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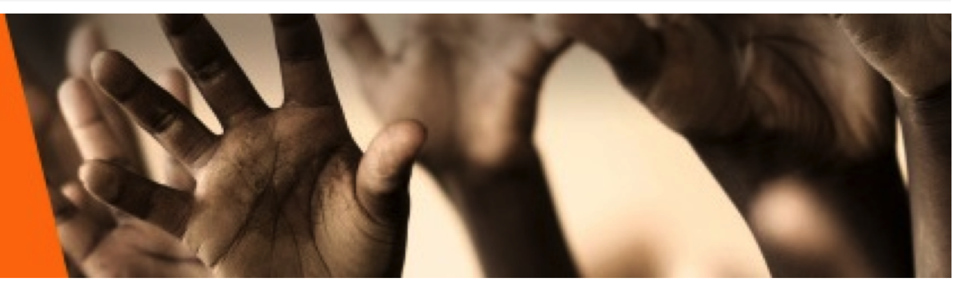

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# The Dynamics of Microcredit Borrowings in Cambodia*

Vathana Roth[†], Abdelkrim Araar[‡], Bopharath Sry[§] and Dalis Phann[¶]

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**Abstract**

We employ 2011–2014 panel data of eleven villages in Cambodia to investigate the impact of microcredit access on paddy quantity and income, expenditure on inputs of paddy production, and self-employment income. The panel data enables us to implement difference-in-differences and triple differences estimators. We find that credit participants observe a 26.1% increase in paddy income, 68.9% in paddy quantity and 26.5% in expenditure on inputs of paddy production. Poorer households benefit more from credit participation. Participants also observe an increased non-land durable assets relative to those of non-participants, particularly agricultural assets. We find week evidence that women participants benefit more from credit programme than male counterparts. Although women are more likely to start self-employment activities with the loans—mainly in informal sector—the income gains are not statistically different from zero relative to what men earn.

**Keywords:** multiple-source borrowing, microcredit, Cambodia, formal and informal sources, paddy production, difference-in-differences, difference-in-difference-in-differences, heterogeneous effects

**JEL codes:** C11, G21, O1, Q11.

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# 1 Introduction

Access to finance has been one of the main, albeit not the most important, factors constraining business growth and expansion in Cambodia. The constraint is even more worrisome for small scale self-employed activities—agriculture and non-agriculture—in rural areas where outreach, breadth and depth of financial services are limited and practically inadequate.

Microfinance, mostly microcredit, has been significant in Cambodia's post-war reconstruction and rehabilitation and, as many believe, a solution to ease credit constraints of borrowing households. As of 1st quarter of 2016, 70 Microfinance Institutions (MFIs) had a combined loan amounts of USD3.1 billion, serving 2 million customers and employing 26,940 people nationwide. On March 2016, the National Bank of Cambodia together with MFIs organised a 3 day National Conference on Microfinance Development, further reiterating the important role of MFIs in helping spur economic growth.

There are, however, mixed reactions to operations of and benefits provided by those financial institutions. Proponents argue that the increased competition both within MFIs and between MFIs and commercial banks has contributed to the lower, and decreasing, interest rates on loans. Also, the existence of MFIs enables rural population to access various financial services that they would have otherwise lacked. Critics, nonetheless, accuse MFIs of charging above-market interest rates and of implementing repayment policies that are not conducive for borrowers and their businesses and that are too much profit-oriented. There have also been accounts, mainly anecdotal, that growing competition among MFIs enable households to participate in multiple credit programmes, potentially contributing to over-indebtedness and repayment defaults[1].

Empirical studies examining the dynamic effects of microcredit access on welfare of borrowing households are numerous in other contexts, but sketchy in the case of Cambodia. The latest empirical work, to the best of our knowledge, investigating the issue and employing some sorts of treatment and control framework is Kang and Liv (2011).

In other contexts, the impact of microcredit[2] access has recently been debated given mixed results from recent empirical research. While proponents (Khandker and Samad, 2014; Pitt and Khandker, 1998; Pitt et al., 2006) have advocated positive social and economic effects of microcredit on borrowers' wellbeing, others (Morduch, 1998; Roodman and Morduch, 2014) demise the long-held wisdom that access is a miracle. Even its social goal of helping the financially excluded poor households has also been scrutinised.

Using longitudinal data spanning 20 years of microcredit operation in Bangladesh, Khandker and Samad (2014) find that microcredit access is still helping the poor increase their household welfare. They find also that the effect is higher for female borrowers than male; and multiple borrowings help raise borrowers' assets and net worth rather than create indebtedness. Khandker and Samad (2013) show that microcredit participants in Bangladesh have benefited and are not likely to trap in debt. Other studies that find similar positive effects include Khandker (2005), Pitt and Khandker (1996, 1998), Pitt et al. (1999), Pitt et al. (2006) and Pitt and Khandker (2012). Using triple difference and propensity score matching methods with panel data of households in Bangladesh, Islam (2011) finds that the benefits of microcredit participation vary with duration, emphasising positive effects accrued to continuous and longer-term participants. Imai et al. (2012) also find positive relation between countries with high MFI's gross loan portfolio per capita and lower poverty rates.

However, a few studies find little or no impact of credit access on outcomes. Using a cross-sectional survey in Bangladesh, Morduch (1998) finds no significant im-

pact of microcredit on the level of consumption. Replication by Roodman and Morduch (2014) provides weak evidence of the impacts suggested by Pitt and Khandker (1998) and Khandker (2005). Results of the current impact evaluations using Randomised Controlled Trails (RCTs)—believed by many as a superior design—have also cast doubts on previous findings of the positive effects of credit access on poverty reduction and improvement in other non-consumption (income) indicators. Duflo et al. (2013) find no significant impact on per capita consumption and no discernible changes on education, health and women's empowerment of treatment households. Fafchamps et al. (2011) conclude that access to credit (in cash and in-kind) is a necessary condition for growth performance of male and female micro-entrepreneurs, but not sufficient, implying that credit access alone is not a quick fix. Akoten et al. (2006) reach similar conclusion. Karlan and Valdivia (2011) show that net borrowing has little or no impact on business activities of the treated group. The authors also find little evidence that access has larger impact for female entrepreneurs. Crepon et al. (2011) find no significant differences between consumption, education and health of treatment and those of controls. However, the effect is heterogeneous depending on whether households have pre-existing self-employed activities. Blattman and Ralston (2015) seem to reject the idea that more microcredit is a solution for employment and poverty reduction in poor and fragile states. Capital injections—be it cash, tools or livestock—the authors argue have the most promises in creating employment and increasing profitability of the poor's 'portfolio of work'. Other RCT studies find positive impact, but the magnitude is 'modest' (Banerjee et al., 2015; Bauchet et al., 2011; De Mel et al., 2008, 2009).

The aim of this research, thus, is to fill the existing gap and to contribute to the debate by testing three propositions. First, we assess the impact of microcredit participation on paddy quantity and income and expenditure on inputs used in paddy production. We also examine the impact on self-employment income (excluding paddy income), non-land durable assets and per capita consumption. We hypothesize that access to microcredit remains important to increased borrowers' wellbeing relative to non-borrowers'. Second, we examine whether multiple borrowings have any impacts on outcomes. On this, we hypothesize that multiple programme membership can contribute to lower wellbeing relative to membership in single programme. Finally, we investigate whether gender has any role in the use of credit. Particularly, we hypothesize along the line of previous studies (for example, Pitt and Khandker, 1998; Khandker, 2005; Islam, 2015)[3] that female participants tend to benefit more than male counterparts. Besides, we shed some lights on the other two aspects of credit participation: (1) whether credit participants who dropped out of the programme continue to benefit relative to those who stayed and (2) whether the benefits are heterogeneous among participants, as Banerjee et al. (2015) put it 'good for some, bad for some' [4].

The study contributes on three main fronts. First, we utilise the new 2014 round of panel dataset[5]. Second, while most previous studies use binary choice as treatment assignment, we attempt to extend this to continuous treatment variable. We are, however, aware of response (measurement) errors in reporting loan values. Lastly, instead of lumping households into clients (treatment) and non-clients (controls), we devise sample households into: new clients, non-clients, continuous clients and drop-outs. This helps reduce heterogeneity of characteristics when comparing outcomes.

Selection bias is common in credit participation. Experimental and quasi experimental methods, RCTs or IV-related models, are believed to be the most suitable estimators in dealing with identification issue. We employ fixed-effect difference-in-difference (DID) and triple difference (DDD) estimators[6] using 2011-2014 panel data of eleven villages.

