The effect of parental migration on the schooling of children left behind in rural Cambodia

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Abstract

Growing rural-to-urban and international migration flows have sparked concerns about the investments in the education of the children left behind in Cambodia. We draw on a panel household-level survey conducted in rural villages in 2014 and 2017 to analyse the relationship between parental migration and children's schooling. The analysis shows that children of migrant parents complete less years of schooling than children of non-migrant parents. We find a bigger effect for children whose parents migrated abroad, for children aged 12 to 17, and for maternal migration. The effect persists over time, with parental migration in 2014 influencing schooling in 2017. We exploit the longitudinal dimension of the data to estimate a placebo, which greatly reduces the concerns related to the possible confounding effect of time-invariant unobserved heterogeneity. The negative effect that we find appears to be driven by the reduced parental input in children's education rather than by an increase in children's work.

Keywords: Cambodia, Parental migration, Education, Child schooling, Propensity Score Matching

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1. Introduction

Cambodia has recently experienced a steady growth in some economic sectors, notably garment, textile, and construction, which are concentrated in Phnom Penh, and which are low-skill labour intensive (Hill and Menon, 2014). The increase in the availability of urban jobs has led to a rise in internal, mostly rural-to-urban, migration flows (Hing et al., 2014). Cambodians do not just move internally, because there are also substantial flows towards foreign countries, with neighbouring Thailand being the main Asian destination (OECD, 2017). While the Cambodian government supports the international migration of its citizens (MLVT, 2010), as other Asian countries, and especially the Philippines, do, it has voiced concerns about the possible negative effects of rural-to-urban migration flows. In particular, the Cambodian Ministry of Agriculture, Fishery and Forestry warned against the possible threat to food security due to the reduction in size of the labour force employed in agriculture (CDRI, 2015). Further worries relate to the condition of the children left behind. A study on internal migration flows commissioned by the Cambodian Ministry of Planning, expressed concern about migrants' children, who are typically left behind with their grandparents (Ministry of Planning, 2012), who are often illiterate or with very little formal education (UNICEF, 2014). This, in turn, has led UNICEF to call the Cambodian government to "conduct a specific household survey to better estimate the number of children left behind […] to understand their wellbeing" (UNICEF, 2017, p. 16)[3].

This paper uses a household-level panel survey conducted by the Cambodia Development Research Institute (CDRI) in rural villages to analyse the implications of parental migration on the educational outcomes of the children left behind. More specifically, we aim to answer the following questions: (i) What is the influence exerted by parental migration on the completed years of schooling of the children that are left behind in rural Cambodia? (ii) Is this influence different depending on the age of the child, the gender of the migrants' parent, and the internal or international nature of migration? (iii) What can explain the pattern that we uncover in the data?

The contribution of this paper is threefold: First, we address a research question that is closely related to the concerns expressed by the Cambodian government and by international institutions. We go beyond the estimation of an average effect, because the design of targeted policy interventions requires a more detailed understanding of the channels that are at work. Second, we resort to an estimation approach, i.e. propensity score matching, in which identification hinges on the hypothesis of selection on

[3] Bilsborrow (2016) observes that this type of concerns about the children left behind, and in particular for those exposed to maternal migration, played a key role in motivating efforts to collect data in migrant-sending areas.

observables, but we are able to test whether a violation of the identifying assumption is biasing our estimates. The longitudinal dimension of the data allows us to test whether the future migration status of a household with no migrant parent in the first wave of the survey correlates with the current educational outcomes of the children, something that would hint at a non-random selection on time-invariant unobservables of migrant households[4] that could explain our results. Third, we contribute to the growing literature which analyses the effect of adult migration on children left behind. Adult migration can affect the schooling outcomes of children left behind through four different channels. First, migrants' remittances can increase household's investments in the education of children (e.g. Yang, 2008; Alcaraz et al., 2012; Adams and Cuecuecha, 2013; Cox-Edwards and Ureta, 2003; Amuedo-Dorantes and Pozo, 2010), even though the remittances could also increase the investments in productive assets which lead to competing demands on children's time (Bhalotra and Heady, 2003). This first channel could reinforce the second channel, children's work: because of adult migration, the children are likely to be more involved in family-run economic activities (Bansak and Chezum, 2009) and in household chores (McKenzie and Rapoport, 2011). This could be compensated by the change in the household composition, which often takes place after migration - left behind members start to co-reside with other family members, and can contribute to household chores (Bertoli and Murard, 2020). Third, the migration of a household member could increase the probability of future child migration, making the child's educational outcome more sensitive to the (possibly lower) returns to schooling that migrants enjoy at destination (McKenzie and Rapoport, 2011; de Brauw and Giles, 2017). Fourth, when parents migrate, children are exposed to a reduction in adult supervision. According to some authors, this could have negative effects on schooling (Giannelli and Mangiavacchi, 2010) and learning (Zhao et al, 2014; Zhang et al, 2014). However, there is not consensus in the literature on this point. Gibson et al. (2011) find a positive impact of parental migration on English literacy and no effect on schooling attainment in Tonga, while Nguyen (2016) shows that parental migration reduces cognitive abilities in India and Vietnam, but not in Ethiopia and Peru. The effect of parental absence on children's education is likely to be more pronounced for older children (McKenzie and Rapoport, 2011; Giannelli and Mangiavacchi, 2010), stronger for maternal migration due to a differential investment in children by the two parents (Cortes, 2015), and for long duration migration episodes (Giannelli and Mangiavacchi, 2010). Similarly, a single parent migration is not likely to have the same effect as a both parent migration - no effect is found by Zhang et al (2014) when only one parent is absent. In this paper, we explore the heterogenous effects of parental migration in Cambodia according

[4] With the term 'migrant household', we refer to households where a child has one or both parents migrated.

to: the age of the child, the gender of the migrant parent, and the domestic or international destination of the parents. We are also able to test the effect on a longer period, thanks to the panel nature of our data. Finally, we explore some of the possible channels that might drive the effect that we uncover. More specifically, we investigate if the effect is caused by a change in the use of child work, or by the lack of parental investment.

We analyse the relationship between parental migration and the number of completed years of schooling of the children. The results show that differences in observables account for most of the difference in the outcome between children in migrant-sending households and other children residing in rural areas, but that parental migration is still associated with a reduction (by 0.3-0.5 years) in the number of completed years of schooling in 2014. The lower number of years of schooling of migrants' children appears to reflect a higher probability of being in a class below that expected for their age or of dropping out of school after the age of 11. When we exploit the longitudinal dimension of the data, to see whether the (future) migration of a parent in 2017 for children whose parents were at home in 2014 is associated with their (current) schooling outcome, we find no significant effect. This placebo is reassuring with respect to the identifying assumption that underpins the propensity score matching. The effect that we uncover is stronger for children whose parents migrated abroad, for children aged 12 to 17 than for younger children, and it is persistent over time. Maternal migration has a more detrimental effect on children's education than paternal migration. We find no association between parental migration and the probability that a child is economically active. Child schooling is not associated with the migration of other adult former household members. These two last findings suggest that our results are mostly related to the detrimental effect of parental absence. The analysis does not exclude the possibility that migrants' children have less schooling because of the high opportunity of future migration, which reduces the return to education.

The rest of the paper is structured as follows: Section 2 briefly illustrates the context of our analysis. Section 3 presents the data and descriptive statistics. Section 4 describes the methodology that we use in the econometric analysis. Section 5 presents the results. Section 6 draws conclusions.

2. Migration and education in Cambodia

Estimates from the Cambodian National Institute of Statistics indicate that around 27 percent of Cambodians could be classified as migrants in 2009 (NIS, 2009). The vast majority (97.3 percent) are internal migrants. Rural-to-rural migrants are 58.4 percent, and rural-to-urban migrants are 24.5 percent (NIS, 2013). Phnom Penh is the main domestic destination, where migrants are employed in the

manufacturing and service sectors (Hing et al., 2014). Urbanisation in the country is forecast to increase, thus leading to a likely increase in rural-to-urban migration (UN, 2011). International migration has steadily increased in the last two decades, with 1.19 million Cambodians estimated to be living abroad in 2015, which represents around 7.6 percent of the total Cambodian population, compared to 3.7 percent in 2000. Thailand is the most common destination country, and receives 68 percent of Cambodian emigrants (OECD, 2017), with Malaysia and South Korea being other important destinations. According to Hing et al. (2014), most international migrants are undocumented in the host countries.

When Cambodians migrate for work, they often leave behind family members, such as a spouse, child, siblings, parents, or some combination thereof. 21 percent of all households of origin of the migrants have at least one child under 18 who has been left behind. Around half of the children left behind live with the other parent, 41.5 percent live with grandparents, and the rest co-reside with other relatives (Zimmer and Van Natta, 2015). The qualitative study by UNICEF (2014) reports that the main caregivers of the children left behind are mostly their grandparents, who are on average 62 years old.

