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The Effect of a Transfer Program for the Elderly in Mexico City on Co-Residing Children's School Enrollment*

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Abstract: This paper studies whether the increase in government transfers, induced by an old-age pension program for individuals age 70 and older in Mexico, affects co-residing children's school enrollment, using a regression discontinuity analysis. Results suggest that while household composition and other household-level characteristics do not change significantly at the cutoff age for program eligibility, co-residing children's school enrollment increases significantly. This suggests that public resources for older adults might generate benefits for other age groups. An additional finding is that the increase in school enrollment takes places mostly at the program eligibility cutoff and not before. Given that the program transfer is known and potentially anticipated by individuals who are only a few years away from being eligible, this suggests that households might have credit constraints.
Keywords: Government transfers, school enrollment.
JEL Classification: J14, I25

Resumen: Este artículo estudia si el incremento en las transferencias gubernamentales, inducido por un programa de pensiones de vejez para individuos con 70 y más años de edad en México, afecta la inscripción escolar de los niños co-residentes, usando un análisis de regresión discontinua. Los resultados sugieren que mientras la composición y otras características a nivel de hogar no cambian significativamente en la edad de corte para elegibilidad del programa, la inscripción de los niños co-residentes aumenta significativamente. Esto sugiere que los recursos públicos para los adultos mayores podrían generar beneficios para otros grupos de edad. Un resultado adicional es que el aumento en la inscripción escolar tiene lugar mayormente en el corte de elegibilidad y no antes. Dado que la transferencia del programa es conocida y potencialmente anticipada por los individuos que están a sólo unos pocos años de ser elegibles, esto sugiere que los hogares podrían tener restricciones de crédito.
Palabras Clave: Transferencias gubernamentales, inscripción escolar.

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1 Introduction

This paper estimates the impact of a cash transfer program for individuals age 70 and older in Mexico City on the schooling enrollment of co-residing children. This is relevant for at least two reasons. First, government programs for the elderly that are parallel to traditional social security have recently expanded in Mexico and other developing countries, due to large fraction of individuals that do not qualify for a contributory pension. In addition to investigating the effects on the economic outcomes of the direct beneficiaries of such programs, some studies have shown that public resources paid to the elderly are shared with younger individuals through the crowding out of private transfers (Jensen (2004); Juarez (2009)), reductions in labor supply(Bertrand et al. (2003); Juarez (2010)) and human capital investments(Edmonds (2006); Duflo (2003)). Second, given that eligibility for many of such programs is exclusively conditioned on age, these transfers constitute an anticipated increase in permanent income for individuals who are close to eligibility age. In the absence of liquidity constraints, such individuals should smooth out their investments and consumption over time. As a consequence, estimating whether the timing of benefits matters for outcomes, like the school enrollment of children, could provide evidence of liquidity constraints, as argued by Edmonds (2006), which is relevant from a policy perspective.

We focus on a cash transfer program for the elderly that started in 2001 in the part of Mexico City that belongs to the state of Distrito Federal (DF).[1] The program *Pensión Alimentaria para Adultos Mayores* (Nutrition Transfer for Senior Adults, PAAM henceforth) pays a monthly cash transfer of about 75 USD to all DF residents who are at least 70 years old. This is not conditioned on gender, past labor history or other household and individual characteristics. Furthermore, at the end of 2003, few years after the first implementation of PAAM, the program became part of a state law, thus becoming a more permanent component of the state government social policy. As a result, individuals close to the age eligibility cutoff can reasonably expect to receive the transfer when they reach 70 years old. In addition, the transfer is paid for life, so it constitutes a permanent and anticipated income shock. As argued by Edmonds (2006), estimating the impact of this type of income shocks on school enrollment has advantages over other exogenous sources of variation in income, such as rainfall shocks, which are also likely to affect wages and, as a result, the opportunity cost of attending school.

[1]Mexico City's urban sprawl extends to parts of the state of Mexico.

Our data comes from the Mexican Income and Expenditure Survey (ENIGH, by its Spanish acronym). The ENIGH is a nationally representative cross-sectional survey, which is carried out every two years by the Mexican Institute of Statistics (INEGI). We use the 2004, 2006 and 2008 rounds, collected after the PAAM program became a state law. We restrict our sample to households in DF with at least one member aged 55 or older, and at least one child between the ages of 13 and 18. Our outcome of interest is the school enrollment rates of children age 13 to 18 living in the household. We focus on the schooling enrollment of adolescent children, because enrollment of children age 6 to 12 in primary school is universal in Mexico.

For our empirical strategy, we exploit the age eligibility cutoff in a regression discontinuity (RD) design. First, we present exploratory graphs that validate this RD design and provide a preview of our main findings. Then, we run a series of local linear regressions on either side of the discontinuity to provide a local Wald estimate of the impact of the program. These results are then contrasted with a fully parametric approach. Finally, we carry out a series of robustness checks to support the validity of our main findings.

Other papers have estimated the effect of the introduction of the PAAM program on private transfers and time use (Juarez (2009, 2010); Chavez and Juarez (2010)). To our knowledge, this is the first paper that estimates the program's effects on the school enrollment of co-residing children. Within the development literature, our paper is closest to Edmonds (2006), who exploits the implementation of a similar old-age pension program in South Africa to estimate the impact of anticipated pension income on school enrollment. The author finds significant positive impacts, and interprets them as evidence of liquidity constraints among black elder males.[2]

An important difference between our paper and that of Edmonds (2006), who uses a Regression Discontinuity (RD) design with a flexible polynomial in an OLS approach, is that our estimation technique allows for a more flexible relationship between the age of the eldest household member and the school enrollment of children. Nevertheless, as mentioned before, we also present results from a parametric approach, similar to that of Edmonds (2006), for robustness and comparability. Another difference is that Edmonds (2006) focuses on a sample of rural households, whereas our households are in the largest Mexican city.

[2]For a similar study in the Brazilian context, see de Carvalho Filho (2012). Another study that has looked at the effect of the South African pension on the human capital of children, measured as anthropometric measures, is Duflo (2000).

We find positive and statistically significant impacts of the PAAM pension on the school enrollment of co-residing children. Our preferred estimates range from 20 to 33 percentage points in the fraction of children age 13 to 18 in the household that are enrolled in school. Comparing these figures with the school enrollment rates of same-age children in non-eligible households suggests an increase to universal enrollment among these children. In addition, we provide evidence suggesting that the program decreases the labor force participation of children in this age group. However, these estimates for labor force participation are smaller and slightly less robust than those obtained for school enrollment, which suggest that the increase in the latter is not fully accompanied by a similar decrease in labor supply. When allowing the program impacts to vary with the gender of the child, the increase in enrollment seems more robust for girls than for boys, but both are within the range estimated for the pooled sample. Finally, we find no evidence suggesting that the program effects vary with the gender of the beneficiary.

In summary, our results confirm that the public resources targeted to the elderly are partially reaching other age groups. Thus, the recent expansion in these programs in Mexico could generate large and potentially long-lasting benefits for the younger relatives of beneficiaries. Nevertheless, given that we find an increase in school enrollment mostly right at the age eligibility cutoff, our findings also suggest that households in DF face considerable liquidity constraints that restrict their investment in co-residing children's human capital, despite being in a highly urbanized context. Several explanations for the coexistence of relatively high returns to education and low school enrollment rates in developing countries are that parents (i) may have wrong beliefs about the true returns to schooling (as documented by Jensen (2010); (ii) may not fully internalize their childrens' lifetime utility (as in Munshi and Rosenzweig (2006)); or (iii) they may be liquidity constrained. As in Edmonds (2006), our estimates provide some support for the third explanation and imply that the secondary benefits of this type of programs for the elderly might not be fully realized.

