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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. We provide evidence on the repayment motive using data from the Mexican conditional cash transfer program PROGRESA/Oportunidades (PO). PO 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 PO should decrease these transfers—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 PO 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. Results suggest a repayment motive exists. That is, PO is causing adult children to transfer less resources to their parents.

Keywords Parental schooling investments · Schooling repayment hypothesis · Intergenerational transfers · PROGRESA/Oportunidades

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JEL Classification D19 · J18 · J19

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: 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. We provide evidence of the repayment motive for these transfers using data from Mexico's PROGRESA/Oportunidades program (PO hereafter).

Addressing the school repayment hypothesis empirically is challenging because the expenditures are generally endogenous to the ability of each child. Additionally, unobserved family characteristics affecting the transfers received from children might also be correlated with the human capital investment in children. For example, the altruism of the parents (reflected in high investments into children) might be transmitted to the children explaining large transfers on their part. As a result, for both developed and developing countries, previous empirical work examines the *determinants* of the transfers that adult children give to their parents and vice versa.[1] 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, but without controlling for the endogeneity of education.[2]

To provide evidence on the repayment motive, we exploit the features and randomized design of PO, a Mexican antipoverty program that pays a cash transfer to rural parents for sending their children to school. The schooling transfer from the program—the largest fraction of total program benefits for most households—is conditioned on children's enrollment and substantial attendance to school. By design, when PO 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

[1] A recent paper on what motivates transfers from adult children to parents in South Korea is Park (2014). For surveys of the literature on the determinants of transfers from adult children to parents, see Laitner (1997) and Arrondel and Masson (2006). For developing countries, see, for instance, Cox and Jimenez (1992) and Cox et al. (1998).

[2] 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 Malaysia. Also using Malaysian 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.

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 PO 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 PO rural evaluation sample. We focus on poor parental households that had children 0–16 years old in 1997. Any parent with at least one child older than 16 in 1997 is dropped from our sample.[3] Nonpoor households are excluded from the main analysis, but are used to perform a falsification test.

There are two important aspects of our data that may complicate finding an effect. First, both parents and children are still relatively young in 2007 to be receiving and giving important amounts of private transfers, so it might be too early to detect any significant program effects. Second, it has been documented that PO increases children's education (Behrman et al. 2005, 2011; Schultz 2004) and improves their health (Gertler 2000; Behrman and Hoddinott 2001). These improvements should lead to an increase in children's earnings which—under relatively weak assumptions—should generally lead to an increase in transfers to parents. That is, the combination of more schooling and better health works against finding evidence of the repayment hypothesis. Nevertheless, despite this, as discussed later, we do find some evidence consistent with the repayment motive.

Our identification strategy exploits the exogenous variation in the amount of cash transfers a parental household receives from PO for sending its children to school. This variation is induced by the age of the child in 1997, before the start of the program, and the year 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 of 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.

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 his children and from

[3] By doing this, we keep only families with age-qualifying children in 1997 to reduce heterogeneity and reduce noise in our transfers data. Children who are older than 16 are more likely to transfer money to their parents because they are (relatively) the oldest children. However, it is unlikely that these children have been affected by PO. If we add these children to our sample, results remain qualitatively the same, but statistical significance gets lost.

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, we estimate the effect of the number of children in different age groups that 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.

Despite the limitations of our data, we find that longer exposure to PO decreases the transfers coming from children potentially exposed to the program, and not those coming from children who left the household *before* the start of the program, or from other friends and relatives. Hence, we interpret our results as suggestive evidence in favor of the repayment hypothesis.

We conduct a number of robustness and additional empirical checks to further support our findings. For example, we perform a falsification test by re-estimating our transfer equations using the sample of nonpoor households and find no significant effects of our key interactions, which is reassuring. In addition, we use the information of poor individual children in our sample to estimate the effect of

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

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

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

2 Empirical strategy and descriptive statistics

Our identification strategy exploits the exogenous variation in the amount of cash transfers a household receives from PO for sending its children to school. This variation is induced by the age of children in 1997 and the starting date of treatment of each household. Table 1 shows in column 6 the potential years of differential exposure to PO by 2007 for a given child, depending on her age in 1997 and the moment her locality was incorporated to the program (May 1998 or December 1999). For calculating the years of exposure, we assume the age-grade relationship shown in columns 1 and 2.[4] The actual transfers from the program are conditioned on the school grade and not on the age of the child, thus in Table 1 we are abstracting from any grade repetition or from re-entry of older children to school after the program was implemented in their localities. Our measure is a proxy for the schooling costs that parents were compensated for by the program and 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 1 shows that depending on the age of the child in 1997, the additional years of schooling compensation received from into PO varies between 0, 1 and 2 years. Note that given our assumptions about the age-grade relationship and grade progression, children who were 14–16 years old in 1997 had no exposure to PO educational grants, regardless of the community they lived in. However, grade repetition seems to be an issue in our sample.[5] Thus, a number of children who were 14–16 years old in 1997 might actually have received the benefits of the program. Hence, we will consider this to be the case from now on.