Cambodia's Gross Enrolment Rate (GER) in primary education was very low up to the 1970s (27.8 percent in 1971), and increased very quickly during the following decades (115 percent in 1981), according to UIS data[5]. Thus, most of the grandparents of today's Cambodian children had almost no education. The Cambodian Constitution states that children aged 6 and above are entitled to a free, compulsory 9-year basic education[6]. According to MoEYS, the GER at primary school in rural areas was at 113.5 percent in 2014 at the Net Enrolment Rate at 96.5 percent cent. This difference can be explained by the late enrolment of children. In addition, the average drop-out rate for rural children in grades 1 to 6 was quite high, at 6.6 percent, and repetition rate at 7.2 percent in 2014-15. All these factors combined imply that the average age of completion of primary school is 15 rather than 12, as would be expected with no late enrolment and no year repetition (MoEYS, 2015).

In 2014, the GER in rural Cambodia for lower secondary school was 53.3 percent and for upper secondary school was 20.2 percent, far lower than for primary school. Rural drop-out rates were even higher for lower secondary school (20.6 percent), and upper secondary school education (26.0 percent). High drop-out rates at secondary school suggest that the country is struggling with the challenge of increasing the level of its human capital in rural areas.

[5] UIS data: uis.unesco.org.

[6] General education in Cambodia consists of 12 years of schooling, which is categorized into primary education (6 years), lower-secondary (3 years) and upper-secondary education (3 years).

3. Data and descriptive statistics

This study uses two waves of the *CDRI Household Questionnaire*, a household-level panel survey conducted by CDRI in March 2014 and in March 2017, in 11 rural villages spread across Cambodia (see Figure A.1 in the Appendix). These are follow-ups of a survey that began in 2001. The 11 villages in the survey were selected from the rural areas of four geographical zones (i.e. the Mekong and the Tonle Sap plains, the upland plateau, and the coastal zone)[7]. The surveys contain standard information on demographic characteristics and on household economic conditions. The questionnaire also provides information on individuals who used to co-reside with the members of the surveyed household[8].

1,183 households were interviewed in 2014 and 1,104 of them were re-interviewed in 2017, with new households replacing those lost by attrition. The attrition rate is around 6.7 percent. Table A3 in the Appendix shows that attrition is not systematically related to relevant observables in 2014. We use information on 1,661 children, aged 6 to 17 in March 2014 belonging to 839 households[9]. We only focus on children and grandchildren of the household head, because for children with a different relationship to the household head the information contained in the household roster does not allow unambiguous identification of their parents and hence it is not possible to define their migration status[10].

We define as an international migrant any former household member who was residing abroad at the time of the survey[11]. As in Roth and Tiberti (2017)[12], we define as an internal migrant any individual who left her household of origin and moved to another Cambodian province for job-related reasons. The migration module of the survey also collects information on individuals who moved to other parts of Cambodia for other reasons; we do not consider them to be internal migrants[13].

[7] Although the sample has not been designed in order to be representative of the rural areas of Cambodia, Table A2 in the Appendix shows that some of the main variables of the 2014 CRDI survey are similar to those from the Cambodian Socio-Economic Survey (CSES), a national representative survey conducted annually by the National Institute of Statistics (NIS) and the Ministry of Planning (MoP).

[8] Table A1 in the Appendix shows the questions included in the migration module of the two waves of the survey.

[9] When we replicate the analysis with children aged 6 to 19, we obtain the same results.

[10] There are 30 children with a different relationship with the household head.

[11] In other words, we classified as international migrants all individuals registered in the migration module reported in Table A1 in the Appendix, for which the answer to Question 8 is 3 or 4.

[12] We classified as internal migrants all individuals registered in the migration module reported in Table A1, for which the answer to Question 8 is 1 or 2, and the answer to Question 10 is 1 or 2.

[13] We consider children whose parents moved internally for other reasons than work as internal migrants in a robustness check presented in Section 5.2.

1,537 children (92.5 percent of the sample) do not have migrant parents, 29 have a migrant mother, 58 have a migrant father and 37 have both parents who migrated, so that we have in total 124 children who have at least one parent who migrated internally or internationally before March 2014. Table 1 describes the main characteristics of the sample children by the migration status of their parents. For children of migrants, household characteristics (i.e. household size, the number of working-age members, the dependency ratio, and share of women among working-age members) are computed using information on the migrant member, so they represent the household composition in a counterfactual no-migration scenario. Migrants' children are younger, and have, on average, parents who are more likely to have completed primary education. They live in poorer households, with fewer working age (15-64) members, a lower share of women among working age members, and a higher dependency ratio. Table 1 also shows that migrants' children completed 1.18 years of school less than other children in 2014.

Table 1 around here

4. Methodology

In order to analyse the implications of parental migration on the educational outcomes of the children left behind, we resort to a matching approach, where the main treatment is defined as having at least one parent who migrated internally or internationally before March 2014, and the outcome is represented by the number of years of schooling completed by the child at the same time. Additional outcomes are explored in Section 5.3.

Let d_i be a dummy variable that takes the value of one if child i is exposed to the treatment, i.e. at least one of her parents is a migrant, and 0 otherwise. Let T and U represent the sets of treated, i.e. $d_i = 1$, and untreated children respectively, i.e., $d_i = 0$. Let y_i be the outcome of interest, i.e. the years of completed schooling of child i. More precisely, let y_{i1} be the outcome for child i when exposed to the treatment, and y_{i0} when not treated, with just one of the two being clearly observed for us. The causal effect of exposure to the treatment on the treated, i.e., the average of $y_{i1} - y_{i0}$ for all children $i \in T$, requires estimation of the unobserved (counterfactual) outcome y_{i0} for treated individuals.

Let x_i be a vector of characteristics of the child and of her household that influence both the probability of exposure to the treatment $P(x_i)$ (the propensity score), and the counterfactual outcome y_{i0}. If the counterfactual outcome y_{i0} is orthogonal to the assignment to the treatment d_i conditional upon the value of the propensity score $P(x_i)$, then the unobserved counterfactual outcome y_{i0} for the treated can

be estimated through an average of the (observed) outcome y_{i0} for the untreated children, i.e. children belonging to the set U, with a similar value of the propensity score (Rosenbaum and Rubin, 1983).

Formally, under the conditional independence assumption that we have just described, we have:

$$E_T[y_{i0}|i \in T, P(x_i) = p] = E_U[y_{i0}|i \in U, P(x_i) = p]$$

The average treatment effect on the treated can then be estimated through a simple double average for treated children only: the average of the difference $y_{i1} - y_{i0}$, where the unobserved counterfactual outcome has been replaced by the average of the observed outcomes for untreated observations with a similar value of the propensity score $P(x_i)$. We denote as $C \subseteq U$ the set of control children, i.e. the untreated children matched to at least one treated child in this double average.

The large difference between the number of treated and untreated children (124 vs 1537) provides a large pool of potential controls for matching, thus reducing the distance in terms of the estimated propensity score between each treated child and the matched untreated child(ren). Moreover, the limited size of the treated with respect to the non-treated, reduces the risk that the treatment could be giving rise to general equilibrium effects, thus jeopardizing the Stable Unit Treatment Value Assumption (SUTVA) which underlies our estimations.

Threats to identification

The simplicity, and the intuitive appeal, of the identification of a causal effect through propensity score matching, crucially depends on the plausibility of the conditional independence assumption. Once we have conditioned on the propensity score, is the assignment to treatment orthogonal to unobserved determinants of the outcome variable? The threats to identification are (as with any other econometric approach) intimately related to the selection of the variables that are included in the analysis via the vector x_i.

These covariates should be orthogonal to the exposure to the treatment. This requires either that the covariates are measured before the child is exposed to the treatment, or that the exposure to the treatment influences in a non-systematic way the covariates (Lechner, 2008), so that the post-treatment measurement introduces a zero-mean measurement error. A variable that does not satisfy this condition would be, for instance, a dummy describing whether the child i belongs to a female-headed household. Headship is defined in surveys among resident members (thus excluding migrants), and exposure to the treatment unambiguously raises the share of female-headed households in any country in which the man

is almost invariably designed as the household head when a married couple co-resides[14]. Female headship is a strong correlate of the receipt of remittances or the presence of a migrant among former household members (e.g. Adams and Cuecuecha, 2013) given that it is influenced in a systematic way by the exposure of the treatment itself. The inclusion of this variable in the vector x_i would result in a biased estimate of the average treatment effect, as it would increase the share of female-headed households in the control group, and worsen the educational outcomes of the children in the control group.[15] Variables related to the demographic structure of the household, such as the share of working-age members, should also be computed using information on the migrant members (as in Bertoli and Marchetta, 2014), to reconstruct the composition of the household before the treatment.[16] Covariates that are measured after the treatment to gauge the socio-economic conditions of the household to which child i belongs, such as a measure of household assets or household consumption per capita, should be widened to reduce the concerns related to their endogeneity with respect to the receipt of remittances.

The vector x_i should not include variables which only influence the probability of exposure to the treatment d_i but are orthogonal to the outcome y_{i0}. As noted by Caliendo and Kopeining (2008), the objective of the estimation of the propensity score $P(x_i)$ is not to maximize the ability to predict the assignment to the treatment, but rather to control only for the covariates that have a simultaneous influence on d_i and y_{i0}. Adding further covariates that influence just d_i would be wrong: first, it would reduce the overlap between the distribution of the propensity score for the treated and untreated children (thus hindering the possibility of finding adequate matches for each treated child); second, these variables would satisfy the desired relevance and excludability properties of an instrumental variable, hinting at the possibility of adopting an alternative approach to identification.

