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Estimating the Economic Effects of Remittances on the Left-Behind in Cambodia

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Abstract

Using propensity score matching with the 2009 Cambodia Socio-Economic Survey of households, the study examines the effects of remittances on a number of households' well-being indicators: poverty, consumption and labour participation of non-migrant members. The theoretical framework is built upon a "new economics of labour migration" hypothesising that the emigration decision is jointly determined by households and individual migrants and that remittances are contractual arrangements between them. The results indicate that households with at least one migrant member and receive remittances could reduce their headcount poverty rate by 3-7 percentage points vis-à-vis their matched controls. Remittances also reduce depth and severity of poverty of treated households. On the contrary, remittances generate a "dependency effect" due to reduced weekly hours worked of 5-9 percent by adult working age who are employed. The impact of remittances on labour participation and salary income is, however, vulnerable to unobservable factors.

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1. Introduction

Migration in Cambodia is an old phenomenon but has recently received increased attention from the government and development partners to find solutions and to design suitable policies and regulations to protect migrants and to ensure that this socioeconomic occurrence creates socially optimal results. Cambodia is among the top four remittance-receiving countries in ASEAN, behind the Philippines, Vietnam and Timor-Leste (IFAD 2013: 12). Remittances in 2008 contributed about 3.4 percent of Cambodia's gross domestic product, equivalent to USD325 million. However, remittances decreased to USD256 million in 2012, only 1.8 percent of GDP (Hing and Lun 2011: 89; IFAD 2013; World Bank 2011). Although remittances as a percentage of GDP are still insignificant, Cambodia is expected to have a growing flow of migrant workers both internally and cross-border, particularly after the implementation of the ASEAN Economic Community and remittances could have important role in smoothing consumption and easing credit constraints of migrant-sending households.

Most, if not all, of the previous empirical studies postulate a positive impact of migration (remittances) on poverty reduction (for examples, Todaro 1969; Stark and Bloom 1985; Lokshin *et al.* 2007; Acosta *et al.* 2007a; Acosta *et al.* 2007b; Du *et al.* 2005; Adams 2004; Adams and Page 2005; Taylor *et al.* 2005; Osaki 2003; Yang and Choi 2007; Gyimah-Brempong and Asiebu 2011; Gupta *et al.* 2009). These studies have shown that the positive effect of migration holds for both internal and international movement, the latter having been found to have a bigger and more significant impact. Nonetheless, results on other well-being indicators, particularly labour participation of left-behind members of source household, have been inconclusive.

Rodriguez and Tiongson (2004) find that international migrants reduce the probability of labour supply of non-migrant household members in urban Philippines due to the reliance on remittances. This reduces the household's wage earnings in the domestic market. Also, Osaki (2003) demonstrates that in Thailand's case remittances to source households might create a "dependency" effect. However, Cox-Edwards and Rodriguez-Oreggia (2009) find limited evidence of the impact on Mexican households. Adams (2011) provides a comprehensive review of empirical literature on the impact of international remittances on developing countries and finds that while cross-border movement are negatively associated with poverty and positively with health, remittances could have negative effect on labour supply, education and economic growth. Using a panel technique on a sample of countries, Chami *et al.* (2003) also postulate a negative association between remittances and GDP growth, arguing that remittances are not intended to serve as capital flow for development. Kim (2007) examines the effects of remittances on labour participation in Jamaica and finds that remittances increase the "reservation wage" of receiving households, thus reducing the propensity of households to supply labour. Using longitudinal data from the 1998 and 2001 Living Standard Measurement Surveys in Nicaragua, Funkhouser (2006) shows a similar decreasing trend of labour supply of migrant households.

This paper investigates the impact of remittances on the livelihood of left-behind members. Outcome variables of migrant-sending households include: a class of poverty measure (poverty headcount, gap and squared gap), consumption disaggregated to food and non-food expenditure, salary earning and hours worked[1] of employed left-behind members.

We contribute on three fronts to the studies of migration in general and migration in Cambodia in particular. First, as regards to Cambodian migration, theoretical and empirical studies using micro household survey data have been scarce and sketchy, partly because of a lack of quality data sets. Some recent studies, which are mostly descriptive, include Hing and Lun (2011), Chan (2009) and MoP (2012). Using the methodology proposed by Adams (2004) with the 2007 Cambodian Socio-Economic Survey, Tong (2011) finds that remittances reduce poverty and its severity. Although this study might be the first to employ an empirical model to assess the impacts, the scope of the study is relatively limited since it examines only the effect of emigration on consumption of migrant-sending households. The latest report on migration in Cambodia was by MoP (2012), but the analysis is descriptive and could not provide in-depth causal investigation. Second, we attempt to differentiate between migration and remittances, as the two could have distinct socioeconomic impacts, especially on migrant families and the village of origin. Although McKenzie and Sasin (2007) show that it is more appropriate to define migration in a broader term, meaning that the impact of migration on outcomes not the impact of remittances only, we test both definitions[2] to check whether the estimated results vary in accordance with definitions. Third, we utilise important characteristics of migrants (e.g., age, gender and education) to measure pre-treatment variables necessary to meet the unconfoudedness assumption.

The study employs propensity score matching (PSM) to establish counterfactual information with which outcomes of migrant households are compared. We utilize the Cambodian Socio-Economic Survey 2009 (MoP 2009a). CSES has been available since 1993 and is the most comprehensive survey of households providing a wide range of socio-economic indicators on migrant-sending households and migrants.

Nearest neighbour matching estimator indicates that internal and international remittances have negative impact on poverty headcount and the effects are statistically significant at 5% confident level. For instance, poverty incidence declines by 3-6 percentage points in households with at least one internal migrant and receive remittances compared to its matched non-migrant households. International remittances have a bigger impact at 4-7 percentage points. Remittances also help the poorest of the poor given its statistically significant effects on poverty depth and severity. Yet the impact is practically relatively small at 1 percentage point. The decreased poverty incidence is consistent with the increased total consumption of remittance-receiving households, 8-9 percent for internal and 11-14 percent for international remittances. We additionally find that remittances could generate "dependency effect"

[1]Refer to Table 1 for definitions of outcome variables.

[2]An emigrant household, in the first definition, is a household with at least one person emigrating internally and/or cross-border, regardless of whether she or he sent remittances. The second definition is restricted only to migrants (defined as before) who sent remittances.

among left-behind members given the decreased household salary income and average weekly hours worked by employed members. For instance, average members of internal remittance-receiving households work 5-9 percent less than members of non-migrant households. The effects are even bigger for international remittances, at 6-9 percent, but not statistically significant at conventional levels. This might be self-explanatory, for left-behind members could rely on remittances. Although there is a statistically significantly negative impact of remittances on labour force participation of non-migrant members, the effect is vulnerable to unobservables. We also caution the results of Kernel matching estimator given the small sample size.

The paper is structured as follows. Section 2 discusses underlying assumptions of propensity score matching and its implementation. Section 3 highlights characteristics of data used, defines variables of interest and presents selected descriptive statistics. Section 4 discusses results and Section 5 concludes.

2. Estimation: Propensity Score Matching

Assume that households $i = \{1,2,3, \ldots n\}$ in a sample are subject to a treatment, $\tau_i = 1$, or not, $\tau_i = 0$; Denote T the subsample of treated households, and C the subsample of controls. Also, let Y_{i1} be specific outcomes (e.g., poverty and consumption expenditure) of households received treatment ($\tau_i = 1$) and Y_{i0} are outcomes of households who were not subject to treatment ($\tau_i = 0$). Thus, the treatment effect on i households is defined by:

$$\Delta_i = Y_{i1} - Y_{i0} \tag{1}$$

The average treatment effect on the treated T could also be written as:

$$E^T(\Delta_i|\tau_i = 1, \chi_i) = E(Y_{i1}|\tau_i = 1, \chi_i) - E(Y_{i0}|\, \tau_i = 1, \chi_i) \tag{2}$$

where χ_i is a set of observable household characteristics. In observational data, while the post-treatment outcomes $E(Y_{i1}|\tau_i = 1)$ of i households are observed, the counterfactual value $E(Y_{i0}|\, \tau_i = 1)$ is not. The only information that could be used to construct counterfactual outcomes is $E(Y_{i0}|\, \tau_i = 0)$. Thus, the estimate provides unbiased and consistent average treatment effect if and only if

$$E(Y_{i0}|\tau_i = 1, \chi_i) = E(Y_{i0}|\, \tau_i = 0, \chi_i) \tag{3}$$

A number of experimental and non-experimental approaches have been used to estimate the counterfactual, specifically to ensure that equation (3) holds. The ideal approach has been randomised control trials, in which individuals are randomly assigned to treatment and control groups. The average effect of the treatment is simply the difference between the outcomes of treatment and control groups. Although this method provides unbiased and consistent coefficient estimates, it is hard and costly to implement, especially in low-income and developing countries.