The rest of the paper is presented as follows. Section 2 provides a brief overview of the PAAM program and describes the data used. Section 3 explores a graphical analysis of the data, and specifies the parametric approach to estimating the effect of the transfer. Section 4 presents our results. Section 5 discusses the robustness of the main findings, section 6 explores if the estimated impacts differ by the child's and beneficiary's gender, while Section 7 concludes.

2 The Nutrition Transfer for Senior Adults Program (PAAM)

The PAAM is a state program that pays a cash transfer of about 75 USD per month to individuals who are at least 70 years old and live in the part of Mexico City that belongs to the state of Distrito Federal (Federal District, DF). This transfer, which represents about 30 percent of the mean income for eligible individuals in our data, is conditioned exclusively on age and DF residence, and not on any other individual or household characteristics.[3] Thus, for qualifying individuals, and those who are close to the age cutoff, eligibility for the program is not correlated with their past labor and saving decisions, or with preferences about schooling investments or enrollment of children, at least in the early years of the program.

This program started in early 2001. In the first two years of operation, the program gave priority to households in poor DF neighborhoods. However, by the end of 2003, the program became a state law. As a result, eligibility was extended to all adults age 70 and older, with at least three years of residence in Mexico City, regardless of their individual or household income, and of the neighborhood they lived in. Thus, the program expanded and became a more permanent component of the state social policy.

Our data cover the period after the program became a law, in which take-up rates have been consistently above 90 percent, even in the richest municipalities or *delegaciones*[4]. In 2009, the PAAM cutoff age was lowered to 68, and it has stayed the same since then. However, for the period covered by our data (i.e., survey rounds in 2004, 2006 and 2008), the cutoff age stayed constant at 70 years old.

After the implementation of PAAM in 2001, several Mexican states implemented their own programs, conditioning benefits mostly on age and state residence. On top of these local programs, the federal government started its own in 2007: the *70 y Más* (70 and Older) program. The latter initially covered individuals age 70 and older residing in localities with up to 2,500 inhabitants—the smallest ones in the country—, and was gradually expanded to cover larger, more urban localities. The existence of this federal program does not contaminate our results because, in the period covered by our data, it operated only in areas with up to only 20,000 inhabitants, none of which are in DF.

[3]Although the name of the program suggests that it is aimed at subsidizing food expenditures for older adults, in practice the transfer from the program is paid through a debit card, with no actual restrictions on what the money is spent on. The program also provides free health care services to beneficiaries in the public hospitals run by the state.

[4]DF is divided into sixteen municipalities, called *delegaciones* in Spanish. For basic statistics about the program, see the DF government website: http://www.redangel.df.gob.mx/

3 Empirical Strategy

3.1 Data

We use the Mexican Household Income and Expenditure Survey (ENIGH), which is a cross-sectional, nationally representative survey, collected every two years by the Mexican Institute of Statistics (INEGI). This survey contains detailed information on individuals' sources of income, including government transfers. Children's school enrollment and labor force participation, as well as dwelling and individual sociodemographic characteristics are also reported.

We use the 2004, 2006 and 2008 ENIGH rounds, collected after the PAAM program became a law and was extended to all age-eligible DF residents. To estimate the effect of the program on co-residing children's school enrollment, we use a subsample of households in DF with at least one adult age 55 or older and at least one child between the ages of 13 and 18. As mentioned before, we focus on the schooling enrollment of adolescent children, because enrollment rates of children age 6 to 12 in primary school are close to 100 percent in Mexico. Once we restrict our sample, we are left with a total of 499 households for all three post-program survey rounds. Of these, 221 households (44% of the sample) are observed in 2004, 88 (18% of the sample) in 2006, and the remaining 190 households (38%) correspond to 2008.[5]

3.2 Graphical Analysis

Using the sample described above, we relate our outcomes of interest at the household level with the age of the eldest household member within the household. These outcomes are the log of one plus the government transfers received by the household, the fraction of children age 13 to 18 in the household that are enrolled in school, and the fraction of children in this age group that participate in the labor force. We seek to identify sharp changes in each of these variables when the eldest household member is right above the cutoff age that determines eligibility for the program benefits, with respect to those right below it. Implicitly, we are assuming that, in the absence of the program, the relationship between the age of the eldest household member and each of the outcomes analyzed can be approximated by a

[5]The 2004, 2006 and 2008 surveys contain information on 22,474, 20,774, and 29,122 households, respectively, of which approximately 9% are in DF.

smooth function. Out of 499 observations in our sample, in 338 households are ineligible for the PAAM benefits and 161 are.[6] Our main estimates do not allow the estimated impacts to differ by the gender of the eldest household member, but we explore this relationship in Section 6.

We start by providing graphical evidence of the sharp increase in government transfers received by households when their eldest member becomes eligible for PAAM benefits. Our data do not allow us to identify the specific source -state or federal- or program where these transfers come from, except for Progresa and Procampo.[7] So, we use the sum of other government transfers received by the household, excluding these two programs. Figure 1 plots the smooth relationship between the age of the eldest hosehold member and the log of government transfers, illustrating the idea behind our identification strategy. Some ineligible households declare positive transfers and few households with eligible members declare not receiving any. This might be due to measurement error or incomplete take-up rates. Nonetheless, a steep increase in public transfers starting at the age cutoff can be observed. To illustrate this relationship better, in Figure 2 we present the results obtained from a local linear regression for the log of government transfers, using the lowess method on either side of the age cutoff. As can be seen, government transfers received experience a large upward jump at the age cutoff, and the trends are mostly linear on either side. This exploratory evidence confirms that the PAMM program effectively induces a discontinuity in the public transfers received by a given household when their eldest household member becomes eligible, thus validating our RD design.[8]

We conduct a similar analysis for school enrollment and labor force participation of children age 13 to 18 in the household. Figure 3 shows a sharp increase of about 10 percentage points in children's school enrollment when their eldest household member becomes PAAM-eligible. An obvious concern with this graph is the quadratic nature of the trends on either side of the cutoff, especially given the convexity on the left and the concavity on the right. Nevertheless, this is a first indication that an effect on co-residing children's school enrollment actually exists. Lastly, Figure 4 indicates a decrease of the labor force participation of children at the age cutoff. This graphical evidence suggests that the PAAM program is in-

[6] Note that this is eligibility status, not whether they actually received the PAAM pension. Our data does not allow us to distinguish PAAM benefits from other government transfers. However, the high take-up rates of the program guarantee a good first stage of receiving the PAAM pension on age eligibility.

[7] Progresa and Procampo are among the largest federal cash transfer programs in Mexico, but they target mostly rural households.

[8] An additional concern could be a bunching of observations at or around the cutoff age. The histogram in Figure 5 in the Appendix shows that this is not the case.

creasing school enrollment and decreasing children's LFP. The next subsection presents the empirical strategy we use to estimate these effects more formally.

3.3 Econometric Estimation

To estimate the effect of the program on our outcomes of interest, we first perform a semi-parametric analysis that approximates the smooth relationship between the outcome variables and the age of the eldest household member on either side of the age-eligibility cutoff. As a second step, we validate these estimates by using a parametric approach, somewhat similar to that in Edmonds (2006). We will also use a parametric approach to estimate heterogeneous effects by the recipients' gender. The semi-parametric specification is the following:

$$\begin{aligned} y_{it} &= E_{it}\beta + f(S_{it}^{-}) + g(S_{it}^{+}) + \epsilon_{it} \qquad (1) \\ E_{it} &= 1[A_{it} \geq 71] \\ S_{it}^{-} &= (1 - E_{it})(A_{it} - 71) \\ S_{it}^{+} &= E_{it}(A_{it} - 71) \end{aligned}$$

where A_{it} is the age of the eldest member of household i in survey year t; S_{it}^{-} and S_{it}^{+} denote the distance in years of the eldest household member to the eligibility age cutoff from below and above, respectively; E_{it} is a dummy variable taking value of one if the eldest household member is eligible to receive the program benefits and ϵ_{it} is an error term. We use 71 as the eligibility cutoff, because beneficiaries can apply for the program after their 70th birthday, and thus we would expect some delay in the receipt of benefits due to paperwork. We use a triangle kernel and the optimal bandwidth on each side of the age cutoff according to Imbens and Kalyanaraman (2011), which minimizes the mean square error. We compute standard errors by bootstrapping over 100 repetitions and clustering at the age of the eldest household member. Under this specification, our coefficient of interest is β, which measures the impact in the outcome variable when the eldest household member becomes eligible to receive PAAM benefits.