In our case, the covariates which belong to the vector x_i must influence both the probability of having at least one migrant parent, and the number of completed years of schooling, by child i. We include household size, the number of working-age members, the dependency ratio, and the share of women among working-age members; for treated households, all these variables are computed using information on the migrant members (as in Bertoli and Marchetta, 2014), to reconstruct the composition of the

[14] If a woman migrates, household headship is unchanged, while a woman might become household head after the migration of a male household member.

[15] Female headship among non-migrant households is closely correlated with negative economic shocks, such as the death of, or the divorce from, a male household member (Fafchamps and Quisumbing, 2008).

[16] A correlation between the occurrence of a migration episode and further variations in household composition (Bertoli and Murard, 2020) would pose a threat to the analysis, but only if new household members differ systematically with respect to the characteristics that are measured.

household before migration. We also include mother's and father's age and education, and age and gender of the child. Age is included with a set of dummies in order to control for the age differences between treated and control children in a more flexible way. Quartiles of consumption per capita are included among covariates to control for the socio-economic conditions of the household to which the child belongs. We are conscious that consumption can be endogenous with respect to parents' migration, e.g. because of remittances sent back by the migrants. This is why we decided to use quartiles of consumption to mitigate the problems related to the post-treatment measure of consumption. Clearly, there are changes in the quartiles over time, but a test on children interviewed both in 2011 and in 2014 shows that those movements are not systematically different for the treated[17]. Dummies for the 11 villages are also included among the covariates, to take into account all village-specific characteristics that can influence the probability to be treated and the education outcome. The inclusion of village dummies makes it unnecessary to include any supply side variable of the education production function, such as the distance from educational facilities, or past local weather conditions, that could have influenced agricultural productivity.

The most direct way to test whether the proposed specification of the vector of covariates is able to reproduce the ideal setting of a randomized assignment to treatment is to conduct a placebo test. The longitudinal dimension of the survey data used in this analysis offers such an opportunity. We define a dummy variable d_i^{t+1} which takes the value of one if the child i at time $t+1$ is exposed to the treatment, and 0 otherwise. We estimate the propensity of exposure to this (future) treatment using the same vector of covariates x_i (still measured at time t), and we estimate the average (future) treatment effect on the treated using the set of untreated children at time t. If this is significant, then it would strongly suggest that our estimated effect captures unobserved heterogeneity among the treated and the untreated children, as under the conditional independence assumption the future exposure to the treatment should be orthogonal to the current outcome.

Our placebo test does not deal with unobserved time-varying factors that affect both migration and children's schooling simultaneously. The survey includes some questions on the (self-reported) occurrence of various types of negative shocks, such as the death or the illness of a household member, or crop failure or damage. These variables allow us to compute the correlation between the exposure to treatment (parental migration in 2014) and shocks reported in different waves of the survey. When we

[17] In both treated and non-treated group, around 40 percent of children belonged to the same quartile in 2011 and in 2014, while 44 percent moved to a higher or a lower one.

aggregate all the shock variables into a dummy that signals whether at least one negative shock has been reported, this correlation is 0.13 for shocks reported in 2014, and -0.02 for shocks reported in 2011. The low correlation between the occurrence of a negative shock and parental migration might appear surprising, but this is consistent with the existence of liquidity constraints that can hinder investment in international (Angelucci, 2015) and domestic migration (Bryan et al., 2014), and which are likely to become more limiting after a negative shock. This allows partial mitigation of the legitimate concern about time-varying confounders which could simultaneously increase the probability of parental migration, and at the same time induce migrants' children to drop out of school.

Matching methods

We use either nearest neighbour matching (with replacement, and with a varying number of matches), or Kernel matching, to define the set of controls $C \subseteq U$. With Kernel matching, all untreated observations are used as matches for each treated child i, but their weights are inversely proportional to the distance between their propensity score and $P(x_i)$. After the match, we re-estimate the propensity score on the joint subset of matched treated and control observations (weighting these to reflect their influence in the estimation of y_{i0}), and we gauge the ability of the propensity score to act as a balancing score via the induced reduction in the pseudo-R^2, which reflects the reduced ability to predict assignment to treatment (Sianesi, 2004). We correct for the bias arising from an imperfect balancing of the covariates (a likely outcome given the small size of our sample) through a regression adjustment using as regressors the same variables as vector x_i (Abadie et al., 2004).

5. Results

5.1. Main Results

Table 2 shows the results of the estimation of the logit model where the dependent variable is a dummy taking the value of one for the children having at least one parent who migrated. The estimation is conducted on the sample of 1,661 children aged 6 to 17 in 2014. The overall goodness of fit, measured by the pseudo-R^2, of the logit model is 0.306. When we compute the percentage of good predictions, we find that 81 percent of the predictions are correct, using as a threshold the frequency of exposure to the treatment in our sample of children (7.5 percent) to establish when a child is predicted to be exposed to

the treatment. However, a good match also requires a good number of incorrect predictions (i.e. treated and untreated individuals whose estimated values on the propensity score are close to each other). We computed 20 quantiles of the propensity score distribution in order to observe the percentage of treated and non-treated in each percentile. The 20th quantile of the distribution contains 44 percent of untreated while the 19th quantile contains 61 percent of untreated. This means that we have a large number of untreated with a high propensity score, which is reassuring with respect to the goodness of fit of our logit model.

The estimated propensity score $\hat{P}(x_i)$ estimated from the logit model is used to define the subsample of non-treated children that form the control group, and hence to estimate the ATET.

Tables 2 and 3 around here

Table 3 presents the results obtained with nearest neighbour matching (with the number of neighbours varying between 1 and 10), and with Kernel matching. Each line in Table 3 corresponds to the estimate obtained from a different matching method. The estimation of the propensity score on the subsample of matched treated and control children has a pseudo-R^2 that is far lower than on the entire sample (and declines with the number of neighbours), revealing that $\hat{P}(x_i)$ acts well as a balancing score, even though we also correct for any residual imbalance in the vector of covariates x_i between the group of treated and control children following Abadie et al. (2004). Such an adjustment is necessary given the limited size of our sample, which hinders the achievement of a perfect balance.

When we purge the ATET from the bias associated with the residual imbalance of the covariates, we obtain a negative and statistically significant effect. The number of the matching method and the numbers of neighbours for the nearest-neighbour matching do not significantly influence the results, as shown in the different lines of Table 3. The bias-adjusted ATET ranges from -0.30 years to -0.40 years, a figure that is below the negative difference of 1.18 years that is obtained from a simple (unconditional) comparison between treated and untreated children. Thus, differences in observables explain around three quarters of the difference in the number of completed years of schooling, but the residual difference is significantly different from zero at conventional confidence levels. This result is, *per se*, insufficient to conclude that parental migration exerts a causal negative impact on the schooling outcome of the children left behind, because such an interpretation crucially hinges on the identifying assumption of selection on observables. The children left behind might differ in terms of relevant unobservable characteristics, e.g. their parents

might have a lower concern for their education, and their input might have been lacking even if they had not left the household.

As described in Section 2 above, the longitudinal dimension of the dataset allows us to conduct a placebo test to gauge the empirical relevance of the possible violations of the identifying assumption. We first restrict the sample to the 1,122 children who are untreated in 2014, i.e. whose parents were not reported to have migrated in March 2014, and who are re-interviewed in 2017. We define the placebo treatment as having at least one parent reported as a migrant three years later, in March 2017 (there are 36 children in the data subject to this placebo). We then estimate the probability of having at least one parent migrated in 2017 on this restricted sample, using the same vector of covariates x_i as in Table 2, and still measured in 2014. Finally, we estimate the ATET of this placebo treatment on the completed years of schooling in 2014. Under the conditional independence assumption, the exposure to the treatment in 2017 should be independent to the outcome in 2014. Conversely, if treated children differ in unobservables, and this was driving the significant effect shown in Table 3, then even the placebo should be negatively associated to the current outcome. Table 4 presents the results obtained with nearest-neighbour matching (with 5 and 10 matches) and with Kernel matching[18]; the estimated propensity score is presented in data column 1 of Table A4 in the Appendix. The ATET of the placebo treatment is not significant at conventional confidence levels, thus mitigating the concern that the estimates in Table 3 are just picking up a non-random selection on unobservables of the children left behind.

Table 4 around here

This test confirms that parent migration has detrimental effects on children's education, which is subject to further robustness checks in Section 5.2 below. The lack of parental supervision and emotional distress for the children left behind, the substitution of migrant labour, and the higher migration perspectives of left-behind children could be the underlying mechanisms explaining the reduction in child education. Section 5.4 attempts to disentangle these alternative channels.

5.2. Robustness checks

We initially want to check our results with respect to the definition of internal migrant we adopted. As explained in Section 3 above, we defined an individual as an internal migrant if she left the household of

[18] We also estimated for this and for all following models, results with one to four and six to nine matches. For brevity reasons, we did not report the results here, but they are available from the authors upon request.

origin to take or look for a job in a province other than their own, as in Roth and Tiberti (2017). We thus do not include in the main definition of internal migrant those individuals who moved to other parts of Cambodia for non-work-related reasons[19]. Table 5 shows the effect of parental migration on the number of completed years of schooling when individuals leaving for other reasons than work are included in the definition of internal migrants; the estimated propensity score is presented in data column 2 of Table A4 in the Appendix. The bias-adjusted ATET obtained with nearest neighbour matching are very similar to the ones presented in Table 3, our benchmark specification. We can thus conclude that our estimated effect is not influenced by our definition of internal migrants. Moreover, these results let us assume that the absence of a parent who leaves for a job-related reason has a similar effect on children's education as a parent's absence for other reasons.

