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THESIS

Parental Migration and Schooling Choices. A Study on Children Left Behind in Ecuador.

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Michelle Tello S.

DEDICATION

A mis padres, porque sus decisiones durante mi formación me han puesto en los lugares y momentos apropiados para tener oportunidades y alcanzar diferentes metas.

Michelle Tello S.

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CONTENT

Abstract

This study investigates the role of parental migration on schooling choices of children left behind in Ecuador. Specifically, we use household-level data from the 2006 Encuesta Condiciones de Vida survey to estimate the effects of paternal migration on the probability of attending a paid school and the amount paid for school tuition. We find that children with a migrant father had, on average, 15.0 – 16.6 percentage points higher probability of attending a paid school relative to children with no migrant father. School tuitions were on average, 3.47-3.75 times larger than children with no migrant father. Despite these positive effects, we did not find significant effects on years of schooling and on the gap of years of schooling measured as the difference between the age-appropriate grade for a child and her/his actual grade. Various robustness checks were conducted, the results held up to various specifications and other sensitivity analysis. This is evidence that households with immigrant fathers invest more in their children education but those investment do not necessarily translate into improved schooling outcomes, at least not in the short term.

Keywords: paternal migration, matching estimators, schooling, left-behind children.

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ABBREVIATIONS

PS	Propensity Score
nnmatch	Nearest neighbourhood match
CIA	Conditional indepedence assumption
ATT	Average Treatment on the Treated
RB	Rosenbaum Bounds

1 Introduction

Migration, under the right conditions, is part of the development process particularly for low-middle income countries (United Nations, 2020). Nevertheless, the positive impact of migration remains a subject of debate. On the one hand, some economic literature has emphasize the negative impacts of migration, primarily concerning labor market outcomes in recipient countries (Borjas, 2017; Olivieri et al., 2022; Pedrazzi & Peñaloza-Pacheco, 2023) and even on the countries of migrants' origin (Murakami et al., 2021). On the other hand, a group of evidence has reaffirmed the role of migration on development showing significant positive effects on different outcomes for the population of the origin countries such as a reduction of poverty (Azizi, 2021; Bertoli & Marchetta, 2014) and inequality (Akçay, 2022). Even more, studies have been conducted to show the positive effects on the next generations of immigrants, in the case where the parents migrated with their children (Abramitzky et al., 2021) and also in children that stayed in their home country (Amuedo-Dorantes et al., 2010; Amuedo-Dorantes & Pozo, 2010; Botezat & Pfeiffer, 2020; Datt et al., 2020; Sanchez-Soto, 2017).

Children whose parents have migrated leaving them in the country of origin, are commonly referred in the literature as "children left behind". These children are raised in their home country under the care of close relatives, friends, or even on their own. There is no official data related to how many children are raised under these circumstances (United Nations Children's Fund Unicef, 2018). Moreover, the limited availability of data identifying children in this situation hinders their study.

A particular topic of interest of migration and children in the home country is schooling choices. From a theoretical perspective, parent migration can exert opposing effects on children education. Positive effects might be driven by additional income due to the remittances send during school age. In this regard, a broad literature has looked at the effect of remittances on children living on recipient countries, in some cases finding a positive effect in school enrollment (Bansak & Chezum, 2009; Calero et al., 2009; Stanley & Fleming, 2019), enrollment in private school (Valatheeswaran & Khan, 2018), and school related expenses (Hines & Simpson, 2019; Karki Nepal, 2016).

This paper investigates the effect of parental migration on schooling choices of children left behind in Ecuador during 2000s. We focused on the effect of paternal migration because migration in Ecuador in the period of study was mainly explained by immigrant fathers rather than mothers (Herrera et al., 2005). This implies that in our sample we have a small proportion of children with migrant mothers. The outcomes of interest were the probability of attending a paid school and amount of school tuitions. In addition, we looked at the effects on years of completed education and on the gap of completed years of schooling, defined as the difference between the desired or age-appropriate years of schooling for a child and her/his actual years of schooling (Datt et al., 2020).

This study contributes to the relatively scarce evidence analyzing the impacts of parental migration on human capital formation of children left behind. In this regard, a commonly used approach has been instrumental variables. For instance, Cortes (2015) explored the effect of mother's migration on her children with children with migrant father as a control group in Philippines. The empirical strategy in this study consisted of using the shocks to destination countries' demand for migrants as a credible instrumental variable. The study found that children with a migrant mother are more likely to drop out of school compared to children with a migrant father. In the study from Raut and Tanaka (2018) the predicted probabilities of migration are obtained from a bivariate probit model in the first stage, and these are used as instrument in the second stage. This study analyzed both effects, effects of remittances and effects of parental absence, on educational investment in children left behind in Nepal. The findings revealed that while parental absence has a significant negative effect, remittances have a positive effect on children's education variables including school enrolment and education expenditure.

In the Latin-American region, a more recent study conducted by Fiore (2022) looked at the effect of parental migration on education of children from Mexico. Her study focused on the impact of the timing of parental migration on children's completed years of school, thus, the treatment variable corresponded to: (i) child experienced parental migration when he/she is 6-7 years old, or (ii) child experienced parental migration when he/she is 12-13 years old, since these are the periods when school decisions on entering a new school level must be made. In this case, the empirical strategy to estimate the causal effects consisted in family fixed-effects comparing siblings that experienced parental migration at ages of schooling stages different to 6-7 and 12-13 years old as a control group. The results showed that mother's migration has a significant negative impact on children's schooling and these effects holds at ages 6-7 and 12-13 years old. The effects are stronger as the duration of mother's absence increases.

Regarding paternal migration, evidence is even scarcer. We found two studies that have specifically assessed paternal migration. Both studies were conducted in Mexico. Antman (2011) found that paternal migration reduced child's hours of study and increase his hours of labor, this effect is especially significant for boys between 12-15 years old. A more recent study by Song and Glick (2022) reported a positive effect of log-term father migration on girls' school enrollment. Our study adds evidence in this line of research looking at other educational variables, specifically, schooling choices.

This paper also contributes to understand the consequences of high migration flows experimented in Ecuador during the 2000s. Despite that previous studies have been conducted on this subject in this country (Bertoli & Marchetta, 2014; Calero et al., 2009), to the best of our knowledge, this is the first study dedicated to look at the effects of migration on schooling choices for the case of Ecuadorian children. These findings also add evidence related to the positive effects of remittances and migration on human capital formation in Ecuadorian children reported in previous studies (Antén, 2010; Ponce et al., 2011).