Propensity score matching has been used to estimate the impacts of policy and programme interventions (e.g., Rosenbaum and Rubin 1983; Motohashi 2002; Aerts and Czarnitzki 2004; Criscuolo *et al.* 2007; Caliendo and Kopeining 2008; Mole *et al.* 2008). However, this technique is subject to a set of strong assumptions, namely conditional independent assumption (CIA) and sufficient region of common support. The former assumption can be written as:

$$\left(Y_{i1}^{T}, Y_{i0}^{C}\right) \coprod \tau_i \mid \chi_i \tag{4}$$

Equation (4) states that outcomes of treatment and controls are independent of treatment assignment given a set of observable covariates χ_i. This suggests that all covariates that influence participation or assignment and potential outcomes have to be simultaneously observable to researchers. Beside CIA, PSM also requires that there has to be a sufficient area of common support or overlap condition. That condition can be given as:

$$0 < P(\tau_i = 1 \mid \chi_i) < 1 \tag{5}$$

Equation (5) ensures that households with similar χ_i have positive probability of being participants and non-participants. Matching on covariates could create a "curse of dimensionality" and is computationally tedious. To avoid such problem, Rosenbaum and Rubin (1983) prove that it is mathematically possible to condition the matching on propensity score rather than covariates. Therefore, equations (4) and (5) could be written as:

$$\left(Y_{i1}^{T}, Y_{i0}^{C}\right) \coprod \tau_i \mid P(\chi_i)$$

and

$$0 < P\big(\tau_i = 1 \mid P(\chi_i)\big) < 1$$

Thus, equations (2) and (3) could also be given as:

$$E^{T}[\Delta_i | \tau_i = 1, P(\chi_i)] = E[Y_{i1} | \tau_i = 1, P(\chi_i)] - E[Y_{i0} | \, \tau_i = 1, P(\chi_i)]$$

and

$$E[Y_{i0} | \tau_i = 1, P(\chi_i)] = E[Y_{i0} | \, \tau_i = 0, P(\chi_i)]$$

CIA and common support, if fulfilled, constitute what Rosenbaum *et al.* (1983) call "strong ignorability" that allows researchers to mimic results obtained from experimental approach by using observational survey data. Finally, the average treatment effect on the treated can be written as:

$$E^T[\Delta_i|\tau_i = 1, P(\chi_i)] = \frac{1}{\mathrm{T}}\left[\sum_{\mathrm{i}\in\mathrm{T}} \mathrm{y_i^T} - \sum_{\mathrm{i}\in\mathrm{C}} \varphi(\mathrm{i},\mathrm{j})\mathrm{y_j^C}\right] \quad (6)$$

where T is the number of migrant-sending households and $\varphi(\mathrm{i},\mathrm{j})$ is the weight used to aggregate outcomes for the matched non-migrant households.

PSM estimates average treatment effect of policy or programme impacts between treatment and matched control groups in the region of common support. Treatment and control groups are matched based on the probability of migration or propensity score of participation estimated from a household's observed characteristics and other factors (Khandker *et al.* 2010; Gertler *et al.* 2011). Thus, estimating propensity score is a necessary first step in PSM implementation. The score is the probability of participation and non-participation and is usually estimated using a logit or probit model the choice of which is less debatable when the treatment is binary (Caliendo and Kopeining 2008). This study uses logistic regression for the estimation of propensity score.

PSM does have disadvantages compared to alternative non-experimental approaches—prominently the instrumental variable approach. If the two above-mentioned assumptions (conditional independence and sufficient region of common support) are met, it is theoretically feasible to obtain consistent and unbiased estimates of programme participation. Violation of assumption (2) is more likely to be resolved with a large sample size; yet failure to meet assumption (1)—which is called selection bias—is more severe and produces biased results because it is practically difficult to observe all covariates. It is also almost impossible to test objectively uncounfoundedness assumption. Becker and Ichino (2002) note that PSM can only help reduce, not eliminate, bias resulting from "unobservable confounding factors".

2.1 Selection of Covariates

The matching to estimate average treatment effect on the treated is dependent on the CIA, requiring that outcomes are not influenced by the treatment assignment. Thus, estimating propensity score would demand a set of covariates that is exogenous to treatment (Rosenbaum & Rubin 1983). To ensure that, variables need to be measured before treatment or fixed over time(Caliendo and Kopeinig 2008). Lechner (2008) argue that post-treatment covariates can be included unless they are systematically affected by treatment. Although there are formal statistical tests to select covariates, there is no single rule of thumb to include predictor variables. The inclusion should be based on economic theory, previous studies and thorough understanding of institutional and administrative arrangements of the programme and local context where the programmes are implemented (Caliendo and Kopeinig 2008: 39). Heckman *et al.* (1997) point out that matching estimators perform well if the following three criteria are met: (1) information of treatment and controls come from the same set of questionnaires, (2) participants and non-participants are in the same local labour market, and (3) the data contain a comprehensive set of variables that influence the decision to participate.

Table 1 illustrates covariates of household and village used in the estimation of propensity score[3]. These are pre-treatment variables, as they incorporate information about migrants. Majority of the literature on migration in Cambodia and other contexts we reviewed do not have characteristics of migrant individuals constraining them to use characteristics of left-behind as covariates which could be influenced by emigration and remittances (e.g., Clément 2011). One exception is found in Bertoli and Marchetta (2014). Tables 2 & 3, respectively, show different characteristics of migrant-sending households with or without migrants and between internal and international destinations. For instance, including migrants, average household size is 7.2. The average size declines to 5.1 after emigration. This is expected and including post-emigration household size in the regression would bias the coefficient and subsequently matching estimates. The same rationale is applied to other characteristics listed in Table 2. Therefore, the availability of migrant information (e.g., age, gender, education) gives us an advantage in obtaining pre-treatment variables although all variables are measured post-treatment. All covariates reflect average attributes of households rather than individual members (e.g., household head or migrants). In addition, households sending at least one member internally tend to have more members, lower dependency ratio, more working-age members, and are more educated compared to households with international migrants (Table 3). The differences suggest separate estimations of the impact of internal and international remittances.

One of the influential unobservable variables mentioned in migration literature is migration networks (e.g., Adams *et al.* 2008; Taylor *et al.* 2003). We also incorporate proxies for migration networks to address potential unobservable indicators, and we use a proxy variable of the percent share of migrants to the total population in the district. 2008 Cambodia's census is used to calculate the network variable (MoP 2009b). The level of income, production, land allocation, assets and wealth could influence household decision to send migrants (Zhao 2005: 298; Hare 1999). Nonetheless, these attributes could be affected by emigration through remittances. Therefore, we do not include these variables in the estimation of propensity score.

An important issue worth discussing is the length of emigration, for it could have important implications on the types of emigration episodes (seasonal, short and long) and the changes in familial structures and characteristics of left-behind members. Unfortunately, data did not provide sufficient information to categorise emigration episodes. Migrant-sending households' heads were asked only the year of migration of their migrating member(s), while we need also month in the classification. On the latter implication, 89 percent of all emigrants emigrating to take or look for a job had emigrated for 10 years or less at the time of the survey. More precisely, 48 and 78 percent of internal and international emigrants had been away for 5 years or less, respectively. This indicates that emigration is recent, implying minimal impacts on important post-treatment characteristics of left-behind members.

[3] Selection of most predictor variables is based on previous studies, one of which is Bertoli and Marchetta (2014).

2.2 Matching Techniques

The average treatment effect is calculated using matching methods that compare outcomes of treated households with those of controls who have same or similar propensity score. Given observable covariates, matching estimates balance differences between treatment and control groups prior to comparison. PSM estimators could vary not only on how the matched controls are defined but also on how weights are assigned to matched controls. A few matching techniques have been proposed in the literature: nearest neighbour, caliper and radius, stratification and interval and kernel and local linear. Caliendo and Kopeinig (2008) provide a summary of each method and show the trade-offs in terms of bias and efficiency when choosing one not the other. Nonetheless, literature does not provide a clear guidance on which matching estimator is the best one and the choice seems to be dependent on the question being examined. The rule of thumb is that more than one technique should be used and estimates are compared. If the various matching methods provide robust coefficients, we could be confident on our estimated results.

The study uses two matching techniques: Kernel (KB) and Nearest Neighbour (NN). KB method estimates average treatment effect by matching outcomes of treated units with a weighted sum of outcomes of matched controls, assigning greater weights to matched control units with the closest propensity score (Heckman *et al.* 1998). One of the major merits of this approach is that it reduces variance, for more information about treatment and controls is used. Nonetheless, the technique could increase bias because outcomes with different propensity score might also be matched—what Caliendo and Kopeinig (2008: 43) call "bad matches". Albeit the disadvantage, with the large sample size our study employs, it is beneficial to use more information.

The matching is formally written as:

$$\widehat{\mathrm{E}}^{\mathrm{C}}[\Delta_{i0}|\tau_i = 1, P(\chi_i)] = \sum_{\mathrm{j}\in \mathrm{I}_0}\left(\frac{\mathrm{K_h}\left[\mathrm{P}(\chi_\mathrm{j}) - \mathrm{P}(\chi_\mathrm{i})\right]}{\sum_{\mathrm{j}\in \mathrm{I}_0}\mathrm{K_h}\left[\mathrm{P}(\chi_\mathrm{j}) - \mathrm{P}(\chi_\mathrm{i})\right]}\right)y_j$$

where $\hat{E}^C$ is the average effect of control units; j is a set of non-participants and i is a set of participants; $\mathrm{P}(\chi_\mathrm{j})$ is the propensity score of control units and $\mathrm{P}(\chi_\mathrm{i})$ is the propensity score of treated units; and

$$\mathrm{K_h}\left[\mathrm{P}(\chi_\mathrm{j}) - \mathrm{P}(\chi_\mathrm{i})\right] = \mathrm{K}\left[\frac{\mathrm{P}(\chi_\mathrm{j}) - \mathrm{P}(\chi_\mathrm{i})}{\mathrm{h}}\right]$$

where K is the kernel function and h a "bandwidth". Thus, the average treatment effect on the treated is given by:

$$\widehat{\mathrm{E}}^{\mathrm{T}}[\Delta_{i1}|\tau_i = 1, P(\chi_i)] = \frac{1}{\mathrm{N}}\sum_{\mathrm{i}=1}^{\mathrm{n}}\left[\mathrm{y_i} - \sum_{\mathrm{j}\in \mathrm{I}_0}\left(\frac{\mathrm{K_h}\left[\mathrm{P}(\chi_\mathrm{j}) - \mathrm{P}(\chi_\mathrm{i})\right]}{\sum_{\mathrm{j}\in \mathrm{I}_0}\mathrm{K_h}\left[\mathrm{P}(\chi_\mathrm{j}) - \mathrm{P}(\chi_\mathrm{i})\right]}\right)\mathrm{y_j}\right] \tag{7}$$

where N is the number of treated households. Kernel[4] could use matching techniques such as the Gaussian which uses information of all observations or other alternatives, for example Epanechnikov that utilises information of observations within a specified bandwidth. Our study uses Epanechnikov matching with sampling weights. Thus, equation (7) could be rewritten as:

$$\widehat{\mathrm{E}}^{\mathrm{T}}[\Delta_{i1}|\tau_i = 1, P(\chi_i)] = \frac{1}{\mathrm{N}}\sum_{\mathrm{i}=1}^{\mathrm{n}}\left[\mathrm{y_i} - \sum_{\mathrm{j}\in \mathrm{I_0}}\left(\frac{3/4[1-\mu^2]1_{\{|\mu|<1\}}}{\sum_{\mathrm{j}\in\mathrm{I_0}} 3/4[1-\mu^2]1_{\{|\mu|<1\}}}\right)\mathrm{y_j}\right] \qquad (8)$$

where $\mu = \mathrm{P}(\chi_\mathrm{j}) - \mathrm{P}(\chi_\mathrm{i})$ and $1_{\{\ldots\}}$ is an indicator function. Caliendo and Kopeinig (2008: 43) state that choice of kernel function and the bandwidth has to be made when performing kernel matching, the former of which is less important in practice. The choice of bandwidth is relatively more crucial, as it influences the trade-off between small variance and unbiased estimators. To test whether results are robust, the paper approximates average treatment effect on the treated with different bandwidth.