Table 5 around here

The effect of international parental migration on child education outcomes might be stronger than the effect of internal parental migration, even though international migrants are likely to be sending home more remittances. Internal migrants come back home more regularly and may be able to exert a greater control on the time children spend in school, and on their school performance.[20] Furthermore, children are likely to suffer emotionally less from irregular parent absence than from a prolonged absence. Table 6 reports the effect of parent(s) who migrated abroad on the number of completed years of schooling; the estimated propensity score is presented in column 3 of Table A.4. Looking at the bias-adjusted ATET, it can be seen that international parent(s) migration leads to a reduction in the completed years of schooling of between 0.53 and 0.54 years, a figure that is higher than the point estimate that we obtained when considering children whose parents migrated either internally or internationally.

Table 6 around here

We expect the negative effect of parental migration on children's education outcome to be stronger for older children, for a number of reasons. First, almost all children currently enrol in primary school in rural Cambodia. However, the gross enrolment rate for lower secondary school was 53.3 percent in 2014 (and 20.2 percent for upper secondary school), as mentioned above; we thus expect that primary school enrolment (and so completed years of education for children of primary school age) is less influenced by

[19] As shown in Table A.1, the possible answers to the question 'Why did [name] move to current location?' were (i) to take a job, (ii) to look for a job; (iii) to study; (iv) other.

[20] de Laat (2014) provides evidence of the extensive resources invested by rural-to-urban migrants to control the household members left behind.

any external factor than secondary school enrolment. Second, children perform better at school when adults actively assist them with their homework; migrants' children often co-reside with grandparents, who may be possibly able to help them with primary school homework, but not with secondary school homework. Third, older children can better substitute for adults in farm and family work.

Table 7 shows the effect of parent(s) migration on completed years of schooling for children aged 12 to 17; the estimated propensity score is presented in column 4 of Table A.4. Children aged 12 to 17, who have at least one migrated parent, complete between 1.0 years and 1.25 years less of schooling. The result is robust to the use of different matching methods and number of neighbours.

Table 7 around here

Cortes (2015) suggests that maternal migration is more detrimental to children's education than paternal migration. One of the possible reasons is that in countries with rigid gender roles, like Cambodia, mothers are the primary caregivers for the children and they consecrate more time to domestic activities, including childcare. Maternal migration could thus imply a bigger reduction in parental investment than paternal migration.

In order to explore the possible differential effects, we define the treatment as maternal (paternal) migration, excluding from the sample of untreated the children who do not co-reside with their fathers (mothers), i.e. untreated children are not exposed to parental migration. The bias-adjusted ATET ranges between -0.34 years to -0.40 years for children left behind by mothers, and between -0.26 years and -0.30 years for children left behind by fathers (Table 8 and Table 9). The estimated propensity score is shown in Table A4, columns 5 and 6. In addition, the bias-adjusted ATET is never significant at most commonly used confidence levels when it is the father who migrates. These results confirm the intuition that maternal migration has stronger negative effects on the education of children left behind.

Table 8 around here

Table 9 around here

As a last robustness check, we evaluate if the estimated effect persists over time. In our main specification, we observe schooling outcomes in 2014, and we also have information on schooling outcomes in 2017 for most of the children. Table 10 shows the estimated ATET when we use the years of schooling completed by March 2017 as the outcome; the estimated propensity score is reported in Table A4, column 7. A slightly different sample is used here since we have added 301 children aged 3 to 5 years in 2014 to the

analysis and we did not use children that were not re-interviewed in 2017. Bias-adjusted ATET coefficients are significantly negative, at around 0.6, indicating that the observed effect persists over time.

Table 10 around here

Notice that all results are robust to the exclusion from the sample of all children whose parents passed away, or were not co-residing with them for reasons unrelated to migration (results not presented here but available from the authors upon request).

5.3. Other education outcomes

In previous sections, we showed that children of migrants have less schooling than children whose parents did not migrate. This result can be explained by several alternative and not mutually exclusive mechanisms: (i) migrants' children have a higher probability of dropping out of school early; (ii) they enrol later in primary school; (iii) they are more likely to repeat one or more school years. In this section, we explore the relation between parental migration and other education outcomes to understand if those mechanisms are at work[21].

About 24 percent of sample children had already dropped out school at the time of the survey. Table 11 shows that migrants' children do not have a higher probability of dropping out at any age, but that they have a 15 percent higher probability of dropping out after the age of 11, when the data show a substantial reduction in school enrolment[22].

Tables 11 panels A & B around here

[21] The survey does not provide further information on school performance, as for example test scores or results to national exams, nor on school attendance.

[22] We estimated the propensity score on the same sample and using the same covariates as in the main estimation, presented in Table 3, so the results of the estimation of logit model are reported in Table 2.

Unfortunately, the survey does not provide information on late enrolment at primary school (which should start at age 6 in Cambodia) and on repeated years. Thus we follow Cortes (2015) and we look at the probability for the children who are still enrolled in school of lagging behind, i.e. of being enrolled in a year group below the one expected for their age[23]. Our outcome is a dummy variable equal to one if the difference between age of the child minus six and the year group in which she is currently enrolled is higher than one. About 30 percent of the sample children still enrolled at school are behind their expected year group. An enrolment in a level below the expected age could be due to a late enrolment or to a year repetition. Table 12 shows that migrants' children do not lag behind untreated children at all ages, but they do have a 20 percent higher probability of lagging behind after the age of 11. The estimated propensity score is reported in Table A4, columns 8 and 9.

Tables 12 panels A & B around here

This set of results thus suggests that the lower number of years of schooling of treated children appears to reflect a higher probability of lagging behind or dropping out of school after the age of 11.

We also looked at the probability for children older than 11 to have completed primary school. About 45 percent of children over 11 have completed primary school; this percentage drops to 29 percent for migrants' children. Table 13 shows that the probability of completing primary school is about 12 percent lower for the migrants' children, albeit the difference is marginally not significant when we use the nearest neighbour matching (Table 13)[24].

Tables 13 around here

5.4. Which channels are at work?

The results presented above provide robust evidence for the negative effect of parental migration on child education. However, they do not help us to understand if this effect works through the reduction in the direct parental investment in children education or through the child work channel. Parental absence can lead to a reduction in the time adults devote to children's education, which is an important input in the education production function. If the child work channel is operating, children could replace the migrant

[23] Cortes (2015) combines this outcome with the drop out to measure the probability to lag behind in school. Her main outcome is a dummy variable that takes a value of one if the child has dropped out of school or if she is enrolled in a level below the expected for her age.

[24] We estimated the propensity score on the same sample and using the same covariates as in the estimations reported in Table 7, so the results of the estimation of logit model are reported in Table A4, column 4.

parent in family or farm work, with negative consequences on school attendance and performance. In this paragraph, we test the existence of these channels (i) estimating the effect of parent(s) migration on child work, and (ii) estimating the effect of the migration of any other adult member of the household except parents on children education.

We define a child as working if she is economically active, either within or outside the household[25]. 32 percent of children aged 6 to 17 are economically active, but the percentage decreases to 14.5 percent for children with at least one migrated parent. Most of the children work within the households, with only 14 percent of children being engaged in 'active labour', i.e. they work outside the household.

Table 14 shows the effect of parent(s) migration on child work[26]. The bias adjusted ATET is never significant at conventional confidence levels, suggesting that parent(s) migration has no effect on the likelihood of being engaged in economic activities for the children left behind.

Table 14 around here

We also estimate the effect of the migration of any other member of the households. If the child work channel is operating, a worsening of education outcomes should be observed not only when parents migrate, but also when any other working age member migrates (besides parents, other migrants are mostly brothers or sisters of the children). The result, presented in table 15 (logit model presented in Table A4, column 10), show that the bias adjusted ATET is never significant at conventional confidence levels. It indicates that there is no negative effect on education of the children when it is not the parent who migrates.

Table 15 around here

These two tests allow us to conclude that the negative effect of parent(s) migration on children' schooling outcome does not pass through the child work channel - at least on an extensive margin[27], but it is rather

[25] The question states as follows: "Is the member economically active?" We define the child as economically active if she replies to be engaged in "active labor", in "family work/house work" or in "study and work", or if she "can do some work".

[26] We estimated the propensity score on the same sample and using the same covariates as in the main estimation, presented in Table 3, so the results of the estimation of logit model are reported in Table 2.

