ASSESSING THE POVERTY IMPACTS OF REMITTANCES WITH ALTERNATIVE COUNTERFACTUAL INCOME ESTIMATES

Eliana V. Jimenez and Richard P.C. Brown*, School of Economics Discussion Paper No. 375, October 2008, School of Economics, The University of Queensland. Australia..

Abstract

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EPrint Type:	Departmental Technical Report
Keywords:	migration, remittances, poverty, Pacific islands, Tonga.
Subjects:	,
ID Code:	JEL Classification: D13. O39. O15
Deposited By:	

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ABSTRACT

We estimate the impacts of remittances on poverty with survey data from Tonga, a poor Pacific island country highly dependent on international migrants' remittances. The sensitivity of poverty impacts to estimation method is tested using two methods to estimate migrants' counterfactual incomes; bootstrap prediction with self-selection testing and propensity score matching. We find consistency between the two methods, both showing a substantial reduction in the incidence and depth of poverty with migration and remittances. With further robustness checks there is strong evidence that the poorest households benefit from migrants' remittances, and that increased migration opportunities can contribute to poverty alleviation.

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Introduction

Remittances have the potential to play an important development role by providing households in low-income countries with much needed social protection. Formal systems of social protection face severe obstacles in developing countries where they tend to overstretch fiscal and institutional capacities of governments and cannot easily be extended to those in the informal sector (Guhan 1994; Holzmann and Jorgensen 2000; Norton 2002). As a result, households in developing countries have traditionally relied on informal systems of social protection to provide for adequate levels of welfare in the face of otherwise uninsured risks (Deaton 1997). The importance of informal social protection in developing countries should not be underestimated. Sen (1981) has shown that starvation can occur in the midst of abundant food supply. He argues that institutionalized systems of social protection rather than adequate supply of food explain the absence of starvation in developed countries. Informal instruments of social protection typically consist of income support in the form of cash and in-kind transfers provided by the extended family and kinship networks.

In many countries of the South Pacific the ability of social networks to provide financial support in times of need has come under increasing stress (Abbott and Pollard 2004; ADB 2004; UNDP 2006). Not only are family networks exposed to covariate risks that erode their capacity to provide co-insurance (Morduch and Sharma 2001), but high population growth rates and youth unemployment are placing social networks under enormous pressure (Duncan *et al* 2006). Tonga is no exception as evidenced by the recent riots.

International migrants' remittances, unlike in-country internal transfers, are not affected by covariate risks and therefore could potentially provide effective social protection in the face of adverse macroeconomic conditions or natural disasters. Furthermore, in many developing countries, including Tonga, socio-cultural institutions encourage migrants to use their remittances to fulfil their extended family and kinship obligations in times of need.[1] To the extent that this occurs, and that poor households have access to international migration opportunities will determine the extent to which, if at all, migration and remittances contribute to the alleviation of poverty. This issue is also important in the context of the current migration policy debates in

[1] For a comprehensive discussion of the determinants of migrants' remittances see Rapoport and Docquier (2006). See also Adams (2008) for a good cross-country analysis of the determinants of remittances.

the region, in view of the potential poverty alleviating role of increased migration opportunities for Pacific islanders to the two main destination countries, Australia and New Zealand.[2]

Findings from recent empirical literature on the impacts of migrants' remittances indicate that in most instances remittances reduce poverty in the recipient country (Adams, 1989; 2006; Adams and Page, 2005; Barham and Boucher, 1998; Lopez-Cordoba, 2004; McKenzie, 2006; Rodriguez, 1998; Yang and Martinez; 2006). However, the strength of the estimated effect varies and in some instances it was found that the income gains from remittances were not sufficient to offset the estimated losses from migration, resulting in an overall increase in poverty (Acosta *et al.*, 2007). It is not known whether these differences are real, arising from the different country migration histories and conditions, or simply reflections of the different analytical methods applied.[3] At this point non-one, to our knowledge, has tested the sensitivity of the results obtained from a given dataset to the choice of analytical method for estimating counterfactual income.

The main purpose of this paper is to estimate the effects of remittances on poverty indicators in a remittance-dependent country and examine the extent to which the results are robust to the choice of counterfactual methodology used to estimate migrant households' income. We use a dataset compiled by the authors from a customized household survey in Tonga. Specifically, we compare the results of the bootstrap prediction method developed by Adams (1989) and Barham and Boucher (1998) with the Propensity-Score Matching (PSM) method.[4] We also test the robustness of the results to different model specifications and, in the case of the PSM method, to alternative matching procedures. We find that the estimated poverty-reducing impacts of remittances are consistent across the different methods, indicating that remittances have a strong impact on poverty reduction, in terms of both the extent and depth of poverty in Tonga.

In estimating the effects of international migrants' remittances on the income of recipient households a number of important methodological issues and challenges are now widely acknowledged in the economics literature. First, remittances cannot be treated simply as an exogenous addition to the income of the recipient

[2] At the time of writing both New Zealand and Australia had initiated a 'guest worker' scheme for unskilled labor from the South Pacific. For an extensive discussion of the case for such a scheme, see World Bank (2006).

[3] It is often argued that early migrants contribute to the reduction of migration costs by providing both cash and in-kind assistance to other community members wishing to migrate. The newcomers draw on existing networks of migrants to find jobs and accommodation. Thus, the longer the history of migration in a community, the lower the migration costs and so the greater the accessibility of poor households to international migration opportunities (Massey, 1990; Massey *et al.*, 1994).

[4] Although the PSM method has not been used previously to assess poverty impacts of remittances this method has been used to estimate the effects of migration on the income of the migrant-sending households (McKenzie *et al.*, 2006).

household, given that this ignores both what the migrant would have earned had migration not occurred, and, the possible effects that the absence of the migrant and the subsequent inflow of remittances could have on the activities and earnings of those remaining. For this reason a major methodological challenge addressed in recent remittances research is the estimation of counterfactual income of migrant households, from which the poverty impacts of remittances can be assessed.