Table 1 also shows the educational level—primary or secondary—financed by those additional years of program support. For children 6–10 years old in 1997, the early treatment financed part of their primary education, whereas for children 11–13 years old in 1997, it financed their secondary one. Finally, in column 3, it can be seen that these (adult) children are still quite young by 2007.

The data allow us to create parent–child pairs for each child the head of the parental household had in 1997. We also observe sociodemographic characteristics of both heads and children. Ideally, we would like to observe the private transfers each individual child gave to the head and link this information with the individual

[4] These are the standard entry ages to each schooling level in Mexico. When calculating program exposure, we also take into account that PO starts paying schooling transfers to parents when their children get enrolled and attend third grade, which is when children are about 8 years old, and that the program started providing schooling grants for high school in 2001.

[5] In our data, 74 percent of 14 year olds in 1997, 56 percent of 15 year olds, and 39 percent of 16 year olds were enrolled in 1997 in a grade that would made them eligible to receive transfers from PO by the time the program started in 1998. Furthermore, Behrman et al. (2005) suggest that some children re-entered school after the program was implemented in their localities.

characteristics of the head and child. But, as mentioned above, this information cannot be disaggregated by child. We only observe whether the parental household, and who within the household, received a private transfer from another household, the amount, and 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).[6] 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 his children and other types of donors.

To provide evidence on the repayment motive, we estimate the following equation by OLS:

$$T_{hl} = \alpha + \beta_1 X_{hl} + \beta_2 D98_l + \sum_g \gamma_g C_{ghl} + \sum_g \delta_g \left(D98_l \times C_{ghl}\right) + \sum_g \rho_g A_{ghl} + \sum_g \pi_g \left(D98_l \times A_{ghl}\right) + \phi_l + \varepsilon_{hl}$$

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;[7] $D98_l$ is a dummy equal to 1 if the parental household is in a PO locality that started treatment in 1998, and 0 otherwise; C_{ghl} and A_{ghl} are the number of children in age group g the head of parental household h had in 1997 and those who are absent from the parental household in 2007, respectively; 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;[8] and ε_{hl} is an idiosyncratic error term. Following the exposure differentials shown in Table 1, the four age groups we consider are: 0–5, 6–9, 10–13 and 14–16 years old in 1997, before the start of PO.[9]

The coefficients of interest are π_g, because they measure the effect of having an additional 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 PO on the private transfers due to a repayment motive, because we are already controlling for C_{ghl}, $D98_l \times C_{ghl}$, and 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 1, only children age 6 and older in 1997 in early treated localities had a longer exposure to the program's schooling grants. The income effect of receiving

[6] Throughout the paper we refer to children who left the parental household by 2007 as "absent children".

[7] Age, gender, years of schooling, a dummy for married and the number of male children he had in 1997.

[8] Our results do not change significantly if we remove the locality fixed effects from our regressions. If anything, our standard errors become a bit smaller.

[9] Children who were 10 years old in 1997 had two additional years of program exposure if their household was incorporated into PO in 1998, compared to same-aged children incorporated into PO 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 differentially exposed to PO only during their primary school years.

the PO cash benefits for a longer time on private transfers is appropriately controlled for with the interaction of the number of children in different age groups in 1997 and the early treatment dummy ($D98_l \times C_{ghl}$).[10]

Table 2 presents the descriptive statistics of parental household heads and their households by the date their treatment started (May 1998 or 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 small: 92 pesos for those receiving treatment early and 58 pesos for those receiving treatment later. About 84 % of the private transfers received by the head come from his children and, of those, 74 % come from children who left the household after 1997. On average, we find no statistically significant difference for the transfers received by the heads receiving treatment early or later. 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. For parental households, private transfers received in 2007 are larger. For both groups, about 54 % of the private transfers come from the head's children and, of these, 77 % come from children who left the parental household after 1997. For both groups, about 46 % of private transfers come from other donors, whereas for heads alone only 4–16 % do.