In this setting, estimates tend to be sensitive to the choice of bandwidth. A smaller bandwidth usually reduces bias, but increases variance. In a fuzzier design like ours, the optimal bandwidth tends to be larger given the variance in the treatment around the discontinuity. We explore how sensitive our estimates are to the choice of bandwidth by reporting results for half and twice the optimal bandwidth. In addition, to confirm our main results, we perform a parametric analysis using OLS.

Sepcifically, we estimate the following equation for household-level outcomes y_{it}:

$$y_{it} \quad = \quad E_{it}\beta + S^{-}_{it}\lambda + S^{+}_{it}\pi + X_i\gamma + \phi_t + u_{it} \tag{2}$$

Where X_{it} is a vector of household characteristics, ϕ_t is a year fixed effect, and u_{it} is an error term, and all other variables are defined as above. Our coefficient of interest in this case is again β, which indicates the impact of having a PAAM-eligible household member on outcome y_{it}. Once again, we cluster standard errors at the age of the eldest household member. We add a quadratic and cubic terms in both of the spread variables S^{-}_{it} and S^{+}_{it}. We abstain from including higher order polynomial terms for these variables. As Gelman and Imbens (2014) point out, regressions based on higher order polynomials are very sensitive to the specific order chosen, and econometric theory provides no specific guidance for computing the optimal order. Furthermore, the confidence intervals on these types of regressions are often misleading in the sense that they exclude zero with a probability higher than the usual type I error rate. Lastly, note that Figures 3 and 4 suggest that a quadratic fit is the most appropriate one in our setting.

To explore whether the estimated impact varies when the eldest household member, i.e. the one turning eligible at the discontinuity is male or female, we use the following OLS specification :

$$y_{it} = E_{it}\beta_1 + G_{it} * E_{it}\beta_2 + S^{-}_{it}\lambda_1 + S^{+}_{it}\pi_1 + G_{it} * (S^{-}_{it}\lambda_2 + S^{+}_{it}\pi_2) + X_i\gamma + \phi_t + u_{it} \tag{3}$$

Where G_{it} is a dummy variable indicating if the eldest household member is female, and all other variables are defined as above. We allow the relationship between the age spread and the outcomes analyzed to vary by the gender of the eldest age member and also present the results of the specification allowing for a quadratic spread on each side of the discontinuity, by gender. All other variables are defined as above. In this case, our coefficients of interest are β_1 and β_2. The first one (β_1) measures the change in the outcome variable when the eldest household member turns eligible to receive the PAAM benefits; the second (β_2) indicates whether such change is different when the eldest household member is female.

4 Results

For our RD design to consistently estimate the causal impact of a household receiving the PAAM transfers on outcomes, no sharp differences should be observed at the age discontinuity for other household characteristics. For example, any changes in household composition ocurring right when the eldest household member starts receiving the program benefits would threaten our identification strategy.

To address this concern, in Table 1 we present descriptive statistics of our outcomes of interest and other control variables capturing household composition and asset holdings Columns 1 and 2 show the mean and standard deviation of these variables by whether the household eldest member is eligible for PAAM or not. We also indicate whether for each of these variables we can reject the hypothesis that the difference in means between eligible and ineligible households is significantly different from zero. As shown in those columns, some mean differences are statistically significant between households on each side of the discontinuity. Eligible households have higher average government transfers, which is expected due to the program. On average, eligible households have a slightly higher fraction of children age 13-18 in school, compared to ineligible households, and a lower force participation rate for these children, but these differences are significant at 10 percent only. Eligible households also have a lower mean labor force participation rate among members age 55 and older and the eldest household member. Regarding household composition, eligible and ineligible households appear significantly different, except notably for the number of co-residing children who are younger than 18. Eligible households also have more rooms and TV sets in their dwellings, compared to ineligible households, but no other mean differences in assets are statistically significant.

It is not surprising that eligible and ineligible households differ in mean characteristics, given that we are using a sample of households with at least one member age 55 years and older. On the contrary, given this relatively broad age window, it is reassuring to find no statistically significant mean differences in the number of coresident children younger than 18 and in most household assets. Nevertheless, it must be stressed that for our RD analysis to correctly identify the impact of eligibility on our outcomes of interest, we must observe no sharp and significant differences in the control variables presented in Table 1 exactly at age eligibility cutoff. To verify this, in column 3 of Table 1 we present the result of running a semi-parametric RD analysis, as specified in Equation 1, including survey round fixed effects and using the optimal bandwidth according to Imbens and Kalyanaraman (2011). Significant differences are observed for the total amount of government transfers received by the household, the school enrollment of children age 13 and 18 and the labor force participation of children, particularly for boys, which are precisely the outcomes that we expect to be affected at the eligibility cutoff. Reassuringly, no significant discontinuities are observed for the household composition or asset holding variables.

We now turn to our main results. Table 2 presents the local Wald estimates for the log of government transfers (Panel A), the fraction of children age 13 to 18 in the household that are enrolled in school (Panel B). and the fraction the children age 13 to 18 in the household that participate in the labor force (Panel C). In column 1, we include no other covariates in the estimation. In column 2, we add survey round fixed effects, and in column 3 we include a set of covariates in a linear fashion. These controls consist of the number of male adults age 55 and over, the number of female adults age 55 and over, the number of boys age 6 to 12, the number of girls age 6 to 12, the number of boys 13 to 18 years old, the number of girls 13 to 18 years old, the gender of the eldest household member,[9] and the number of rooms in the dwelling. Note that, in general, adding covariates to this local Wald estimation procedure tends to improve efficiency by reducing the number of relevant factors that would otherwise be captured in the error term. On the other hand, including these covariates might increase estimation bias if any of them were actually endogenous, i.e. affected by the implementation of the PAAM exactly at the age eligibility cutoff. However, as shown in Table 1, we find no evidence of such potential endogeneity. Finally, it is important to recognize that including these covariates could increase the estimation bias if the implicit assumption of linearity is violated.

For our three outcomes of interest, we present the estimate using the optimal bandwidth from Imbens and Kalyanaraman (2011) first. Then, for completeness and robustness, we also show estimates obtained by using half and twice this optimal bandwidth. In Panel A, the estimates are positive and significant at 1 percent, confirming that PAAM effectively induced a sharp increase in the government transfers received by the household. The estimates in column 3 suggest that the program increased such transfers by 3 to 4 times. For co-residing children's school enrollment, in Panel B we find positive and significant impacts, ranging from 19 to 54 percentage points. The magnitude of these impacts depends on the choice of bandwidth and the inclusion of additional controls, but the statistical significance remains unaffected at

[9]In some instances (10 of our 499 observations), there are two household members who share the same age and are the eldest. We then include a dummy variable taking value of one when this is the case as an additional control). Table 10 in the Appendix shows the distribution of this variable across eligibility groups.