Alternatively, we also estimate coefficients using nearest neighbour (NN), partly to check robustness of estimated results. NN matches outcomes of each participant with those of non-participants who have the closest propensity score. The matching can be with or without replacement ($n \geq 1$). Caliendo and Kopeinig (2008) show that NN without replacement reduces bias of estimated results but increase variance (decreased efficiency), whereas NN with replacement provides opposite results. The former matching might be problematic in small sample size since it would be hard to find non-migrant households who have the closest score. Therefore, to avoid losing information on controls, we employ NN with replacement and estimate results using different number of matches per treatment observation to check whether estimates converge. Instead of using the estimation command proposed by Becker and Ichino (2002), we utilise the nearest neighbour Stata command given by Abadie *et al.* (2004), for it provides flexibility in matching. For instance, researchers can request the number of matches not necessarily 1-to-1. The command also allows for bias-corrected matching estimators and proper account of sampling weights. The correction is introduced given potential biases due to the inexact matching of covariates; that it adjusts the different values of covariates between treatment and controls.

Another important issue to be considered when implementing PSM is the use of sampling weights. Unfortunately, literature has provided little, sometimes unclear, guidance. The debate lies on whether sampling weights should be employed in the estimation of propensity score or in the calculation of the average treatment effect on the treated or both. Zanutto (2006) and Frölich (2007) argue that sampling weights should not be used in the first step of PSM since the purpose of the estimation is to balance the score for subsequent estimates not to obtain population inference. Also, they postulate that coefficients of the logit or probit

[4]We modified the Becker-Ichino Kernel matching estimator in order to incorporate sampling weights.

regression should not depict behaviour of treatment and controls. Thus, sampling weights are used only to estimate the average treatment effects on the treated. Our study does not use sampling weights in the first step but in the estimation of average treatment effect on the treated.

2.3 Common Support Restriction and Balancing Property

Balancing test is another important feature when implementing matching. The test is required to ensure that plausible counterfactual information is created to estimate the average treatment effect on the treated. The idea behind the test is to check whether observations with the same $P(\chi_i)$ have the same distribution of covariates χ_i, independent of assignment. Currently, different tests have been introduced in the literature, yet again there is no clear guidance to which one is the most useful.

Some studies distinguish between before and after matching balancing tests, calling it specification and balancing tests, respectively (Lee 2006; Ham *et al.* 2005; Smith and Todd 2005). Lee (2006) provides a good summary of four commonly used balancing tests discussed in the literature: the balancing test proposed by Dehejia and Wahba (2002) that tests mean difference within strata of propensity score; the test for standardised differences by Rosenbaum and Rubin (1985); the test of equal mean of each covariate across groups using t-test by Rosenbaum and Rubin (1985); and the test for joint equal mean of covariates across groups using the Hotelling test or F-test by Smith and Todd (2005). The author also distinguishes between before and after matching balancing tests, arguing that before matching test is a test of the validity of PSM specification and post matching is a significant test that checks balancing score of controlled units.

Our study uses "pscore" Stata command by Becker and Ichino (2002) to perform specification test. Amongst the seven steps of algorithm this command performs, the balancing test comes when propensity score is divided equally into k blocks and test the null hypothesis that the mean difference of $P(\chi_i)$ of treated and control units equals to zero. New specifications need to be established if the test rejects the null hypothesis, for the balancing property is not satisfied. Higher order or interaction of variables can also be included in the old specification if it fails specification test. We also re-estimate propensity score on the matched sample between treated and matched controls as in Sianesi (2004) and Bertoli and Marchetta (2014) to test balancing property. The pseudo-R^2 after matching should be small implying no systematic differences in characteristics between treatment and matched controls.

2.4 Sensitivity Check

The unconfoundedness assumption either conditional on covariates or score is strong and almost impossible to test. It could be easily violated if there are unobservable household fixed effects that simultaneously influence potential outcomes and migration decision. Therefore, it is crucial to perform sensitivity or robustness check of estimated results from hidden bias. Becker and Caliendo (2007: 71) state that "checking the sensitivity of the estimated results with respect to deviations from this identifying assumption has become an increasingly important topic in the applied evaluation literature". The estimates might not only be sensitive to unobservable variables but also to different specifications even though there are studies which argue that matching results are independent of specifications (Zhao 2005). Dehejia (2005) and Ravallion (2001) recommend that sensitivity check should be performed given changes to various specifications. However, robustness check can only reduce biases of matching estimates, not eliminate.

Sensitivity check of estimates can be done by using various matching methods and if each method provides consistent results we can conclude that matching estimates are fairly reliable. The 'nnmatch' introduced by Abadie *et al.* (2004) can also be used in sensitivity check.

Another approach is the bounds method proposed by Rosenbaum (2002). Let assume that there are unobservables ε_i simultaneously affecting potential outcomes and treatment assignment. Thus, the CIA could be modified as:

$$\left(Y_{i1}^{T}, Y_{i0}^{C}\right) \coprod \tau_i \mid P(\chi_i), \varepsilon_i$$

The probability of being in the treatment is given by:

$$P\left(\tau_i = 1 \mid P(\chi_i, \varepsilon_i)\right) = F(\beta\chi_i + \psi\varepsilon_i)$$

where χ_i is a set of observable covariates, ε_i is a set of unobservables and $F(.)$ is logistic distribution. ψ needs to be zero if the estimated results are free of hidden bias, meaning that treatment assignment is only conditional on observables. We further define the odd of being in the treatment and control group, respectively, as: $P(\chi_i)/\left(1 - P(\chi_i)\right)$ and $P(\chi_j)/\left(1 - P(\chi_j)\right)$. Thus, the odd ratio could be written as:

$$\frac{P(\chi_i)/\left(1 - P(\chi_i)\right)}{P(\chi_j)/\left(1 - P(\chi_j)\right)} = \frac{P(\chi_i)\left(1 - P(\chi_j)\right)}{P(\chi_j)\left(1 - P(\chi_i)\right)} = \frac{\exp\left(F(\beta\chi_i + \psi\varepsilon_i)\right)}{exp\left(F(\beta\chi_j + \psi\varepsilon_j)\right)}$$

The CIA requires that χ_i and χ_j should be the same ensuring that units with similar characteristics have equal chance of receiving treatment. Therefore, equation (9) can be modified as:

$$\frac{P(\chi_i)\left(1 - P(\chi_j)\right)}{P(\chi_j)\left(1 - P(\chi_i)\right)} = exp\left[\psi\left(\varepsilon_i - \varepsilon_j\right)\right] \qquad (9)$$

Equation (9) shows that the CIA can be violated if $\psi \neq 0$ and $\varepsilon_i \neq \varepsilon_j$. There is no hidden bias if the odd ratio equals 1. This equation calculates ψ and $\varepsilon_i = \varepsilon_j$ to determine how strong the unobservable variables have to be to undermine the matching estimates. Assuming that $\Gamma = e^{\psi}$, Rosenbaum bounds method is given as:

$$\frac{1}{\Gamma} \leq \frac{P(\chi_i)\left(1 - P(\chi_j)\right)}{P(\chi_j)\left(1 - P(\chi_i)\right)} \leq \Gamma \qquad (10)$$

Adapting this framework, Becker and Caliendo (2007) propose "mhbounds" Stata command to perform such test. The Mantel-Haenszel statistics given by mhbounds is applicable only to categorical outcomes. Thus, the paper uses rbounds Stata command (e.g., Clément 2011; DiPrete & Gangl 2004; Gangl 2004; Aakvik 2001) when outcomes are continuous variables.

3. Data, Migration Definitions and Descriptive Statistics

The study uses the 2009 Cambodia Socio-Economic Survey (CSES). A major objective of the CSES is to understand the socioeconomic characteristics of Cambodian households nationwide to assist in policy making and to monitor policy interventions. Specifically, the CSES aims to measure household income and consumption/expenditure and other important household characteristics. Indicators in main social-political-cultural areas are covered: demographic characteristics, housing, agriculture, education, labour force, health and nutrition, victimisation, household income and consumption and current and past migration.

Stratified sampling in three stages was used to select the sample. The required number of PSU (villages) was selected in the first stage. Then, Enumeration Areas (EAs) were picked from each village, and the number of households was selected from each EA in the last stage (NIS 2005). This sampling design does have implications for our calculation of population characteristics, e.g., mean income/consumption or poverty headcount. Failing to take the sampling design into account can potentially generate biased and unrepresentative results. Thus, household weight—already calculated and available in the data set—is used to calculate nationally representative measures. The survey sample size of the CSES varies in each round, with a complete sample of 15,000 households every five years. Surveys of a smaller sample of 3,500 households have been conducted every year since 2007. 2009 CSES contains 15,000 surveyed households.

One of the important matters to consider when examining migration is its definition. The literature has a number of differently defined concepts of migration taking into accounts time, place, purposes and remittance decisions. Gubert (2002) states that a migrant is a person who has left the household for more than six months to live or work elsewhere, either internally or abroad. Litchfield and Waddington (2003) define migrants as "adult (aged 15 or older) household members who either were not born in their current residence, or if they were, have lived elsewhere for a period of 12 months or longer". De Jong (2000) considers one month or more as the time horizon of migration. As shown, the time dimension has been widely discussed in defining migration; however, it has also been criticised for its arbitrariness.