[27] These two tests do not completely rule out that the negative effect of parents' migration on children' schooling outcome does not pass through the child work channel because changes in child work could not only occur on the extensive margin but also on the intensive margins as in Chen (2013). In order to test if child work increases on the intensive margin, we would need information on the time use of children. Unfortunately, the survey asks the number of hours of work only if the child is employed in 'active labour', while the majority of economically active children

explained by the parental investment channel. The fact that it is not the migration of any adult member but the migration of a parent which is detrimental for children education, suggests that the parental investment channel is at work. This idea is also corroborated from the data on the household composition of children left behind. 73 percent of treated children live in a household where the head is a grandparent, while the percentage stands at 13 percent for untreated children. Moreover, the average age of adult household members is 49.6 for migrants' children and 40.3 for the other children. This implies that migrants' children are more likely to co-reside with elderly people, who are less educated than parents. If caregivers of the children left behind are less educated than the parents, this could have negative effect on their schooling.

Finally, a concurrent channel could be, as in McKenzie and Rapoport (2011) and de Brauw and Giles (2017), that children of migrants perceive a lower return to schooling because parental migration increases the potential for their own migration in the future and because the return to education is lower at destination. Indeed, the migrant status of the parents in 2014 is positively and significantly related to the migrant status of the children in 2017 in a regression when we also control for the age of the children and village dummies (results not shown here but available from the authors upon request). This could indicate that the perspective of migration channel could work with lower parental investment to explain the lower schooling outcomes of migrants' children.

6. Conclusion

Growing rural-to-urban and international migration flows have sparked concerns about the investments in education of the children left behind in Cambodia. Children whose parents migrated, internationally or domestically, could suffer from the parent's absence, which implies a reduction in the time adults devote to children's education. They could also be asked to replace the migrant parent in family or farm work. Both situations could have negative consequences on their school attendance and performance.

In this paper, we draw on a panel household-level survey conducted in rural villages in 2014 and 2017 to analyse the relationship between parental migration and child schooling. The analysis reveals that children of migrant parents lag significantly behind in terms of completed years of schooling; the estimated average effect is about half a year, a quite substantial effect in a country where the average number of

are rather employed in household chores or in family work, as showed in Table 1. This does not allow us to verify if migrants' children are spending more time in non-income generating activities.

completed years of schooling is 7[28]. The effect we identify is not sensitive to the definition of internal migration we use. A placebo test, in which we tested whether the future (2017) migration status of parents who did not migrate in the 2014 wave of the survey significantly correlates with the educational outcomes of the children in 2014, allows us to conclude that our results cannot be explained by the non-random selection on time-invariant unobservables of migrant households.

We find a stronger effect for children aged 12 to 17, consistently with Giannelli and Mangiavacchi (2010) and McKenzie and Rapoport (2011), and for children exposed to maternal migration, in line with Cortes (2015). The estimated reduction in schooling is also larger for children whose parents migrated abroad. The effect persists over time, as schooling in 2017 is also affected by parental migration observed in 2014. The lower number of years of schooling of treated children appears to reflect a higher probability of lagging behind or dropping out of school after the age of 11. To understand what factors explain the negative effect on schooling that we identify, we analyse the relationship between parental migration and child work, and we find that child work does not increase at the extensive margin. This finding, coupled with the fact that no negative effect on education is observed for children who experienced the migration of other adult former household members, suggests that the negative effect that we find is mostly related to the detrimental effect of parental absence, rather than the increase in child work. Our data also indicate that the perspective of migration channel could concur to explain the lower schooling outcomes of migrants' children.

Our results confirm that the concern for the education outcomes of left behind children is well grounded. Since the lack of parental support seems to be one of the main factors which explains the negative effect of parental migration on children's education outcome, policymakers could probably consider the setup of a special education programme to support children left behind in rural Cambodia. This programme could be partially funded by remittance flows.

[28] Source: *CDRI Household Survey 2014*. Answer to the question "how many years(?) has …. attended school?", that is asked to all individuals who attended some school. It does not take into account the 20 percent of the population who never attended school.

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Tables

Table 1. Descriptive Statistics

Variables	Treated	Non-treated	t-test
Years of schooling	2.362	3.543	-1.180***
Dropout	0.145	0.243	-0.098**
Lagged behind (for kids still enrolled)	0.292	0.310	-0.018
Completed primary (for kids aged 6-17)	0.289	0.455	-0.166***
Economically active	0.145	0.329	-0.184***
In active labor	0.048	0.147	-0.098***
Consumption per capita	98.798	142.093	-43.294***
Consumption per capita, 1st quartile	0.362	0.175	0.187***
Consumption per capita, 2nd quartile	0.282	0.228	0.053
Consumption per capita, 3rd quartile	0.169	0.285	-0.116***
Consumption per capital, 4th quartile	0.185	0.310	-0.125***
Household size	6.467	6.242	0.225
Number of working age members	3.241	3.555	-0.313**
Dependency ratio	1.202	0.942	0.260***
Share of female working age members	0.515	0.605	-0.090***
Age of mother	33.418	41.155	-7.737***
Mother, any education	0.241	0.283	-0.041
Mother, primary not completed	0.524	0.577	-0.053
Mother, primary completed	0.233	0.138	0.095***
Age of father	32.940	43.262	-10.322***
Father, any education	0.137	0.191	-0.054
Father, primary not completed	0.443	0.524	-0.080*
Father, primary completed	0.419	0.283	0.135***
Gender, boy	0.459	0.518	-0.058
Age	10.314	11.776	-1.461***
Observation	124	1,537	

Source: Authors' elaboration from *CDRI Household Survey* 2014.

Table 2. Estimation of the propensity score, logit model. Outcome: parental migration.

	Parent(s) migrated
Consumption per capita, 2nd quartile	-0.411
	(0.310)
Consumption per capita, 3rd quartile	-1.322***
	(0.339)
Consumption per capita, 4th quartile	-1.473***
	(0.347)
Household Size	-0.244
	(0.147)
Number of working age members	0.267
	(0.257)
Dependency ratio	-0.153
	(0.293)
Share of female working age members	-1.344*
	(0.673)
Age of the Mother	0.00530
	(0.0241)
Mother, primary not completed	-0.370
	(0.309)
Mother, primary completed	-0.294
	(0.391)
Age of the Father	-0.157***
	(0.0239)
Father, primary not completed	-0.205
	(0.375)
Father, primary completed	0.319
	(0.396)
Gender, boy	-0.0469
	(0.244)
Age dummies	Yes
Village dummies	Yes
Number of observations	1,507
Pseudo R^2	0.306
$\chi^2(24)$	262.6

Notes: t statistics in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The dependent variable is a dummy taking the value of one for the children having at least 1 parent who migrated. The estimation is conducted for the sample of 1,661 children aged 6 to 17 in 2014. 154 observation from the village of Bos have been dropped from the estimation because no children in the village was treated.

Source: authors' elaboration from *CDRI Household Survey* 2014.

Table 3. The effect of parental migration in 2014 on completed years of schooling in 2014.

Non-matched samples					
	Children				
	Treated	Non-treated	Pseudo-R^2	Difference in completed years of schooling	
	124	1,537	0.3065	-1.180***	
				(0.248)	
Matched samples					
	Children				
n	Treated	Matched controls	*Pseudo-R^2*	ATET	ATET bias-adj.
1	120	90	0.0876	-0.108	-0.340*
				(0.304)	(0.191)
2	120	155	0.0318	-0.25	-0.40**
				(0.291)	(0.157)
3	120	210	0.0372	-0.275	-0.376**
				(0.273)	(0.155)
4	120	256	0.0299	-0.270	-0.376**
				(0.265)	(0.149)
5	120	292	0.0233	-0.221	-0.355**
				(0.260)	(0.148)
6	120	327	0.0262	-0.227	-0.350**
				(0.260)	(0.146)
7	120	356	0.0221	-0.216	-0.338**
				(0.254)	(0.143)
8	120	384	0.0217	-0.170	-0.306**
				(0.255)	(0.144)
9	120	411	0.0204	-0.215	-0.305**
				(0.256)	(0.142)
10	120	431	0.0200	-0.229	-0.314**
				(0.254)	(0.143)
Kernel	115	1,177	-	-0.230	-0.312**
				(0.266)	(0.146)

Notes: Standard errors are in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The treatment is having at least 1 parent who migrated in 2014. The outcome is the number of the completed years of schooling in 2014. The sample include 1,661 children aged 6 to 17 in 2014. The estimation is conducted using the nearest neighbour matching with replacement, with a number of matching varying between 1 and 10. n represents the number of nearest neighbours used in the matching. We also report the result of the Kernel matching. The *pseudo-*R^2 is derived from the re-estimation of propensity score on the sample of matched children only.

Source: authors' elaboration from *CDRI Household Survey* 2014.

Table 4. Falsification test: the effect of migration in 2017 on completed years of schooling in 2014

Non-matched samples				
	Children			
	Treated (2017)	Non treated	*Pseudo-R^2*	Difference in completed years of schooling
	36	1,086	0.3086	-0.105
				(0.444)

Matched samples					
	Children				
n	Treated (2017)	Matched control	*Pseudo-R^2*	ATET	ATET bias-adj.
5	32	98	0.0555	0.243	-0.008
				(0.569)	(0.271)
10	32	157	0.0493	0.096	-0.090
				(0.526)	(0.227)
Kernel	30	320	-	0.210	-0.096
				(0.521)	(0.259)

Notes: Standard errors are in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The treatment is having at least 1 parent who migrated in 2017. The outcome is the number of the completed years of schooling in 2014. The sample includes 1,121 children aged 6 to 17 in 2014 whose parents were not reported to have migrated in 2014 and who were re-interviewed in 2017. The estimation is conducted using the nearest neighbour matching with replacement, with a number of matching varying between 1 and 10. n represents the number of nearest neighbours used in the matching. We also report the result of the Kernel matching. Stata © does not estimate the standard errors of the ATET bias-adjusted coefficient with Kernel matching when age dummies are included as controls; we thus replaced them with 2-years age cohorts. is not The *pseudo-R^2* is derived from the re-estimation of propensity score on the sample of matched children only. Source: authors' elaboration from *CDRI Household Survey* 2014.