5 percent. Based on the estimates in column 3, which include all covariates and year fixed effects, the impact of the PAAM program on the school enrollment of children age 13 to 18 ranges between 20 and 33 percentage points. Taking the mean enrollment from ineligible households in Table 1 as a base for comparison, these estimates would imply that the program helps reach universal enrollment rates among these children.[10] Finally, in Panel C, we show that the estimated impact of the program on the labor force participation of children in this age group is consistently negative, but the magnitude and statistical significance are more affected by the choice of bandwidth and the inclusion of additional controls. Specifically, in column 3 the estimated reduction in labor force participation ranges from 5 to 12.5 percentage points, but none of these estimates are statistically significant. Note that comparing the impacts in columns 1 and 3 in Panel C, the impacts are not that different in magnitude when controls are either excluded or included, except for when we choose half the optimal bandwidth. However, the loss of statistical significance implies that this evidence is more suggestive than conclusive. While indicative of a potential shift from the labor force into school, the differences in the estimated magnitudes suggests that the impact on school enrollment is not fully driving children out of the work force.

5 Robustness checks

We conduct a series of robustness checks using only the fraction of children age 13 to 18 as a dependent variable. As mentioned before, we compare our local linear regression estimates with those obtained from estimating Equation 2 by OLS. Table 3 shows these results. We present estimations including a linear term in the spreads above and below the cutoff in columns 1 and 4, and also quadratic (columns 2 and 5) and cubic terms (columns 3 and 6) in those spreads. In addition, the first three columns include no controls, whereas the last three include both year fixed effects and household characteristics. These characteristics are the same as those included in the semi-parametric estimations.

Columns 1 and 4 show that including only the linear terms in S^-_{it} and S^+_{it} yields an estimated impact that is small and not statistically significant, regardless of whether other controls are included or not. Including a quadratic term in such spread variables, as in columns 2 and 5, yields an positive, and statistically significant, estimate of about 24-26 percentage points, both with and without additional controls. Finally, columns 3 and 6 show that adding a cubic

[10] This is consistent with what Edmonds (2006) finds for rural household in South Africa.

term in the spreads reduces both the magnitude and statistical significance of the estimated impacts. The results in columns 2 and 5 are our preferred OLS estimates, given our previous discussion in Section 3 about the potential problems caused by the inclusion of higher order terms based on Gelman and Imbens (2014), and the quadratic nature of our exploratory graph in Figure 3. Furthermore, these estimates are within the 20-33 percentage points range of those obtained from our preferred local linear regressions in Table 2.

In Tables 4 to 7 we present additional robustness checks to validate our main findings. First, we perform OLS regressions similar to our preferred ones in Table 3, i.e. those including only linear and quadratic terms in the spread variables, but for observations within tighter windows around the age eligibility cutoff. Second, we present local Wald estimates at eligibiliy cutoffs different from 71. Third, we conduct placebo tests and report both local Wald and OLS estimates for households with at least one member age 55 and older, and at least one co-resident child age 13 to 18, in DF before 2001, the year that the PAAM program first started; and for similar households in municipalities of State of Mexico that are contiguous to DF after 2001. Note that due to the timing and the state residence requirement of PAAM, these two groups of households should not have been affected by the program.

In Table 4, we present the OLS results for tighter windows around the age cutoff. We define three windows: households with the eldest member between the ages of 59 and 83 (± 12 years from the cutoff age), between 66 and 76 (± 5 years), and finally between 68 and 74 (± 3 years). As expected, our number of observations dwindles quickly under these constraints. For each window, we present results with and without year fixed effects and controls. The estimates are all positive, and significant except for the specification that includes no fixed effects nor controls using the 66-76 years old window. For the tightest window of three years around the cutoff age, the estimates are highly significant and - as expected - they are much larger in magnitude than those obtained from the local linear regressions.

In Table 5, we present local Wald estimates obtained by varying the age eligibility cutoff from 66 to 76 years old. Column 6 corresponds to the real cutoff age, whereas the rest of the columns correspond to fake cutoffs. These estimates include year fixed effects, but no other covariates. Once again, we present estimates using the optimal bandwidth, together with half and twice its value. As can be seen, most of the estimates are not significantly different from zero and they vary in sign at different hypothetical cutoff ages. The only exceptions are the estimates for age 67, 68 and 71 cutoffs. For the first one (age 67), the estimate is negative; for the second (age 68) the estimate and positive and significant, but around half of the estimate for the actual age 71 cutoff. So, overall these estimates seem to support that the effect of PAAM pension on school enrollment is indeed positive and significant, and not simply misconstrued from some fortuitous variation in the data.[11]

Exploiting both the regional and temporal variation in the implementation of the PAAM program, Tables 6 and 7 present the same set of semi-parametric and parametric regressions for two groups of households that are similar to our main sample. The first one is composed by similar households in DF, who were surveyed before the PAAM pension was announced. For this group, we consider the 1996, 1998 and 2000 ENIGH rounds. Imposing the same restrictions on our sample as before, we are left with 154 observations.[12] The second group consists of households in the municipalities of State of Mexico that share a border with DF, sampled after the implementation of the program. These households are highly comparable to DF households, except for the fact that they do not reside in DF and, as a result, they are not eligible for PAAM. Our sample of DF neighboring households contains 152 observations.[13]

Table 6 presents the local Wald estimates obtained for these two additional samples. We find no significant jump in co-residing children's school enrollment for households where the eldest member's age is 71, confirming that these samples do not exhibit the discontinuity we found for the post-2001 DF sample. Table 7 shows the OLS estimates for these same groups, first without and then with year fixed effects and additional controls as before. Once again, we find no significant impacts, indicating that the effects that we found for our post-2001 DF sample are due to the introduction of the PAAM pension, and not to some other factor affecting households differentially around that age cutoff.

6 Heterogeneous Effects

In this section, we explore whether the impacts of the PAAM program on the schooling enrollment of children age 13 to 18 differ by the gender of the child and the gender of the eldest household member. For both of these, we rely on OLS estimates.

[11]Figure 8 in the Appendix shows a graphical representation of these estimates using the optimal bandwidth and including the 95% confidence interval to clarify the difference among them.

[12]The breakdown of these observations by year is the following: 64 households in 1996, 68 in 1998, and 22 in the year 2000.

[13]Our sample of municipalities that border DF contains 42 households in 2004, 35 in 2006, and 75 in 2008.

For the first exercise, we redefine our dependent variables as the fraction of boys or girls age 13 to 18 in the household who are enrolled in school, and estimate a specification similar to that in Equation 2. Given that we restrict our sample to households having at least one child, not all households will have both a boy and a girl. Of our 499 original observations, we are left with 275 that have at least one boy, and 292 that have at least one girl. The results are shown in Table 8, both with and without controls and fixed effects. The first two columns show the results for boys, and the last two columns those for girls. For boys, the estimates in columns 1 and 2 are both positive and of similar magnitude, as those obtained for the full sample, but the inclusion of controls and fixed effect erodes their statistical significance. For girls, the estimates in columns 3 and 4 are positive, a bit larger than those obtained for boys, but of the same order of magnitude as those obtained for the full sample. In addition, the estimates for girls are always statistically significant at 5 percent, thus they seem to be more robust to the inclusion of controls than those obtained for boys.

Finally, we explore whether the estimated impacts differ by the gender of the beneficiary. It is important to acknowledge that households in which the eldest household member is male might not necessarily be comparable to those in which that member is female, because of marriage patterns and gender differences in longevity. As a result, gender differences in the estimated impact cannot be solely attributed to the beneficiary's gender, but we present them as suggestive evidence. Table 9 shows the results of estimating Equation 3 by OLS, including increasing controls. The dummy for whether the eldest member is PAAM eligible measures the effect of the program, and the interaction of this variable with a dummy for whether the eldest member is female measures the differential impact when the eligible person is female. As shown in Table 9, the coefficient on eligibility alone is positive and within the range of our main estimates in all columns, but it is statistically significant at 5 percent only in the first three columns. In the fourth column, significance disappears after controlling for a differential quadratic spread on each side of the discontinuity by gender, but the coefficient is similar in magnitude to those on other columns. Throughout specifications, the coefficient of the interaction term measuring the differential impact of the program when the beneficiary is female is small and not statistically significant. Thus, we find no evidence of a differential impact of the program on schooling enrollment by the gender of the beneficiary. Some previous studies find some evidence suggesting that resources in the hands of elderly women have differential impacts than those in the hands of elderly men (Edmonds, 2006; Duflo 2003). However, our data are not ideal to provide a definite answer in this respect. Households in which the eldest household member is male might not necessarily be comparable to those in which such member is female, because of marriage patterns and gender differences in longevity. Our small number of observations do no allow us to appropriately control for these differences, so we take these results as merely suggestive.