Second, as it cannot be assumed that those who migrate are randomly selected and thus share the characteristics of non-migrants, using non-migrant household earnings to estimate migrant households' counterfactual earnings requires appropriate self-selection testing and, where required, adjustment for self-selection bias in the earnings estimations. For this reason another important methodological challenge is to employ appropriate methods to allow for possible self-selection bias in deriving estimates of migrants' counterfactual income from the observed earnings of non-migrants.

Third, it is usually, and most often implicitly, assumed that only migrant households have access to international remittances (e.g. Adams, 2006). Parameter estimates from non-migrant households are then used to estimate the counterfactual income of migrant households. However the customized Tongan survey which focused on the households' migration status, showed that households without migrants, also received remittances.[5] It is therefore necessary also to test whether remittances received by non-migrant households have an impact on their other sources of income.

These challenges have given rise to a number of innovative methods for estimating the poverty impacts of migration and remittances when the researcher is limited to a single, cross-sectional dataset as in our case. The alternative approaches can be grouped into two broad categories; those that rely on models using instrumental variables (IVs) for migration and remittances to estimate cross-regional or household-level comparisons of observed poverty rates; and, those that rely on the comparison of poverty rates using observed 'with migration and remittances' household incomes, and estimated counterfactual household incomes in a hypothetical 'without migration and remittances' scenario. Within each of these categories, different estimation methods have been used.

[5] The household was defined broadly in terms of as those 'eating from the same pot' and migrant household members were those currently living abroad, who either resided with the household before leaving, or, who would reside with the household if they were to return. Non-migrant households could be receiving remittances from extended family members not treated as members of the household, or, friends or distant relatives.

The studies in the first category focus mainly on estimating the effects of cross-regional variations in migration and remittances (instrumented) on regional-level poverty rates (Adams and Page, 2005; Lopez-Cordoba, 2004; McKenzie, 2006), or the exploitation of a natural experiment to instrument remittances (Yang and Martinez , 2006). In the absence of natural experiments and datasets with a sufficiently large number of regional-level observations the analyst needs to rely on the second form of analysis where the main challenge is to estimate what the households' income would have been in a hypothetical without-migration-and-remittances scenario. The resulting measures of poverty derived from the estimated counterfactual incomes are then compared with the poverty measures based on the households' observed income, including remittances.

The counterfactual income approach was first used in the migration and remittances literature by Adams (1989) and subsequently refined by Barham and Boucher (1998) and others. A variety of estimation and prediction procedures have been used to allow for potential self-selection and other endogeneity problems, each with its particular strengths and weaknesses and potential estimation bias. Invariably, researchers adopt one preferred method which is believed to be less susceptible to bias than the others.

The rest of this paper is organized as follows. Section 2 discusses briefly the alternative methodological approaches to estimating migrant households' counterfactual income. Section 3 discusses the empirical analysis employing both methods for estimating counterfactual income. Section 4 presents and discusses the resulting poverty measures in the observed and counterfactual scenarios and in section 5 conclusions are presented.

2. Alternative approaches to estimating counterfactual income

Estimating counterfactual incomes of migrant households requires appropriate treatment of potential self-selection issues since migrant households cannot be treated *a-priori* as a purely random selection of the population. Two methodological approaches to deal with self-selection issues are examined and applied in this study. The first is the more commonly used bootstrap prediction method which uses a Heckman self-selection model to test and control for self-selection in predicting migrant counterfactual incomes from non-migrant earnings (Barham and Boucher, 1998; Adams, 2006; Acosta *et al.*, 2007). The other is the Propensity Score

Matching (PSM) method which uses a method of matching migrant households with comparable non-migrant households from whom income is then imputed.

Bootstrap Prediction

A counterfactual income approach was initially developed by Adams (1989) in his study of the effects of remittances on poverty and inequality in a sample of households in Egypt. The underlying methodological challenge arises from need to remove both the direct and indirect effects of migration and remittances on the earnings of remaining household members to predict and then impute the home earnings of migrants had they not migrated. Adams estimated a mean regression of incomes for non-migrant households and used the resulting parameter estimates to predict the counterfactual incomes of migrant households. These predicted incomes were then used to calculate the usual poverty rates for the counterfactual, no-migration and remittances scenario. A similar methodology was used by Rodriguez (1998) to study the impact of migration in the Philippines.

It has been acknowledged that this method could give rise to biased estimates for two reasons. First, there is potential self-selection bias. If migrant households are not a random draw of the population, using the regression parameters derived from the sample of non-migrant households would bias the predicted income estimates for migrant households. Second, there would be a problem of underestimated variance of predicted incomes for migrant households if these are based only on the estimated parameter coefficients, without the inclusion of the stochastic term. In effect, this is set equal to zero distorting the range and distribution of predicted household incomes in the counterfactual scenario (Barham and Boucher, 1998; Rodriguez, 1998).

Potential self-selection bias was addressed by Barham and Boucher (1998) using individual survey data from Nicaragua. To estimate counterfactual household income they first tested for self-selection of non-migrant households using a Heckman self-selection model, and then estimated a mean regression of incomes for non-migrant households from the second-stage, OLS equation. The resulting parameters were then used to predict the counterfactual incomes for migrant households. However, they also added a stochastic term component to predicted incomes drawn randomly from the empirical distribution of residuals obtained in fitting the income regression to their sample. Since the error term is drawn randomly, the procedure was replicated one thousand times to derive their bootstrap predictions.[6]

The same bootstrap prediction method to that employed by Barham and Boucher (1998) was followed by Acosta *et al*. (2007) in an analysis of poverty impacts of migration and remittances in 11 Latin American countries. They found that in Mexico, the Dominican Republic and Nicaragua, estimated poverty rates in the counterfactual scenario were lower than in the observed with-migration and -remittances scenario, suggesting that for some households remittances were not sufficient to compensate for the foregone migrant's income, pushing their per capita income below the poverty line.