Table 5. The effect of parental migration in 2014 on completed years of schooling in 2014, alternative definition for internal migrants.

Non-matched samples				
	Children			
	Treated	Non-treated	Pseudo-R^2	Difference in completed years of schooling
	297	1,367	0.2665	-0.538***
				(0.171)

Matched samples					
	Children				
n	Treated	Matched control	Pseudo-R^2	ATET	ATET bias-adj.
5	291	557	0.0339	-0.042	-0.298**
				(0.245)	(0.150)
10	291	766	0.0227	-0.115	-0.293**
				(0.232)	(0.145)
Kernel	271	1,194	-	-0.152	-0.353***
				(0.232)	(0.125)

Notes: Standard errors are in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The treatment is having at least 1 parent who migrated in 2014. The outcome is the number of the completed years of schooling in 2014. The sample includes 1,661 children aged 6 to 17 in 2014. Differently from estimations in Table 3, here we include the children whose parents migrated to other Cambodian provinces for other reasons than work in the group of treated. The estimation is conducted using the nearest neighbour matching with replacement, with a number of matching varying between 1 and 10. n represents the number of nearest neighbours used in the matching. We also report the result of the Kernel matching. The *pseudo-*R^2 is derived from the re-estimation of propensity score on the sample of matched children only. Source: authors' elaboration from *CDRI Household Survey* 2014.

Table 6. The effect of parental migration in 2014 on completed years of schooling in 2014, international migrants only.

Non-matched samples				
	Children			
	Treated	Non-treated	Pseudo-R^2	Difference in completed years of schooling
	77	1,584	0.3948	-1.348***
				(0.311)

Matched samples					
	Children				
n	Treated	Matched control	Pseudo-R^2	ATET	ATET bias-adj.
5	74	152	0.0361	-0.370	-0.502**
				(0.320)	(0.196)
10	74	236	0.0231	-0.345	-0.535***
				(0.307)	(0.182)
Kernel	72	1,130	-	-0.034	-0.415**
				(0.363)	(0.187)

Notes: Standard errors are in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The treatment is having at least 1 parent who migrated abroad in 2014. The outcome is the number of the completed years of schooling in 2014. The sample includes 1,661 children aged 6 to 17 in 2014. The estimation is conducted using the nearest neighbour matching with replacement, with a number of matching varying between 1 and 10. n represents the number of nearest neighbours used in the matching. We also report the result of the Kernel matching. The *pseudo-*R^2 is derived from the re-estimation of propensity score on the sample of matched children only. Source: authors' elaboration from *CDRI Household Survey* 2014.

Table 7. The effect of parental migration in 2014 on completed years of schooling in 2014, children aged 12 to 17.

Non-matched samples				
	Children			
	Treated	Non-treated	Pseudo-R^2	Difference in completed years of schooling
	45	850	0.4001	-1.036***
				(0.358)

Matched samples					
	Children				
n	Treated	Matched control	Pseudo-R^2	ATET	ATET bias-adj.
5	33	110	0.0599	-1.018**	-1.253***
				(0.445)	(0.396)
10	33	172	0.0254	-1.133**	-1.092***
				(0.447)	(0.357)
Kernel	31	408	-	-1.372***	-1.396***
				(0.484)	(0.344)

Notes: Standard errors are in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The treatment is having at least 1 parent who migrated in 2014. The outcome is the number of the completed years of schooling in 2014. The sample includes 895 children aged 12 to 17 in 2014. The estimation is conducted using the nearest neighbour matching with replacement, with a number of matching varying between 1 and 10. n represents the number of nearest neighbours used in the matching. We also report the result of the Kernel matching. The *pseudo-*R^2 is derived from the re-estimation of propensity score on the sample of matched children only. Source: authors' elaboration from *CDRI Household Survey* 2014.

Table 8. The effect of maternal migration in 2014 on completed years of schooling in 2014.

Non-matched samples				
	Children			
	Treated	Non-treated	Pseudo-R^2	Difference in completed years of schooling
	66	1,388	0.3343	-1.082***
				(0.338)

Matched samples					
	Children				
n	Treated	Matched control	Pseudo-R^2	ATET	ATET bias-adj.
5	62	170	0.0406	-0.035	-0.347
				(0.362)	(0.229)
10	62	270	0.0299	-0.187	-0.359
				(0.349)	(0.220)
Kernel	58	633	-	-0.494	-0.378*
				(0.348)	(0.209)

Notes: Standard errors are in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The treatment is having mother migrated in 2014. The outcome is the number of the completed years of schooling in 2014. We exclude from the sample of untreated children those who do not co-reside with their fathers. The sample includes 1,454 children aged 6 to 17 in 2014. The estimation is conducted using the nearest neighbour matching with replacement, with a number of matching varying between 1 and 10. n represents the number of nearest neighbours used in the matching. We also report the result of the Kernel matching. The *pseudo-*R^2 is derived from the re-estimation of propensity score on the sample of matched children only. Source: authors' elaboration from *CDRI Household Survey* 2014.

Table 9. The effect of paternal migration in 2014 on completed years of schooling in 2014.

Non-matched samples				
	Children			
	Treated	Non-treated	Pseudo-R^2	Difference in completed years of schooling
	95	1,484	0.3920	-1.210***
				(0.283)

Matched samples					
	Children				
n	Treated	Matched control	Pseudo-R^2	ATET	ATET bias-adj.
5	91	198	0.0363	-0.085	-0.265
				(0.327)	(0.190)
10	91	300	0.0342	-0.126	-0.265
				(0.322)	(0.182)
Kernel	84	977	-	-0.375	-0.269
				(0.392)	(0.175)

Notes: Standard errors are in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The treatment is having father migrated in 2014. The outcome is the number of the completed years of schooling in 2014. We exclude from the sample of untreated children those who do not co-reside with their mothers. The sample includes 1,579 children aged 6 to 17 in 2014. The estimation is conducted using the nearest neighbour matching with replacement, with a number of matching varying between 1 and 10. n represents the number of nearest neighbours used in the matching. We also report the result of the Kernel matching. The *pseudo-*R^2 is derived from the re-estimation of propensity score on the sample of matched children only. Source: authors' elaboration from *CDRI Household Survey* 2014.

Table 10. The effect of parental migration in 2014 on completed years of schooling in 2017

Non-matched samples				
	Children			
	Treated	Non-treated	Pseudo-R^2	Difference in completed years of schooling
	88	1,334	0.3346	-1.549***
				(0.348)

Matched samples					
	Children				
n	Treated	Matched control	Pseudo-R^2	ATET	ATET bias-adj.
5	86	203	0.0448	-0.516	-0.659**
				(0.390)	(0.263)
10	86	318	0.0235	-0.587	-0.617***
				(0.374)	(0.236)
Kernel	80	958	-	-0.711*	-0.611***
				(0.379)	(0.220)

Notes: Standard errors are in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The treatment is having at least 1 parent who migrated in 2014. The outcome is the number of the completed years of schooling in 2017. We exclude from the sample used in the estimations shown in Table 3 the children that were not re-interviewed in 2017 and we add children aged 3 to 5 years in 2017. The sample includes 1,422 children aged 6 to 20 in 2017. The estimation is conducted using the nearest neighbour matching with replacement, with a number of matching varying between 1 and 10. n represents the number of nearest neighbours used in the matching. We also report the result of the Kernel matching. The *pseudo*-R^2 is derived from the re-estimation of propensity score on the sample of matched children only. Source: authors' elaboration from *CDRI Household Survey* 2014.

Table 11. The effect of parental migration in 2014 on school dropout in 2014.

Panel A : children aged 6 to 17

Non-matched samples					
	Children				
	Treated	Non-treated	Pseudo-R^2	Difference in school dropout	
	124	1,537	0.3065	-0.098**	
				(0.039)	
Matched samples					
	Children				
n	Treated	Matched control	Pseudo-R^2	ATET	ATET bias-adj.
5	120	292	0.0233	-0.005	-0.011
				(0.042)	(0.039)
10	120	431	0.0200	-0.011	-0.017
				(0.040)	(0.037)
Kernel	115	1,177	-	-0.015	-0.011
				(0.045)	(0.035)

Panel B: children aged 12 to 17

Non-matched samples					
	Children				
	Treated	Non-treated	Pseudo-R^2	Difference in school dropout	
	45	850	0.3065	-0.046	
				(0.072)	
Matched samples					
	Children				
n	Treated	Matched control	Pseudo-R^2	ATET	ATET bias-adj.
5	33	110	0.0599	0.169*	0.168**
				(0.096)	(0.077)
10	33	172	0.0254	0.130	0.146**
				(0.089)	(0.071)
Kernel	31	408	-	0.224**	0.208***
				(0.096)	(0.072)

Notes: Standard errors are in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The treatment is having at least 1 parent who migrated in 2014. The outcome is a dummy variable that takes the value of one if the child is not enrolled at school in 2014. For panel A, the sample includes 1,661 children aged 6 to 17 in 2014. For panel B, the sample includes 895 children aged 12 to 17 in 2014. The estimation is conducted using the nearest neighbour matching with replacement, with a number of matching varying between 1 and 10. n represents the number of nearest neighbours used in the matching. We also report the result of the Kernel matching. The *pseudo-*R^2 is derived from the re-estimation of propensity score on the sample of matched children only. Source: authors' elaboration from *CDRI Household Survey* 2014.