We find that microcredit borrowing has a positive impact on paddy quantity and income and expenditure on inputs of paddy production. That is, borrowing households observe a 26.1% increase in paddy income, 68.9% in quantity and 26.5% in expenditure on factors of paddy production. Poorer borrowing households benefit more from credit relative to richer ones, indicating that poor households are much more credit contraint and easing that help boost production performance. There is a statistically positive impact of microcredit on non-land durable assets—increased investment in agricultural equipments.

The results also indicate that multiple programme participation—borrowing from formal and informal sources—increases paddy income and quantity; the effects are statistically significant at 5% level. Nonetheless, borrowing from informal sources is more expensive than that from formal ones—6.6% to 2.6% in monthly interest rates, respectively. We raise concerns that repayment might be an issue for multiple-borrowing households. The reason is, for households whose first loan is from MFIs, majority of them obtain the second loan from informal sources, mainly moneylenders. This is worrisome given that the monthly interest rates charged by the informal lenders is roughly 2.5 times higher relative to that charged by MFIs. There is little evidence that MFIs exercise excessive lending practices that could contribute to over-indebtedness and payment difficulties of clients. Although borrowing from more than one MFI increases during the survey periods, the rise is not statistically significant.

On women and credit, we find weak evidence that women participants tend to benefit more from microcredit than their male counterparts. However, women participants are more likely to start some kinds of self-employment activities with the loans, mainly in the informal sector. The effect is only significant at 20% confidence level. Lastly, the results show that continuous borrowers $(1,1)$ are likely to benefit more relative to drop-outs $(1,0)$.

The remaining of the paper is organised as follows. Section 2 discusses econometric models and presents outcome and treatment variables. It also highlights attrition issues. Section 3 describes data and discusses results. Section 4 concludes.

# 2 Econometric models

## 2.1 Main identification

This study uses difference-in-difference and triple difference approaches. Both these approaches are implemented in regression framework to ensure additional controls[7]. We start with the following growth model:

$$Y_{ijt} = \theta_i t + \delta CD_{ijt} + \beta X_{ijt} + \mu_{ij} + \xi_j + \epsilon_{ijt} \tag{1}$$

where $Y_{ijt}$ is the outcome of interest of household $i$ in $j$–th village at time $t$; $t$ is time-dimension used to measure economy-wide changes that possibly affect outcomes of both participants and non-participants in the absence of credit participation; $CD_{ijt}$ is amount of outstanding credit borrowed of $i$–th household in village $j$ at time $t$; $X_{ijt}$ is a vector of characteristics of households $i$ in village $j$ at time $t$. $\mu_{ij}$ a vector of households' time-invariant components in $j$–th village and $\xi_j$ is a set of village time-invariant factors. $\epsilon_{ijt}$ is error term assumed to have zero conditional mean. $\theta$, $\delta$ and $\beta$ are unknown parameters to be estimated.

Equation (1) could be estimated using fixed-effect method by first differencing. Nonetheless, fixed-effect method would be bias if $\mu_{ij}$ and $\xi_j$ are time-varying. To address that and to achieve a more robust and consistent estimate and to partly address one of the

weaknesses of strong assumptions on the error term pertaining to extrapolation methods, Difference-in-Differences (DID) is used. In regression form, DID could be written as:

$$Y_{ijt} = \alpha_i + \beta X_{ijt} + \gamma_T T + \gamma_G G_{ijt} + \gamma_{TG} GT_{ijt} + \epsilon_{ijt} \tag{2}$$

$\gamma_{TG}$ is said to indicate the benefits credit borrowers obtain from participation. Beside using DID[8], we also explore the triple differences. Because DID regression has a strong assumption of homogenous impact within the treated group, triple–differences can control for different shocks affecting two distinct groups (male and female in this study). The unaffected potential in the control group can be used to differentiate away the unobservable factors. Thus, we compare households with credit and those without credit at period $t = 2014$. For the triple-difference estimator, we use double-difference estimation for credit borrowing households minus difference–in–differences estimator for those who left the program $(1, 0)$ or those who are not involved in the program at all $(0, 0)$. In the sample there are 27% and 15% that leave the program $(1, 0)$ and that do not involve in the program $(0, 0)$, respectively.

$$\begin{aligned} Y_{ijt} = \alpha_i + \beta X_{ijt} + \gamma_A A + \gamma_G G_{ijt} + \gamma_T T + \gamma_{AG} AG_{ijt} \\ + \gamma_{AT} AT_{ijt} + \gamma_{TG} GT_{ijt} + \gamma_{AGT} AGT_{ijt} + \epsilon_{ijt} \end{aligned} \tag{3}$$

where $A$ takes the value of 1 if the household is headed by female and 0 otherwise; $G$ is the treatment variable taking the value of 1 if the household is outstanding loans and 0 otherwise; $T$ is time (0 before the treatment and 1 after). Figure 1 illustrates the casual effects of credit participating households headed by female and those headed by male.

For the choice between the random and fixed effect models, we use Auxiliary test, proposed by Mundlak (1978) which is valid even under heteroscedasticity, while (Durbin-Wu) Hausman test is based on the assumption of the homoscedasticity of the error term. Thus, to test the random model assumption of unrelated effect (UE) or the non-correlation between the error term and the observables (X), we use the following auxiliary regression.

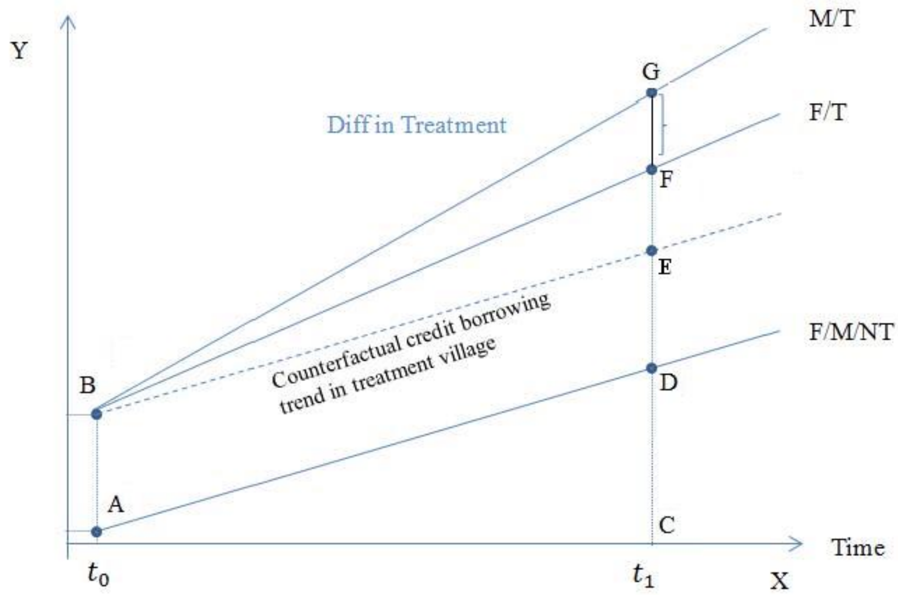
$$Y_{ijt} = \alpha_i + \theta_i t + \delta CD_{ijt} + \beta X_{ijt} + \lambda Z_{ij} + \xi_j + \epsilon_{ijt} \tag{4}$$

In Equation (4), $Z_{ij} = 1/T Z_{ijt}$ are the time averages of all time-varying regressors. Time fixed $\xi_j$ included if RE and FE estimation is counted in the regression. The test of the UE assumption is equivalents to a joint Wald-test of zeros $\lambda$ coefficients. There is a very strong assumption for UE of the RE model. For this study, we find that the FE model is the most appropriate.

Internal validity of the above estimators rests on the correction of time-invariant unobservable differences between credit participants (treatment) and non-participants (controls). Failures, however, to control for time-varying unobservables could still lead to biased estimates. In other words, the discussion clinges on determinants affecting demand for credit.

Using the same dataset as ours, but the 2014 round, Lun (2013) shows that demand for rural credit is positively dependent on income shocks—the odd of borrowing is 0.5 time larger for households experiencing such shocks relative to those without ones. Although we control for household-level shocks that could systematically influence borrowing decision, the sample could be non-random initially if households self-select themselves into borrowing due to the shocks. We show that this is not the case in our estimators. First, as shown in Table 5, 42.9% of the households reported experiencing some sorts of negative shocks in 2011. That experience declined roughly by 9.2% in 2014. Disaggregated by borrowing status, the experience dropped among all groups, but new comers. The increase, however, is not statistically significant at the typical level. This

Figure 1: Causal effects in difference-in-difference-in-difference estimator




- DC: time effect
- ED: selection bias effect
- EC: ATT of female
- EG: ATT of male
- FG= EG – EC: Difference in treatment M/F.