A migrant-sending household in our study is the household that had at least one member (15 years of age or older but less than 65 and either head, head's spouse or children of head) absent from home to take or search for work and had received a positive amount of remittances for the last 12 months. We also estimate the impact of migration which is broadly defined; that is, the household had at least one member (15 years of age or older but less than 65 and either head, head's spouse or children of head) absent from home to take or search for work, regardless of remittances[5]. The purpose is to examine whether results vary with definitions. The impacts of internal and international remittances are estimated separately.

Table 4 illustrates some characteristics of migrants moving to take or look for a job. 79.6 percent of emigration was internal, from less developed provinces to Phnom Penh. The majority of migrants were aged between 15 and 34, and 55.1 percent of them were male. This might indicate that young men are more likely to migrate given their freedom and independence than their female counterparts.

Another debatable conclusion concerns the factors and their frequency that push migrants to remit to their source households. Some studies consider that the decision to remit is a contractual arrangement among household members, while others postulate that migrants remit because they look at the household's bequests. One of the questions asked is, "Have any members of the household received transfers or gifts in cash and kinds from migrants in the last 12 months?" Some 71.2 percent reported having received remittances. This partly indicates that the remittance decision can be a contractual arrangement between migrants and source households. Informal means of remittance transfer are still prevalent (95.5 percent). Only a fraction (3.8 percent) used formal transfer channels, including Western Union and banks.

Table 5 compares a number of socio-economic variables of migrant-sending households with those of non-migrants and provides statistical tests. On average, migrant households have higher total consumption and non-food consumption than non-migrant households. However, the difference is small and not statistically significant. Poverty headcount of migrant households is 4.6 percentage points lower relative to non-migrant households and it is statistically significant. Migrant households also observe lower poverty gap and squared gap but there is no statistical difference. Descriptive statistics also illustrates differences of demographic and socio-economic characteristics between migrant and non-migrant households. For instance, migrant-sending households tend to have more members, come from rural areas, have more working age members especially females, more female, more educated members and lower dependency ratio. The differences are statistically significant.

4. Results and Discussions

Tables 7 and 8 present matching estimates of the effects of internal (Spec. 2) and international remittances (Spec. 3) on poverty headcount of remittance-receiving households. The matching is performed using Epanechnikov kernel and nearest neighbour with different numbers of bandwidth and matches, respectively.

Using nearest neighbour match, the poverty headcount of treated households receiving internal remittances ranges between 3-6 percentage points lower compared to matched non-migrant households, holding other factors fixed. International remittances depict a larger negative impact on the level of poverty migrant-sending households, at 4-7 percentage points.

[5]McKenzie and Sasin (2007) recommend checking robustness of estimates with various definitions. Results of broadly defined emigration are not presented, but available upon request.

Estimated results are consistent across different numbers of matches. This is explanatory as remittances could help ease consumption constraints, at least in the short term. Kernel matching estimator provides similar results for internal remittances, but international remittances given the limited sample size of international-remittance-receiving households. More specifically, of 182, only 36 households are below the poverty line compared to 2,197 poor non-migrant households.

The Tables also report pseudo-R^2 after matching and Mantel-Haenszel (MH) statistics that measures the influence of potential hidden bias on the estimates. In other words, MH bounds present the highest critical values that the average treatment effect on the treatment remains statistically different from zero.

As expected, pseudo-R^2 after matching is low indicating that characteristics of treated and controlled households are balanced. The positive effect of internal remittances on poverty reduction is robust to the assumption of overestimation given the high critical values, greater than 2. Yet, the critical value of underestimation is low, highest being 1.30. The relatively low $\mathbf{e}^{\Psi}$ of MH lower bound might indicate that households with the same observable variables would differ in the odds of sending at least one migrant member by a factor of 1.30, or 30 percent.

Most previous studies, whether they control for reverse causality, omitted variable bias, and selectivity, find that emigration (internal and international) and remittances have statistically significantly positive impact on poverty reduction of migrant-sending households even though the impact is modest varying between 3-5 percent (Adams 2011: 815). Using Ravallion-Huppi decomposition, the World Bank (2013: 38) concludes that migration in Cambodia which is reflected by population shifts almost has no impact on poverty reduction. The report estimates that population movement accounts for only 0.5 percent of poverty reduction during 2004-2011. However, some studies find significantly positive impact of migration and remittances on poverty reduction. Using methodology proposed by Adams (2004) with the 2007 Cambodia Socio-Economic Survey, Tong (2011) finds that in Cambodia internal and international remittances could reduce poverty headcount of migrant-sending households by 4.7 and 7.4 percentage points, respectively. He also shows that remittances have even larger impact on poverty gap and squared poverty gap. For instance, internal and international remittances could reduce squared poverty gap by 26.9 and 60.8 percent relative to non-recipient households'. Lokshin *et al.* (2007) postulate that at least 20 percent of the reduction in poverty during 1995-2004 was attributable to migration. In addition to the strong effect of international migration, the authors argue that internal movement plays an important role. Our study also finds that remittances reduce depth and severity of poverty of treated households and the impacts are statistically significance. The size of the estimates, however, is relatively small, at 1 percentage point.

It is worth discussing how robust the effects of remittances are on poverty headcount of migrant households. The robustness results are presented in Figure 1. Headcount poverty curve for migrant households does not dominate that of non-migrant one over the whole range of possible poverty lines. Poverty headcount of non-migrant household is lower than that of migrant ones, specifically before the normalized poverty line of 55, but the difference is not statistically significant at 95% confidence interval. Headcount of migrant households is statistically significantly lower than that of non-migrants starting from the poverty line of 64 and beyond. Overall, we can conclude that the effect of remittances on poverty incidence is fairly robust, except at very low levels of consumption (Less than 65 of the poverty line).

Estimates of the impact of remittances on remittance-receiving households' daily per capita consumption are presented in Table 10. Results are measured using nearest neighbour matching methods given by Abadie *et al.* (2004)[6]. Sampling weights are used in both matching techniques. Generally, matching estimates show a positive impact of remittances on households' total daily per capita consumption—8-9 percent for internal and 11-14 percent for international remittances—higher than the control households, significant at a 1 percent level. The positive effects corroborate the findings of most previous studies in different contexts (e.g., Clément 2011; Adams and Cuecuecha 2010).

Two outcome variables are used to measure the impact of remittances on economic activities of left-behind members: monthly per capita salary income earned by working age members (15-64) and employed and weekly per capita hours worked by working age members (15-64) and employed. Primary and secondary occupations in all kinds of employment status (i.e. employee, employer, own-account and unpaid family workers) are considered. Table 11 presents the estimates for the latter outcome. Overall, matching estimates show a negative impact of remittances on both outcomes, indicating a "dependency effect" of non-migrant members given an additional income from remittances. Migrant-sending households observe a 5-9 percent reduction in weekly per capita hours worked compared to non-migrant households'. Nonetheless, while the impact of internal remittances on hours worked is statistically significant, that of international remittances could hardly achieve significant level.

This finding is consistent with most of the previous on the impact of migration, particularly cross-border, on labour supply and participation of non-migrant members. In his review, Adams (2011) concludes that "virtually all of the studies reviewed also find that international migration and remittances reduce labour supply and participation, because non-migrants substitute increased income for more leisure." The effect might be different if emigration changes familial structure, specifically household head. For instance, if previous household head emigrate and spouse or one of the adult members becomes head, reduced labour participation might be expected. Osaki (2003) finds that migration induces dependency among non-migrant members whereas Cox-Edward and Rodriguez-Oreggia (2009) find no evidence that "persistent" remittances change labour participation behaviour of non-migrant members. However, data in their study indicates that remittances are an important part of household's income earning strategy.

Rosenbaum bounds of the effects of remittances on other outcome variables, except poverty headcount, are presented in Tables 12 and 13. The sensitivity analysis is performed for both specifications. Nearest

[6] Kernel-based estimates are not presented, but available upon request.

neighbour with $\boldsymbol{n} = \mathbf{4}$ is the matching technique. Overall, the lowest critical value of $\boldsymbol{\Gamma}$ ranges from 1.10 to 6.00 and varies significantly between Hodges-Lehmann point estimate and 95% confidence interval. For instance, when the treatment variable is internal remittances (Table 12), the lowest critical value for poverty gap that includes zero is 5.00 (Hodges-Lehmann point estimate) and 4.00 (95% confidence interval). That would constitute strong evidence that the effects are robust. Similar sensitivity results are obtained for poverty severity. For total consumption, the lowest critical value of Hodges-Lehmann point estimate is 1.35 and that of the 95% confidence interval is 1.15. The sensitivity analysis produces relatively low critical value for labour participation, implying that the effects of remittances on these outcomes are more sensitive to hidden bias compared to poverty incidence and consumption. For hours worked, the critical value is 1.1 for Hodges-Lehmann point estimate[7].