The mean differences between those receiving treatment early and later are very small and never statistically significant for almost all of the characteristics reported in Table 2. 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. Expenditures per capita and the total value of households' assets in 1997 are balanced as well. So, the PO 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 later are those in the children's average years of PO exposure. For the average parental household treated early, the program financed between 1.6 and 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. We do not control for parental household size and its composition, the total value of its assets and the number of members who are absent in 2007, because PO could potentially affect these outcomes.[11]

[10] The income effect refers to the effect on transfers received by adult children that can be attributed to the fact that the parental household might have more income after receiving the PO transfers. The altruistic model would predict this effect is negative, but in other models, like in the exchange one, it could be positive under certain conditions.

[11] For our main estimations, we check whether including these potentially bad regressors changes our results and find no evidence of this.

Table 2 Descriptive statistics by year of treatment start

	Treatment started in May 1998		Treatment started in December 1999		Difference	
	Mean	SE	Mean	SE	T98–T00	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 exposure to PROGRESA/Oportunidades in 2007						
All children	5.02	0.04	3.36	0.05	1.65***	0.06
Absent children	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
Expenditure per capita in 1997	218.00	4.29	215.00	4.42	3.00	6.17
Total value of household assets in 1997	7014.00	355.00	6990.00	380.00	24.00	520
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 household assets	22,371	2066	19,722	1679	2649	2662
Number of absent members	2.10	0.03	2.09	0.04	0.009	0.05

Table 2 continued

	Treatment started in May 1998		Treatment started in December 1999		Difference	
	Mean	SE	Mean	SE	T98–T00	SE
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

3 Results

Table 3 shows the results from OLS regressions on the amount of private transfers received by the parental head (Panel A) and household (Panel B) in 2007. Only the coefficients on the early treatment dummy and the key interactions with the number of children in different age groups in 1997, who are absent in 2007, are shown. In all estimations, the standard errors are clustered at the locality level.

Column 1 shows the results for the transfers received from children. For both parental heads and households, the key interactions for children older than 6 in 1997, absent in 2007, are negative, but not statistically significant. We interpret these results as the first piece of suggestive evidence supporting the repayment hypothesis, because absent children with longer program exposure, seem to transfer less to their parents. Further, the effect increases in absolute value as we consider older children. Pre-intervention data from 1997 show that at age 11, children start dropping out of school and start participating in the labor market. Thus, when children turn 11 years old, the school-work trade-off 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 trade-off would be even more important for children age 14–16 in 1997, who have the largest negative effect on the private transfers received by early treated heads. They have even better labor market opportunities and a greater probability of being absent from the parental household.[12]

In column 2, for the transfers received from children who left the household after 1997, who might have been exposed to PO, the key effects are similar to those in column 1. Interestingly, some become larger and statistically significant. In particular, in Panel A the coefficient for the oldest group becomes significantly different from zero at 5 %. If instead of a two-tailed t test, for each coefficient 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 (−66.9 pesos) at 10 % and the one for children age 14–16 (−185.2 pesos) at 2 %. This reinforces our interpretation of these coefficients as evidence of repayment, because these transfers are coming precisely from children potentially

[12] For instance, in 1997, before the start of PO, 54 percent of children age 14 was enrolled in school and 23 percent was already working in the labor market. These percentages are 30 and 36, respectively, for children age 16.

Table 3 OLS regressions for private transfers received in 2007 by poor parental households and heads

	From children (1)	From children who left after 1997 (2)	From children who left before 1997 (3)	From other donors (4)
Panel A: received by the head of household				
Treatment 1998 dummy	97.41	80.80	16.62	−23.92
	(87.53)	(86.63)	(24.11)	(17.49)
Treatment × number of children in the household 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	2271	2271	2271	2271
Adjusted R-squared	0.0386	0.0410	0.0162	0.023
Panel B: received by the household as a whole				
Treatment 1998 dummy	73.11	69.33	3.777	2.424
	(135.3)	(134.2)	(33.19)	(117.5)
Treatment × number of children in the household 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	2271	2271	2271	2271
Adjusted R squared	0.0479	0.0666	0.0428	0.0526

Sample: poor households who 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, the number of children in different age groups in 1997 and its interactions with the treatment dummy, the number of children in different age groups in 1997 who are absent in 2007, and locality dummies. Standard errors clustered at the locality level are reported in parentheses

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

exposed to the program. In Panel B, a similar pattern is observed for the transfers received by the parental household. An additional child age 14–16 years old decreases the transfers received by the household by 215 pesos per year, effect statistically different from zero at 10 %. 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 %.

In column 3, in both Panels A and B, 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. So, the negative effects of the number of absent children exposed longer to PO 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 before.