A problem with this approach is that the results are potentially compromised by biases in the counterfactual estimators given the strong distributional assumptions of the Heckman self-selection model (Deaton, 1997). Differing results in relation to the estimated impacts of remittances on poverty from the different studies make it all that more necessary that appropriate robustness tests are performed. Ideally, experimental data should be used to assess the extent of any such bias. In the absence of experimental data a next-best robustness check is to use alternative counterfactual income estimation methods with the same sample. In none of these previous studies has this been attempted.

Propensity Score Matching

PSM estimators have a long tradition in the evaluation literature, which has devoted considerable attention to the methodological complexities involved in estimating the '*average treatment effect*' of social programs[7]. The objective of the PSM approach is to assess the causal effect of a treatment (migration in our case), on a particular outcome (household income in our case), experienced by those affected by the treatment, after correcting for non-random selection of participants (Ravallion, 2005). The non-treatment outcome of the treated group is not observable. Therefore, in the absence of natural experiments, which could generate a control group with the same observables and unobservables as the treated observations, non-experimental estimators are required to solve the non-random selection problem and to estimate the counterfactual outcome

[6] This method has the advantage of allowing for comparison of the counterfactual income scenario with observed rather than predicted income for the with-migration-and-remittances scenario. A potential disadvantage of this methodology is that in the presence of heteroskedasticity, the error terms added to the predicted income of migrant households could be drawn from households with very different levels of income, which would then affect the income estimations of migrant households (Schafer, 1999).

[7] See Becker and Ichino (2002); Dehejia and Wahba (2002; Heckman, Ichimura, Smith and Todd (1996); Heckman Ichimura and Todd (1997); LaLonde (1986); Ravallion (2005); Rosenbaum and Rubin (1983); Smith and Todd (2005) and Winship and Morgan (1999).

for the treated group in the hypothetical absence of treatment (Winship and Morgan, 1999). In our case, this method is used to impute a counterfactual income to the 'treated' households such that each household in the sample has an outcome (income) with and without treatment (Wooldridge, 2002).

PSM estimators have been developed to correct for non-random selection and to pair each treated observation (migrant households) with a similar control observation (non-migrant households) on the basis of their propensity scores, and to interpret the outcome of the control observation as the counterfactual outcome of the treated observation in the absence of treatment.

The PSM methodology applied here consists of the following steps. First, similar to the Heckman self-selection tests discussed previously, we estimate a probit regression model of the treatment variable; that is migration in our case. However, with the PSM method, the dependent variable of the probit model needs to be re-defined as the probability of a household engaging in international migration[8]. Second, the parameters of the estimated probit model are used to calculate the propensity score, that is, the predicted probability to migrate for each household, based on the observed characteristics included in the model. Third, using the estimated propensity score, each migrant household is matched with the nearest non-migrant household, using the 'nearest neighbor' matching procedure with replacement.[9] Fourth, once a migrant household has been matched with the nearest non-migrant household, the observed income of the latter is imputed to the former.[10] Fifth, checks for potential biases in the counterfactual income estimates are undertaken by testing for 'balance' and 'common support' after matching.

The use of PSM estimators to correct for self-selection relies on the assumption that there exists a set of observable conditioning variables (*X*), for which the non-migration outcome (Y_0) is independent of migration status (*M)*, that is, $Y_0 \perp M \mid X$. In other words, PSM assumes that there is a set of observable conditioning variables that capture all the relevant differences between the treated and the control groups so that the non-treatment outcome is independent of treatment status, conditional on those characteristics

[8] In the probit model of the Heckman two-step estimation, the dependent variable is specified as the probability of not engaging in international migration (see section 3.2).

[9] While it needs to be acknowledged that matching with replacement could affect the variance of the estimates, the alternative approach of not allowing replacement during matching suffers more serious defects; it usually results in many treated observations being matched with control observations that have very different propensity scores; and, the results depend on the sorting order of the data. To test the robustness of the results to the number of matching observations, migrant households are re-matched using the 'two nearest neighbours' procedure.

[10] When applying the ‘two nearest neighbours’ procedure the imputed income of the migrant household is given by the mean income of the two nearest matching observations.

(Wooldridge, 2002; Smith and Todd, 2005). Clearly, this is a potential limitation of extending the PSM methodology to estimate migrants' counterfactual income, for it is conceivable that unobservables, such as an entrepreneurial predisposition of household members, could be correlated with the migration decision. In the absence of data, ideally from a natural experiment, to test for possible bias, we follow a 'next best' approach of checking for robustness by comparing the results obtained from the two, very different counterfactual income estimation methods.

3. Econometric estimations

3.1 The data and preliminary testing

The data are from a sample of 500 households in Tonga, surveyed in the first half of 2005.[11] Table 1 shows the composition of the sample in terms of whether the household had a migrant or not and whether it received remittances or not, in the preceding year.[12] As expected, an extremely high proportion of households had at least one migrant (58.2 per cent) and almost all of these received remittances. [13] Although the high incidence of remitting migrants was to be expected from previous knowledge of remittances and migration networks in the region, what was not expected was the high proportion of households (78.5 per cent) without a migrant that had also received remittances, despite the use of a broad definition of household. In total, almost 90 per cent of households received remittances.

[11] For details of the design of the survey instrument, selection of enumeration areas, sampling and survey administration, see Appendix C of Brown *et al*, 2006.
[12] In this study remittances are defined broadly to include cash transferred both formally through the financial system and informally, hand-carried, as well as remittances in kind (eg. goods sent or carried by the migrant), as well as payments such as airfares, made by the migrant on behalf of the recipient household.
[13] The percentage for Tonga is considerably higher than the 75 per cent found in the 2001 *Household Income and Expenditure Survey*. The most likely explanation for the difference is that the HIES used a rather general question about cash remittances only, while this questionnaire asks numerous questions with cross-checks to assist the respondent in recalling transfers that might not have been considered remittances, such as in-kind transfers, and bills paid on behalf of the household. The 91 per cent figure is also very similar to what was observed in a similar survey over a decade earlier (Brown, 1995).