Source: Authors' preparation.

might indicate that the negative shocks induce households to borrow. Second, although negative shocks are likely to increase the demand for credit, the supply side is also important. Lenders might tighten lending during difficult times. Lastly, micro-insurance is almost non-existent in rural credit market in Cambodia and it is unlikely that MFIs would lend against shocks. Hence, it is fairly reasonable to argue that household-level shocks in this context has a very minimal influence on the non-randomness of the studied sample.

The second main determinant shown in Lun (2013) is the size of landholdings; that is, a one hectare increase in land holding increased the odd of borrowing by 1.1 times larger relative to small landholders. One of the reasons is that land could be used as collaterals. That lending practice, nonetheless, has subsided to one that is based mainly on business proposal plans and/or existing businesses. Land ownership and types of land title remain popular collaterals for formal borrowing. Kang and Liv (2011) show that 98% of 1,876 MFI clients reported owning land at the time of survey relative to 88% of 568 non-clients. 17% of clients possessed hard land title compared to 23% of non-clients. Relative to non-clients, clients possess more soft land title–61% to 52%. Our dataset shows similar trend on the status of land ownership (Table 5). That is, 97.1% of new clients reported owning residential land they were living in 2014, a 2.4 percentage point increase from 2011. It is 98.3% for non-clients, a 1.5 percentage point rise from the same period[9]. Although types of land title might explain selection into borrowing, our data and those of Kang and Liv (2011)[10] indicate no systematic influence on the studied sample[11].

Another crucial issue to be discussed is the possible effects of non-credit services. Banerjee et al. (2015) note that in the six randomised control trials studies they report, none investigates the impact of non-microcredit provisions that are increasingly popular among MFI institutions (for example, microsaving, training courses on entrepreneurial activities, financial management and microinsurance)[12]. One thing that we can infer from this observation is that the supplemental services might possibly confound the causal links between microcredit borrowings and outcomes[13]. Thus, understanding the prevalence of such institutional arrangements is an important factor for econometric identification.

In Cambodia, microfinance services have grown in numbers, variations and sophistication. The law on microfinance is, however, still restrictive on service provisions of MFIs. For instance, only 8.0% of the 59 microfinance institutions are allowed to take deposits. Based on the results of in-depth interview with CEOs and senior managers of 14 MFIs and a review of relevant documents, Kang and Liv (2011) highlight that none of the MFIs provided complementary services to clients on a regular basis even though the interviewed CEOs and managers expressed their intention to expand those services. That said, we argue that microcredit provision in Cambodia is more common than the non-microcredit services, resulting in a low probability of confoundedness and spillover[14].

The growing discrepancies of estimated results on the impact of microcredit access among empirical research (some find positive, some negative and some no impact) has led one to hypothesize that effects could be heterogeneous. Banerjee et al. (2015) postulates that efforts should be made to understand how microcredit participation affects different groups of borrowers—what they call the ‘distributional effects’. In this study, we analyse the heterogeneous effects of credit participation[15] by estimating the following quantile difference-in-difference regression (QDD)[16].

$$Y_{(\tau)} = \alpha_i + \beta X_{ijt} + \gamma_T T + \gamma_G G_{ijt} + \gamma_\tau GT_{ijt} + \epsilon_{(\tau)ijt} \tag{5}$$

where $Y_\tau$ is outcomes by quantile. $\gamma_\tau$ is the effect of credit access on different quantiles of the examined outcomes. $\epsilon_{(\tau)ijt}$ is assumed to have zero conditional mean—$E(\epsilon_\tau | X_{ijt}, T, G, GT) = 0$.

## 2.2 The choice of the econometric models

As far as empirical methods are concerned, the current wave of impact studies of credit access on borrowers’ outcomes employs Randomized Controlled Trials (RCTs) given its explanatory power to deal with endogeneity issues and its relaxed assumptions (Duflo et al., 2013; Dupas and Robinson, 2013; Karlan and Zinman, 2010; Karlan and Valdivia, 2011; Gine et al., 2011; Fafchamps et al., 2011; Brune et al., 2011; Crepon et al., 2011; De Mel et al., 2008, 2009; Akoten et al., 2006)[17].

RCTs are, however, no panacea to the statistical problems evaluators face when using quasi-randomised or non-experimental approaches. Deaton (2010) argues that "actual randomization faces similar problems as does quasi-randomization, notwithstanding rhetoric to the contrary." Generalisation of RCT results might also be context-dependent, indicating that positive (negative) impacts of credit access on outcomes found in a certain context might not equally imply positive (negative) effects in others. One, thus, is still able to replicate the same study in a slightly, sometimes completely, different setting to examine whether the same patterns could be observed (Bauchet et al., 2011). Banerjee (2013) also highlights difficulties that plague RCT implementation when assessing the impact of microcredit. In addition, sample size in RCTs is often small compared to that of non-experimental surveys, including treatment and controlled units, affecting

representation of the selected sample (Bauchet and Morduch, 2010). The length of programme evaluation is also rather short (between 12-18 months) that might not capture full dynamism of the effects (Khandker and Samad, 2014). Field management is another critical concern in randomized survey failure of which would have significant effects on data reliability[18].

The second common estimator in the realm of non-experimental tools is instrumental variable approach. Example studies investigating similar research questions as ours and using such estimator include Khandker and Samad (2014), Khandker and Samad (2013), Pitt and Khandker (2012) and Pitt et al. (2006). Khandker and Samad (2014) uses land eligibility criteria as placement variable, allowing the authors to implement fixed-effects-instrumental-variable (FE-IV) estimator.

That said, the updated approach is no more or less vulnerable than RCTs and IV techniques and is probably the best design that available data could provide in Cambodia's context. Choices of research design in impact evaluations, moreover, seem to be arbitrary, depending on the question(s), data that are available or to be collected, and the knowledge of programme administration (Heckman et al., 1999). Another reason to incorporate non-parametric approach is to avoid the arbitrariness in modelling distributions of error terms, (Islam, 2011).

## 2.3 Outcome and treatment variables

Previous studies examine the effects of microcredit on a range of dependent variables, typical one of which is consumption (income). Attention has also been given to investigating the impact on non-consumption dimensions including education and health, improvement in durable assets and entrepreneurial ability. We estimate the impact on agricultural production and income, particularly income from paddy. We also examine the effects on self-employment income, per capita daily consumption (equivalent scale) and value of non-land assets. Choice of the former outcomes is motivated by the fact that one of the aims of micro-credit is to enhance self-employment activities [19]. Logarithmic form of all outcomes, but total value of non-land durable assets and off-farm self-employment income, is used to normalise and to reduce the effects of outliers, achieving a more homoscedasticity of error term (Khandker and Samad, 2014).

Treatment variable is both binary and continuous; outstanding loan amount is used for the latter. For DID, we categorise treatment variable into three groups: (1) continuous participants—households who reported having outstanding loans in both periods $(1, 1)$; (2) new comers—households who reported not having outstanding loans in 2011 but 2014 $(0, 1)$; and (3) drop-outs who reported having outstanding loans in 2011, but 2014 $(1, 0)$. Households who reported not having outstanding loans in both periods $(0, 0)$ are controls.

Hence, DID estimator compares outcomes of participating households $(0, 1)$ and those of non-participants $(0, 0)$. The effects of continuous participation are also estimated by comparing outcomes of households with outstanding loans in the two periods with those of households who dropped out of the loan programme in period $t = 2014$.

## 2.4 Attrition bias

5.07% of the households left the survey between 2011 and 2014[20]. Though relatively low, it is crucial to examine characteristics of the attritors, for estimates can be biased if characteristics of the drop-outs are significantly different from those of households that stay. To deal with attrition biases, we regress decision to stay in the sample on households'

characteristics by using probit model. The predicted probability of staying is, then, used to calculate the mean inverse probability weights which is later included in the main regression.

The probit model is written as:

$$\Pr(A_{it} = 1) = \beta_1 + \beta_2 X_{it-1} + V_{it-1} + \sigma_i + \psi_{it} \tag{6}$$

where $A_{it}$ takes the value of 1 if the household chose to stay in the survey between 2011 and 2014 and 0 otherwise; $X_{it-1}$ are other household characteristics at $t = 2011$; $V_{it-1}$ is village dummy; $\sigma_i$ is household-level fixed effect; and $\psi_{it}$ is a stochastic error term.