Ecuador experienced an unprecedented wave of international migration in the context of the sociopolitical and economic crises during the last years of 1990s. Between 2000-2003 over 130 000 Ecuadorians migrated each year with the main destinations being USA and Spain (Herrera et al., 2005). By 2005, Ecuadorians represented the largest group of Latin American migrants in Spain numbered almost half a million (Deere & Alvarado, 2016). Previous studies regarding the effects of migration have been conducted with data collected in 2005 (Bertoli & Marchetta, 2014; Calero et al., 2009) and more recently using data from the 2010 Ecuadorian Census (Bucheli et al., 2018). We draw the data for our analysis from the *Encuesta Condiciones de Vida*, ECV, conducted by the INEC in 2005. These data have the advantage of providing detailed information of migrant members including sex, age of migration, year of migration, years of schooling, destination country and if they left sons or daughters under the age 18.

The main issue when estimating effects of migration is the self-selection problem. We rely on propensity score (PS) matching and obtained two types of estimates including bias-adjusted matching (Abadie & Imbens, 2006) and blocking estimator (Imbens, 2014). This strategy was preferred since we did not have panel data available to evaluate children's outcomes before and after his/her father migrated. Furthermore, we lack data to construct a credible instrument as previous studies (Antén, 2010; Ponce et al., 2011). In addition, the PS and other matching approaches have been used in previous similar studies related to the effects of migration on different outcomes (Bertoli & Marchetta, 2014; Cox-Edwards & Rodríguez-Oreggia, 2009; Dey, 2015; Jimenez-Soto & Brown, 2012; Zhou et al., 2014) and recently have also been used to analyze schooling outcomes related to different treatments (Courtney et al., 2023; Hernandez et al., 2022; Sondergeld et al., 2020). We obtained the results with different specifications of the PS to test its sensitivity. Additional robustness checks were conducted using the baseline covariates as pseudo-outcomes and we also used paternal absence not related to migration as a pseudo treatment. Ronsenbaum bounds were obtained as additional evidence of uncounfoundedness. The results of these robustness checks confirmed the validity of the identification strategy and the significant positive effect on the child's likelihood of attending a paid school and on the amount of school tuition. The absence of significant effects on years of schooling and on the completed years of schooling gap were also confirmed.

The remainder of the paper is organized as follows. The next section describes the data used in the study. The third section is dedicated to describing the empirical approach and methods. The results are presented in the fourth section. Finally, concluding remarks are derived from the analysis in section five.

2 Data description

We use data from the survey "Encuesta Condiciones de Vida" (ECV). The fifth wave of this survey was carried out during 2006. Five datasets are available in this survey, we use the datasets *e5rEMLA* which records information related to migrants and the dataset *e5rPER* which registered detailed information of every surveyed household member. The dataset *e5rEMLA* counts with 1024 observations, however, not all cases correspond to migrants who left children behind. Thus, we filter this database based on the item “Did the immigrant household member leave sons or daughters with 18 or less years old?”. A total of 461 respondents said “yes”. From these 461 participants, 249 observations were reported to be men/fathers and 212 were women/mothers. Among these cases, 148 fathers were head of household during their time living in the country of origin. These observations were used to carry out the matching procedure.

In addition, we only included cases of children living with their mother as head of household, for the following reasons: (i) an accurate matching procedure was only feasible for these cases since we need to know the characteristics of the husband (age, years of schooling), (ii) the proportion of migrant mothers identify as head of household was low (12.26%), this also means that the household structure of the children with a migrant where more diverse.

Our final treated group correspond to children living with their mother at head of household with ages between 6 to 19 years old. A total of 194 cases were matched with children characteristics. The estimated proportion for cases of children out of the age range 6-19 and living with other relatives as head of household was 22.78%. In addition, the estimated proportion of cases without match was 19.82%. This means that, after conducting the matching procedure (described in Appendix C) our retained rate was 80.17%.

A final data base was constructed where each observation is the child i in the household h with information regarding: (i) his/her socio-demographic characteristics (e.g. age, schooling, gender), (ii) information about the household (e.g. members, income, among other), and (iii) characteristics of their parents (e.g. mother schooling). Moreover, given that children with ages less than six years old did not report years of schooling, we only include observations with ages more than six years old and less than 19 years old. The final data set is composed of 8 742 observations, 194 correspond to children with migrant fathers and 8 558 observations correspond with children without migrant fathers. This allows us to have a large pool of potential matches in the control group, making feasible the matching procedure (Peikes et al., 2008).

Our main outcome of interest corresponded to schooling choices variables. Specifically, two outcome variables related to schooling choices were analyzed: (i) a dummy indicating whether the child attend a paid school, (ii) the amount of money in USD paid on school tuitions. In addition, we looked at two schooling outcomes: (i) years of schooling and, (ii) the gap of completed years of schooling, defined as the difference between the desired or age-appropriate years of schooling for a child and her/his actual years of schooling (Datt et al., 2020). Table B.1 presents a detailed description of all study variables. Summary statistics of all outcome variables and covariates are reported in Table 1.

Table 1 *Summary Statistics*

	Mean / %
(a) Child's characteristics	
Female	48.18%
Age	11.78 (3.84)
(b) Household and household members characteristics	
Rural	41.66%
Owns land	5.45%
Owns house	53.10%
Monthly income	272.47 (234.12)
Years of schooling household members	7.88 (4.07)
Ratio working age members	0.450 (0.148)
Ratio female members	0.493 (0.167)
Mother years of schooling	7.34 (4.72)
(c) Child's education outcomes	
Attends to paid school	19.34%
Years of schooling	5.64 (3.13)
Difference of years of schooling	0.084 (1.68)
School tuitions	75.65 (200.17)

Note. Standard deviation in parenthesis.

3 Empirical Approach

The general setup for estimating the effects of father migration on the children schooling outcomes can be described as the standard one in the literature of potential outcome framework. Let $W_i = 1$ if a child has at migrant father and $W_i = 0$ otherwise. Then, define the outcome (e.g. years of schooling, attainment a paid school, school tuition) for the children left behind as $Y_i(1)$ and the outcome for children that does not have a migrant father $Y_i(0)$. The target average treatment effect on the treated ATT (i.e., the effect of having migrant parent in those children whose parent migrated) will be τ_{treat} and this is given by:

$$\tau_{treat} = \mathbb{E}[Y_i(1) - Y_i(0)|W_i = 1] = \mathbb{E}(Y_i(1)|W_i = 1) - \mathbb{E}(Y_i(0)|W_i = 1)$$

Given that we observe $\mathbb{E}(Y_i(1)|W = 1)$, but we do not observe $\mathbb{E}(Y_i(0)|W = 1)$ (i.e., the effect on schooling of children whose father did not migrate in the case he had migrated) a robust empirical strategy is required to obtain a credible estimator. To achieve this, we resort to bias-adjusted matching estimator and blocking estimator with trimming. These estimators are based on two key assumptions *unconfoundedness* and *overlap*. These assumptions tell us the average causal effects can be estimated by adjusting for difference in covariates in the treatment and control units. The *unconfoundedness* assumption implies that $\mathbb{E}(Y_i(0)|X_i, W_i = 1) = \mathbb{E}(Y_i(0)|X_i, W_i = 0)$, thus the ATT can be obtained as

$$\tau_{treat} = \mathbb{E}[\mathbb{E}(Y_i(1)|X_i, W_i = 1) - \mathbb{E}(Y_i(0)|X_i, W_i = 0)|W_i = 1]$$

And this can be redefined in terms of the propensity-score considering the theorem proposed by Rosenbaum and Rubins (1983). In this case the ATT can be estimated as an iterative averaging procedure written as

$$\tau_{treat} = \mathbb{E}[\mathbb{E}(Y_i(1)|e(X_i), W_i = 1) - \mathbb{E}(Y_i(0)|e(X_i), W_i = 0)|W_i = 1]$$

Where $e(X_i)$ is the propensity score i.e.
$e(X_i) = \mathbb{E}(W_i|X_i = x) = \Pr(W_i = 1|X_i = x)$.