Table 1 Composition of Sample

	Migrant	Non-Migrant	Total
Remittances	**284**	**164**	**448**
	(56.8%)	(32.8%)	(89.6%)
No-Remittances	**7**	**45**	**52**
	(1.4%)	(9.0%)	(10.4%)
Total	**291**	**209**	**500**
(%)	(58.2%)	(41.8%)	(100.0%)

The summary statistics in Table 2 show the mean incomes and remittances of households in each of the four categories, indicating that while households with migrants enjoy considerably higher mean incomes (excluding remittances), the variability of their income is also much greater.[14] On the other hand, it can also be observed that as expected, average remittances received are substantially larger for households with overseas migrants.

Table 2 Mean Per Capita Income and Remittances by Migration Status (All values in US$ 2004; Standard deviations in parentheses)

	Migrant	Non-Migrant
Remittances		
Income/Capita	1651.7	865.4
	(4,397,7)	(706.8)
Remittances/Capita	987.7	332.9
	(1,967.6)	(421.9)
No-Remittances		
Income/Capita	6,431.0*	1,242.7
	(14,342.7)	(992.5)
Remittances/Capita	----	----
Total		
Income/Capita	1,766.7	946.6
	(4,864.8)	(790.0)
Remittances/Capita	963.9	261.2
	(1,949.6)	(397.9)

* As there are only 7 households in this category this value should be treated with due caution.

[14] Income includes estimates of subsistence income derived from the survey data.

The observation about non-migrant households receiving remittances is important as counterfactual income estimation methods are based on the observed income of non-migrant households. It is usually assumed that only migrant households have access to international remittances (Adams, 2006).[15] With there being four categories of household: (a) migrant households receiving remittances; (b) migrant households not receiving remittances; (c) non-migrant households receiving remittances; and (d) non-migrant households not receiving remittances, the question then arises as to which non-migrant category to use when estimating migrant household income; (c), (d) or the two combined?

The small size of the sample, and in particular the sub-sample of non-migrant households not receiving remittances (45), does not allow us to estimate counterfactual income of migrants from the sub-sample of households with neither migrants nor remittances. Instead, we use all non-migrant households irrespective of whether they receive remittances or not. As this implies treatment of remittances to non-migrant households as exogenous additions to their income, it becomes necessary to test for possible effects on non-migrant households' income. Clearly there is no opportunity cost in the form of forgone migrants' income, but it is possible that remittances received by non-migrant households have indirect effects on their observed income. This hypothesis was formally tested by regressing the natural log of household income on remittances for non-migrant households using instrumental variable techniques (Jimenez and Brown, 2008). It was found that remittances have no statistically significant effect on income of non-migrant households. It is therefore considered reasonable to use the observed income of non-migrant households (excluding remittances) to estimate the counterfactual non-remittances income of migrant households.

As discussed previously, both counterfactual income estimation methods require preliminary estimation of a migration *vs*. non-migration probit model. The bootstrap prediction method requires a Heckman-type self-selection test with a probit migration equation as the first step in estimating non-migrant income. The PSM method requires a probit regression model of the migration *vs*. non-migration selection criterion. The estimated probit migration model and the regression results used in both methods are discussed in Appendix 1.

[15] A recent study in Ghana by Adams *et al.* (2008) similarly finds a significant number of non-migrant households in receipt of remittances.

3.2 Counterfactual Income Estimations

Bootstrap Prediction Method

Following the standard approach to bootstrap predicted income as described in section 2, to test for self-selection two regression models of the natural log of income for non-migrant households are estimated and compared, one with a self-selection control and one without. Formally, the following models for non-migrant households are estimated:

$$Y = \beta_0 + \beta_1 Z + \beta_2 \lambda + \upsilon \qquad (1)$$

$$Y = \beta_0 + \beta_1 Z + \upsilon \qquad (2)$$

where Y represents the natural log of household income from all sources (excluding remittances) which is a function of characteristics of the household and individual household members (Z). In equation (1), λ is the selection control variable taken from the probit migration model reported in Appendix Table A1.3. In both equations υ is the stochastic error term. Definitions of variables and descriptive statistics are shown in Appendix Tables A1.1 and A1.2 respectively, and both sets of regression results are reported in Table 3.

The purpose of the comparison is to ascertain whether the OLS estimates without selection controls (equation 1) would be biased, by comparing the coefficient estimates of the two models and the statistical significance of the selection variable coefficient λ in the second model (equation 2). If evidence of self-selection is found, to estimate household income a Heckman two-step self-selection model should be used.

It can be seen that the coefficient on the selection variable (λ) is small and not statistically significant, indicating that the sub-sample of non-migrant households can be considered a random selection from the population. This is reinforced by the finding that the estimated coefficients in the two models are very similar.[16] This can be explained by historical migration patterns in Tonga where migration flows have been particularly substantial since the 1960s, with just over half of all ethnic Tongans now living outside the country and over 58 per cent of households having at least one migrant abroad (Table 1). Although relatively more skilled and wealthier households might have dominated the early migration waves, the long history of migration from Tonga, as well as the widespread access to international remittances suggests a lowering of

[16] This self-selection test was repeated using cash income only, excluding subsistence income, with similar results, available from the authors on request.

migration costs, thus facilitating migration for the poor. A wider range of migration opportunities is also available to Tongans, including the New Zealand lotto system, skilled migration, and family-reunion visas in most destination countries (Lee, 2003).[17]

Table 3 Non-Migrant Household Income Regression Results (Dependent Variable: Natural Log of Household Income)