7 Conclusion

This paper estimates whether the exogenous income variation induced by a cash transfer program for older adults in Mexico City has an impact on co-residing children's school enrollment. We find a clear positive and significant impact on this outcome, and suggestive evidence of a negative impact on these same children's labor force participation. This is consistent with previous findings showing that public transfers for the elderly are shared with younger individuals through investments in human capital, among other channels. In addition, given that the transfer from the PAAM program induces an anticipated and permanent change in the eligibles' income, we interpret these findings as indirect evidence that these households face liquidity constraints, so that they are unable to smooth out this anticipated positive shock, as Edmonds (2006) does. Our findings imply that the increase in the public resources targeted to the elderly, caused by the recent expansion of both state and federal programs of this type in Mexico, could generate important and potentially long-lasting benefits for their younger coresidents. However, alleviating the liquidity constraints that prevent household from fully exploiting the benefits of this anticipated income shock remains a pending issue from a policy perspective. As in any RD study, an important caveat about the applicability of our findings is that we are focusing on households around the age eligibility cutoff. In addition, it is important to acknowledge that, in the early years of PAAM, those eligible and soon-to-be eligible for the program had already taken most of their lifetime labor and saving decisions. Thus, the long term impacts of these age-based pensions might be different from those estimated in this paper if households and individuals, who are still young enough, change such lifetime decisions as a result of these pensions.

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Table 1

Descriptive Statistics by Eligibility Status and RD Estimates

	Not Eligible (1)	PAAM Eligible (2)	RD Estimate (3)
Log HH Transfers	0.94***	6.43***	2.594***
	(2.46)	(2.91)	(0.798)
School Enrollment and LFP			
Percent Children 13-18 y/o in School	0.75*	0.82*	0.540***
	(0.40)	(0.37)	(0.150)
Percent Males 13-18 y/o in School	0.75	0.83	0.422**
	(0.42)	(0.37)	(0.203)
Percent Females 13-18 y/o in School	0.74	0.8	0.406*
	(0.43)	(0.40)	(0.246)
LFP Children 13-18 y/o	0.15*	0.10*	-0.127*
	(0.34)	(0.28)	(0.0675)
LFP Male Children 13-18 y/o	0.2	0.13	-0.291***
	(0.39)	(0.32)	(0.0453)
LFP Female Children 13-18 y/o	0.12	0.07	-0.198
	(0.32)	(0.26)	(0.236)
LFP Adults 55+ y/o	0.57***	0.13***	-0.152
	(0.46)	(0.28)	(0.162)
Eldest HH Member Works	0.60***	0.10***	-0.166
	(0.49)	(0.30)	(0.142)
Composition of the Household			
Total HH Members	5.37*	5.74*	0.254
	(2.10)	(2.00)	(0.576)
HH Members <13 y/o	0.74	0.74	0.149
	(1.02)	(0.98)	(0.192)

Table 1 (continued)

	Not Eligible (1)	PAAM Eligible (2)	RD Estimate (3)
HH Members 6-12 y/o	0.46	0.48	-0.123
	(0.79)	(0.72)	(0.318)
HH Members 13-18 y/o	1.30	1.30	-0.129
	(0.64)	(0.50)	(0.167)
HH Members 19+ y/o	3.33***	3.70***	0.064
	(1.35)	(1.32)	(0.269)
HH Members 30+ y/o	2.57***	3.19***	0.374
	(0.93)	(1.06)	(0.249)
HH Members 40+ y/o	1.92***	2.57***	-0.046
	(0.65)	(0.99)	(0.090)
HH Members 50+ y/o	1.49**	1.62**	-0.230
	(0.52)	(0.75)	(0.207)
HH Members 55+ y/o	1.26***	1.39***	0.004
	(0.45)	(0.56)	(0.049)
HH Members 60+ y/o	0.70***	1.29***	-0.024
	(0.66)	(0.48)	(0.263)
HH Members 65+ y/o	0.32***	1.22***	-0.166
	(0.52)	(0.41)	(0.136)
Eldest Member Is Female	0.417***	0.578***	-0.037
	(0.027)	(0.039)	(0.091)
Household Assets			
Number of Rooms	4.83***	5.30***	0.463
	(1.85)	(1.72)	(0.391)
TV Sets	1.92*	2.09*	0.265
	(1.05)	(1.03)	(0.245)
Computers	0.42	0.47	0.249

Table 1 (continued)

	Not Eligible (1)	PAAM Eligible (2)	RD Estimate (3)
	(0.58)	(0.59)	(0.283)
Refrigerator	0.96	0.99	-0.0136
	(0.27)	(0.22)	(0.0219)
Blenders	1.04	1.06	-0.0312
	(0.25)	(0.31)	(0.167)
Irons	1.08	1.14	0.188
	(0.37)	(0.45)	(0.158)
Microwave	0.67	0.67	0.280
	(0.51)	(0.52)	(0.382)
Observations	338	161	499

Columns (1) and (2): Standard deviations in brackets. Difference in means test performed (t-statistic not reported, just significance levels).
Column (3): Local linear regressions estimated on both sides of the cutoff age, using a triangle kernel with the optimal bandwidth from Imbens and Kalyanaraman (2011). Estimates include year FE. Bootstrapped standard errors in parentheses (100 repetitions), clustered at the eldest age cell.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 2

Regression Discontinuity: Local Linear Regressions

Panel A: Log Household Transfers			
	(1)	(2)	(3)
Local Wald Estimate (optimal bandwidth)	2.843***	2.594***	3.101***
	(0.826)	(0.798)	(0.918)
Local Wald Estimate (half the optimal bandwidth)	5.316***	5.051***	4.478**
	(1.082)	(1.164)	(2.000)
Local Wald Estimate (twice the optimal bandwidth)	3.544***	3.368***	3.862***
	(0.675)	(0.734)	(0.665)
Panel B: School Enrollment of Children 13-18 y/o			
	(1)	(2)	(3)
Local Wald Estimate (optimal bandwidth)	0.480***	0.540***	0.335**
	(0.119)	(0.150)	(0.136)
Local Wald Estimate (half the optimal bandwidth)	0.333***	0.374***	0.225***
	(0.0974)	(0.106)	(0.0731)
Local Wald Estimate (twice the optimal bandwidth)	0.189**	0.235**	0.200***
	(0.0866)	(0.102)	(0.0727)
Panel C: Labor Force Participation of Children 13-18 y/o			
	(1)	(2)	(3)
Local Wald Estimate (optimal bandwidth)	-0.135***	-0.127*	-0.125
	(0.0468)	(0.0675)	(0.103)
Local Wald Estimate (half the optimal bandwidth)	-0.156***	-0.154**	-0.0641
	(0.0520)	(0.0674)	(0.146)
Local Wald Estimate (twice the optimal bandwidth)	-0.0480	-0.0526	-0.0556
	(0.0573)	(0.0724)	(0.0616)
Year FE	No	Yes	Yes
Controls	No	No	Yes
Observations	499	499	499