We also test linear hypothesis of probit coefficients after estimation. Among the main household characteristics, we include household-level shocks given that the shocks can induce households to drop out of the sample (for example, Islam, 2011). Table 1 reports the estimates[21] indicating that both households are statistically different on a few characteristics; that is, households with high socio-economic status are less likely to leave the sample. The findings are consistent with those of Khandker and Samad (2014). Household-level shocks do not influence the probability of leaving. Although crop failure positively explains the drop-out, the effect is not statistically significant. The Chi-squared statistics rejects the null hypothesis of simultaneously null coefficients at a high statistical level implying that these independent variables explain attrition which might not be random[22].

A number of approaches have been used to correct for attrition bias: two-step selection model with appropriate instruments[23] (Heckman, 1979), inverse probability weight (Khandker and Samad, 2014; Baulch and Quisumbing, 2011; Fitzgerald et al., 1998) and non-parametric approaches in Das et al. (2011). In this study, we calculate inverse probability mean weight[24], which is used to estimate all results. It should be noted that the validity of inverse probability weight rests on the assumption that attrition is attributable to observables. If it is due to unobservables, the selectivity model is more appropriate[25]

# 3 Applications and results

## 3.1 Data

Table 2 presents total sampled and panel households. The data has been collected since 2001 with 3 years interval, yet we use the latest two rounds given the larger panel sample household (1,123) and consistency in information reported[26]. The survey covers a number of socio-economic and demographic characteristics of individuals, households and villages; those include: household demographics, housing conditions, land ownership and transactions, credit markets, food and non-food consumption, non-land assets, livestock ownership, household income, agricultural production, production expenditure and wages and self-employment[27]. Village information is also used. All survey rounds were employed by other researchers some of whom include Tong (2012, 2013) and Lun (2013), but 2014 survey.

## 3.2 Results and discussions

Table 3 presents basic information about loan characteristics. There observed a decline in interest rate (1st loan) of 0.42 percentage point to 3.5% in 2014 and the decrease is statistically significant. Borrowing from informal sources is the most expensive relative to that from formal ones, averaging 6.6% per month in 2014. There was also a slight increase

in the cost of borrowing informally, but it is not statistically significant. Borrowing from MFIs costs roughly 2.6% a month in the same year; the level basically remained unchanged in this last 3 years.

Table 4 compares outcome and demographic variables by credit participation status in each year. Credit participants tend to have smaller household size, fewer working-age adults, less migrant members, and higher asset value. However, there seems to be no common pattern on other outcomes such as paddy harvested area, paddy production, paddy value, self-employment income and per capita daily consumption. We further examine changes in durable assets, crop mix and livestock in general and by borrowing status in particular (Table 5). Overall, all surveyed households observed statistically significant increases in the number of motorbike, hand tractor and telephone. By borrowing status, the number of hand tractor rose for non-borrowing and borrowing households. Yet, new and continuous credit clients observed a big change in the number of hand tractor relative to non-clients and drop-outs, reflecting partially that credit help borrowing households buy more agricultural equipment. On quantity of crops produced per hectare harvested, both new comers and continuous clients of microcredit observed a higher quantity of dry-season and irrigated rice than non clients and drop-outs and the differences are statistically significant.

Table 6 describes differences of outcomes and demographic variables between borrowing households who had one outstanding loan, regardless of sources, and those who had more than one. Overall, there are no statistically significant differences of demographic characteristics between the two groups. Although households with multiple loans could produce more rice per hectare harvested, thus earn higher sale income, and spend more on rice production, particularly in 2014, the effects are not consistent across observed periods. Nonetheless, it seems clear that credit participants spent more on factor inputs in paddy production (e.g., fertilisers, water and hired labour) relative to non-participants. The trend is also consistent among multiple-source borrowers (formal and informal)[28].

Table 8 shows the results of the fixed effects difference-in-differences model assessing the average treatment effect on the treated (ATT) of entering the programme $(0, 1)$. Thus, the natural counterfactual group in this case is those that do not involve in the program $(0, 0)$. The credit program positively impacts by 26.1% on gross paddy income and 68.9% on quantity of paddy production. The effects are statistically significant at 1%. Credit participants also observe a 26.5% increase in input expenditure on paddy production. We also find a statistically positive effect of credit on total non-land durable assets. This finding is consistent with that of descriptive statistics. That is, credit participants invest more on hand tractors relative to non-participants. Total consumption of participating households increases by 4.8% but not statistically significant even at 20% level.

The above findings are consistent with most previous studies assessing similar hypotheses and using different or similar estimators (for example, Kang and Liv, 2011; Khandker and Samad, 2013, 2014; Islam, 2011). The results further confirm that microcredit access potentially helps ease financial constraints of borrowers so as to increase production and income. On the weak evidence of credit access on per capita consumption, results are aligned with those of a few RCT and non-experimental studies which include Crepon et al. (2011), Duflo et al. (2013), Morduch (1998) and Roodman and Morduch (2014). Nonetheless, the impact could be heterogeneous with respect to household characteristics. For instance, Banerjee et al. (2009) show that consumption of households with pre-existing self-employment activities actually dropped prior to participating in microcredit programme. Crepon et al. (2011) draw similar conclusion. Kaboski and Townsend (2012) find positive effects of village credit programme on borrowers' consumption in the

case of Thailand, but durable assets growth. However, one of the potential caveats of this insignificant or even negative effect both in the results of Kaboski and Townsend (2012) and ours is the length of exposure (short-term) before the borrowing is materialised.

The DID results in Table 9 also indicate that borrowing from both formal and informal sources entails no negative effects on, at least, paddy production and income. Multiple-source borrowing results in a 7.9% increase in paddy income and 13.3% in paddy quantity. Multiple-source borrowing also contributes to higher expenses on paddy production (6.7%), even though the effect is only statistically significant at 20% level (Table 9). Nonetheless, borrowing from informal sources is more expensive than that from formal ones—6.6% to 2.6% in monthly interest rates, respectively. It is interesting to note the followings. First, borrowing multiple sources increased during the survey periods by about 0.1%, but not statistically significant. Second, for multiple-borrowing households whose first loan was from formal sources, there is high probability that the second loan was from informal source, moneylenders in particular. For instance, in 2014, of the 43 multiple-borrowing households whose first loan was from ACLEDA, 28.0% obtained their second loan from moneylenders[29]. Similar pattern is observed with other formal sources. This might raise payment issues, for borrowing households are subject to dual payment and the fact that interest rates from informal source is 2.5 times higher relative to that from formal sources. Even though calculating costs and benefits of multiple borrowing is not our main focus, this is a cause of concern and an indication that MFIs as well as the government need to watch out. As argued, competition among MFIs and between MFIs, moneylenders and other credit operators is a double-edged sword. On the one hand, such competition helps reduces the cost of borrowing. On the other, it might contribute to credit lenders compromised borrowing standard to increase the number of loans. However, our data does not support the latter; that is, although borrowing from more than one MFI increases, it is not statistically significant.

Table 10 illustrates relative benefits of credit participation among continuous and drop-out participants. Specifically, drop-out participants (1, 0) observe a 21.4% decrease in paddy production expenditure relative to that of those continued borrowing (1, 1). The positive impact is statistically significant at 5% level. That contributes to lower paddy production and, thus, income. Per capita consumption of drops-out, for instance, declines 30.9% relative to the level consumed by continuous credit participants. Islam (2011) also postulates that, in Bangladesh, benefits in food and non-food consumption and self-employment income accrue more to long-term borrowers of credit programme.

Based on the reported results in Table 11, we observe that coefficients of the triple interaction, which are assumed to be able to capture the difference in the impact of credit for between female and male, are negative for gross paddy (income and quantities) and the total expenditures on rice production. However, the impacts are hardly statistically significance, indicating weak evidence that female participants benefit more from credit than male participants. Results also indicate, however, that the credit access for female is likely to increase self-employment income. This can be explained, inter alias, the available opportunities for female, and where they tend to activate more in the informal sector with small amounts of credits and high returns. The result is consistent with that of other studies. Using panel data in the case of Bangladesh, Khandker and Samad (2014) indicate that female borrowing results in higher increases in almost all outcomes compared to those of male borrowing. The extent to which women participants could benefit from microcredit access might be beyond the standard income and substitution effects to gains in empowerment as shown in Pitt et al. (2006). But assessing that is beyond the current study[30].