To ensure *overlap* we resort to trimming as suggested in previous literature (Crump et al., 2006; Crump et al., 2009; Imbens, 2014; Stürmer et al., 2021). We take advantage of the numerous control observations to drop units with values of the covariates such that they have no counterparts in the treatment group (Imbens, 2014). This procedure allows us to improve overlap in the covariates, usually disregarding observations on the treatment and control group based on the predictions of the propensity score (PS) obtained by a logit or probit regression with the set of covariates as independent variables of the treatment status (e.g. having a migrant father). It is important to notice that the trimming method alters the population reference points, which means that the estimator loses to some extent external validity but internal validity may be improved with more credible and accurate causal effects than in the original full sample (Imbens & Rubin, 2015).

We follow to some extent the strategy given by Imbens (2014), which can be summarize as follows: (i) estimating the PS, (ii) trimming the data based on extreme values of the PS, (iv) assessing common support and balance, (iv) obtaining bias-adjusted matching and blocking estimators with the trimmed sample (Y^T, W^T, X^T), (v) sensitivity and *unconfoundedness* assessment. Additionally, we conducted a sensitivity analysis using the bounding approach (Caliendo & Kopeinig, 2005).

The matching estimator and its corresponding standard errors were obtained according to Abadie and Imbens (2006). The blocking estimator correspond to the one on Imbens (2014), this type of estimator can be described as a procedure of *blocking with the regression*. Its calculation is relatively simple. Using the estimated PS, the covariates and the treatment indicator, a series of linear regressions are conducted in different partitions of the data based on the range of the PS. After these procedure J estimates of the ATT are obtained, one for each partition, these ATT are then averaged using the proportion of units in each block as weights.

In addition, since current functions implemented to calculate the match and blocking estimator do not calculate clustered robust standard errors, we conducted a post estimation with the matched sample to obtained clustered errors. Recent literature (Abadie & Spiess, 2022) has been dedicated to show valid clustered level standard errors for the case of matched sample through least square estimators, concluding that this type of estimates are valid when matching is done without replacement. Thus, we obtained clustered standard errors through linear regression using the matched sample without replacement. The standard errors were cluster at household level to account for the potential effects on children living with siblings[1].

4 Empirical Results

4.1 Propensity score matching

Table 2 reports logit estimates for five models to obtain the PS for the treatment of having a migrant father. The set of appropriate covariates to be included in X correspond to: (i) baseline covariates, (ii) covariates associated with the treatment status, (iii) covariates that affect the outcome, (iv) both, covariates that affect the treatment status and the outcome; in contrast, post-baseline covariates that may be influenced or modified by the treatment should not be included (Austin, 2011). The first specifications, Model (1), considers all available covariates based on theory and previous studies (Bertoli & Marchetta, 2014; Deng & Law, 2020; Jimenez-Soto & Brown, 2012; Tran et al., 2012). Model (2) considers a reduced number of covariates, after evaluating overlapping between different specifications. Model (3) only include significant covariates. Model (4) adds significant second order terms.

These series of estimations were conducted to assess the sensitivity of the propensity score. Following Imbens (2014) the correlation of the log odds obtained from the four specifications are reported in Table B.2 in Appendixes. All Models reported a correlation were close to one (>0.70), suggesting that the distributions of their log odds did not differ substantially.

4.2 Common support and balance

To achieve common support and balance we trimmed the sample omitting observations with extreme values of the PS obtained in the estimation of Model (2) which showed a better overlap and retained more observations. The threshold to drop observations with extreme values was defined as $\alpha = 0.10$ which is the recommended practice suggested by the study simulations on Crump et al. (2009). This procedure leaves us with a total sample of n = 412, 104 observations in the treated group and 308 in the control group.

[1] An additional estimation was conducted using a control dummy variable indicating if more than one child lives in the household. This estimation is shown in Table B5.

Table 2 *Logit Estimations for the Prediction of the Propensity Score*

	Model (1)	**Model (2)**	**Model (3)**	**Model (4)**
rural	-0.353 (0.237)	-0.087 (0.221)		-0.476 (0.248)
ratio working age members	-7.42*** (0.945)	-6.32*** (0.804)	-6.21*** (0.784)	-26.50*** (2.86)
ratio female	6.59*** (0.678)	5.31*** (0.571)	5.25*** (0.568)	6.47** (0.698)
college	0.267 (0.483)	-0.210 (0.442)		0.100 (0.483)
years schooling	0.277*** (0.072)	0.078 (0.050)		0.326*** (0.071)
owns land	1.25** (0.393)	0.305 (0.391)		1.34** (0.395)
owns house	1.12*** (0.201)	0.657*** (0.179)	0.614*** (0.174)	1.08*** (0.204)
child age	0.115*** (0.029)	-0.026 (0.024)		0.097** (0.030)
child female	-1.24*** (0.215)	-0.965** (0.197)	-0.962** (0.196)	-1.23*** (0.220)
mother schooling	-0.070 (0.049)	0.074 (0.043)	0.140*** (0.021)	-0.084 (0.049)
household income	-0.005*** (0.000)	-0.004*** (0.000)	-0.004*** (0.001)	-0.005*** (0.001)
father age	-0.162*** (0.017)			-0.173*** (0.018)
father schooling	-0.472* (0.227)			-0.529* (0.225)
ratio working age members square				21.73*** (2.84)
X2	595.86	789.57	590.98	834.67
Pseudo-R2	0.332	0.447	0.330	0.473