Local linear regressions estimated on both sides of the cutoff age, using a triangle kernel.
Optimal bandwidth calculated from Imbens and Kalyanaraman (2011)
Bootstrapped standard errors in parentheses (100 repetitions),
clustered at the eldest age cell.
*** p<0.01, ** p<0.05, * p<0.1

Table 3

OLS Estimates: PAAM Effect on School Enrollment of Children 13-18 y/o

	(1)	(2)	(3)	(4)	(5)	(6)
Eldest Is PAAM-Eligible	0.0234	0.241***	0.197*	0.0631	0.258***	0.145
	(0.0578)	(0.0747)	(0.0973)	(0.0599)	(0.0764)	(0.0875)
Spread below cutoff	0.00311	-0.0464***	-0.00898	-0.0167**	-0.0714***	0.00922
	(0.00443)	(0.0135)	(0.0407)	(0.00674)	(0.0171)	(0.0471)
Spread above cutoff	0.00235	-0.0181	-0.0314	0.00245	-0.0163	-0.0319
	(0.00375)	(0.0112)	(0.0248)	(0.00444)	(0.0116)	(0.0244)
Spread below cutoff squared		-0.00277***	0.00229		-0.00302***	0.00842
		(0.000684)	(0.00575)		(0.000866)	(0.00661)
Spread above cutoff squared		0.00104**	0.00268		0.000968*	0.00290
		(0.000469)	(0.00250)		(0.000519)	(0.00269)
Spread below cutoff cubed			0.000190			0.000432
			(0.000223)			(0.000257)
Spread above cutoff cubed			-4.96e-05			-5.88e-05
			(6.63e-05)			(7.58e-05)
Constant	0.784***	0.625***	0.690***	0.418***	0.247*	0.360**
	(0.0466)	(0.0575)	(0.0751)	(0.121)	(0.122)	(0.145)
Observations	499	499	499	499	499	499
Year FE & Controls	No	No	No	Yes	Yes	Yes
R-squared	0.008	0.023	0.025	0.040	0.054	0.055

Robust standard errors in parentheses, clustered at the eldest age cell.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 4

Robustness Check: OLS Estimates At Tighter Windows Around the Cutoff Age

	Eldest member age 59-83		Eldest member age 66-76		Eldest member age 68-74	
	(1)	(2)	(3)	(4)	(5)	(6)
Eldest Is PAAM-Eligible	0.267***	0.236**	0.233	0.335	0.739***	0.709**
	(0.0906)	(0.0857)	(0.141)	(0.188)	(0.00107)	(0.229)
Spread below cutoff	-0.0448*	-0.0413	-0.0283	-0.110	-0.527***	-0.521
	(0.0247)	(0.0277)	(0.0975)	(0.138)	(0)	(0.271)
Spread above cutoff	-0.0377	-0.0336	-0.0964	-0.101**	-0.278***	-0.178*
	(0.0260)	(0.0275)	(0.0559)	(0.0390)	(0.00213)	(0.0780)
Spread below cutoff squared	-0.00272	-0.000534	-0.00488	-0.0138	-0.120***	-0.104
	(0.00201)	(0.00212)	(0.0163)	(0.0217)	(0)	(0.0659)
Spread above cutoff squared	0.00291	0.00233	0.0177	0.0181**	0.0761***	0.0455
	(0.00220)	(0.00258)	(0.00990)	(0.00714)	(0.000648)	(0.0321)
Constant	0.630***	0.243*	0.692***	0.224	0.260***	-0.256
	(0.0648)	(0.142)	(0.125)	(0.330)	(0)	(0.448)
Year FE & Controls	No	Yes	No	Yes	No	Yes
Observations	363	363	167	167	112	112
R-squared	0.012	0.055	0.028	0.145	0.048	0.184

Robust standard errors in parentheses, clustered at the eldest age cell.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 5

Robustness Check: RD Estimates for School Enrollment at Different Cutoff Ages

	Cutoff=66	Cutoff=67	Cutoff=68	Cutoff=69	Cutoff=70	Cutoff=71
	(1)	(2)	(3)	(4)	(5)	(6)
Local Wald Estimate (optimal bandwidth)	0.0661	-0.154***	0.304***	-0.109	-0.224	0.540***
	(0.1353)	(0.0513)	(0.0943)	(0.129)	(0.267)	(0.150)
Local Wald Estimate (half the optimal bandwidth)	0.0780	-0.138**	0.151***	0.0605	-0.208	0.374***
	(0.0596)	(0.0545)	(0.0164)	(0.0542)	(0.196)	(0.106)
Local Wald Estimate (twice the optimal bandwidth)	-0.00574	-0.0366	0.175***	0.0126	-0.0590	0.235**
	(0.1071)	(0.0506)	(0.0506)	(0.0848)	(0.184)	(0.102)
Observations	499	499	499	499	499	499

	Cutoff=72	Cutoff=73	Cutoff=74	Cutoff=75	Cutoff=76
	(7)	(8)	(9)	(10)	(11)
Local Wald Estimate (optimal bandwidth)	-0.580	0.167	0.151	-0.0189	-0.0908
	(0.431)	(0.515)	(0.193)	(0.109)	(0.163)
Local Wald Estimate (half the optimal bandwidth)	0	0	0	0.0174	-0.0148
	(0)	(0)	(0)	(0.0123)	(0.114)
Local Wald Estimate (twice the optimal bandwidth)	-0.283	-0.0497	0.0670	0.00927	-0.0749
	(0.235)	(0.150)	(0.0942)	(0.0660)	(0.0985)
Observations	499	499	499	499	499

Local linear regressions estimated on both sides of the (hypothetical) cutoff age, using a triangle kernel.
Estimates include year FE, and no other covariates.
Bootstrapped standard errors in parentheses (100 repetitions), clustered at the eldest age cell.
*** p<0.01, ** p<0.05, * p<0.1

Table 6

Robustness Check: Local Linear Regressions for Other Samples

	DF Before 2001 (1)	DF Neighbors After 2001 (2)
Local Wald Estimate (optimal bandwidth)	-0.0925	-0.0974
	(0.281)	(0.338)
Local Wald Estimate (half the optimal bandwidth)	0.249	0.125
	(0.197)	(0.558)
Local Wald Estimate (twice the optimal bandwidth)	-0.191	0.00737
	(0.203)	(0.173)
Observations	154	152

Local linear regressions estimated on both sides of the cutoff age, using a triangle kernel. Estimates include year FE, and no other covariates.
Bootstrapped standard errors in parentheses (100 repetitions), clustered at the eldest age cell.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 7

Robustness Check: OLS Estimates for Other Samples

	DF Before 2001		DF Neighbors After 2001	
	(1)	(2)	(3)	(4)
Eldest Is PAAM-Eligible	-0.142	-0.107	0.110	0.0938
	(0.126)	(0.165)	(0.129)	(0.163)
Spread below cutoff	0.0191	0.0229	-0.0304	-0.0189
	(0.0256)	(0.0468)	(0.0346)	(0.0446)
Spread above cutoff	0.0123	0.00839	-0.0353	-0.0108
	(0.0198)	(0.0260)	(0.0311)	(0.0273)
Spread below cutoff squared	0.000950	0.000292	-0.00256	-0.000837
	(0.00149)	(0.00248)	(0.00218)	(0.00243)
Spread above cutoff squared	-0.000251	-0.000238	0.00272	0.00139
	(0.000776)	(0.00102)	(0.00202)	(0.00172)
Constant	0.818***	1.206***	0.727***	0.262
	(0.0828)	(0.347)	(0.106)	(0.233)
Year FE & Controls	No	Yes	No	Yes
Observations	154	154	152	150
R-squared	0.006	0.127	0.042	0.216

Robust standard errors in parentheses, clustered at the eldest age cell.