Rosenbaum bounds particularly of the impacts of remittances on labour participation are considered, by some researchers, low. The validity and precision of sensitivity analysis on hidden bias, however, are still debated. The majority of the literature that use propensity score to examine causal inference in social science reports the range of $\boldsymbol{\Gamma}$ to be between 1.1 and 2.0 (e.g., Bertoli and Marchetta 2014; Clemént 2011; Caliendo *et al.* 2005; Duvendak and Palmer-Jones 2012). Guo and Fraser (2010: 318) consider, however, that $\boldsymbol{\Gamma} = \mathbf{1.43}$ is low indicating high sensitivity of the estimates to unobservable factors. Duvendak and Palmer-Jones (2012: 12) share Gou and Fraser's argument and, somewhat, draw a general conclusion that $\boldsymbol{\Gamma} < \mathbf{2}$ implies influences of unobservables on the causal inference. Nonetheless, Aakvik (2001: 132) provides sensitivity test-statistics on the effects of participating in a Norwegian training programme. He argues that $\boldsymbol{\Gamma} = \mathbf{1.25}$ would require 25 percent difference in odds of participants and non-participants to deviate the estimated results whereas $\boldsymbol{\Gamma} = \mathbf{2}$ would require 100 percent difference in the odds, which is a large number given that we have controlled for differences in observables. Studying the impacts of Social Fund for Development in Egypt, Abou-Ali *et al.* (2010: 543) report $\boldsymbol{\Gamma} = \mathbf{1.17}$ and conclude that the impacts are 'relatively robust from hidden bias'. Becker and Caliedo (2007: 81) argue that critical values of test-statistics which are bigger than 1 are "worst-case scenario" implying that estimated results might be sensitive to hidden bias but it does not imply existence of unobservables. They also argue that this test-statistics does not test the validity of the CIA. Nonetheless, the authors also advise some attention to result interpretation when the value of $\boldsymbol{\Gamma}$ is low (e.g., $\boldsymbol{\Gamma} = \mathbf{1.15}$). In addition, Watson (2005: 26) postulates that the critical value is lower in social science (between 1.1 and 2.2) compared to that in natural science (as high as 6). Overall, we are cautious in interpreting the effects of remittances on labour participation given their comparatively low critical value.

[7] Rosenbaum bounds on these outcomes are also conducted for the two specifications using Epanechnikov kernel matching with different bandwidth (results are available upon request).

5. Concluding remarks

Migration and its effects on individual migrants and left-behind members have long been studied even though the causal relations of such labour movement and socioeconomic status of migrants and their households have been mixed. In this study, we use the 2009 Cambodia Socio-Economic Survey combined with Propensity Score Matching method to investigate counterfactual effects of remittances on household well-being: poverty and labour participation of non-migrant members. The use of PSM is to establish causal inference and to reduce biases from using cross-sectional data sets. The theoretical framework of the study is adapted from the new economics of labour migration, which hypothesises that an emigration decision is jointly dependent on other household members and migrants and a decision to remit is a prior agreement between them. One of the important contributions of the study is the incorporation of migrant characteristics (e.g., age and years of schooling) with those of non-migrant members specifically to construct pre-treatment covariates necessary to meet unconfoundedness assumption.

The PSM method shows consistent signs and sizes of the impacts even though not all outcome variables achieve the desired statistically significant levels. We find evidence that internal and international remittances reduce poverty headcount of migrant-sending households by 3-7 percentage points compared to non-migrant households'. Treated households also observe lower poverty depth and severity, yet the magnitude is practically small, about 1 percentage point. Matching estimates illustrate negative effects of remittances on household economic activities: salary income and weekly hours worked by left-behind members reflecting the "dependency effect" of additional income from remittances. The impact on labour participation and salary income is sensitive to unobserved factors, signalling further investigation. We caution the induced negative effects of international remittances on the level, depth and severity of poverty and labour participation of sending households mainly because of the limited observations. This is a point of further investigation when comprehensive datasets with sufficient sample size are handy.

The paper provides two strands of recommendations. One of the priorities for Cambodia's government is improving emigration data given the limitations and insufficiency of the current nationally representative household data sets, specifically the Cambodia Socio-Economic Surveys. Information on migration section should be more comprehensive incorporating retrospective questions about migrants and migrating households and the situations at destination cities/provinces and countries. The improvement would allow researchers and academia to investigate important research questions about the link between emigration and development and resolve some selection issues when there is no credible baseline information.

The second strand is policy suggestions that might directly or indirectly emerge from the analysis. First, emigration, particularly remittances, is seen as a short-term strategy towards poverty reduction. Second, the government should enact policies which encourage emigrants to use formal channels to transfer remittances to source households as transfer fees through informal channels are believed to be high. A medium-term strategy is to work with Microfinance Institutions to widen financial access of migrant-sending households targeting villages with high emigration rate. In addition, as majority of international emigrants are illegal, especially to Thailand, what Cambodia's government could help is to continue working with Thai counterpart to formally register illegal workers so that they could access to financial services. Lastly, productive use of remittances should be encouraged. To do so, emigrants and source households should be given counselling services on productive uses of remittances. Policies that link source households to financial institutions to obtain loans for starting up local businesses would also useful in encouraging productive investment of remittances.

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Annex

Table 1: Variable definitions

Variable	Definition
Outcomes	
Poverty (Headcount, gap and squared gap)	Percent of households whose daily per capita consumption is below national poverty line. Daily per capita consumption is normalised by poverty line and multiplied by 100, so that poverty line is 100 for everyone.
Consumption	Daily per capita consumption of households (riels)
Food consumption	Daily per capita food consumption of households (riels)
Non-food consumption	Daily per capita non-food consumption of households (riels)
Salary	Monthly per capita salary income earned by working age members (15-64) and employed in primary and secondary occupations (riels) (wage, own-account and unpaid family employment)
Hours worked	Weekly per capita hours worked by working age members (15-64) and employed in primary and secondary occupations (wage, own-account and unpaid family employment)
Covariates	
Dependency ratio	Ratio of members age below 6 and above 65 to working age members
Rural areas	Dummy variable taking the value 1 if the household resides in rural area and 0 otherwise
Household age	Average age of members (15-64)
Household size	Total number of members in the household before migrating
Years of schooling	Average years of completed schooling of members age 15-64
College education	Dummy variable taking value 1 if the household has at least one member completing or currently studying college
Female ratio	Ratio of working age female (15-64) to working age members (15-64)
Village irrigation	Per capita irrigated land available in the village
Village government project	Dummy variable taking value 1 if the village has at least one functioning government project and 0 otherwise
Network	Ratio of migrants (15-64) to the total population in the district
Provincial dummies	Dummy variable of 24 provinces, cities and municipality (Phnom Penh omitted)

Source: Authors' preparations

Table 2: Characteristics of migrant-sending households, with or without migrants

Variable	Migrant members: SD Obs.	SD with	SD without	QSD Obs.	QSD with	QSD without
Household size	831	7.221	5.086	1080	7.199	5.183
Dependency ratio	831	0.122	0.227	1080	0.112	0.204
Age	831	32.602	36.551	1080	32.508	36.248
Working age members	831	5.756	3.255	1080	5.747	3.351
Working age female	831	2.950	1.721	1080	2.886	1.747
Female ratio	831	0.520	0.559	1080	0.508	0.548
Years of schooling	831	6.331	5.083	1078	6.204	5.040
High school education	831	4.266	2.282	1080	4.234	2.332

Note: Sampling weights are used. SD is strictly defined definition of emigration; QSD is quite strictly defined one.

Source: Authors' calculations

Table 3: Characteristics of remittance-receiving households, internal vs. international

Variable	Internal Obs.	Internal Before	Internal After	International Obs.	International Before	International After	Diff (B-B)	Diff (A-A)
Household size	665	7.355	5.037	166	6.703	5.276	0.652***	-0.239
Dependency ratio	665	0.113	0.222	166	0.160	0.246	-0.047**	-0.024
Age	665	32.481	36.578	166	33.071	36.444	-0.590	0.134
Working age members	665	5.930	3.268	166	5.087	3.204	0.843***	0.064
Working age female	665	3.067	1.717	166	2.502	1.736	0.565***	-0.019
Female ratio	665	0.674	0.835	166	0.711	0.914	-0.037	-0.079
Years of schooling	665	6.494	5.204	166	5.705	4.611	0.789***	0.593**
High school education	665	4.374	2.285	166	3.851	2.271	0.523***	0.014

Notes: Total observations are 831 remittance-receiving households. 249 non-recipients are excluded. B-B represents pre-migration differences between internal and international migrant-sending households; A-A is the post-migration differences. Sampling weights are used in means calculations. *** $p < 0.01$, ** $p < 0.05$.

Source: Authors' calculations

Table 4: Descriptive statistics (individual migrants)

Variable	All (n = 1,753)	Internal (n = 1,396)	International (n = 357)
Migration destination (%)	100.0	79.6	20.4
Age (%)			
[15-19]	16.3	13.3	3.0
[20-24]	29.0	23.8	5.2
[25-29]	24.2	19.3	4.9
[30-34]	12.6	9.2	3.4
[35-39]	9.1	6.9	2.2
[40-	8.9	7.2	1.7
Sex (%)			
Male	55.1	41.9	13.2
Female	44.9	37.7	7.2
Education of migrants (%)			
No class completed	4.4	3.1	1.3
[0-3]	11.6	8.7	2.9
[4-7]	45.0	34.6	10.4
[8-11]	25.4	21.6	3.8
[12-	12.4	11.0	1.4
DK	1.3	0.7	0.6
Others			
Whether households received remittances (%)			
Yes	71.2	57.2	14.1
No	28.8	22.5	6.3
Means/channel used to send money (%)			
Western union	0.8	0.4	0.4
Bank transfer	3.0	0.9	2.1
From or by other person	95.5	92.6	2.9
Other	0.7	0.4	0.3
Amounts remitted in previous 12 months (KHR)	812,582 (2,083,971)	561,758 (1,045,684)	1,859,367 (4,074,285)
Male	836,712 (2,523,598)	493,147 (857,751)	1,977,438 (4,847,115)
Female	786,940 (1,481,833)	627,897 (1,196,413)	1,669,211 (2,359,709)

Notes: Emigrating individuals are those who have been away to take or look for a job. The time horizon for receiving remittances was the last 12 months. We do not know the frequency with which migrants remitted home during this period. Figures in parenthesis are standard deviation. Exchange rate in July 2009 was USD1 = KHR4108.