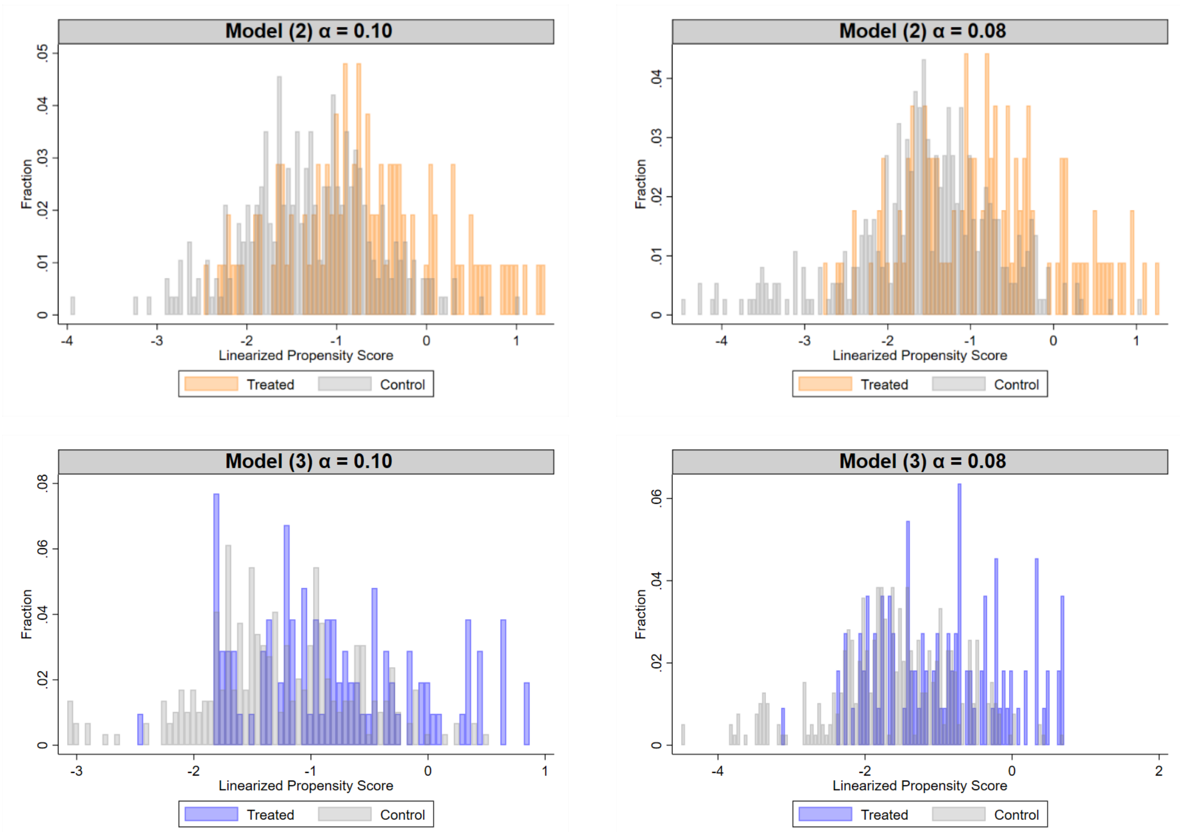
Note. Models (1)-(4) have province dummy controls. Standard errors in parentheses. *** $p < .001$, ** $p < .01$, * $p < .05$

To show evidence of sufficient overlap between the treated and control group, Figure 1 reports the distribution of the estimated PS when modeling with specifications of Models (2) and Model (3)[2] for trimmed data considering $\alpha = 0.10$ and $\alpha = 0.08$. The graphed variable is

[2] The PS distribution for Models (4) is shown in Figure A 1 in Appendixes. This achieved less overlap between the groups.

the linearized propensity score equal to $\ln[p(X_i)/(1-p(X_i))]$. Better overlapping of the distributions is achieved with $\alpha = 0.10$. The further analysis was carried out with specification Model (2)[3] and $\alpha = 0.10$.

Figure 1 Distribution of the Propensity Score based on Different Specifications and α Thresholds.



Note. This graph shows the distribution of the estimated linearized propensity score obtained from Models (2)-(3). The linearized PS was calculated as $\ln[p(X_i)/(1-p(X_i))]$. The treated group corresponds to children with migrant father. The control group correspond to children living with both parents.

Next, we calculated the normalized differences in the full sample and the trimmed sample as additional insights about the improved balance between the samples of the treatment and control group achieved with the trimming procedure. The trimmed sample shows in general less differences across variables between the groups. Only two variables (working age

[3] Despite that with $\alpha = 0.10$ the sample is smaller; later we show that the results did not differ when using $\alpha = 0.08$ in Table B4 in Appendixes.

members and female share) reported differences greater than 0.25, which is the value considered as not suitable for performing regression adjustment (Imbens & Rubin, 2015). However, these differences are further eliminated in the matched sample as shown in Table 3.

Additionally, we perform the joint significance and Pseudo-R2 test as suggested in previous studies (Sianesi, 2004). In this test the same logistic regression is estimated in the matched sample, evidence of sufficient overlapping is given by a fairly low pseudo-R2 and a non-significant F-test (Caliendo & Kopeinig, 2005). Table B.3 in Appendixes shows the results for this regression which yielded a pseudo-R2 of 0.023 and a non-signifi.cant F test with $p = 0.659$.

Table 3 *Standardized Differences in the Full Sample, Trimmed Sample and Matched Sample*

	Raw Full sample	**Trimmed sample**	**Matched with trimmed sample**
rural	-0.212** (2.86)	-0.235** (2.43)	-0.186 (1.55)
working age members	0.691*** (9.37)	0.249** (2.61)	-0.175 (1.45)
female share	-0.677*** (8.69)	-0.070 (0.758)	-0.098 (0.823)
college	0.058 (0.875)	0.260** (2.89)	0.202 (1.96)
years schooling	0.006 (0.075)	0.122 (1.26)	0.165 (1.37)
owns land	-0.022 (0.309)	0.017 (0.244)	0.004 (0.019)
owns house	0.11 (1.53)	0.025 (0.277)	0.065 (0539)
child age	0.138 (1.91)	0.056 (0.606)	0.005 (0.046)
child female	0.031 (0.441)	0.015 (0.148)	-0.041 (0.335)
mother schooling	-0.026 (0.363)	0.119 (1.26)	0.203 (1.71)
household income	0.727*** (11.27)	0.119 (1.23)	0.050 (0.403)

Note. Absolute value of t-test in parentheses. ***$p < 0.001$, **$p < 0.01$, *$p < 0.05$

4.3 Effects on schooling choices

Table 4 shows the results for the ATT effects for the outcome variables related to schooling choices. Panel A reports the estimates with the full sample and Panel B reports the estimates for the trimmed sample. We report the coefficients for the adjusted matching estimates and blocking estimators. We found significant effects on attending a paid school and on the amount of school tuition. Based on the adjusted estimates for the matching and blocking estimators for the trimmed sample, which are our preferred estimates, the results show that having a migrant father increases the likelihood of attending a paid school by 15% to 16.6%. Moreover, school tuitions for children with migrant fathers could be notably larger, ranging from 3.47-3.75 times larger compare to those of children without migrant fathers.

4.4 Effects on schooling outcomes

The results revealed non-significant effects on child's schooling outcomes. For the case of years of schooling, the estimates are close to zero and even negative in the bias-adjusted matching estimator. In the case of the variables related to the gap of education years, the estimates go from positive estimates in the full sample, to negative and close to zero estimates in the trimmed sample. In general, the coefficients' values varied significantly between samples and estimators for the case of schooling outcomes.