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

Table 8

OLS Estimates:
PAAM Effect on School Enrollment of Children 13-18 y/o by Gender

	Boys		Girls	
	(1)	(2)	(3)	(4)
Eldest Is PAAM-Eligible	0.237***	0.164	0.298**	0.384**
	(0.0865)	(0.108)	(0.120)	(0.150)
Spread below cutoff	-0.0553**	-0.0427	-0.0500**	-0.112***
	(0.0230)	(0.0397)	(0.0234)	(0.0339)
Spread above cutoff	-0.0130	-0.0128	-0.0288	-0.0335*
	(0.0173)	(0.0179)	(0.0204)	(0.0191)
Spread below cutoff squared	-0.00338**	-0.00252	-0.00284**	-0.00452***
	(0.00128)	(0.00238)	(0.00119)	(0.00157)
Spread above cutoff squared	0.00104	0.00106	0.00134	0.00185**
	(0.000840)	(0.000815)	(0.000857)	(0.000821)
Constant	0.603***	0.511***	0.587***	0.187
	(0.0662)	(0.178)	(0.0974)	(0.249)
Year FE & Controls	No	Yes	No	Yes
Observations	275	275	292	292
R-squared	0.034	0.135	0.021	0.041

Robust standard errors in parentheses, clustered at the eldest age cell.
*** p<0.01, ** p<0.05, * p<0.1

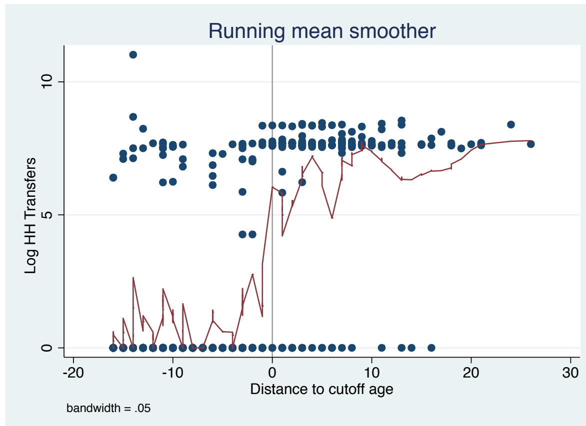
Table 9

OLS Estimates:
PAAM Effect on School Enrollment by Gender of Eldest Household Member

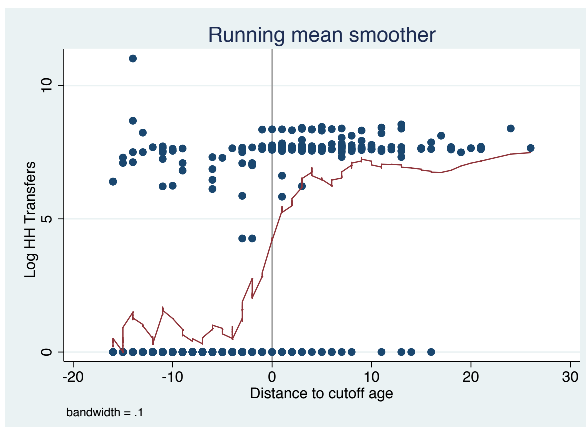
	(1)	(2)	(3)	(4)
Eldest Is PAAM-Eligible	0.272***	0.294***	0.246**	0.200
	(0.0868)	(0.101)	(0.106)	(0.120)
Eldest Is PAAM-Eligible & Female	-0.0442	-0.0616	0.00952	0.0771
	(0.0541)	(0.0804)	(0.108)	(0.145)
Constant	0.625***	0.256*	0.241	0.208
	(0.0576)	(0.127)	(0.173)	(0.175)
Year FE & Controls	No	Yes	Yes	Yes
Differential Linear Spreads	No	No	Yes	Yes
Differential Quadratic Spreads	No	No	No	Yes
Observations	499	498	498	498
R-squared	0.024	0.120	0.122	0.125

All regressions include a linear and quadratic spread above and below the cutoff. Differential linear (quadratic) spreads refers to controlling for the interaction between the variable indicating if the eldest household member is a woman and linear (quadratic) spreads above and below the cutoff age.
Robust standard errors in parentheses, clustered at the eldest age cell.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

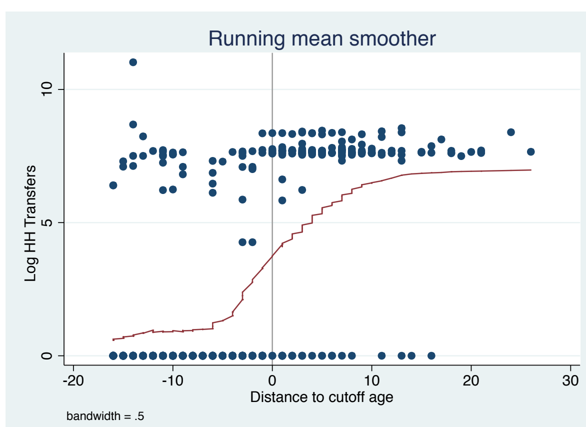
Figure 1: Relationship between Log Transfers and Age of Eldest HH Member



(a) Bandwidth 0.05



(b) Bandwidth 0.1



(c) Bandwidth 0.5

Figure 2: Log Household Transfers (Lowess)

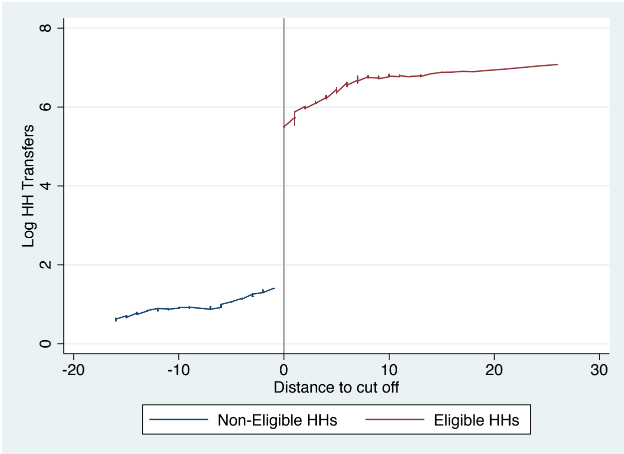


Figure 3: Percent Children 13-18 y/o Enrolled in School (Lowess)

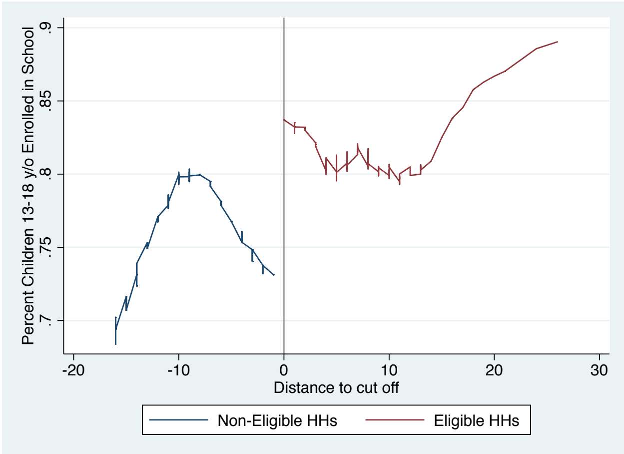


Figure 4: Labor Force Participation Children 13-18 y/o (Lowess)