The observed gender-differentiated effects of credit borrowing on self-employment

activities might be attributable to a few factors. First, the number of female borrowers relative to that of male counterparts might be overwhelming in the sample, masking the effects by male borrowers. This is more likely to be the case of microfinance borrowing in Bangladesh, for instance, with the objective to target women (Islam, 2015). This, however, is not the case in our sample. That is, 30% of the 223 credit participating households were headed by female at baseline ($t = 2011$). Second, one of the arguments advanced in credit literature is the empowerment women are likely to obtain from participation. This would allow them to be more effective and productive in any investment decision they subsequently make (for example, Pitt and Khandker, 1998; Khandker, 2005; Islam, 2015). Third, women are more likely to involve in self-employment activities, either farm or off-farm, than men. In our sample, 43% of female-headed households were engaged in agriculture, predominantly farming, compared to 21.4% of male-headed ones. Also, 17.1% of street food sellers were female relative to almost none of male-headed households. Disaggregated by credit participation, the percent share of occupation among female and male remains similar [31]. Male household heads are more inclined to be wage/salary workers (30% of 271 with reported occupations).

On the heterogeneous effects shown in Figure 2, the effect of microcredit has a larger positive impact on participating households who are within 0th–20th quantile of paddy income and quantity, indicating that microcredit benefits poorer households disproportionately than richer ones, an argument put forth by proponents—Islam (2015) reaches similar conclusion. The effect seems to drop significantly after the 20th quantile and is almost zero for households at the top quantile. This might make intuitive sense as richer households either do not participate in microcredit programmes in the first place or drop out when their income gets higher, implying that small loans are no longer fitted with the scale of the businesses in terms of operations and finance. The QDD results also indicate consistent pattern of the positive effects of microcredit on paddy production expenditure and non-land durable assets for participating households at the lower end of the distribution. On off-farm self-employment income, there is no borrowing effect for poor households, specifically those at 50th or below quantile of the self-employment income distribution. This partially indicates challenges in nudging poor families to start non-farm activities with the small loans. The impact might also be dependent on the pre-existing business activities as pointed out by Crepon et al. (2011), postulating that households with pre-existing activities tend to save and borrow more to expand business activities; whereas households without such activities are more likely to increase consumption. Our paper's results also show that the effect of credit dropped for households whose consumption is within 60–80th quantile, which might imply a saving effect to further expand the businesses.

We also estimate the ATT on paddy quantity over propensity to borrow, education of household head, number of current emigrants, and total outstanding loan amount. As shown in Figure 3, the impact of credit borrowing is positive across households with propensity to borrow, indicating that making credit access widely available is a policy the government should pursue. The effect, however, seems to depict a U-shaped pattern benefiting households at the lower and upper ends of the propensity score, not so much for households in the middle. The effect is also varied in accordance with the level of education of household head, having heads who finished, at least, upper secondary or higher benefited more from credit borrowing. The finding makes intuitive sense as educated household heads could be more careful and effective in investing the borrowed amounts. It is also interesting to note that how much a participant could borrow has implication on the effect of credit borrowing. Figure 3 indicates that credit borrowing starts to have positive effects only with loan value of KHR1 million (USD248)[32] or greater.

# 4 Conclusions

The positive impact of microcredit access on a client's well-being has long been reported in numerous empirical studies. However, a growing number of studies using quasi- and experimental research designs have found no or modest benefits of credit participation– further casting doubts on the long-held view that microcredit is a miracle.

To contribute to the debate, we test the following hypotheses: that credit participation remains important; that participation in multiple loan programme is harmful; and that women participants are more likely to benefit more from micro-loans than their male counterparts.

We find evidence of a positive and statistically significant impact of credit participation on paddy income and quantities and expenses on factor inputs (for example, fertilisers, water, and hired labour). Participating households also observe an increased non-land durable assets, most importantly, investment in hand tractor, relative to non-participating households. Borrowing from both formal and informal sources seem not to have negative effects, at least, on paddy income and production. In fact, multiple-source borrowing induces borrowers to spend more on paddy production relative to non-borrowers—an indication that microcredit helps ease financial constraints. However, there are concerns that borrowing is still costly, particularly from informal channels when borrowers pay roughly 79.2% of annual interest rates. Moreover, we find that continuous participants tend to benefit from credit participation relative to the dropped out. The probability of starting self-employment activities is relatively high among female borrowers.

The following policy implications can be drawn from the paper's results. First, credit access remains an important contributor to increased borrowers' wellbeing, especially in easing financial constraints in paddy production. Nonetheless, the impact might depict heterogeneity benefiting certain households than others. Lending policies of microcredit institutions tailored to borrowing households with average annual real paddy income of KHR63 million (USD15,000)[33]; households with heads having finished upper secondary or higher; and households with more emigrants are recommended. Second, the effects on other important outcomes (off-farm self-employment income, non-land durable assets and consumption) are much more modest, implying that extending credit access is not a quick fix as argued by some proponents. Lastly, existing institutional and national microcredit policies that give advantages to female borrowers should be continued, hoping to nudge more women into self-employment activities.

One of the study's caveats is confined generalization of the findings given data coverage—concerning only eleven, mostly rural, villages. It is unfortunate that nationally representative panel data of households is not handy.

# Notes

[1]The number of microfinance institutions increased almost 4 folds to 59 in 2015 from 17 in 2006. Credit has grown significantly, averaging 53% per annum during 2005–2015. The number of MFI clients grew, on average, 20% a year in the same periods, reaching 2 million in 2015 (National Bank of Cambodia, 2016). It is, however, harder to obtain accurate information on the number of loans participants and non-participants are servicing. Kang and Liv (2011) report that 6% of microfinance clients in their sample were servicing more than one loan at the time of survey. The reported figure might understate the actual number given that respondents could conceal true information. The authors triangulate the figure by examining audit reports of individual MFIs and see that the cross-lending rate ranges between 10% and 20% of the clients.

[2]Literature distinguishes microcredit and microfinance; the latter covers a range of services (for example, provision of saving accounts, training courses on financial management and investment) while the former focuses mainly on provisions of small loans to, as many believe, the financially underserved customers. Nonetheless, there are studies that use the term interchangeably; Islam (2011) is an example. In this study, we treat both terms differently, using only the term microcredit or access to it. The possible impacts of noncredit services on the average treatment effects on the treated (ATT) of credit access are briefly discussed in Section 2.1.

[3]Pitt et al. (2006) distinguish gender-based effects of microfinance participation between 'empowerment' and standard 'income and substitution' effects. The authors combine a number of proxy variables to estimate the latter: women role in household decision making, social networks, bargaining power in the household, etc. The authors find that women participation in microfinance provides more empowerment in those outcomes. In Cambodia's context, Kang and Liv (2011) postulate that women in households with microcredit are more empowered in terms of decisions relative to women in non-participating households. They tend to also be active in the community. While assessing the impact of microfinance on such outcome variables is important, we attempt to provide assessment only on income and substitution effects.

[4]Analysing heterogeneous effects of credit and other programmes has become popular as there are discrepancies of the total average treatment effects among empirical studies, RCTs included (for example, Islam, 2015; Verhofstadt and Maertens, 2015; Mutuca et al., 2013; Brand and Xie, 2010. Hence, failure to thoroughly understand such varying effects would lead one to prematurely nullify the positive effects of credit participation altogether.

[5]Although Kang and Liv (2011) employ treatment and control framework in their impact evaluation, the data is cross-sectional limiting the dynamic understanding of the effects.

[6]This is because we are unable to spot a good and strong IV.

[7]Studies investigating economic effects of credit participation on participants' outcomes must deal with endogeneity that simultaneously affects credit demand and outcomes of interest and selection bias. Given that it is highly likely that credit participation is self-selection, a range of econometric techniques has been used to tackle the issue, some common of which include: instrumental variable, fixed-effect and fixed-effect with IV, lagged depend variable (first or more lagged) and fixed-effect weighted by propensity score. Khandker and Samad (2014) employ all of these methods to control for time-varying factors and measurement errors and use Durban-Wu-Hausman test to determine which of them is more appropriate. The authors show that although p-score weighted fixed-effect method appears to be a bit more appropriate compared to other methods, estimates produced by these estimators are not significantly different.

[8]Islam (2011) combines propensity score matching with DID and DDD to ensure that comparable households are created before outcome comparison is made.

[9]Unfortunately, questions on various types of land title were not asked in these survey rounds.

[10]The authors also show that none of the CEOs and senior managers interviewed reported using some kinds of screening criteria to lending. Rather, they try to devise plans and products to feed the needs of clients.