Source: Authors' calculations

Table 5: Descriptive statistics (households)

Variable	All Migrant (1)	Internal (2)	International (3)	Non-migrant (4)	(1)-(4)	(2)-(4)	(3)-(4)
Consumption	7632.937	7724.508	7160.419	7458.256	174.687	266.252	-297.837
	(278.118)	(326.015)	(460.233)	(77.856)			
Food consumption	3847.607	3823.914	3893.198	3950.563	-102.956	-126.649	-57.365
	(74.031)	(93.851)	(182.457)	(22.690)			
Non-food consumption	3785.331	3900.594	3267.22	3507.693	277.638	392.901	-240.473
	(243.291)	(286.059)	(332.438)	(67.184)			
Poverty headcount (%)	15.535	14.550	19.958	20.178	-4.643***	-5.628	-0.220
	(0.235)	(0.019)	(0.037)	(0.444)			
Poverty gap (%)	3.006	3.051	3.741	4.130	-1.124**	-1.079	-0.389
	(0.004)	(0.006)	(0.009)	(0.002)			
Squared poverty gap (%)	0.917	1.014	1.153	1.257	-0.337	-0.243	-0.104
	(0.002)	(0.002)	(0.006)	(0.001)			
Household size	7.220	7.355	7.011	5.500	1.715***	1.855	1.511***
	(0.086)	(0.108)	(0.238)	(0.026)			
Rural area	0.878	0.885	0.848	0.811	0.067***	0.074*	0.037
	(0.012)	(0.011)	(0.037)	(0.002)			
Working age members	5.756	5.930	5.277	3.965	1.799***	1.965**	1.312***
	(0.080)	(0.101)	(0.201)	(0.023)			
Working age female	2.950	3.067	2.668	2.033	0.917***	1.034	0.635***
	(0.053)	(0.063)	(0.122)	(0.013)			
Female ratio	0.520	0.674	0.711	0.521	0.001	0.153**	0.190*
	(0.006)	(0.012)	(0.037)	(0.001)			
Age	32.602	32.481	32.757	32.274	0.328*	0.207**	0.483
	(0.169)	(0.192)	(0.396)	(0.053)			
Years of schooling	6.331	6.494	5.718	5.255	1.076***	1.239***	0.463*
	(0.096)	(0.012)	(0.239)	(0.029)			
High school education	4.266	4.374	3.811	3.490	0.776***	0.884***	0.321**
	(0.064)	(0.085)	(0.144)	(0.022)			
Dependency ratio	0.122	0.113	0.157	0.159	-0.037***	-0.046	-0.002
	(0.007)	(0.008)	(0.018)	(0.002)			

Notes: Linearised standard errors are in parenthesis; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$; sampling weights are used to calculate the means; adjusted Wald test is performed to test the null hypothesis of equal means. 249 observations of migrant-sending households were excluded from (1), 415 from (2) and 898 from (3). This is to avoid counting migrant households, but had not received remittances for the last 12 months, as non-migrants. Refer to Table 1 for definitions of variables.

Source: Authors' calculations

Table 6: Logit regression to estimate propensity score

Variable	Spec. 1 All	Spec. 2 Internal	Spec. 3 International
Dependency ratio	-0.187	-0.387*	0.248
	(0.170)	(0.200)	(0.297)
Rural areas	0.680***	0.519***	0.646**
	(0.151)	(0.162)	(0.270)
Household age	0.019***	0.018**	0.017
	(0.007)	(0.008)	(0.014)
Household size	0.780***	0.818***	0.551***
	(0.080)	(0.088)	(0.146)
Household size (squared)	-0.035***	-0.036***	-0.017**
	(0.005)	(0.006)	(0.008)
Years of schooling	0.300***	0.357***	0.285
	(0.050)	(0.058)	(0.101)
Years of schooling (squared)	-0.011***	-0.013***	-0.017**
	(0.003)	(0.004)	(0.037)
College education	0.570***	0.534***	0.449
	(0.134)	(0.142)	(0.289)
Female ratio	0.702***	0.995***	0.009
	(0.228)	(0.251)	(0.459)
Village irrigation	-1.094***	-1.144**	-0.543
	(0.420)	(0.468)	(0.689)
Village government project	-0.149*	-0.197**	0.066
	(0.082)	(0.091)	(0.162)
Network	0.014	-0.014	0.065**
	(0.019)	(0.023)	(0.031)
Provincial dummies	Yes	Yes	Yes
_cons	-10.087***	-10.748***	-9.837***
	(0.502)	(0.571)	(0.916)
All controls	10,093	10,890	10,890
Treatment	807	665	182
Number of obs.	10,724	11,282	9,729
Prob>χ^2	0.0000	0.0000	0.0000
Pseudo R^2	0.1616	0.1854	0.1257
Log likelihood	-2373.265	-2029.695	-787.251
Balancing property	Satisfied	Satisfied	Satisfied
# of blocks	8	9	6

Notes: Standard errors are in parenthesis; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$; sampling weights are not used in the first step to calculate propensity score following Zanutto (2006) and Frolich (2007). Spec. 1 excludes 249 observations of migrant-sending households from 1080 observations, 415 from Spec. 2 and 898 from Spec. 3.

Source: Authors' calculations

Table 7: The impact of internal and international remittances on poverty headcount of migrant-sending households (matching estimator = Epanechnikov kernel)

Bandwidth	Spec. 2					Spec. 3				
	Households			MH bounds		Households			MH bounds	
	Treated	Matched controls	PATT	p_mh_{+}	p_mh_{-}	Treated	Matched controls	PATT	p_mh_{+}	p_mh_{-}
0.100	652	10029	-0.044** (0.017)	6.0	1.1	181	8585	0.000 (0.033)	1.00	1.00
0.200	652	10029	-0.052** (0.020)	6.0	1.1	181	8585	0.002 (0.040)	1.00	1.00
0.300	652	10029	-0.053** (0.020)	6.0	1.1	181	8585	0.002 (0.039)	1.00	1.00
0.400	652	10029	-0.054** (0.019)	6.0	1.1	181	8585	0.002 (0.039)	1.00	1.00
0.500	652	10029	-0.054*** (0.017)	6.0	1.1	181	8585	0.002 (0.037)	1.00	1.00
0.600	652	10029	-0.054*** (0.016)	6.0	1.1	181	8585	0.002 (0.038)	1.00	1.00
0.700	652	10029	-0.054*** (0.017)	6.0	1.1	181	8585	0.002 (0.034)	1.00	1.00
0.800	652	10029	-0.054*** (0.017)	6.0	1.1	181	8585	0.002 (0.036)	1.00	1.00
0.900	652	10029	-0.054*** (0.016)	6.0	1.1	181	8585	0.002 (0.042)	1.00	1.00

Notes: Estimated results are in level. Standard errors are in parenthesis; *** $p < 0.01$;**$p < 0.05$; sampling weights are used in the estimation of ATET. 415 and 898 observations of specifications (2) & (3), respectively, are excluded from the calculation of the ATET. Mantel-Haenszel statistics represents the highest critical values of upper and lower bounds that ATET is statistically different from zero at $p < 0.05$.$\mathbf{e}^{\Psi} = \mathbf{6}$ is the highest value tested. The p-value for lower bound increases at lower critical values ($1 < \mathbf{e}^{\Psi} < 2.5$), but decrease at high values**.** PATT is population average treatment effects on the treated. After-matching balancing properties are satisfied. Source: Authors' calculations

Table 8: The impact of internal and international remittances on poverty headcount of migrant-sending households (matching estimator = nearest neighbour)

	Spec. 2							Spec. 3						
	Households					MH bounds		Households					MH bounds	
n	Treated	Matched Controls	PATT	PATT bias-adj.	Pseudo R^2 after matching	p_mh_+	p_mh_-	Treated	Matched Controls	PATT	PATT bias-adj.	Pseudo R^2 after matching	p_mh_+	p_mh_-
1	652	780	-0.031*	-0.031*	0.0052	1.00	1.00	181	254	-0.074**	-0.060*	0.0020	1.00	1.00
			(0.018)	(0.018)						(0.035)	(0.033)			
2	652	1199	-0.048***	-0.045***	0.0091	6.00	1.25	181	399	-0.054*	-0.051*	0.0017	1.00	1.00
			(0.016)	(0.016)						(0.032)	(0.030)			
3	652	1574	-0.040**	-0.040**	0.0141	6.00	1.15	181	545	-0.038	-0.043	0.0036	1.00	1.00
			(0.015)	(0.015)						(0.030)	(0.028)			
4	652	1918	-0.035**	-0.037**	0.0171	6.00	1.15	181	688	-0.049*	-0.053*	0.0031	1.00	1.00
			(0.015)	(0.014)						(0.028)	(0.027)			
5	652	2238	-0.034**	-0.037***	0.0221	6.00	1.15	181	830	-0.055*	-0.061**	0.0047	6.00	1.05
			(0.015)	(0.014)						(0.029)	(0.028)			
6	652	2525	-0.039***	-0.041***	0.0261	6.00	1.20	181	958	-0.060**	-0.063**	0.0049	6.00	1.05
			(0.014)	(0.014)						(0.028)	(0.028)			
7	652	2782	-0.040***	-0.041***	0.0293	6.00	1.25	181	1100	-0.063**	-0.064**	0.0056	6.00	1.10
			(0.014)	(0.014)						(0.028)	(0.027)			
8	652	3014	-0.039***	-0.041***	0.0342	6.00	1.25	181	1229	-0.065**	-0.067**	0.0068	6.00	1.10
			(0.014)	(0.014)						(0.028)	(0.026)			
9	652	3223	-0.041***	-0.043***	0.0378	6.00	1.25	181	1346	-0.062**	-0.065**	0.0083	6.00	1.10
			(0.014)	(0.014)						(0.028)	(0.027)			
10	652	3410	-0.041***	-0.043***	0.0412	6.00	1.30	181	1454	-0.062**	-0.066**	0.0106	6.00	1.05
			(0.014)	(0.013)						(0.028)	(0.027)			

Notes: Estimated results are in level. Standard errors are in parenthesis; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$; sampling weights are used in the estimation of the average treatment effect on the treated. 415 and 898 observations of specifications (2) & (3), respectively, are excluded from the calculation of the ATET. Mantel-Haenszel statistics represents the highest critical values of upper and lower bounds that ATET is statistically different from zero at $p < 0.05$. $\mathbf{e}^{\Psi} = \mathbf{6}$ is the highest value tested. The p-value for lower bound increases at lower critical values $(1 < \mathbf{e}^{\Psi} < 2.5)$, but decrease at high values. PATT is population average treatment effects on the treated. After-matching balancing properties are satisfied. Source: Authors' calculations

