

Sample: Poor heads of household who had children 0-16 years old in 1997. Only heads of households with at least one child absent in 2007 are included. All estimations include the head's age and years of education, dummies for whether the head is male or married,, the number of male children the head had in 1997, and locality dummies. Standard errors clustered at the locality level are reported in parentheses.

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

Figure 1: School enrollment and labor force participation in 1997 of girls 8-17 years old

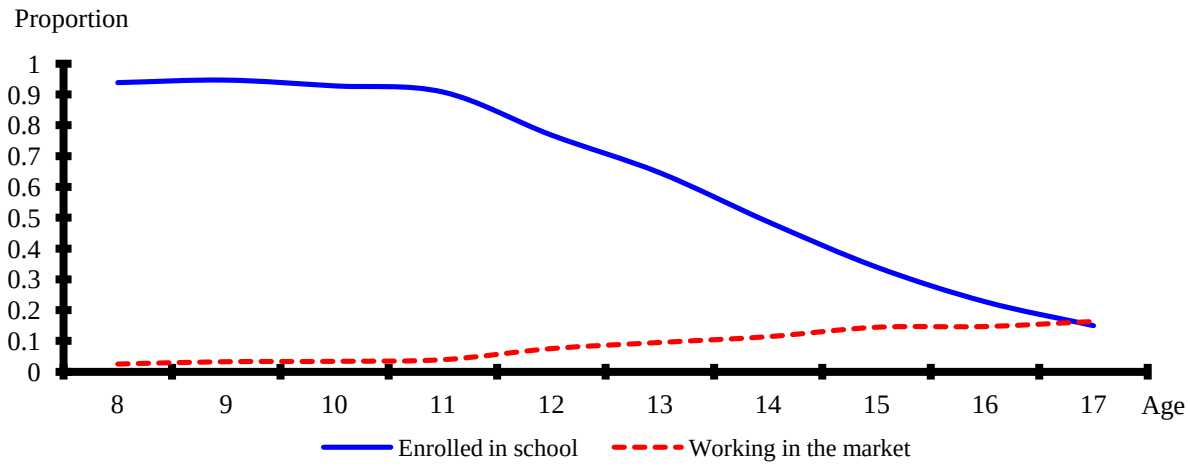
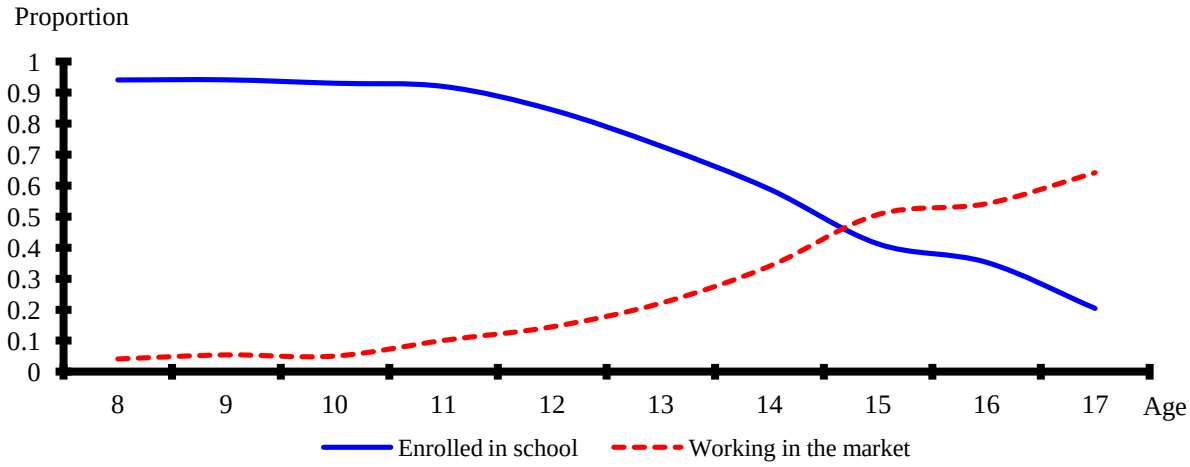


Figure 2: School enrollment and labor force participation in 1997 of boys 8-17 years old



Source: Authors' calculation using ENCASEH97.