[11]On estimating the impact on production outputs, some authors suggest using specific types of production function given that the technique could address unobserved changes like productivity shocks that could affect the use of intermediate inputs, thus, outputs (for example, Levinsohn and Petrin (2003) and Loecker (2013)). We do not, however, consider the technique in this study due to three reasons. First, our estimations do not solely rest on paddy output, but other outcomes (refer to Appendix A). Second, it is difficult, for the time being, to decide on which type of production (Cobb-Douglas or Translog) function to be used. Even when the decision could be resolved, further assumptions on various parameters of the function would have to be made. Lastly, we believe that it is fairly reasonable in this context to assume that credit clients and non-clients are subject to similar production technology and intermediate inputs. The use of inputs such as capital, labour and fertilizers are shown in Tables 4 and 5. Other factor shocks—such as access to agricultural training or extension services—that might increase Total Factor

Productivity and/or labour efficiency through increased knowledge is unwarranted in our data. Table 5 shows decreased information access to technique that could improve rice yield for the survey periods. The decrease happens for credit clients and non-clients alike.

[12] Karlan and Valdivia (2011) and Dupas and Robinson (2013) are two examples of experimental studies on noncredit services.

[13]McKernan (2002) finds positive effects of noncredit services on self-employment income.

[14]This does not mention the possibly low take-up rate of such services.

[15]A few example studies examining heterogeneity of credit access and other programme participation are Verhofstadt and Maertens (2015); Islam (2015); Abebaw and Haile (2013); Mutuca et al. (2013); Abebaw and Haile (2013); Doan et al. (2011).

[16]For robustness check, we also run propensity score matching estimator to estimate the heterogenoeus effects of the ATT on a number of household characteristics. Figure 3 presents the results.

[17]Bauchet et al. (2011) provide a concise review of findings from some of the above randomized evaluations.

[18]Levitt and List (2009) provide a concise review of the evolutions using experimental design in observational settings and important caveats that need to be attentive to ensure validity and reliability of results. Deaton (2010) also gives comprehensive accounts of the drawbacks of Randomised Controlled Trials. Banerjee and Duflo (2009) present pros and cons of utilising experimental approach in development economics. Banerjee et al. (2015) also briefly highlight specific drawbacks of RCTs.

[19]Appendix A presents definitions of outcome, treatment and other control variables at household and village levels.

[20]Khandker and Samad (2014) point out that the extent of attrition is not as important as the degree of non-randomness. Thus, attention should be paid to testing whether attrition is random.

[21]Fitzsimons and Mesnard (2014) show that attrition in their sample was mainly due to 'unwillingness' to respond and less so to migration. We, however, include migration in our attrition estimator given that it is a common phenomenon, especially in rural villages such as ours. Our results show that attrition was attributable mainly to migration than to unwillingness to answer.

[22]We also separate attrition regressions for microcredit clients and non-clients and find similar results. Chi-squared statistics rejects the null hypothesis of simultaneously null coefficients at $p - value = 0.0000$ for both estimators. Results are available upon requests.

[23]Quality of the interview and characteristics of interviewers can be used as instruments since they possibly affect the decision to leave the sample but not the outcomes of interest. Fitzsimons and Mesnard (2014), for instance, use day of the month of the interview and whether head or spouse answered the questionnaire.

[24]Do file to calculate inverse probability weight is available upon requests.

[25]See Baulch and Quisumbing (2011) for strengths and caveats of inverse probability weight. The effects of attrition on estimates are still inconclusive. Nonetheless, majority of literature we reviewed find that attrition even if it is non-random has insignificant effects on estimates (e.g., Islam, 2011; Fitzgerald et al., 1998; Ziliak and Kniesner, 1998).

[26]If all survey rounds are considered (2001, 2004, 2008, 2011 and 2014), there are 760 panel households in 9 villages. The attrition rate is 22.1% of 1,005 households. Village information could not be used, for it was not recorded before 2011.

[27]See Tong (2013) for a detailed description of the data.

[28]We also examine descriptive statistics on the same outcomes and demographic variables specified in Table 4 using different definitions of control and treatment groups outlined in Section 2.1. Estimated results are similar.

[29]The Chi-squared test statistically confirms row differences.

[30]A few RCT studies that find no discernible impact of credit access between female and male include Duflo et al. (2013) and Karlan and Valdivia (2011).

[31]About 35% of 521 households had no information on the primary occupation of household head. The percent share might have been higher if there were no missing values.

[32]USD1=KHR4034 as of June 2016 (National Bank of Cambodia, 2016).

[33]The amounts are only for households with reported figures and activities.

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Table 1: Probability of staying in the sample

| Explanatory variables | *Coeff.* | *Std.Error* |
|---|---|---|
| *Demographic Variables* | | |
| Household size | 0.0084 | 0.0423 |
| Age of household head | 0.0180** | 0.0078 |
| Average years of schooling of households | -0.0010* | 0.0005 |
| Sex of household head ($1 = male, 0 = otherwise$) | 0.0271 | 0.1954 |
| Marital status of household head ($1 = married, 0 = otherwise$) | 0.2127 | 0.2699 |
| Occupation of household head ($1 = agriculture, 0 = otherwise$) | 0.0108 | 0.1415 |
| % of members aged 0-7 | 0.1363 | 0.4956 |
| % of members aged 7-14 | 0.1761 | 0.3363 |
| % of members aged 15-24 | 0.6844* | 0.3765 |
| % of members aged 25-35 | 0.9171* | 0.4930 |
| Housing condition ($1 = thatchhouse, 0 = otherwise$) | -0.1927+ | 0.1773 |
| *Assets* | | |
| Non-land assets (*log*, ten thousand *riels*) | 0.0066 | 0.0190 |
| Number of agricultural land (*log*) | 0.0148 | 0.0193 |
| Total value of livestock (*log*) | 0.0321*** | 0.0071 |
| *Household Shocks* | | |
| Health shock (1 if a household has at least a member died or seriously ill, 0 otherwise) | 0.1128 | 0.1136 |
| Crop failure (1 if a household experienced crop failure or damage due to flood, 0 otherwise) | -0.1286 | 0.2369 |
| *Programme Participation* | | |
| Household participated in agriculture extension advice or assistance since 2008 ($1 = yes, 0 = otherwise$) | 0.2774+ | 0.2532 |
| *Village* | | |
| Village dummies (0 = Dang Kdar) | | |
| Tuol Krasang | -0.8607*** | 0.0948 |
| Andong Trach | -0.8909*** | 0.1161 |
| Trapen Prey | -0.3280*** | 0.0892 |
| Khsach Chiros | 0.3723*** | 0.0744 |
| Prek Kmeng | -0.1361 | 0.1187 |
| Kanhchor | 0.3569*** | 0.0930 |
| Ba boang | 0.8425*** | 0.0906 |
| Prey Nobmuy | -0.3275*** | 0.1221 |
| *Constant* | 0.3270*** | 0.1221 |
| $Prob > chi2$ | 0.0000 | |
| *Pseudo R2* | 0.1208 | |
| *Obs.* | 978 | |
| $p - value$ of equality of probit coefficients of independent variables | 0.0000 | |

*Notes:* Dependent variable is 1 if the household stayed in the sample 0 otherwise. Standard errors are robust and clustered at village-level. We drop Kompong Thnaot and Bos due to the insufficient number of households leaving the sample. Thus, sampling weights of the villages will be used as inverse probability weights for households in the two villages. $^{+}\ p < 0.30$, $^{++}\ p < 0.20$, $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.
Source: Authors' calculations.

Table 2: Sample panel households by village

| Village | 2011 | 2014 | # | % |
|---|---|---|---|---|
| Tuol Krasaing | 120 | 106 | 14 | 11.67 |
| Andong Trach | 85 | 76 | 9 | 10.59 |
| Trapeng Prey | 76 | 72 | 4 | 5.26 |
| Khsach Chiros | 120 | 115 | 5 | 4.17 |
| Dang Kdar | 125 | 122 | 3 | 2.40 |
| Kompong Thnoat | 120 | 120 | 0 | 0.00 |
| Prek Khmeng | 120 | 117 | 3 | 2.50 |
| Kanhchor | 120 | 115 | 5 | 4.17 |
| Bos | 85 | 84 | 1 | 1.18 |
| Ba baong | 127 | 115 | 12 | 9.45 |
| Prey Nobmuy | 85 | 81 | 4 | 4.71 |
| Total | 1,183 | 1,123 | 60 | 5.07 |

Source: Authors' preparation.