Variable	With Selection control	Without Selection control
Log Household Size	0.42(0.75)	0.50(0.99)
Log Household Size^2	-0.05(-0.32)	0.08(0.40)
Dependency Ratio	0.84(1.96)**	-0.08(-0.21)
Female Head	-1.02(-1.76)*	-0.86(-1.76)*
HH Head Age	0.11(1.93)**	0.08(1.74)*
HH Head Age ^{2}	-0.00(-2.14)**	-0.00(-1.91)*
Urban	0.53(2.27)**	0.53(1.64)*
Outer-Island	0.33(0.69)	0.18(0.43)
Tertiary Educated	0.11(0.94)	0.21(2.59)***
Female Head and Tertiary Educated	1.11(2.36)**	1.07(2.27)**
Agricultural Land	0.08(0.32)	0.09(0.35)
Outer-Island and Agricultural Land	0.26(0.70)	0.29(0.88)
Selection control variable (λ)	0.80(1.47)	----
Constant	4.08(2.61)***	4.79(3.42)***
Wald Chi^2(12)	127.38	146.26
Probability > Chi^2	0.00	0.00
R-Squared	----	0.19
Adjusted R-Squared	----	0.14
Observations (Uncensored)	500 (209)	209

Notes: Standard errors clustered at the community (PSU) level. z-statistics in parentheses.
***statistically significant at 1% level, **statistically significant at 5% level, * statistically significant at 10% level.

Looking then at the results of the OLS model without a selection control it is evident that the coefficients for age of household head and its square have the expected signs and are statistically significant, indicating that total income initially increases but at a decreasing rate with the age of household head. Urban households have an income 53 per cent higher than non-urban.[18] As expected, female-headed households are also poorer and earn 86 per cent less than male-headed households. However, for female-headed households,

[17] It is worth noting that McKenzie *et al.* (2006) found evidence of self-selection among Tongan migrants in their study. However their study focused on a very specific sub-set of migrants; viz. recent migrants to New Zealand, who had entered under the lotto system. Moreover, they tested for self-selection of individual migrants and not for migrant households as we do. This leaves open the possibility that these individuals might belong to households that already have other migrant members. In addition, their tests were based only on earnings from wages, excluding subsistence and other non-wage sources of income.

[18] To facilitate interpretation of the coefficient of the dummy variables, the procedure implemented by Goldstein (2007) is used to gauge the percentage impact of the dummy variables on household income.

if there is an additional household member with tertiary education household income increases by 107 per cent.

In section 4 we use the parameter estimates from the OLS model without a self-selection control to estimate counterfactual income of migrant households applying the bootstrap prediction method outlined in section 2.

The Propensity Score Matching Method

Using the same sample data, the sequence of steps to estimate counterfactual income applying the PSM method (described in section 2) was followed.[19] For the matching estimators to exhibit least potential bias, it is important that: (i) both the treatment (migrant) and the control group (non-migrant) are located in the same markets; (ii) the outcome variable is measured in an identical manner for both the treated and the control groups; and (iii) the dataset includes a relatively large number of observable variables that would capture the 'non-ignorable' determinants of treatment[20] (Heckman *et al*., 1996; 1997; LaLonde, 1986; Smith and Todd, 2005). In our case, both the migrant and the non-migrant groups are obviously located in the same markets and were administered the same household questionnaire, satisfying conditions (i) and (ii). However, given that we use a cross-sectional dataset, the choice of exogenous observable variables is restricted, indicating that condition (iii) could be violated. To address this, two specifications of the PSM model were estimated, one with and one without additional community-level migration variables. The two sets of results were then compared in relation to the two criteria of 'balance' and 'common support' which constitutes in integral part of the PSM procedures as discussed later in this section .

In the first step a similar probit migration equation estimated for the Heckman self-selection model is used, except in this instance we estimate the probability that the household is a migrant rather than a non-migrant household, and the identifier variable (*community migration history* is omitted).[21] In the alternative specification with additional conditioning variables we estimate the probit model:

$$P(M_1 = 1) = \beta_0 + \beta_1 Z + \beta_2 K + \varepsilon \qquad (3)$$

[19] We used the "psmatch2" procedure in STATA developed by Leuven and Sianesi (2003).

[20] Indeed, the larger the number of co-variates included, the more relevant differences between the treated and the control groups that would be captured.

[21] With M_1 representing the propensity to be a migrant household, we estimate $P(M_1 = 1) = \beta_0 + \beta_1 Z + \varepsilon$.

where the vector of community variables *K* consists of the following: the average maximum length of absence of migrants from the community; and, the average amount of annual remittances received by households in the community. The community-level variables were constructed from the household survey data, treating households in each the primary sampling unit as the 'communities', but always excluding the observation from the household in question. The inclusion of these community migration variables also facilitates a check for robustness by allowing us to gauge the extent to which the results are sensitive to the choice of variables. The regression results for both models are shown in Appendix Table A2.1. Each of the subsequent steps of the PSM procedure was accordingly performed using both sets of probit predicted migration results, with and without community-level conditioning variables.

Following the PSM procedure outlined in section 2, the estimated coefficients from the two probit models were then used to calculate, for each household, the predicted probability of it being a migrant household, and then each migrant household was matched with the nearest non-migrant household, using the 'nearest neighbor' matching procedure,[22] with replacement.[23] Once the counterfactual income of the migrant household has been estimated by imputing to it the observed income of the nearest non-migrant household, we test and compare the two sets of PSM results for 'balance' and 'common support' after matching, and then test the robustness of the preferred specification estimates to alternative matching rules.