4.5 Robustness checks

The robustness checks involved two sections. First, we tested the sensitivity of the propensity score with the different especifications presented in Table 2. This procedure is reported for all the estimates in Section 4.4.1. Second, the CIA was tested through different approaches including, a pseudo-treatment or placebo, pseudo-outcomes and Rosenbaum bounds approach. The implementation for these tests and their results are presented in Section 4.4.2.

Table 4 *Estimation Results of ATT Effects on Schooling Choices and Schooling Outcomes*

		(a) Paid school			**(d) ln (school tuition)**			**(b) ln(years of schooling)**			**(c) Gap on years schooling**		
		Match	**Block**	**Cluster**	**Match**	**Block**	**Cluster**	**Match**	**Block**	**Cluster**	**Match**	**Blocking**	**Cluster**
a. Full sample	Coeff.	0.195***	0.159***	0.184**	1.299***	1.08***	0.896**	-0.033	0.025	0.013	0.069	0.166	0.020
	s.e.	0.036	0.031	0.061	0.262	0.21	0.318	0.034	0.039	0.035	0.136	0.139	0.188
	T = 0	319	7260	7260	346	8550	8550	339	8291	8291	346	8557	133
	T = 1	169	169	151	185	185	171	181	181	166	185	185	185
	Blocks		9			7			7			7	
b. Trimmed sample	Coeff.	0.150**	0.166***	0.144+	1.06**	0.906**	1.298*	-0.077	0.025	0.015	0.003	0.063	0.001
	s.e.	0.05	0.045	0.085	0.356	0.305	0.517	0.055	0.055	0.068	0.209	0.195	0.281
	T = 0	119	265	58	139	308	67	136	299	65	139	308	67
	T = 1	94	94	94	104	104	104	102	102	102	185	104	104
	Blocks		4			3			4			3	

Note. Coefficients correspond to the bias-adjusted matching and blocking average treatment on the treated (ATT) estimates. Dependent variables are: (a) dummy variable that indicates if the child goes to a paid school, (b) natural logarithm of school tuitions, (c) natural logarithm of child's years of schooling, (d) difference between the desired or age-appropriate years of schooling for a child and her/his actual years of schooling. Estimations include all variables used to construct the propensity score and province dummies. The matching estimator correspond to the bias adjusted estimator with a max of three matches implemented with nnmatch command in Stata. The matching estimator was implemented using nnmatch function in Stata by Abadie et al., (2004). The blocking estimator was implemented with the psreg function in Stata by Bazzoli et al. (2020). The trimmed sample considered the threshold of alpha = 0.10 for the PS estimated by Model (2). The standard errors (s.e) correspond to robust s.e. Obs = number of observations. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

4.5.1 Sensitivity analysis

Since the covariates selection to obtain the PS, which is further used in the regression adjustment, is a key step in the trimming and hence in the matching process, we tested the sensitivity of the results to the different specifications. According to Caliendo and Kopeinig (2005), previous literature has proposed arguments in favor and against of including a broad set of covariates in the PS estimation. On the one hand, some authors suggested that including a numerous covariates, especially in small samples, might cause higher variance due to over-parametrization, thus it is better to estimate the PS discarding covariates that appear not to or weakly influence the treatment (Bryson et al., 2002). On the other hand, other authors considering that discarding variables without enough justification is not appropriate, thus if there are doubts about whether to include or not a covariate, it is advisable to include the relevant variables in the propensity score estimation based on theory even if there is no statistical significance (Rubin & Thomas, 1996). Taking into account both arguments, we estimated the ATT using Model (3) with a reduced number of covariates and using Model (4) with a broader set of covariates considering second terms as suggested in some studies (Benedetto et al., 2018; Dehejia & Wahba, 1999; Imbens, 2014).

Table 5 reports the ATT considering the Model (3) and Model (4) specifications of the PS. All estimates remained significant. Moreover, the range of estimates for the outcome attending to paid school in the preferred specification Model (2) was [0.150 – 0.166] with a width of 0.016. In the case of the alternative specifications, the range for the PS estimator with Model (3) was [0.131 – 0.141] with width of 0.010 and the Model (4) range estimates was notably wider [0.103 – 0.258] with a wider difference of 0.155. For the outcome variable of school tuition, the estimates also remained relatively stable. The preferred specification for the PS Model (2) had an estimate range of [0.906 – 1.01] with a width of 0.104 and for the alternative specifications Model (3) and Model (4) the ranges were [0.804 – 1.01] and [0.885 – 0.962], respectively. It is worth noticing that the preferred specification, Model (2), produces the ranges with small differences in both cases, in contrast with Model (3) and Model (4) which show wide intervals in school tuition and paid school, respectively.

Table 5 *Estimation Results of ATT Effects on Education Outcome Variables based on Alternative Specifications for the PS.*

		(a) Paid school		**(b) ln (school tuition)**		**(b) ln(years of schooling)**		**(c) Gap on years schooling**	
		Matching	**Blocking**	**Matching**	**Blocking**	**Matching**	**Blocking**	**Matching**	**Blocking**
a. Model (3)	Coeff.	0.141**	0.131**	1.01**	0.804*	-0.073	-0.001	0.255	0.032
	s.e.	0.053	0.047	0.347	0.332	0.052	0.098	0.233	0.204
	Obs T = 0	116	261	134	310	130	379	109	310
	Obs T = 1	92	92	104	104	102	112	163	104
	Blocks		4		4		6		4
b. Model (4)	Coeff.	0.165*	0.187***	0.812*	1.71*	-0.095	-0.081	0187	0.830
	s.e.	0.056	0.078	0.390	0.876	0.056	0.144	0.215	0.828
	Obs T = 0	145	233	167	270	262	262	270	270
	Obs T = 1	103	107	114	115	112	112	115	115
	Blocks		4		4		4		4

Note. Coefficients correspond to the bias-adjusted matching and blocking ATT estimates. Dependent variables are: Dependent variables are: (a) dummy variable that indicates if the child goes to a paid school, (b) natural logarithm of school tuitions, (c) natural logarithm of child's years of schooling, (d) difference between the desired or age-appropriate years of schooling for a child and her/his actual years of schooling. Estimations include all variables used to construct the propensity score and province dummies. Model (3) considers only significant covariates to estimate the PS. Model (4) includes higher orden terms. The trimmed sample considered the thresshold of alpha = 0.10. The standard errors (s.e) correspond to robust s.e. ***$p < 0.001$, **$p<0.01$, *$p<0.05$

4.5.2 Unconfoundedness

In this section, we undertake three standard robustness checks to assess the plausibility of the CIA. The initial test involved the examination of pseudo-outcomes. The pseudo-outcome variables were chosen from the set of covariates used to estimate the PS[4] (Imbens, 2014). The intuition of this approach is that the treatment should not affect the baseline covariates. In this case, we estimate the effects for three pseudo-outcomes: (i) college dummy, was one of the variables were we found the higher normalized differences, and (ii) household income, because despite literature that supports is used in the PS (Deng & Law, 2020; Tran et al., 2012), it is still arguably that this can be influenced by migration even without considering the remittances. Table 6 shows the ATT for these two pseudo-outcomes, in all cases the estimates are non-significant, and its value varies substantially between the matching and blocking estimators.