Finally, column 4 shows the results for the private transfers received by the parental head and household from other donors (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 PO affected only the transfers from children, due to the repayment hypothesis, and not those from other donors.

4 Confounders, robustness checks and falsification test

Even after controlling for relevant covariates, some confounders could potentially undermine our results. PO is intended to increase its beneficiaries' health and their children's education, and has been found to be effective in doing so (Gertler 2000; Behrman and Hoddinott 2001; Behrman et al. 2005, 2011; Schultz 2004). Precisely because of this, we do not control for health and education outcomes in our estimations. Still, an improvement in these factors can potentially increase adult productivity and earnings, and also the transfers paid to parents as a result. However, note that in such case, our results would be attenuated.

Regarding health, the program could make early treated parental households healthier on average than later treated ones, which could contaminate our results if adult children transfer money to their parents not only due to a repayment motive, but as a response to a health shock affecting them. 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.

In turn, more education could have additional negative effects on the transfers given to the parents for at least two reasons. First, in order to continue studying, adult children may leave the parental household, delay their entry into the labor market (Behrman et al. 2011; Skoufias and Parker 2001), and thus also their transfers to parents.[13] 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.

[13] This postponement could also arise for other reasons not discussed here.

Table 4 OLS regressions for the child's migration probability and motives

	Child is absent in 2007	Motive for migrating			
		Marriage	Studies	Work	Other
	(1)	(2)	(3)	(4)	(5)
Treatment dummy	−0.004	−0.172**	−0.118	0.093	0.150
	(0.042)	(0.081)	(0.122)	(0.152)	(0.107)
Treatment × Age 6–9	−0.020	0.119*	0.083	−0.066	−0.100
	(0.026)	(0.070)	(0.132)	(0.114)	(0.111)
Treatment × Age 10–13	0.011	0.156**	0.072	−0.099	−0.086
	(0.026)	(0.071)	(0.127)	(0.116)	(0.111)
Treatment × Age 14–16	0.015	0.182**	0.089	−0.102	−0.108
	(0.028)	(0.084)	(0.137)	(0.130)	(0.114)
Constant	0.238***	0.195*	0.326**	0.253	0.253***
	(0.059)	(0.114)	(0.125)	(0.190)	(0.082)
Observations	9576	1669	1669	1669	1669
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 characteristics of the child like dummies for ages 6–9, 10–13 and 14–16 (the omitted category is age 0–5), a female dummy, number of siblings, number of male siblings; characteristics of the parent like age, years of schooling, dummies for male and married; and locality fixed effects. Standard errors clustered at the locality level are reported in parentheses

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Another potential confounder is the migration of children. If PO induces treated children to leave the parental household for work, it would be more likely to observe transfers from them to their parents. This behavior would work against our results. The opposite would hold if such effect is negative and our results could not be entirely attributed to the effect of lower repayment. Similarly, as we mention above, if treated children migrate in order to acquire more education, their entry into the labor market may get delayed causing them to have less resources to transfer.

Table 4 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 a migrant questionnaire—the motive for migrating.[14] All estimations control for an early treatment dummy; characteristics of the child;[15] a female dummy; the number of siblings, and the number of male siblings; characteristics of

[14] For this exercise, we use individual level data for the 9576 children 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 1669 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] Dummies for age in 1997 (6–9, 10–13 and 14–16, to be consistent with main estimations in Sect. 2).

the parent;[16] and locality fixed effects. The key independent variables in these regressions, i.e. those measuring the effect of additional exposure to PO are the interactions between the early 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 early treatment dummy on the probability of being absent in 2007, and the interactions of this dummy with the age of the child, are small and not statistically significant (the omitted category are children age 0–5).[17] Thus, our main results in Table 3 cannot be explained by the effect of PO on the children's decision to leave the parental household.

Columns 2–5 show the results of OLS regressions on the motive for migrating for the sample that did migrate. The early treatment dummy by itself has no statistically significant effect on any motive for migrating, except for the negative effect of 17 % on the probability of migrating for marriage. For studies and work, none of the interactions of the early treatment dummy with the age dummies are statistically significant after controlling for the main age effects (not shown). This confirms that children with longer PO 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 key interactions 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 early treatment dummy alone (-0.17), the positive interactions suggest that the effect of being in an early 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.[18] Overall, the results in Table 4 favor our interpretation of the results in Table 3 as evidence of the schooling repayment hypothesis for private transfers.

Finally, to further check the validity of our results, we perform a falsification test. We run the same regressions presented in Table 3, but using data for nonpoor parental households, i.e. those ineligible for PO.[19] 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 2009). Hence, if children transfer money to their parents partly because of repayment, the program has changed the education of noneligible children, but not this motive.