Table 9: The impact of internal and international remittances on poverty gap of migrant-sending households (matching estimator = nearest neighbour)

	Spec. 2					Spec. 3				
	Household					Household				
n	Treated	Matched Controls	PATT	PATT bias-adj.	Pseudo R^2 after matching	Treated	Matched Controls	PATT	PATT bias-adj.	Pseudo R^2 after matching
1	652	780	-0.007 (0.004)	-0.008 (0.004)	0.0051	181	254	-0.012 (0.009)	-0.012 (0.008)	0.0016
2	652	1199	-0.011** (0.004)	-0.010** (0.004)	0.0094	181	399	-0.011 (0.008)	-0.011 (0.008)	0.0015
3	652	1574	-0.008** (0.004)	-0.008** (0.004)	0.0141	181	545	-0.008 (0.008)	-0.010 (0.007)	0.0031
4	652	1918	-0.007* (0.004)	-0.007* (0.003)	0.0184	181	688	-0.009 (0.007)	-0.010 (0.007)	0.0033
5	652	2238	-0.006 (0.004)	-0.007* (0.003)	0.0216	181	830	-0.010 (0.007)	-0.011 (0.007)	0.0042
6	652	2525	-0.007* (0.003)	-0.007** (0.003)	0.0256	181	958	-0.012 (0.007)	-0.013* (0.007)	0.0054
7	652	2782	-0.007* (0.004)	-0.007** (0.003)	0.0302	181	1100	-0.013* (0.007)	-0.014** (0.006)	0.0059
8	652	3014	-0.007* (0.003)	-0.007** (0.003)	0.0345	181	1229	-0.013* (0.007)	-0.013** (0.006)	0.0071
9	652	3223	-0.007** (0.003)	-0.008** (0.003)	0.0374	181	1346	-0.011* (0.007)	-0.012* (0.006)	0.0086
10	652	3410	-0.008** (0.003)	-0.009** (0.004)	0.0406	181	1454	-0.012* (0.007)	-0.013** (0.006)	0.0103

Notes: Estimated results are in level. Standard errors are in parenthesis; *** $p < 0.01$; ** $p < 0.05$, * $p < 0.10$; sampling weights are used in the estimation of the average treatment effect on the treated. 415 and 898 observations of specifications (2) & (3), respectively, are excluded from the calculation of the ATET. The size of the coefficients for international remittances estimated using Epanechnikov kernel is similar, but are not statistically significant at 10%. Results of Kernel matching are available upon request. Source: Authors' calculations

Table 10: The impact of internal and international emigration on total consumption of migrant-sending households (matching estimator = nearest neighbour)

n	Spec. 2					Spec. 3				
	Household				Pseudo	Household				Pseudo
	Treated	Matched Controls	PATT	PATT bias-adj.	R^2 after matching	Treated	Matched Controls	PATT	PATT bias-adj.	R^2 after matching
1	652	780	0.092*** (0.032)	0.089*** (0.030)	0.0057	181	254	0.112* (0.058)	0.110** (0.053)	0.0027
2	652	1199	0.109*** (0.029)	0.017*** (0.027)	0.0094	181	399	0.116** (0.050)	0.113** (0.045)	0.0021
3	652	1574	0.094*** (0.027)	0.099*** (0.026)	0.0138	181	545	0.112** (0.046)	0.119*** (0.042)	0.0028
4	652	1918	0.083*** (0.027)	0.092*** (0.025)	0.0178	181	688	0.134*** (0.045)	0.135*** (0.041)	0.0047
5	652	2238	0.076*** (0.026)	0.089*** (0.024)	0.0217	181	830	0.139*** (0.044)	0.139*** (0.041)	0.0040
6	652	2525	0.087*** (0.026)	0.095*** (0.024)	0.0262	181	958	0.136*** (0.045)	0.134*** (0.041)	0.0053
7	652	2782	0.091*** (0.026)	0.098*** (0.024)	0.0300	181	1100	0.133*** (0.045)	0.129*** (0.040)	0.0054
8	652	3014	0.092*** (0.026)	0.100*** (0.024)	0.0343	181	1229	0.130*** (.044)	0.125*** (0.039)	0.0074
9	652	3223	0.097*** (0.026)	0.105*** (0.024)	0.0376	181	1346	0.125*** (0.044)	0.122*** (0.039)	0.0087
10	652	3410	0.099*** (0.026)	0.105*** (0.024)	0.0407	181	1454	0.125*** (0.044)	0.125*** (0.039)	0.0100

Notes: Estimated results are in level. Standard errors are in parenthesis; *** $p < 0.01$; ** $p < 0.05$, * $p < 0.10$; sampling weights are used in the estimation of the average treatment effect on the treated. 415 and 898 observations of specifications (2) & (3), respectively, are excluded from the calculation of the ATET to avoid counting migrant-sending households who had not received remittances for the last 12 months as non-migrant households.
Source: Authors' calculations

Table 11: The impact of internal and international emigration on hours worked of migrant-sending households (matching estimator = nearest neighbour)

n	Spec. 2					Spec. 3				
	Household					Household				
	Treated	Matched Controls	PATT	PATT bias-adj.	Pseudo R^2 after matching	Treated	Matched Controls	PATT	PATT bias-adj.	Pseudo R^2 after matching
1	589	713	-0.050*	-0.047	0.0051	151	218	-0.061	-0.074	0.0020
			(0.031)	(0.030)				(0.069)	(0.068)	
2	589	1092	-0.076***	-0.073***	0.0095	151	338	-0.067	-0.062	0.0016
			(0.029)	(0.028)				(0.065)	(0.063)	
3	589	1445	-0.079***	-0.074***	0.0131	151	463	-0.088	-0.091	0.0020
			(0.028)	(0.027)				(0.061)	(0.059)	
4	589	1755	-0.079***	-0.071***	0.0158	151	595	-0.092	-0.089	0.0026
			(0.028)	(0.027)				(0.059)	(0.056)	
5	589	2042	-0.081***	-0.071***	0.0200	151	720	-0.091	-0.087*	0.0036
			(0.027)	(0.026)				(0.057)	(0.053)	
6	589	2301	-0.076***	-0.069***	0.0240	151	830	-0.089	-0.083	0.0042
			(0.027)	(0.026)				(0.057)	(0.054)	
7	589	2524	-0.076***	-0.068***	0.0273	151	948	-0.088	-0.078	0.0051
			(0.027)	(0.026)				(0.056)	(0.052)	
8	589	2729	-0.080***	-0.070***	0.0329	151	1059	-0.084	-0.071	0.0064
			(0.027)	(0.026)				(0.056)	(0.053)	
9	589	2916	-0.080***	-0.071***	0.0353	151	1162	-0.086	-0.074	0.0074
			(0.027)	(0.026)				(0.056)	(0.052)	
10	589	3090	-0.083***	-0.074***	0.0397	151	1254	-0.089	0.077	0.0090
			(0.027)	(0.026)				(0.055)	(0.052)	

Notes: Estimated results are in level. Standard errors are in parenthesis; *** $p < 0.01$; **, * $p < 0.10$; sampling weights are used in the estimation of the average treatment effect on the treated. 415 and 898 observations of specifications (2) & (3), respectively, are excluded from the calculation of the ATET to avoid counting migrant-sending households who had not received remittances for the last 12 months as non-migrant households. Kernel estimator provides negative and statistically significant effects for both specifications. Source: Authors' calculations

Table 12: Sensitivity Analysis—Rosenbaum Bounds (Spec. 2, matching=nearest neighbour, n=4)

Outcomes	Γ	Matched pairs	Hodges-Lehmann point estimate Min	Hodges-Lehmann point estimate Max	95% Confidence Interval Min	95% Confidence Interval Max
Gap	1.00	650	-3.5e-07	-3.5e-07	-3.5e-07	-3.5e-07
	2.00		-3.5e-07	-3.5e-07	-3.5e-07	-3.5e-07
	2.90		-3.5e-07	-3.5e-07	-3.5e-07	-3.5e-07
	4.00		-0.0148	-3.5e-07	**-0.0537**	0.0020
	5.00		**-0.0526**	0.0020	-0.0778	0.0469
Squared gap	1.00	650	-0.0022	-0.0022	-0.0032	-0.0012
	2.00		-0.0075	-4.9e-07	-0.0090	-4.9e-07
	2.90		-0.0112	-4.9e-07	-0.0137	-4.9e-07
	3.80		-0.0143	-4.9e-07	-0.0159	-4.9e-07
	5.00		-0.0165	-4.9e-07	**-0.0189**	0.0010
	6.00		**-0.0181**	0.00001	-0.0216	0.0057
Total consumption	1.00	650	0.0721	0.0721	0.0262	0.1194
	1.10		0.0477	0.0972	0.0013	0.1444
	1.15		0.0360	0.1094	**-0.0101**	0.1559
	1.20		0.0251	0.1205	-0.0210	0.1670
	1.25		0.0146	0.1309	-0.0315	0.1781
	1.30		0.0045	0.1414	-0.0417	0.1887
	1.35		**-0.0054**	0.1511	-0.0513	0.1988
Food	1.00	651	0.0814	0.0814	0.0409	0.1221
	1.20		0.0399	0.1232	**-0.0001**	0.1644
	1.35		0.0137	0.1501	-0.0269	0.1924
	1.40		0.0059	0.1585	-0.0355	0.2009
	1.45		**-0.0020**	0.1665	-0.0433	0.2091
Non-food	1.00	651	0.0616	0.0616	**-0.0011**	0.1258
	1.05		0.0448	0.0787	-0.0180	0.1434
	1.10		0.0287	0.0958	-0.0342	0.1605
	1.15		0.0127	0.1120	-0.0494	0.1771
	1.20		**-0.0025**	0.1273	-0.0648	0.1926
Salary	1.00	231	-0.0772	-0.0772	**-0.2128**	0.0486
	1.05		**-0.0983**	-0.0538	-0.2376	0.0691
	1.15		**-0.1380**	-0.0143	-0.2850	0.1104
	1.20		**-0.1575**	0.0023	-0.3063	0.1300
Hours worked	1.00	588	-0.0203	-0.0203	**-0.0669**	0.0246
	1.05		-0.0322	-0.0086	-0.0792	0.0365
	1.10		**-0.0437**	0.0023	-0.0916	0.0473

Notes: The lowest critical values of Γ of Hodges-Lehmann point estimate and 95% confidence interval encompassing zero are bolded. Rosenbaum bounds sensitivity analysis for the different number of matches is also conducted. Results are available upon request.