Table 6 *Estimation Results of ATT Effects Using Pseudo-outcomes.*

		Matching	**Blocking**
b. College dummy	Coeff.	-0.014	-0.005
	s.e.	0.010	0.015
	Obs T = 0	143	313
	Obs T = 1	103	103
	Blocks		4
c. Household income pre remittances	Coeff.	0.1241	-0.535
	s.e.	0.501	0.311
	Obs T = 0	135	363
	Obs T = 1	103	103
	Blocks		4

Note. Coefficients correspond to the bias-adjusted matching and blocking ATT estimates. Estimations include all variables used to construct the propensity score and province dummies. The trimmed sample considered the thresshold of alpha = 0.10. The standard errors (s.e) correspond to robust s.e. ****p* < 0.001, ***p*<0.01, **p*<0.05

[4]Since now college dummy and household income are not part of the covariates, we re-do the entire analysis, including estimating the PS, trimming the sample and re-estimating the PS to obtain the blocking and matching estimators. Figure A.2 shows the distributions of the PS obtained from this procedure; an acceptable overlap was achieved in both cases.

Next, we used this same approach but this time using a pseudo-treatment. For this analysis, we identified a group of children whose fathers are not residing in the household, yet they are not migrants, and consider them as the treatment group. As this group has not experienced the actual treatment, which is the absence of the father due to migration, we anticipate that the results for the schooling choices and school outcomes variables will not be statistically significant. For this case, we also trimmed the sample to achieve overlapping, Figure A.3 shows the linearized PS graphed for the treated and control group in the trimmed sample. Table 7 presents the ATT for children facing paternal absence in non-migrant fathers' households. Most estimates do not exhibit statistical significance at any confidence level, as expected. The blocking estimator in Panel B was significant at a confidence level of 95%, however, this result is not the same as the estimate obtained with the preferred estimator bias-adjusted matching which was no significant.

Finally, an additional sensitivity analysis was conducted to analyze how strong the relationship would have to be between an unmeasured cofounder and the treatment assignment, as well as between the unmeasured cofounder and the outcome, to undermine the ATT (Linden et al., 2020). We obtained the RB (Rosenbaum, 2010; Rosenbaum & Rubin, 1983) for the continuous outcome and an adaptation of RB known as the Mantel and Haenszel bounds (Aakvik, 2001) for the binary outcome. It is worth notice that these tests do not necessarily test the unconfoundedness assumption itself, instead, these provide evidence on the degree to which the significance of the results hinge on this assumption (Becker & Caliendo, 2007)

In these tests, we examine whether the estimates are still credible if the children with the same baseline characteristics (age, gender, rural area, household income, etc.) differ in their probability of having a migrant father by a factor gamma (Γ), meaning, these probabilities might differ due to the presence of unobserved heterogeneity. The parameter Γ can be seen as a measure of the degree of hidden bias on which we test the sensitivity of the ATTs. The sensitivity analysis of the significant estimates is presented in Table 8 reporting the results of the *p*-value for the ATT while setting the level of hidden bias to different values of Γ. The results show that the significance at 5% of the ATT for the dummy attending a paid school holds at a level of $\Gamma = 1.75$, this implies that if the children with the same covariates differ in their probability of having a migrant father by 75% (due to unobserved cofounders) the effect on the likelihood of attending to a paid school becomes statistically no significant. For the case of the variable school tuition, the ATT's significance holds for values as high as 2.00. Usual critical values for Γ are 1.80 – 1.85 (Özbuğday et al., 2020), thus based on these results we can conclude that the estimates are not sensitive to unobserved variables.

Table 7 *Estimation Results of ATT Effects Using Pseudo-treatment.*

		Matching	**Blocking**
a. Paid school	Coeff.	-0.006	-0.088*
	s.e.	0.031	0.036
	Obs T = 0	458	1177
	Obs T = 1	258	198
	Blocks		6
b. ln (school tuition)	Coeff.	-0.026	-0.142
	s.e.	0.189	0.299
	Obs T = 0	458	1436
	Obs T = 1	258	258
	Blocks		8
c. Years of schooling	Coeff.	-0.017	0.001
	s.e.	0.028	0.037
	Obs T = 0	458	1404
	Obs T = 1	258	254
	Blocks		6
d. Gap on years of schooling	Coeff.	0.020	-0.050
	s.e.	0.144	0.254
	Obs T = 0	458	1437
	Obs T = 1	258	258
	Blocks		

Note. Coefficients correspond to the bias-adjusted matching and blocking ATT estimates. Model (2) was used for the estimation of the PS. The trimmed sample considered the thresshold of alpha = 0.10. The standard errors (s.e) correspond to robust s.e. ***p < 0.001, **p<0.01, *p<0.05

Table 8 *Rosenbaum Bounds for ATT*

Γ	***p* Significance level**			
	Paid school	**ln (school tuitions)**	**ln(schooling years)**	**Gap on years of schooling**
1.00	0.002	< 0.001	0.205	0.083
1.25	0.009	0.003	0.525	0.311
1.50	0.023	0.011	0.784	0.592
1.75	0.047	0.024	0.919	0.801
2.00	0.08	0.043	0.974	0.916
2.25	0.122	0.069	0.992	0.968
2.50	0.169	0.101	0.999	0.989
2.75	0.222	0.136	0.999	0.996
3.00	0.276	0.174	0.999	0.999

Note. Rosenbaum bounds for binary variables was calculated using mhbounds in stata by Becker and Caliendo (2007) and Rosenbaum bounds for continuous variable was calculated using rbounds in Stata by Gangl (2004). ATT estimations were conducted usign the trimmed sample with Model (2) and using the function psmatch2 in Stata with one match and no replacement.

5 Concluding remarks

This study examined the effects of paternal migration on schooling choices of children left behind of Ecuador. We focus on schooling choices variables including attending a paid school and amount of school tuition. In addition, this study analyzed the effects of paternal migration on schooling outcomes such as years of schooling and the gap between expected years of education and actual child's schooling years. The problem of selection into migration was partially addressed by implementing the matching bias-adjusted (Abadie & Imbens, 2002) and blocking estimators (Imbens, 2014). This strategy allowed us to obtain credible estimates of the effects of paternal migration on schooling choices, while significant effects on schooling outcomes were not found.