Table 12. The effect of parental migration in 2014 on the probability to lag behind at school in 2014.

Panel A: children aged 6 to 17

Non-matched samples				
	Children			
	Treated	Non-treated	Pseudo-R^2	Difference in lag behind
	106	1163	0.3159	-0.017
				(0.046)

Matched samples					
	Children				
n	Treated	Matched control	Pseudo-R^2	ATET	ATET bias-adj.
5	103	238	0.0275	0.036	0.054
				(0.060)	(0.052)
10	103	353	0.0209	0.042	0.055
				(0.057)	(0.050)
Kernel	100	880	-	0.069	0.075
				(0.056)	(0.047)

Panel B: children aged 12 to 17

Non-matched samples				
	Children			
	Treated	Non-treated	Pseudo-R^2	Difference in lag behind
	32	565	0.3159	0.052
				(0.090)

Matched samples					
	Children				
n	Treated	Matched control	Pseudo-R^2	ATET	ATET bias-adj.
5	20	56	0.1758	0.23*	0.203**
				(0.130)	(0.097)
10	20	88	0.1271	0.22*	0.198*
				(0.122)	(0.107)
Kernel	19	200	-	0.230*	0.219
				(0.130)	()

Notes: Standard errors are in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The treatment is having at least 1 parent who migrated in 2014. The outcome is a dummy variable that takes the value of one if the child is enrolled in a level below the expected for her age. Only children enrolled in school at the time of the survey are included in the estimations. For panel A, the sample includes 1,269 children aged 6 to 17 in 2014. For panel B, the sample includes 597 children aged 12 to 17 in 2014. The estimation is conducted using the nearest neighbour matching with replacement, with a number of matching varying between 1 and 10. n represents the number of nearest neighbours used in the matching. We also report the result of the Kernel matching. Stata © does not estimate the standard errors of the ATET bias-adjusted coefficient with Kernel matching. The *pseudo-*R^2 is derived from the re-estimation of propensity score on the sample of matched children only. Source: authors' elaboration from *CDRI Household Survey* 2014.

Table 13. The effect of parental migration in 2014 on the probability of having completed primary school in 2014.

Non-matched samples				
	Children			
	Treated	Non-treated	Pseudo-R^2	Difference in completed years of schooling
	45	850	0.4001	-0.166**
				(0.075)

Matched samples					
	Children				
n	Treated	Matched control	Pseudo-R^2	ATET	ATET bias-adj.
5	33	110	0.0599	-0.090	0.124
				(0.097)	(0.085)
10	33	172	0.0254	-0.136	0.117
				(0.095)	(0.077)
Kernel	31	408	-	-0.189*	-0.162**
				(0.108)	(0.073)

Notes: Standard errors are in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The treatment is having at least 1 parent who migrated in 2014. The outcome is a dummy variable that takes the value of one if the child has completed primary school. The sample includes 895 children aged 12 to 17 in 2014. The estimation is conducted using the nearest neighbour matching with replacement, with a number of matching varying between 1 and 10. n represents the number of nearest neighbours used in the matching. We also report the result of the Kernel matching. The *pseudo-*R^2 is derived from the re-estimation of propensity score on the sample of matched children only. Source: authors' elaboration from *CDRI Household Survey* 2014.

Table 14. The effect of parental migration in 2014 on child work in 2014.

Non-matched samples				
	Children			
	Treated	Non-treated	Pseudo-R^2	Difference in child work
	124	1,537	0.3065	-0.184***
				(0.043)

Matched samples					
	Children				
n	Treated	Matched control	Pseudo-R^2	ATET	ATET bias-adj.
5	120	292	0.0233	0.011	0.002
				(0.041)	(0.033)
10	120	431	0.0200	0.001	-0.007
				(0.040)	(0.033)
Kernel	115	1,177	-	0.012	-0.002
				(0.040)	(0.032)

Notes: Standard errors are in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The treatment is having at least 1 parent who migrated in 2014. The outcome is a dummy that takes the value of one if the child is economically active. The sample includes 1,661 children aged 6 to 17 in 2014. The estimation is conducted using the nearest neighbour matching with replacement, with a number of matching varying between 1 and 10. n represents the number of nearest neighbours used in the matching. We also report the result of the Kernel matching. The *pseudo-*R^2 is derived from the re-estimation of propensity score on the sample of matched children only. Source: authors' elaboration from *CDRI Household Survey* 2014.

Table 15. The effect of other household members' migration in 2014 on completed years of schooling in 2014.

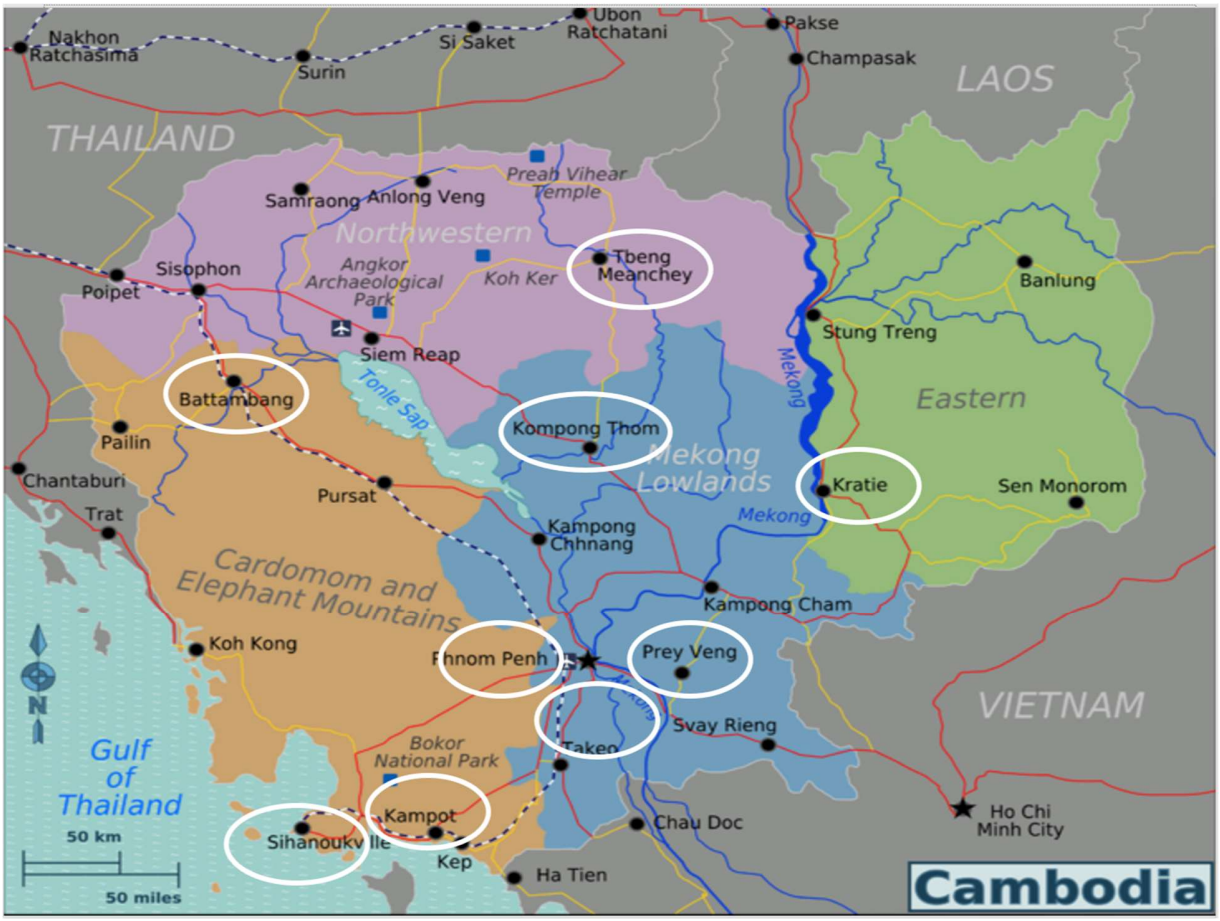
Non-matched samples				
	Children			
	Treated	Non-treated	Pseudo-R^2	Difference in completed years of schooling
	534	1,227	0.4955	0.359**
				(0.141)

Matched samples					
	Children				
n	Treated	Matched control	Pseudo-R^2	ATET	ATET bias-adj.
5	492	401	0.0802	-0.755**	0.072
				(0.313)	(0.194)
10	492	513	0.0769	-0.548*	-0.005
				(0.294)	(0.976)
Kernel	483	909	-	-0.781***	0.106
				(0.284)	(0.164)

Notes: Standard errors are in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The treatment is having at least 1 family member other than the parents who migrated in 2014. The outcome is the number of completed years of schooling in 2014. The sample includes 1,661 children aged 6 to 17 in 2014. The estimation is conducted using the nearest neighbour matching with replacement, with a number of matching varying between 1 and 10. n represents the number of nearest neighbours used in the matching. We also report the result of the Kernel matching. The *pseudo-*R^2 is derived from the re-estimation of propensity score on the sample of matched children only. Source: authors' elaboration from *CDRI Household Survey* 2014.