The problem of 'imbalance' can occur when assignment of treatment is not random, and large differences in the distribution of characteristics between the treated (migrant) and the control (non-migrant) groups can be expected. An important part of the PSM procedure is to achieve balance in the distribution of observables between the two groups such that the distribution of observable covariates is approximately the same for both groups after matching (Lee, 2006; Ravallion, 2005). Common balancing checks used are, the reduction in the absolute 'standardized bias',[24] the reduction in the pseudo R^2 in probit models predicting treatment before and after matching, and, t-tests for equality of means. However, Imai, King and Stuart (2008)

[22] The robustness of the results to use of the alternative 'two nearest neighbours' matching rule was also tested, and found to have a negligible effect. (Results available from authors on request.) Although data limitations prevented additional further matching rules it is reassuring to note that using more than a single nearest neighbour does not necessarily reduce bias of the PSM estimators (Ravallion, 2005; Rubin and Thomas, 2000). The results indicated

[23] Although matching with replacement could affect the variance of the estimates, the alternative approach of no replacement suffers more serious defects as it usually results in many treated observations being matched with control observations that have very different propensity scores, and the results depend on the sorting order of the data (Smith and Todd, 2005).

[24] Measured as the difference of the sample means in the treated and non-treated groups scaled by the square root of the average variances in the original samples (Rosenbaum and Rubin, 1985).

have shown that results from the t-tests are sensitive to sample size and can be misleading. Therefore we use only the first two balancing checks, these results being independent of sample size. The relevant data for the standardized bias check are shown in Appendix Table A2.2 and a more detailed analysis of covariate balance in Appendix Table A2.3. They show that covariate balance is improved after matching both with and without the community-level conditioning variables included. However, the reduction in the standardized bias achieved is substantially larger when community-level variables are included. Similarly, in relation to the Pseudo R^2 balance criterion it can be seen from Appendix Table A2.4 that the value is also substantially lower after matching, indicating that the observed characteristics in the model explain very little of the variation of observed propensity scores in the sample.

Biases could also result if the propensity scores of a substantial number of migrant households lie outside the boundaries of the distribution of propensity scores for non-migrant households; that is 'failure of common support' (Ravallion, 2005). If the characteristics of migrant households are substantially different to those of non-migrant households, using the observed income of the latter to impute counterfactual income of the former could not be justified. We follow the common practice in application of the PSM method to base the estimation of counterfactual income only on those observations with common support (Ravallion).[25] The common support statistics are given in Appendix Table A2.5. These show that when community control variables are included, the number households off common support increases from 8 to 22, but it is reassuring that this still leaves over 97 per cent of the sample on common support.[26] The 22 observations not on common support are dropped from the sample when estimating counterfactual incomes applying the PSM method with community controls.[27]

4. Poverty Measures with Observed and Counterfactual Income

In this section we report and compare the estimated impacts of remittances on both the extent and depth of poverty using the standard *Poverty Headcount Ratios* and *Poverty Gap Ratios* respectively. The former

[25] However, we also tested for any resulting sample bias as a consequence of dropping those observations without common support. The results were very similar. Available from the authors on request.

[26] See also the histogram of the propensity scores for treated (migrant) and untreated (non-migrant) households in Appendix Figure A2.1.

[27] A more stringent test would be to impose common support by dropping a certain percentage of treated observations at which the propensity scores density of the untreated observations is the lowest. The sample was trimmed at 5 and 10 percent. Reassuringly the poverty estimations did not change substantially. Results available from the authors on request.

measures the percentage below the poverty line, while the gap ratio measures, for those in poverty, the mean difference between their income level and the poverty line, expressed as a percentage of the poverty line. In the with-migration-and-remittances scenario observed income (including remittances) of both migrant and non-migrant households is used to calculate the poverty indicators. In the counterfactual scenarios the indicators are calculated using observed income (excluding remittances) for non-migrant households and imputed income (excluding remittances) for migrant households as estimated by the different methods.[28]

To calculate poverty rates, an estimated threshold poverty-level of income for the household, expressed on a per capita basis is required. As there is no official household-level poverty line for Tonga, in this study we use an estimate based on the respondents' self-assessed poverty level which we derived as part of this study and which is discussed in some detail elsewhere (Jimenez, 2008; Brown and Jimenez, 2008).[29] The main results comparing the bootstrap and PSM prediction methods are presented in Table 4.

Table 4 Poverty Indicators With and Without Remittances: Reported and Counterfactual Income Estimates (2004)

Poverty Measure	REPORTED INCOME INCLUDING REMITTANCES (1)	COUNTERFACTUAL INCOME WITHOUT REMITTANCES	
		(2) Bootstrap Prediction#	(3) PSM Prediction+
Head Count Ratio (%)	32.7	49.5	47.4
Gap Ratio (%)	11.6	36.1	22.7
Observations	(n=500)	(n=500)	(n=478)

OLS estimates
+ With community controls and on common support

The first column of Table 4 shows the poverty rates with migration and remittances calculated from observed household income. These can then be compared with the poverty rates using the two counterfactual income estimates (without remittances) reported in columns 2 and 3.

First, it is evident that there is a high degree of consistency between the two counterfactual income methods. Both estimates show a substantial reduction in poverty rates with migration and remittances; from

[28] The estimations of per capita income in the counterfactual scenarios include all household members including migrants and non-migrants, while in the observed, with-migration scenario migrants are not included.
[29] The questionnaire included a section on self-assessed level of need from which a community-level (median) 'required income' was calculated.

47.4 per cent and 49.5 per cent in the two counterfactual scenarios to 32.7 per cent in the observed, with-migration-and-remittances scenario.

Second, the two counterfactual income estimates of the depth of poverty are consistent in showing a substantial reduction in the poverty gap ratio with migration and remittances. However, the magnitude of the difference varies quite substantially between the two methodologies. The bootstrap prediction method shows a reduction from a counterfactual income estimate of 36.1 per cent to 11.6 per cent with migration and remittances, while the PSM estimate shows a reduction from a counterfactual estimate of 22.7 per cent to 11.6 per cent. Despite this difference in magnitude of the effect, it is reassuring that the two very different methodological approaches provide unambiguous evidence that remittances contribute significantly to the reduction in both the incidence and depth of poverty.