The empirical results consistently suggests that paternal migration, in the cases where the children live their mother as head of household, has a significant and positive effect on the probability of attending a paid school and the school tuitions expenses. We find that children with a migrant father had, on average, 15.0 – 16.6 percentage points higher probability of attending a paid school relative to children with no migrant father. Expenses on school tuitions were on average, 3.47-3.75 times larger than children with no migrant father. These findings contrasts with previous studies that have found that paternal migration can have negative effects on children education (Antman, 2011; Song and Glick, 2022). One possible explanation is that our treated group correspond to children with a migrant father living with their mother as head of household, thus, it is possible that the role of the mother is key when facing the father absence and in the use of the remittances. Previous studies have found that when women are who decide in the household, more money is allocated on their children (Duflo, 2012; Saleemi & Kofol, 2022)

These findings add to previous literature that have studied the impact of migration in different indicators of human capital formation. The positive effects of paternal migration on schooling choices contrast with the effects of maternal migration which has been addressed in other studies (Fiore, 2022). In this case, significant detrimental effects on child's schooling were found. Further studies could compare these two migration decisions and confirmed the non-detrimental paternal migration effects on child's schooling versus maternal migration. This is an important question to delve into in the current years since migration flows have been increasing in some countries of the Latin American regions, including Ecuador. Moreover, the decisions of migration are also changing and evidence regarding what outcomes are expected in a child depending on the migration decision (e.g., whether he/she migrates with the parents, his/her father migrates, his/her mother migrates, or both parents migrate) are still unclear.

Furthermore, our results suggest that even when father migration does improve educational investment decisions this do not necessarily translate into better schooling outcomes relative to their peers. However, paid schools, in particular in developing countries, can provide education of higher quality with possible benefits in the long term (Pianta & Ansari, 2018). Given that we lack longitudinal data, further studies should assess different a group of adolescents or adults who experienced paternal absence due to their father migration during their childhood and their impact in schooling and career outcomes.

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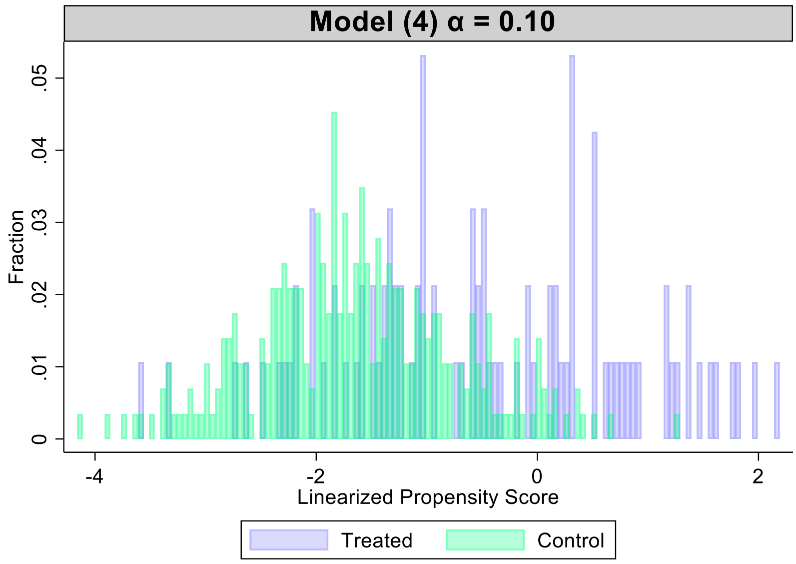
7 Appendixes

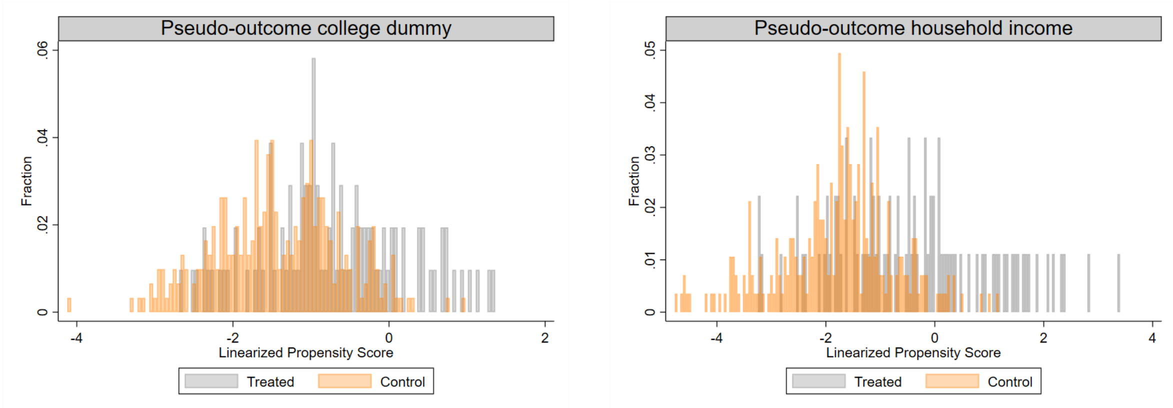
7.1 Appendix A Supplementary Figures

Figure A. 1 Distribution of the Propensity Score when using Model (4)



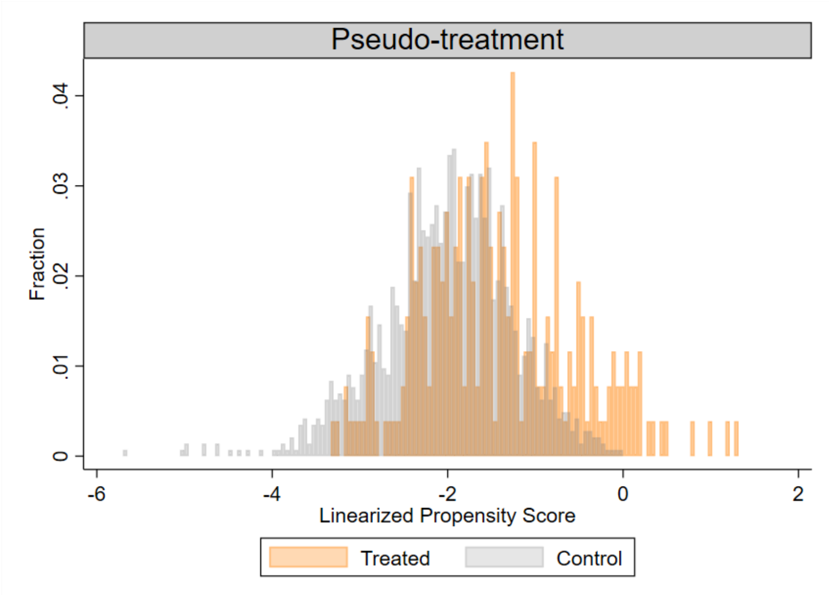
Note. This graph shows the distribution of the estimated linearized propensity score obtained from Model (4). The linearized PS was calculated as $\ln[p(X_i)/(1-p(X_i))]$. The treated group corresponds to children living without their father. The control group correspond to children living with both parents.