Appendix

Figure A.1. Map of survey locations



Source: Authors' elaboration.

Table A.1. Migration module

In addition to the person living in your households, are there any other (spouse or son/daughter), 15 years and older, who previously has been a member of your household but hasn't appeared in this household after September 2013 (Pchum) or no longer living in this household?

No	Name (first names only)	Relationship with hh head (codes below)	Sex 1=M 2=F	Age (year)	Marital status (codes below)	Education (Highest grade-number of year)	Where is [NAME] currently living? (code)	What year did [NAME] move to current location? (month/year)	Why did [NAME] move to current location (code)	What is [NAME] main occupation now? (code)	Have [NAME] send money home in the last 6 month? 1=Yes 2=No (ask the following member)	What is the total cost of the transfers and cash gifts that [NAME] has send to the household in the last 6 months? (moeun riels)	Through what means/channels do you/does your household receive the money? (code) (Primary means)
1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	**11**	**12**	**13**	**14**
20													
21													
22													
23													
24													
25													
26													
27													

Code IB: Question 3: 1= Household head, 2= Husband/wife, 3= Sister-/brother (in-law, sibling), 4= Son or daughter (adopted), 5= Son-/daughter-in-law, 6= Grandchild, 7= Stepchild, 8= Parent (in-law), 9= Grandparent, 10= Niece/Nephew, 11= other (specify)

Question 6: 1= Married, 2= Single, 3= Divorced, 4= Widow/Widower, 5= Deserted

Question 8: 1=Phnom Penh 2=other part of Cambodia 3=Thailand 4=Other countries (specify)

Question 10: 1=To take a job 2=To look for a job 3=To study 4=Other

Question 14: 1=Western Union 2=Bank transfer 3=From the person/by other person 4=Other (specify) 5=Wing

Table A.2. Descriptive statistics of main CDRI 2014 variables comparing with CSES 2014 data

Variable	CSES 2014	CDRI 2014
Household size	4.43	4.92
Share of female working age members	0.54	0.53
Number of working age members	2.93	3.12
Dependency ratio	0.69	0.77
Education attainment of household members		
No education	3.77	3.82
Primary school not completed	45.25	59.87
Upper Secondary not completed	46.11	32.01
Higher education	2.46	3.04
Vocational training	2.38	0.39
Others	0.00	0.05
Don't know	0.03	0.81
Population by age		
0-14 years old	30.31	31.66
15-64 years old	64.22	61.53
65 and above	5.48	6.82

Source: Authors' elaboration from *Cambodian Social Economic Survey* (CSES) 2014 and *CDRI Household Survey* 2014.

Table A.3. Descriptive statistics of main variables comparing with attrited households

Variable	Attrited (n=79)	Non attrited (n=1104)	t-test
Household size	4.51	4.95	-1.82
Female ratio	0.50	0.54	-1.53
Male ratio	0.55	0.50	2.77
Number of working age members	2.83	3.15	-1.77
Dependency ratio	0.71	0.78	-0.78
Food consumption (0000' riels/year)	395.93	446.32	-1.91
Non-Food consumption (0000' riels/year)	446.89	466.80	-0.34
Education attainment of household members			
No education	5.04	3.75	
Primary school not completed	61.63	59.76	
Upper Secondary not completed	31.01	32.08	
Higher education	1.94	3.16	
Vocational	0	0.42	
Don't know	0.39	0.84	
Population by age			
0-14 years old	34.27	31.49	
15-64 years old	61.24	61.55	
65 and above	4.49	6.97	

Source: Authors' elaboration from *CDRI Household Survey* 2014 and 2017.

Table A.4: Estimation of the propensity score, logit models

	(1) Falsification test (table 4)	(2) Alternative definition for internal migrants (table 5)	(3) International migrants only (table 6)	(4) Children 12-17 (table 7)	(5) Maternal migration (table 8)	(6) Parental migration (table 9)	(7) Outcome schooling in 2017 (table 10)	(8) Lagged behind (table 12A)	(9) Lagged behind 12-17 (table 12B)	(10) Migration of other household members (table 15)
Consumption per capita, 2nd quartile	-0.0282	-1.195***	-0.664	-1.805**	-0.371	-0.732	-0.164	-0.253	-2.593**	-0.034
	(0.766)	(0.213)	(0.421)	(0.622)	(0.428)	(0.389)	(0.399)	(0.353)	(0.941)	(0.262)
Consumption per capita, 3rd quartile	-1.487*	-2.322***	-1.351**	-2.132***	-2.449***	-1.629***	-1.290**	-1.303***	-2.765***	-0.091
	(0.753)	(0.237)	(0.425)	(0.590)	(0.591)	(0.411)	(0.425)	(0.389)	(0.815)	(0.261)
Consumption per capita, 4th quartile	-1.643*	-2.932***	-1.547***	-2.023***	-2.287***	-1.673***	-1.699***	-1.280**	-2.462**	-0.794***
	(0.837)	(0.261)	(0.468)	(0.566)	(0.521)	(0.429)	(0.467)	(0.394)	(0.809)	(0.270)
Household Size	-0.989**	-0.550***	0.102	0.677	-0.522*	-0.0772	-0.0875	-0.232	1.366*	-1.264***
	(0.373)	(0.103)	(0.209)	(0.425)	(0.225)	(0.199)	(0.223)	(0.161)	(0.669)	(0.113)
Number of working age members	1.057	0.700***	-0.109	-1.708*	0.518	0.00366	-0.0154	0.241	-3.139*	2.697***
	(0.570)	(0.164)	(0.379)	(0.818)	(0.358)	(0.357)	(0.403)	(0.284)	(1.350)	(0.180)
Dependency ratio	0.408	1.014***	-1.100*	-3.621**	0.480	-0.588	-0.667	-0.0868	-5.456**	2.606***
	(0.637)	(0.213)	(0.521)	(1.232)	(0.428)	(0.493)	(0.513)	(0.311)	(1.837)	(0.238)
Share of female working age members	-1.485	0.365	-2.827**	-1.946	0.982	-2.967**	-1.348	-1.091	-1.321	-2.582***
	(1.281)	(0.430)	(0.986)	(1.003)	(0.952)	(0.926)	(0.842)	(0.800)	(1.467)	(0.455)
Age of the Mother	0.00493	0.0545**	0.0317	-0.0103	0.0190	0.0181	0.00591	-0.00229	-0.0491	0.016
	(0.0520)	(0.0184)	(0.0315)	(0.0437)	(0.0342)	(0.0288)	(0.0308)	(0.0270)	(0.0643)	(0.017)
Mother, primary not completed	-0.729	0.0924	-1.251**	-0.285	0.731	-0.779*	-0.123	-0.526	-0.814	0.373*
	(0.589)	(0.199)	(0.399)	(0.509)	(0.492)	(0.369)	(0.392)	(0.340)	(0.653)	(0.217)
Mother, primary completed	-0.228	0.231	-0.129	0.354	1.516*	-0.491	-0.623	-0.489	0.867	0.447
	(0.759)	(0.279)	(0.499)	(0.710)	(0.601)	(0.473)	(0.527)	(0.432)	(0.912)	(0.291)
Age of the Father	-0.151**	-0.125***	-0.178***	-0.172***	-0.164***	-0.204***	-0.172***	-0.154***	-0.144*	0.021
	(0.0511)	(0.0176)	(0.0315)	(0.0433)	(0.0328)	(0.0296)	(0.0308)	(0.0275)	(0.0619)	(0.016)
Father, primary not completed	-0.676	1.311***	-0.136	-0.136	-0.741	0.0170	-0.556	-0.190	0.650	0.211

	(0.665)	(0.277)	(0.430)	(0.690)	(0.520)	(0.452)	(0.466)	(0.415)	(1.002)	(0.249)
Father, primary completed	0.383	1.182***	-0.441	0.399	-0.156	0.570	0.402	0.307	1.034	0.591*
	(0.720)	(0.309)	(0.499)	(0.708)	(0.577)	(0.474)	(0.500)	(0.442)	(1.036)	(0.279)
Gender, boy	0.567	-0.251	-0.237	-0.167	-0.457	-0.164	-0.155	-0.122	-0.308	0.335*
	(0.470)	(0.167)	(0.324)	(0.409)	(0.364)	(0.293)	(0.296)	(0.283)	(0.576)	(0.172)
Age dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Village dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	732	1664	1291	756	1091	1315	1184	1153	502	1661
Pseudo R^2	0.309	0.266	0.395	0.400	0.334	0.392	0.335	0.316	0.480	0.495

Notes: t statistics in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Source: authors' elaboration from *CDRI Household Survey* 2014.