It is also worth noting that when counterfactual incomes were re-estimated using the bootstrap prediction method with a Heckman self-selection model, the resulting without-migration-and-remittances poverty rate was considerably higher (61.7 per cent). This suggests that introducing a self-selection control with the bootstrap prediction method when there is no evidence of self-selection could introduce a serious bias in the results. One advantage of the PSM method is that if self-selection is present in the migration decision it would be controlled for by the matching procedure, but would not introduce a bias in the absence of self-selection.

It is also important to acknowledge that neither counterfactual income estimation method attempts to correct for the general equilibrium effects of migration and remittances on the earnings of the communities at large, both migrant and non-migrant households. For this reason there could be biases in the estimated counterfactual poverty rates, especially if they are used to assess the potential cost and benefits of a hypothetical no-migration scenario under which all migrants presently abroad are assumed to return home. The direction of the bias will depend primarily on what general equilibrium effects migration and remittances have had on both capital returns and labor returns for the skilled and the non-skilled population in each out-migration community. Further research would be required to examine these which would require analysis of a complex array of factors, including the local institutional context in which labor and capital markets operate.

5. Conclusion

Previous studies estimating the impacts of migrants' remittances on poverty in the migrant-sending country using non-experimental datasets, have applied one of a number of alternative methods for the treatment of potential self-selection bias and other sources of endogeneity. Although an overwhelming majority of these studies indicate that remittances reduce poverty, the magnitude of the estimated effects varies quite considerably, some even indicating *increased* poverty with migration and remittances. In this study we tested the sensitivity of findings to alternative methods and specifications for estimating counterfactual income in the hypothetical no-migration scenario. We used household-level survey data from Tonga applying two different counterfactual income estimation methodologies; the bootstrap prediction method with Heckman self-selection tests, and, the Propensity Score Matching method, hitherto not used in measuring the poverty impacts of remittances.

We found that our results were robust to the counterfactual income estimation method used, showing a substantial reduction in both the incidence and depth of poverty with migration and remittances, taking into account the opportunity costs of migration in terms of the estimated forgone income of migrant household members. Further tests with alternative model specifications and matching procedures showed very little variation in the estimates. The robustness of these results provides strong evidence that in Tonga, even after accounting for the foregone income of the households' migrants, the net improvement in income from remittances contributes substantially to poverty alleviation. These findings are also important in the context of the current migration policy debates in the region, as they show strong support for the potential poverty alleviating role of increased migration opportunities for Pacific islanders to the two main destination countries, Australia and New Zealand.

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Appendix 1 Testing for Migrant Household Self-Selection

A probit regression model is used to estimate the probability (P) of a household *not* having a migrant (M) abroad:

$$P(M = 1) = \beta_0 + \beta_1 Z + \beta_2 K + \varepsilon \qquad \text{(A.1)}$$

Regarding the characteristics of the household and individual migrants (Z) in equation (A.1), two issues should be noted. First, for households with migrants, the characteristics of individual household members include those who are currently living abroad. For example household size includes both migrant and non-migrant household members. Second, variables such as household wealth, which could be endogenous[30], are excluded from the estimated model. On the other hand, though migration costs (K) in equation (A.1) were not directly observable, a variable measuring the history of migration in the community is used as a proxy.[31] Early migrants contribute to the reduction of migration costs by providing both cash and in-kind assistance to other community members willing to migrate. The newcomers draw on existing networks of migrants to find jobs and accommodation. Thus, the longer the history of migration in a community, the lower the migration costs and the higher the expected propensity to migrate of the household (Massey, 1990; Massey *et al*. 1994). This variable (K) identifies the probit equation, since there is no reason to believe migration costs would have any effect on household income. The parameter estimates of the probit model are then used to construct the self-selection variable λ, (the Inverse Mill's Ratio) for non-migrant households.

The definitions and summary statistics of the variables used to estimate the model are presented in Appendix Tables A1.1 and A1.2 respectively.

[30] We only observe the household wealth after migration and remittances have direct and indirect effects on household wealth.

[31] As census data on the migration history of the surveyed communities were not available, a community-level variable was thus constructed from the household data. For each household in the community the length of stay abroad of the first member who migrated was identified. The mean of the first migrant's length of stay was computed for all households in the community, excluding the household observation in each instance.

Appendix Table A1.1 Descriptive Statistics: Variables in Migration Probit Model

Variable	Variable description
Log HH Income	Log of household's total income including subsistence income in US$
Log Household Size	Number of Household Members, including migrants in the case of migrant households
Dependency Ratio	Proportion of household members below 14 and over 60 years of age. Includes migrants in the case of migrant households
Female Head	Dummy for household with female head
HH Head Age	Age of household head
Urban	Dummy for household in urban areas (omitted category household in rural areas)
Outer-Island	Dummy for household in outer-island (omitted category household in rural areas)
Tertiary Educated	Number of household members with tertiary education
Female Head and Tertiary Educated	Interaction dummy female household head and dummy presence of household member with tertiary education
Agricultural Land	Dummy household owns agricultural land in 2003 (omitted category does not own agricultural land in 2003)
Outer-Island and Agricultural Land	Interaction dummy agricultural land, dummy outer-island
Community Migration History	Community variable measuring the average length of stay of the first person who migrated from each community household

Appendix Table A1.2 Descriptive Statistics: Means and Standard Deviations (Standard Deviations in parentheses)

	All Households (n=500)	Migrant (n=291)	Non-Migrant (n=209)
Log Household Income	7.713	7.555	7.934
	(2.982)	(3.610)	(1.754)
Log Household Size	1.787	1.975	1.525
	(0.571)	(0.454)	(0.614)
Dependency Ratio	0.318	0.261	0.397
	(0.227)	(0.178)	(0.2622)
Female Head	0.220	0.261	0.163
HH Head Age	51.894	55.010	47.555
	(14.898)	(14.685)	(14.119)
Urban	0.500	0.539	0.445
Outer-Island	0.250	0.210	0.306
Tertiary Educated	0.478	0.643	0.249
	(1.068)	(1.250)	(0.683)
Female Head and Tertiary Educated	0.074	0.096	0.043
Agricultural Landh	0.514	0.526	0.498
Outer-Island and Agricultural Land	0.164	0.148	0.187
Community Migration History	6.958	7.957	5.567
	(3.475)	(3.192)	(3.380)

Appendix Table A1.3 shows the probit results of the migration equation (A.1)[32]. It should be recalled that the dependent variable is the probability of the household *not* having a migrant.