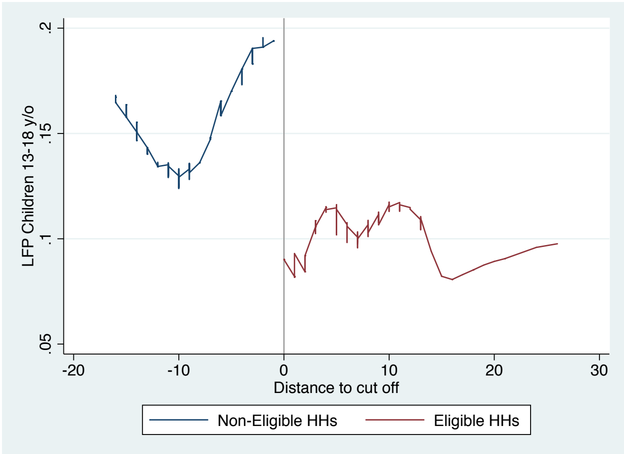
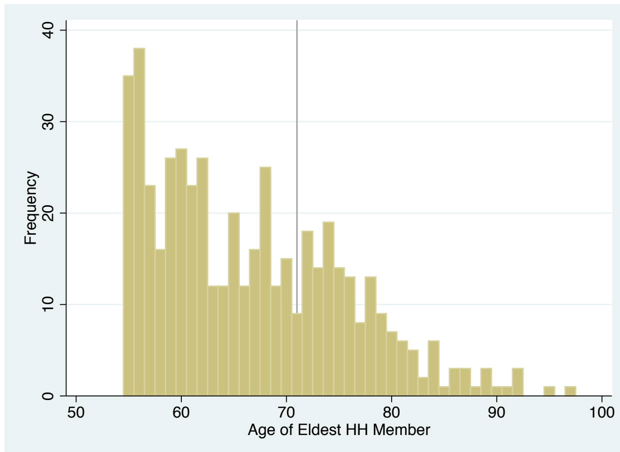


Figure 5: Observations by Age of Eldest HH Member



Appendix

Table 10

Distribution of Sex of the Eldest Household Member by Eligibility Status

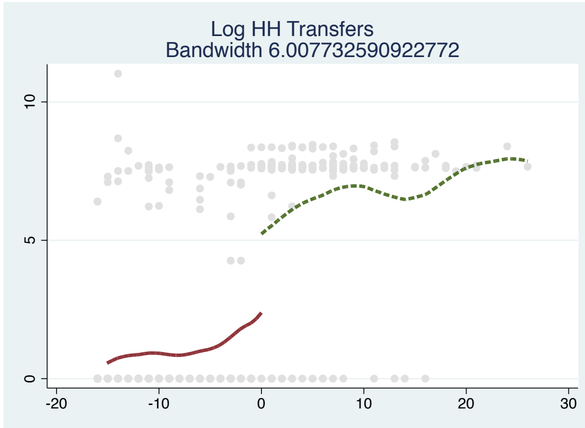
	Not Eligible	PAAM-Eligible	Total
Male	197	68	265
	58%	42%	53%
Female	134	90	224
	40%	56%	45%
Both Male and Female	7	3	10
	2%	2%	2%
Total Observations	338	161	499

Table 11

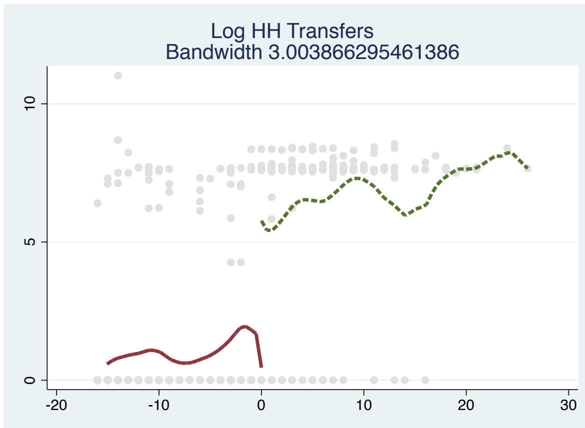
Frequency of Total Number
of PAAM-Eligible Members by Sex of Eldest

Sex of Eldest	PAAM-eligible HH members 0	1	2	Total
Male	197	52	16	265
Female	134	89	1	224
Both Male and Female	7	0	3	10
Total Observations	338	141	20	499

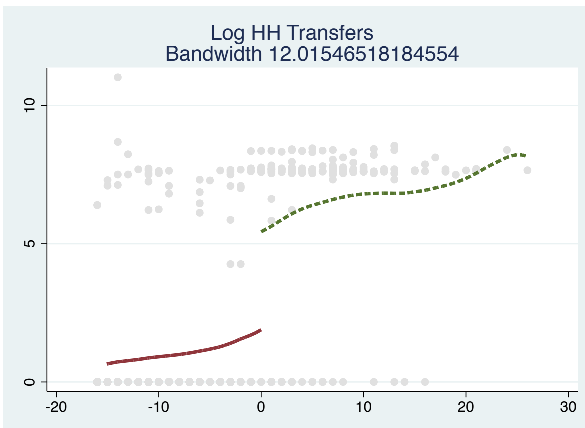
Figure 6: Log Household Transfers



(a) Optimal Bandwidth

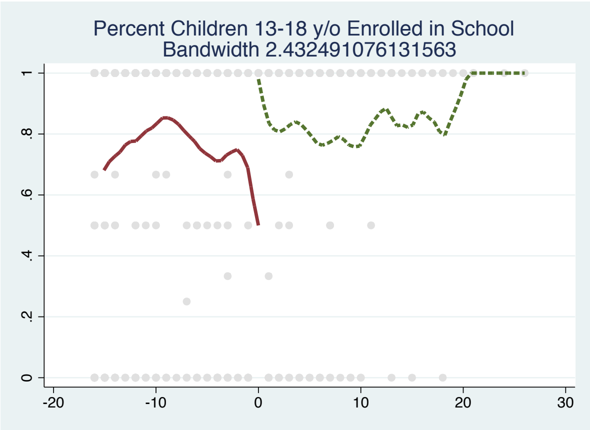


(b) Half the Optimal Bandwidth

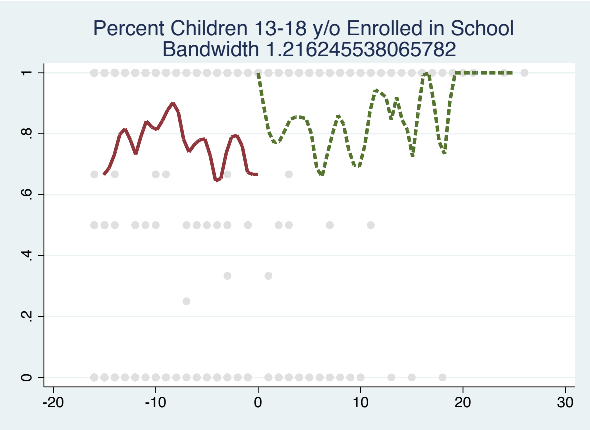


(c) Twice the Optimal Bandwidth

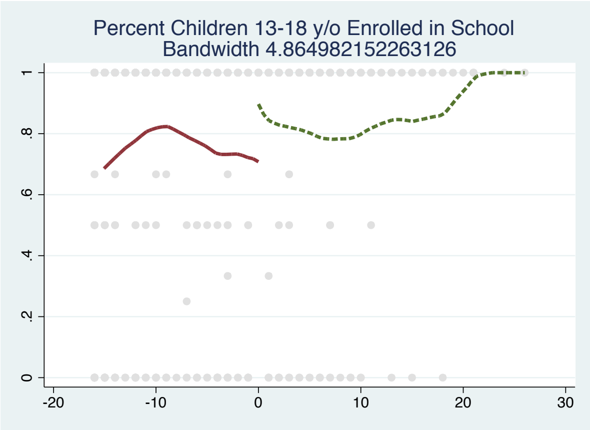
Figure 7: Percent Children 13-18 y/o Enrolled in School



(a) Optimal Bandwidth



(b) Half the Optimal Bandwidth



(c) Twice the Optimal Bandwidth

Figure 8: Local Wald Estimate on School Enrollment for Hypothetical Cutoff Ages