Figure A.2 Distribution of the Propensity Score used to Estimate the ATT on Pseudo-outcomes



Note. This graph shows the distribution of the estimated linearized propensity score obtained from binary logit models on the probability of having a migrant father. These estimations were carried out omitting the covariates college dummy and household income since these were tested as outcomes in the robustness analysis. Variables are: (a) college dummy: a dummy indicating whether at least one household member has college degree, (b) household income: household income in USD pre-remittances. The linearized PS was calculated as $\ln[p(X_i)/(1-p(X_i))]$. The treated group corresponds to children with migrant father. The control group correspond to children living with both parents.

Figure A. 3 Distribution of the Propensity Score used to Estimate the ATT with Pseudo-treatment



Note. This graph shows the distribution of the estimated linearized propensity score obtained from a binary logistic of model on the probability of experiment paternal absence but not related to migration. The linearized PS was calculated as $\ln[p(X_i)/(1-p(X_i))]$. The treated group corresponds to children living without their father. The control group correspond to children living with both parents.

7.2 Appendix B Supplementary Tables

Table B. 1 *Variable Description*

Variable	Description
(a) Child's characteristics	
Female	A dummy variable indicating if the child is 1 = female or 0 = male
Age	Child's age
(b) Household and household members characteristics	
Rural	A dummy variable indating whether the household is located in a 1 = rural or 0 = urban area.
Owns land	A dummy variable indicating whether the head household 1 = owned land or 0 = did not owned land five years ago (before migration).
Owns house	A dummy variable indicating whether the head household 1 = owned a house or 0 = did not owned a house five years ago (before migration).
Monthly income	Averagre monthly household income pre-remmitances.
Years of schooling	Average years of schooling of all household members older than 18 years old.
Ratio working age members	Proportion of household members with workign age (>18).
Ratio female members	Proportion of female household members.
Mother years of schooling	Child's mother years of schooling.
College	A dummy variable indicating whether 1 = at least one household member has a university degree or 0 = none household member has a university degree.
(c) Child's education outcomes	
Attends to paid school	A dummy variable indicating whether the child goes to a 1 = paid school or 0 = public school.
Years of schooling	Child's years of schooling
Difference of years of schooling	The difference between the child's years of schooling and the average years of schooling of children in his/her cohort age.
School tuition	School tuition paid in USD.

Table B. 2 *Correlation between Alternative Specifications of the Propensity Score*

	Model (1)	**Model (2)**	**Model (3)**	**Model (4)**
Degrees of freedom	27	25	20	28
Log Likelihood Function	-488.51	-598.37	-600.81	-465.97
Correlation of Log Odds Ratios				
Model (1)	1.00			
Model (2)	0.863	1.00		
Model (3)	0.859	0.995	1.00	
Model (4)	0.969	0.844	0.839	1.00

Note. a. Pearson correlation. Log Odds Ratios correspond to the linearized PS

Table B. 3 *Logit Estimations for Full Sample and Matched Sample*

	Before matching	**After matching**
rural	-0.087 (0.221)	0.394 (0.365)
network	4.59 (194.80)	8.26 (362.3)
ratio working age members	-6.32*** (0.804)	-2.91 (1.63)
ratio female	5.31*** (0.571)	2.13 (1.12)
college	-0.210 (0.442)	-
years schooling	0.078 (0.050)	0.009 (0.084)
owns land	0.305 (0.391)	-0.352 (0.801)
owns house	0.657*** (0.179)	0.327 (0.043)
child age	-0.028 (0.023)	-0.059 (0.043)
child female	-0.965** (0.197)	-0.462 (0.342)
mother schooling	0.074 (0.042)	0.040 (0.081)
household income pre remittances	-0.004*** (0.000)	-0.001 (0.002)
X2	595.86	15.00
Prob X2	< 0.001	0.662
Pseudo-R2	0.332	0.045
Observations	8742	243

Note. Both models have province dummies. Models before matching corresponde to the full sample, Model After matching correspond to matched sample based on the trimmed sample with the PS from Model (2) in Table 5 . *** $p < .001$, ** $p < .01$, * $p < .05$

Table B. 4 *Estimation Results of ATT Effects on Schooling Choices Variables based on Alternative PS Specifications and Threshold of Alpha*

		(a) Paid school		(b) ln (school tuition)	
		Matching	**Blocking**	**Matching**	**Blocking**
a. Model (2) α = 0.08	Coeff.	0.113*	0.137**	0.979**	0.942**
	s.e.	0.051	0.046	0.329	0.298
	Obs T = 0		352		406
	Obs T = 1		101		113
	Blocks		5		3
b. Model ommiting household income variable α = 0.10	Coeff.	0.103+	0.162***	0.886**	0.654*
	s.e.	0.059	0.046	0.337	0.322
	Obs T = 0		263	169	386
	Obs T = 1		81	111	111
	Blocks		5		3

Note. Coefficients correspond to the bias-adjusted matching and blocking ATT estimates. Dependent variables are: (a) dummy variable that indicates if the child attends a paid school, (b) natural logarithm of school tuition. Estimations include all variables used to construct the propensity score and province dummies. The standard errors (s.e) correspond to robust s.e. ***p < 0.001, **p<0.01, *p<0.05, ^{+}p<0.10

Table B. 5 *Estimation Results of ATT Effects on Schooling Choices Variables considering dummy of more than one child at home*.

		(a) Paid school		(b) ln (school tuition)	
		Matching	**Blocking**	**Matching**	**Blocking**
a. Model (2) α = 0.10	Coeff.	0.135*	0.139**	1.05**	0.81**
	s.e.	0.056	0.050	0.359	0.312

Note. Coefficients correspond to the bias-adjusted matching and blocking ATT estimates. Dependent variables are: (a) dummy variable that indicates if the child attends a paid school, (b) natural logarithm of school tuition. Estimations include all variables used to construct the propensity score and province dummies. The standard errors (s.e) correspond to robust s.e. ***$p < 0.001$, **$p<0.01$, *$p<0.05$, $^{+}p<0.10$

7.3 Appendix C Matching data set procedure

1. La base de datos emla1 fue filtrada para migrantes hombres que dejaron hijos menores de 18 años y eran jefes de hogar.
2. La base de datos personas, fue filtrada para niños con edades entre 6 a 19 años, que viven con su madre como jefe de hogar.
3. Se creó un ID en cada base de datos uniendo los indicadores de región, área, dominio, regional, ciudad, zona, sector, vivienda, hogar.
4. Se matchearon las dos bases de datos con dicho ID. Dado que la base de datos personas, ya está filtrada por edad, los casos en que el padre haya dejado hijos menores de 6 años no tendrán match, así mismo, los casos en los que la madre no sea jefe de hogar.
5. Finalmente, se realizó un proceso de validación (visual y manual) de los 194 casos matcheados comparando las variables hijos que dejó, lugar a que el migró y escolaridad del padre.