Table 3: Interest rates and outstanding loan amounts

| | $1^{st}$ loan | | | $2^{nd}$ loan | | |
|---|---|---|---|---|---|---|
| | 2011 | 2014 | *diff.* | 2011 | 2014 | *diff.* |
| Monthly interest rate (%) | 3.067 | 3.492 | -0.425* | 5.495 | 2.968 | -2.527** |
| –Relatives/Friends | 0.943 | 1.228 | 0.285 | 3.072 | 1.595 | -1.477 |
| –Moneylenders | 6.560 | 6.633 | 0.073 | 8.257 | 4.308 | -3.949++ |
| –MFIs | 2.704 | 2.604 | -0.100 | 2.711 | 2.565 | -.145++ |
| –Others | 2.921 | 2.710 | -0.211 | – | – | – |
| Outstanding amount borrowed for the last 6 months (0,000 riels) | | | | | | |
| Average amount | 160.279 | 291.853 | 131.573*** | 110.037 | 155.288 | 45.252** |
| –Relatives/Friends | 128.403 | 132.5974 | 4.194 | 61.415 | 89.411 | 27.996** |
| –Moneylenders | 157.391 | 185.413 | 28.022 | 125.504 | 222.981 | 97.477*** |
| –MFIs | 191.707 | 336.722 | 145.015*** | 113.9684 | 149.726 | 35.758 |
| –Others | 195.282 | 206.771 | 11.489 | 77.731 | 165.707 | 87.975*** |

Notes: "Others" includes NGOs, Self-Helped Group, etc. $t - statistics$ is calculated using robust standard errors clustered at village level. Loan amounts are within -3 and 3 standardized values. – indicates insufficient observations to obtain the mean. $^{+}$ $p < 0.30$, $^{++}$ $p < 0.20$, $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$.
Source: Authors' calculations.

Table 4: Descriptive statistics, panel households by programme participation

| | 2011 | | | 2014 | | |
|---|---|---|---|---|---|---|
| Demographic Variables | Clients | Non-clients | *Diff.* | Clients | Non-clients | *Diff.* |
| Household size | 6.20 | 5.67 | 0.53* | 5.17 | 4.78 | 0.39 |
| - # of children (14 and below) | 1.79 | 1.40 | 0.39** | 1.44 | 1.24 | 0.20 |
| - # of elderly (65 and above) | 0.34 | 0.55 | -0.21** | 0.38 | 0.66 | -0.28*** |
| - # of working-age adults (15-64) | 4.05 | 3.70 | 0.35** | 3.34 | 2.88 | 0.47** |
| - # of women | 2.96 | 2.80 | 0.15 | 2.57 | 2.46 | 0.11 |
| - # of working-age women (15-64) | 2.01 | 1.76 | 0.25** | 1.63 | 1.47 | 0.16** |
| Age of household head | 53.19 | 56.13 | -0.29** | 53.02 | 60.23 | -7.21*** |
| Years of schooling of household head | 3.18 | 3.12 | 0.06 | 3.45 | 3.23 | 0.21 |
| Average age of household members | 28.55 | 32.69 | -4.14*** | 32.39 | 37.29 | -4.90*** |
| Female headed household $(1,0)$ | 0.198 | 0.209 | -0.011 | 0.188 | 0.284 | -0.097** |
| # of migrant members | 1.80 | 2.06 | -0.26 | 2.52 | 3.23 | 0.70** |
| Asset value (log, 0,000 riels) | 4.73 | 4.72 | 0.007 | 5.40 | 5.09 | 0.31* |
| Outcome Variables | | | | | | |
| Rice harvested area (ha) | 0.88 | 1.14 | -0.26** | 1.16 | 0.98 | 0.18 |
| Rice production (per harvested area, ha) | 2.09 | 2.08 | 0.01 | 2.30 | 2.16 | 0.15 |
| Rice value (per harvested area adjusted, base year=2004, 0,000 riels) | 109.48 | 112.77 | -3.29 | 118.36 | 105.68 | 12.68** |
| Expenses on rice production (0,000 riels per complete session) | | | | | | |
| organic fertiliser, chemical fertilisers and pesticides | 9.63 | 9.38 | 0.25 | 11.40 | 12.13 | -0.72 |
| water fees or pumping cost and soil preparation | 19.72 | 17.26 | 2.45 | 24.75 | 17.25 | 7.51*** |
| hired labour for transplanting and hired labour for harvesting | 24.66 | 24.03 | 0.63 | 26.70 | 22.68 | 4.02* |
| repairs, transports and rentals | 5.71 | 5.18 | 0.53 | 6.25 | 4.19 | 2.06*** |
| total expenses | 77.08 | 74.07 | 3.01 | 85.62 | 71.29 | 14.32* |
| Other outcomes (adjusted, base year=2004) | | | | | | |
| Self-employed income (0,000 riels) | 296.97 | 468.25 | -171.28 | 490.13 | 416.56 | 73.57 |
| Per capita daily food consumption (equivalent scale, riels) | 1615.47 | 1696.56 | -81.09 7 | 1609.34 | 1698.94 | -89.60 |
| Per capita daily non-food consumption (equivalent scale, riels) | 893.23 | 913.60 | -20.37 | 886.64 | 866.90 | 19.74 |
| Per capita daily total consumption (equivalent scale, riels) | 2687.34 | 2639.23 | 48.12 | 2532.05 | 2561.85 | 29.80 |

*Notes:* Sampling and inverse probability weights are used to calculate estimates for pooled and panel households, respectively. Results of all outcome variables are estimated using values between 5th and 95th percentiles to avoid biases due to a few very small and very large values and only for households with available figures. We also estimate characteristics of panel household using sampling weights and the results are not significantly different from those used attrition weights. Results of panel household using sampling weights are available upon request. Observations for each variable are not the same given missing information and due to space limit they are not reported. Adjusted Wald tests are used to test the null hypotheses of equal means between reference periods. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Authors' calculations.

Table 5: Changes in durable assets, crops and livestock

| | (1) all | (2) $(0,0)$ | (3) $(0,1)$ | (4) $(1,0)$ | (5) $(1,1)$ |
|---|---|---|---|---|---|
| **# of durable assets** | | | | | |
| - motorbike | 0.192*** | 0.162*** | 0.060 | 0.338* | 0.212*** |
| - bicycle | -0.198*** | -0.176* | -0.043 | -0.571*** | -0.126++ |
| - television | 0.020 | -0.042 | 0.027 | 0.052+ | 0.049 |
| - telephone | 0.405*** | 0.210++ | 0.599*** | 0.473** | 0.425*** |
| - car | 0.005 | 0.012+ | 0.018++ | 0.008 | -0.008 |
| - water pump | 0.055++ | 0.119* | 0.078++ | -0.086++ | 0.059++ |
| - threshing mechnine | 0.0093 | 0.007+ | -0.004 | 0.063 | -0.007 |
| - rice mill | -0.045** | -0.049++ | -0.054+ | -0.044++ | -0.038** |
| - ox cart | -0.171** | -0.179** | -0.205++ | -0.219** | -0.129* |
| - horse cart | -0.008 | -0.017+ | -0.013 | 0.002 | -0.003 |
| - plough and harrow | -0.385** | -0.315** | -0.441+ | -0.343++ | -0.428** |
| - tractor | 0.002 | 0.008 | – | -0.006 | 0.002+ |
| - hand tractor | 0.132** | 0.113** | 0.155* | 0.074+ | 0.159** |
| **Crops (quantity per hectare harvested, tone)** | | | | | |
| - rice (irrigated or dry season) | 1.028*** | 0.820*** | 1.120*** | 0.746++ | 1.244** |
| - rice (un-irrigated or rainy season) | 0.057 | 0.040 | 0.024 | 0.237+ | -0.001 |
| - cassava | 0.423++ | 2.361*** | -8.128** | 1.577 | -0.139 |
| - other crops | – | – | – | – | – |
| **# of livestock** | | | | | |
| - cow | -0.009 | 0.349 | 0.099 | -0.446+ | -0.159++ |
| - pig | -0.314 | -1.336++ | -0.298 | -0.025 | 0.304* |
| - chicken | 0.615 | 1.272 | 0.315 | -2.195++ | 1.565++ |
| - other animals | -14.320+ | -12.800 | -7.980+ | 0.628 | -25.61++ |
| **Status on land ownership** | | | | | |
| - residential: own $(1,0)$ | 0.027*** | 0.015** | 0.024* | 0.029* | 0.036** |
| - residential elsewhere: own $(1,0)$ | -0.012+ | -0.003 | -0.051+ | -0.012 | 0.004 |
| - agricultural land: own or lease $(1,0)$ | 0.006 | -0.012 | 0.026** | 0.011+ | 0.008 |
| **Household-level shocks** | | | | | |
| 1 negative shock, 0 otherwise | -0.092* | -0.134** | 0.171++ | -0.327*** | -0.084+ |
| **Households' information access to improve farming practices** | | | | | |
| - increase rice yield | -0.082* | -0.166*** | -0.018 | -0.103 | -0.041 |
| - increase yield of other crops | -0.083++ | -0.131* | 0.032 | -0.167* | -0.066++ |
| Observations | 2246 | 596 | 446 | 872 | 332 |

*Notes:* $(0,0)$ indicates non-clients; $(0,1)$ clients; $(1,1)$ continuous clients; $(1,0)$ drop-outs. $t-statistics$ is calculated using robust standard errors clustered at village level. Given space limitations, standard errors are not presented. – indicates insufficient observations. $^{+}p < 0.30$, $^{++}p < 0.20$, $^{*}p < 0.10$, $^{**}$ $p < 0.05$, $^{***}p < 0.01$.
Source: Authors' calculations.