Appendix Table A1.3 Results of Migration Probit Model
(Dependent Variable: Non-Migrant Household = 1)

Variable Name	Coefficient (z-stat)	Marginal Effects
Log Household Size	-1.45(-2.16)**	0.26
Log Household Size^2	0.04(0.21)	0.08
Dependency Ratio	2.87(6.42) ***	0.17
Female Head	-0.17(-0.69)	0.09
HH Head Age	0.07(1.18)	0.02
HH Head Age ^{2}	-0.00(-1.63) *	0.00
Urban	0.21(0.99)	0.08
Outer-Island	-0.01(-0.03)	0.21
Tertiary Educated	-0.13(-1.23)	0.04
Female Head and Tertiary Educated	0.04(0.15)	0.11
Agricultural Land	-0.02(-0.06)	0.10
Outer-Island and Agricultural Land	0.51(1.04)	0.19
Community Migration History	-0.14(-4.01) ***	0.01
Constant	1.26(0.73)	
LR chi2(13)	187.69	
Prob > chi2	0.00	
Pseudo R^2	0.37	
Observations	500	

Notes: Robust z-statistic values in parentheses. Standard errors clustered at community level ***statistically significant at 1% level, **statistically significant at 5% level, *statistically significant at 10% level.

[32] Similar results were also obtained when the average number of migrants in the community was used as the identifying variable in the probit migration equation. Results available from authors on request.

Appendix 2 PSM Balancing and Common Support Test Results

Appendix Table A2.1 Migration Probit Model Results: With and Without Community Controls (Dependent Variable: Migrant Household = 1)

Variable Name	Without Community Controls	With Community Controls
Log Household Size	1.27(2.21) **	1.28(8.41) ***
Log Household Size^2	0.02(0.10)	----
Dependency Ratio	-2.99(-8.48) ***	-2.84(-7.77) ***
Female Head	0.32(1.69) *	0.16(0.79)
HH Head Age	-0.08(-2.33) **	-0.07(-1.92) *
HH Head Age ^{2}	0.00(2.96) ***	0.00(2.70) ***
Urban	-0.07(-0.4)	-0.22(-1.23)
Outer-Island	-0.03(-0.11)	-0.31(-1.36)
Tertiary Educated	0.14(1.94) **	0.13(1.76) *
Female Head and Tertiary Educated	0.08(0.32)	----
Agricultural Land	0.08(0.47)	-0.02(-0.07)
Outer-Island and Agricultural Land	-0.27(-0.84)	-0.09(-0.59)
Community Migration History	----	0.14(5.84) ***
Plcae-Rem	----	4.31E-06(0.07)
Constant	0.17(0.18)	-1.03(-1.08)
LR chi^2 (12)	210.15	247.70
Prob > chi^2	0.00	0.00
Pseudo R^2	0.31	0.36
Observations	500	500

Appendix Table A2.2 Summary Statistics for Absolute Standardized Bias Before and After Matching

	Without Community Controls		With Community Controls	
	Before match.	After match.	Before match.	After match.
Mean Bias	34.86	8.51	38.66	4.99
Median Bias	24.23	7.6	31.7	3.88
SD of Bias	24.61	6.37	24.87	2.96
Minimum Bias	5.63	0.46	5.63	1.83
Maximum Bias	84.75	18.30	83.30	9.38
Explanatory vars.	11	11	12	12

Appendix Table A2.3 Covariate Statistics for Absolute Standardized Bias Before and After Matching

Variable	Sample	Without Community Controls	With Community Controls
HH Size	Before match	84.80	83.3
	After match	1.4	-2.7
HH Dependency Ratio	Before match	-60.70	-60.70
	After match	7.4	8.8
Female HH Head	Before match	24.20	24.20
	After match	7.6	1.8
Age of HH Head	Before match	51.80	51.80
	After match	-3.7	2.2
Square Age of HH Head	Before match	50.30	50.30
	After match	-1.9	4.30
Urban	Before match	19.00	19.00
	After match	0.5	-8.9
Outer-island	Before match	-22.20	-22.20
	After match	-13.1	9.4
Adult Education	Before match	39.10	39.10
	After match	8.6	-2.6
Interaction Female,	Before match	15.40	15.40
Education	After match	17.3	4.3
Own Agricultural Land	Before match	5.60	5.60
	After match	-18.3	-3.0
Interaction Outer-island, Ag.	Before match	-10.40	
Land	After match	-13.80	
Av. Community Maximum	Before match	--	72.70
Length of Stay	After match	--	8.4
Av. Community	Before match	--	19.70
Remittances Received	After match	--	-3.50

*n = 500; ** n =478 observations on common support

Appendix Table A2.4 Pseudo R2 Balance Statistics Before and After Matching

	Without Community Controls		With Community Controls	
	Before match.	After match.	Before match.	After match.
Pseudo R^2	0.30	0.02	0.36	0.01
LR Chi^2	20.57	17.31	247.7	6.41
p-value	0.000	0.10	0.00	0.894

Appendix Table A2.5 PSM Tests of Common Support with and without Community Controls

	Off Common Support	**On Common Support**	**Total**
Without Community Controls			
Non-Migrant HH	0	209	209
Migrant HH	8	283	291
Total	8	492	500
With Community Controls			
Non-Migrant HH	0	209	209
Migrant HH	22	269	291
Total	22	478	500

Appendix Figure A.1 Histogram of Propensity Scores