Source: Authors' calculations

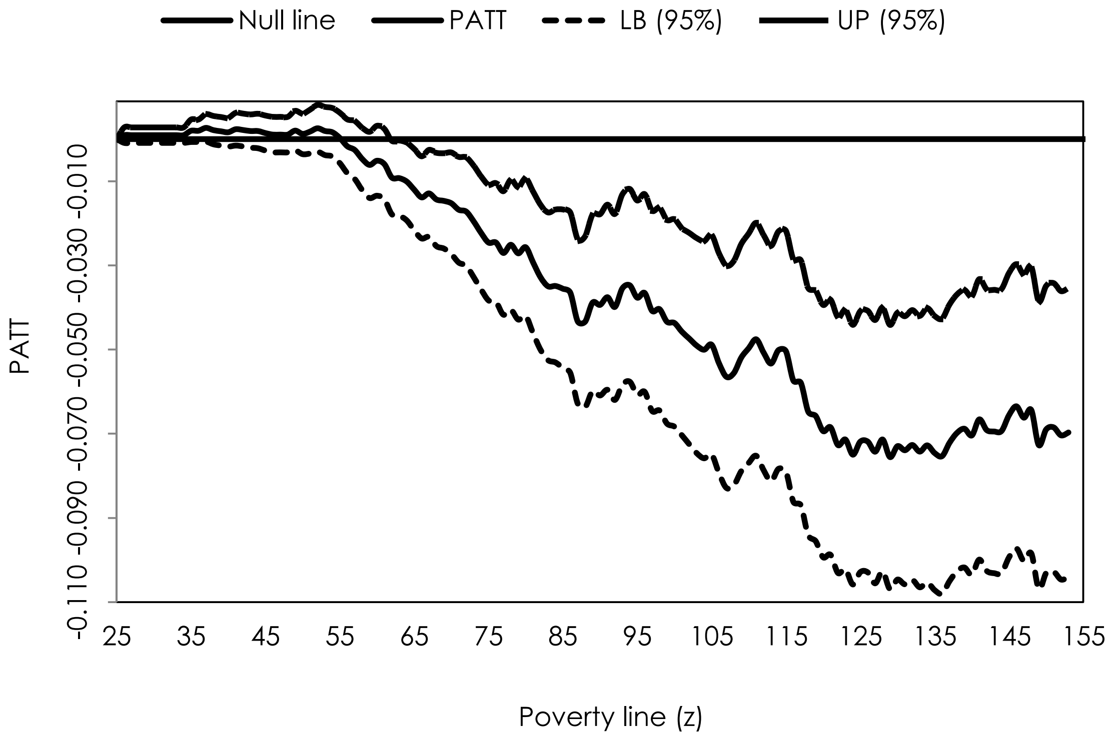
Table 13: Sensitivity Analysis—Rosenbaum Bounds (Spec. 3, matching=nearest neighbour, n=4)

Outcomes	Γ	Matched pairs	Hodges-Lehmann point estimate Min	Hodges-Lehmann point estimate Max	95% Confidence Interval Min	95% Confidence Interval Max
Gap	1.00	179	-0.0206	-0.0206	-0.0343	-0.0082
	2.00		-0.0423	-4.2e-07	-0.0514	-4.2e-07
	2.70		-0.0488	-4.2e-07	**-0.0610**	0.0008
	3.00		-0.0530	-4.2e-07	-0.0643	0.0131
	4.25		**-0.0625**	0.0022	-0.0778	0.0504
Squared gap	1.00	179	-0.0042	-0.0042	-0.0084	-0.0020
	2.00		-0.0124	-0.00006	-0.0163	-2.6e-07
	2.90		-0.0161	-2.6e-07	-0.0220	-2.6e-07
	3.40		-0.0180	-2.6e-07	**-0.0244**	0.00007
	5.00		-0.0240	-2.6e-07	-0.0296	0.0106
	6.00		**-0.0259**	0.0024	-0.0341	0.016585
Total consumption	1.00	179	0.1149	0.1149	0.0399	0.1997
	1.10		0.0937	0.1373	0.0170	0.2237
	1.15		0.0839	0.1486	0.0060	0.2351
	1.20		0.0743	0.1600	**-0.0039**	0.2465
	1.50		0.0253	0.2155	-0.0574	0.3049
	1.60		0.0101	0.2312	-0.0716	0.3236
	1.70		**-0.0039**	0.2466	-0.0858	0.3407
Food	1.00	179	0.1615	0.1615	0.0850	0.2424
	1.50		0.0718	0.2569	**-0.0020**	0.3429
	1.60		0.0582	0.2722	-0.0169	0.3595
	1.90		0.0226	0.3141	-0.0539	0.4042
	2.15		**-0.0018**	0.3429	-0.0810	0.4362
Non-food	1.00	179	0.0820	0.0820	-0.0200	0.1882
	1.15		0.0400	0.1261	**-0.0607**	0.2372
	1.20		0.0260	0.1403	-0.0734	0.2518
	1.25		0.0148	0.1527	-0.0860	0.2666
	1.35		**-0.0073**	0.1762	-0.1080	0.2920
Salary	1.00	61	0.0848	0.0848	**-0.2155**	0.3532
	1.20		**-0.0135**	0.1681	-0.3103	0.4333
Hours worked	1.00	150	-0.0251	-0.0251	**-0.1348**	0.0753
	1.05		-0.0384	-0.0145	-0.1502	0.0862
	1.15		**-0.0665**	0.0123	-0.1814	0.1112

Notes: The lowest critical values of Γ of Hodges-Lehmann point estimate and 95% confidence interval encompassing zero are bolded. Rosenbaum bounds sensitivity analysis for the different number of matches is also conducted. Results are available upon request.

Source: Authors' calculations

Figure 1: Difference between FGT curves of migrants and non-migrants (alpha=0, after matching, nn=4)



Note: PATT is population average treatment effect on the treated. The treatment is internal and international remittance-receiving households. LB and UB are lower and upper bounds, respectively, with 95% confidence interval Emigration is treatment variable. Poverty line ranges from 25 to 153 and 100 is the official line. The average treatment effects are bias adjusted.

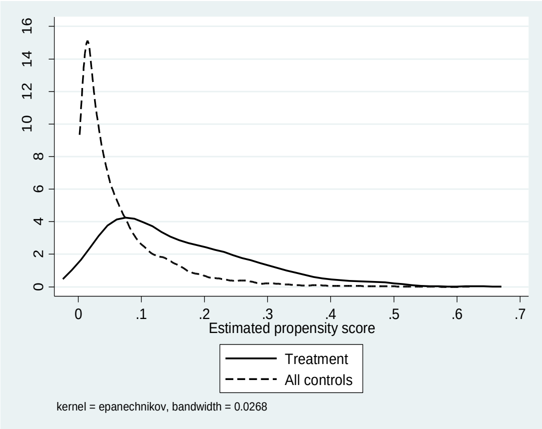
Source: Authors' calculations

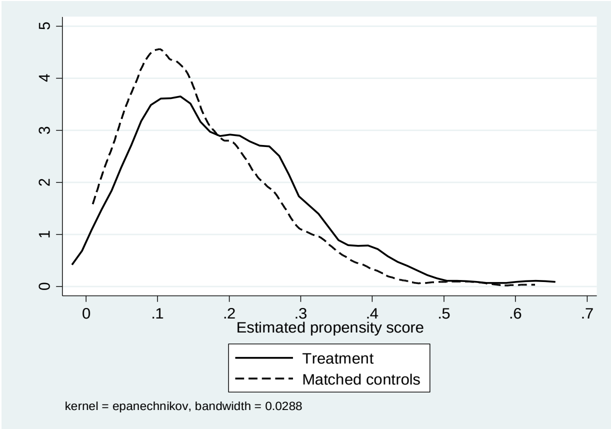
Figure 2: Distribution of propensity score of treatment, controls and matched controls (treatment = poverty headcount, kernel = epanechnikov)

Before

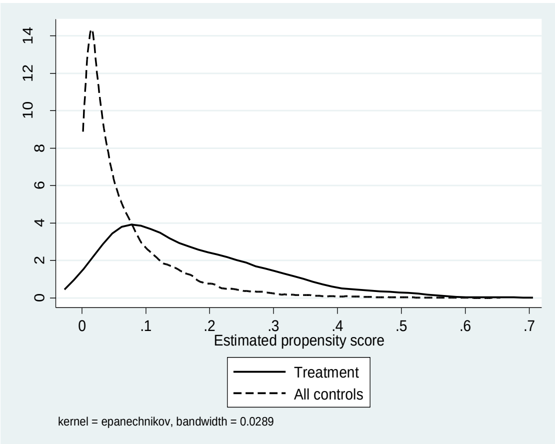
After

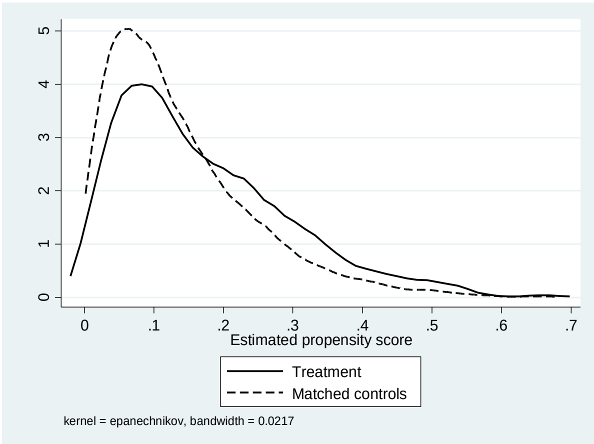
Spec. 1





Spec. 2





Source: Authors' calculations