Table 5 shows 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 early 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

[16] Age, education, dummies for male and married.

[17] Angelucci (2005) finds no effect of PO on migration for those children of secondary and high school age who were exposed to the program.

[18] Indeed, the effect of being in an early treated locality on the probability of migrating is not statistically different from zero.

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

not able to reject the null at any conventional level for any of them. These results further suggest that our findings are a consequence of the additional exposure to PO, and not of some other circumstance that occurred in the localities treated early, or to education making the adult children less reliant or concerned about the social networks in their localities of origin.

5 Additional empirical checks

Lastly, 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 6 presents OLS regressions for the logarithms of the value of parental household assets and consumption per capita in 2007.[20] In both estimations, we include the early 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 early treatment dummy, which capture whether the parent anticipated that the PO 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.[21] In both estimations in Table 6, we control for the same characteristics of the parent as in Table 3.

In column 1 of Table 6, neither the effect of the early treatment dummy nor those of the relevant interactions are statistically significant for the log of household assets. We take this as rough evidence of parents not increasing their asset accumulation, despite the fact that the effect of the PO subsidy received would tend to increase asset accumulation.

In column 2, for the log of total expenditure per capita in 2007, the early treatment dummy and the key interactions with age have mostly small and not statistically significant coefficients. Only the interaction for the number of children age 14–16 in 1997 is negative, the largest in absolute value, and marginally significant at 10 %. 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 %. This result is consistent with the interpretation we have been giving to our

[20] Household assets include properties (except agricultural plots), vehicles, agricultural and nonagricultural 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 6 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.

[21] This would hold if the parent is forward looking and used the PO transfers he received when his children were young to accumulate assets in order to use them for consumption purposes in the future, as he (correctly) anticipated not to receive any transfers from his children.

Table 5 OLS regressions for private transfers received in 2007 by nonpoor parental household and heads

	From children (1)	From children who left after 1997 (2)	From children who left before 1997 (3)	From other donors (4)
Panel A: received by the head of household				
Treatment 1998 dummy	−118.365	−111.974	−6.391	−6.394
	(74.555)	(73.456)	(8.729)	(8.162)
Treatment × number of children in the household 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
Panel B: received by the household as a whole				
Treatment 1998 dummy	314.467	248.202	66.265	74.661
	(386.084)	(384.615)	(80.158)	(166.053)
Treatment × number of children in the household 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 who 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, the number of children in different age groups in 1997 and its interactions with the treatment dummy, the number of children in different age groups in 1997 who are absent in 2007, and locality dummies. 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 parental household assets and consumption in 2007

	Log (hh assets) (1)	Log (expcapita) (2)
Treatment 1998 dummy	0.253	0.065
	(0.291)	(0.143)
Treatment × 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, number of children in different age groups, and locality dummies. Standard errors clustered at the locality level are reported in parentheses

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

main results, because the largest reductions in transfers, and the only ones that are significant, are due to the number of early treated children in this same age group. Taken together, the results in Table 6 suggest that parents did not expect the reduction in transfers potentially due to lower repayment. However, this evidence is not conclusive.

6 Conclusions

In this paper, we provide suggestive evidence of a repayment motive for the private transfers that adult children give to their parents by exploiting the features and experimental design of PO, a Mexican antipoverty program that pays a cash transfer to rural parents for sending their children to school. Even though in our data both parents and their (adult) children are still relatively young to be receiving and giving important amounts of transfers, we find that that the number of absent children who were 14–16 years old in 1997 and had longer exposure to the program reduces the amount of private transfers that parents receive from children in 2007.

Our paper contributes to the literature on the motives for private transfers from adult children to parents by providing suggestive evidence on schooling repayment, and to the evidence on the medium-term unintended effects of PO. 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, i.e. the first to study the intergenerational effects of the program. These effects are of utmost importance given that the design and stated goal of PO is precisely to break the intergenerational transmission of poverty. Our results suggest that the effect on intergenerational transfers from children to parents is negative.

Given the relevance of this result, we further provide crude evidence that parents—at the start of PO—did not expect to be receiving less transfers from their children in the future. As a result, they do not seem to have set aside resources via savings or the accumulation of assets in order to use them for consumption purposes once their children started to transfer less money to them. Thus, the first generation of PO 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 compliance is usually higher, after graduating from the program. However, more research seems due given that we are not able to observe other forms of non-monetary support, like caregiving time, and given the other limitations of our data already mentioned.

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