Table 6: Descriptive statistics, panel households by number of outstanding loans

| | 2011 | | | 2014 | | |
|---|---|---|---|---|---|---|
| Demographic Variables | Multiple | Single | *Diff.* | Multiple | Single | *Diff.* |
| Household size | 6.41 | 6.10 | 0.30 | 5.22 | 5.19 | 0.03 |
| - # of children (14 and below) | 1.92 | 1.71 | 0.21 | 1.62 | 1.39 | 0.23 |
| - # of elderly (65 and above) | 0.37 | 0.33 | 0.04 | 0.43 | 0.36 | 0.06 |
| - # of working-age adults (15-64) | 4.13 | 4.05 | 0.08 | 3.18 | 3.44 | -0.25 |
| - # of women | 3.11 | 2.88 | 0.23 | 2.56 | 2.59 | 0.03 |
| - # of working-age women (15-64) | 2.01 | 2.00 | 0.01 | 1.56 | 1.66 | -0.09 |
| Age of household head | 52.30 | 53.68 | -1.38 | 54.03 | 52.48 | 1.54 |
| Years of schooling of household head | 3.39 | 3.15 | 0.24 | 3.67 | 3.36 | 0.31 |
| Average age of household members | 28.50 | 28.76 | -0.26 | 31.95 | 32.40 | -0.44 |
| Female headed household (1,0) | 0.19 | 0.19 | 0.00 | 0.20 | 0.17 | 0.02 |
| # of migrant members | 1.77 | 1.86 | -0.09 | 2.73 | 2.47 | 0.25 |
| Asset value (log, ten thousand riels) | 4.71 | 4.77 | -0.05 | 5.21 | 5.51 | -0.29 |
| Outcome Variables | | | | | | |
| Rice harvested area (ha) | 1.33 | 1.27 | 0.06 | 1.40 | 1.38 | 0.02 |
| Rice production (per harvested area, ha) | 2.13 | 2.07 | 0.05 | 2.83 | 2.12 | 0.70*** |
| Rice value (per harvested area adjusted, base year=2004, 0,000 riels) | 107.54 | 110.04 | -2.50 | 135.53 | 112.21 | 23.32*** |
| Expenses on rice production (0,000 riels per complete session) | | | | | | |
| organic fertiliser, chemical fertilisers and pesticides | 12.95 | 12.75 | 0.20 | 19.69 | 14.55 | 5.14* |
| water fees or pumping cost and soil preparation | 33.44 | 19.42 | 14.01** | 35.02 | 25.28 | 9.74** |
| hired labour for transplanting and hired labour for harvesting | 32.09 | 25.41 | 6.67 | 34.18 | 30.97 | 3.20 |
| repairs, transports and rentals | 9.95 | 6.04 | 3.90* | 9.33 | 8.09 | 1.24 |
| total expenses | 103.76 | 78.90 | 24.86 | 124.67 | 91.86 | 32.81** |
| Other outcomes (adjusted, base year=2004) | | | | | | |
| Self-employed income (0,000 riels) | 184.24 | 333.71 | -149.46* | 432.82 | 508.91 | 76.08 |
| Per capita daily food consumption (equivalent scale, riels) | 1657.99 | 1600.13 | 57.86 | 1649.90 | 1592.23 | 57.67 |
| Per capita daily non-food consumption (equivalent scale, riels) | 844.37 | 910.66 | -66.29 | 891.1 | 884.78 | 6.34 |
| Per capita daily total consumption (equivalent scale, riels) | 2781.83 | 2652.08 | 129.74 | 2627.26 | 2491.54 | 135.72 |

*Notes:* Sampling and inverse probability weights are used to calculate estimates for pooled and panel households, respectively. Results of all outcome variables are estimated using values between 5th and 95th percentiles to avoid biases due to a few large values and only for households with available figures. We also estimate characteristics of panel household using sampling weights and the results are not significantly different from those used attrition weights. Results of panel household using sampling weights are available upon request. Observations for each variable are not the same given missing information and due to space limit they are not reported. Adjusted Wald tests are used to test the null hypotheses of equal means between reference periods and credit participation. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Authors' calculations.

Table 7: Descriptive statistics, treatment vs. control at baseline ($t = 2011$)

| | treatment ($t_{2014} = 1; t_{2011} = 0$) | controls ($t_{2014} = 0; t_{2011} = 0$) | $diff.$ |
|---|---|---|---|
| Household size | 5.836 | 5.664 | .172 |
| - # of children (14 and below) | 1.611 | 1.324 | .287* |
| - # of elderly (65 and above) | 0.434 | 0.629 | −.195 |
| - # of working-age adults (15-64) | 3.779 | 3.695 | .084 |
| - # of women | 2.814 | 2.870 | −.056 |
| - # of working-age women (15-64) | 1.758 | 1.767 | −.009 |
| Age of household head | 52.880 | 58.170 | −5.294*** |
| Years of schooling of household head | 3.188 | 3.082 | .105 |
| Average age of household members | 30.488 | 33.783 | −3.295 |
| Female headed household $(1, 0)$ | .187 | .221 | −0.033 |
| # of migrant members | 1.759 | 2.277 | -0.518 |
| Asset value (log, 0,000 riels) | 4.781 | 4.735 | .0468 |
| Total land area (ha) | 2.398 | 1.916 | 0.482 |
| Wealth index (PCA, with land) | .148 | 0.291 | -0.143 |
| Total harvested areas of rice (ha) | 1.485 | .926 | .559*** |
| Observations | 223 | 298 | – |

*Notes:* Sampling and inverse probability weights are used to calculate estimates. Adjusted Wald tests are used to test the null hypotheses of equal means between reference periods and credit participation. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Authors' calculations.

Table 8: Impacts of credit borrowing (DID)

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| General growth effects (t) | -0.150$^{++}$ | -0.0903 | -0.200$^{+}$ | 140.5$^{***}$ | 16.70 | -0.123$^{+}$ |
| General growth effects (t) × Treatment | 0.245$^{***}$ | 0.524$^{***}$ | 0.235$^{***}$ | 49.02$^{***}$ | -252.5$^{*}$ | 0.0472 |
| Constant | 4.418$^{***}$ | 7.509$^{***}$ | 0.148 | -26.07 | -6211.2$^{+}$ | 7.566$^{***}$ |
| Observations | 1020 | 1020 | 1020 | 982 | 1020 | 1020 |
| $R^2$ | 0.218 | 0.201 | 0.197 | 0.334 | 0.066 | 0.345 |

*Notes:* The dependent variable for (1) is gross paddy income (log); quantity of paddy produced (kg/ha) (log) for (2); total expenditure on rice production (log) for (3); total durable asset values (non-land) (level) for (4); self-employment income (level) for (5); total consumption (log) for (6). Estimates are in marginal effects. All monetary figures are adjusted for 2004 village prices. The regressions have other control variables at household and village levels. However, given limited spaces, we do not report coefficients and standard errors of those explanatory variables, but available upon request. Approach by Halvorsen and Palmquist (1980) is used for the interpretation of semilogarithmic equations. That is, $\%\Delta\beta=(e^{\beta}-1)\times 100$. Normality and specification tests were also performed, post-estimation. For the latter, estimated coefficients might be biased due to miss specification of functional form if the model is not fully log-linear. Due to space limited, test results are not presented. But, do file is available upon request. $^{+}$ $p < 0.30$, $^{++}$ $p < 0.20$, $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$.
Source: Authors' calculations.

Table 9: Impacts of credit borrowing from formal and informal sources (DID)

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| General growth effects (t) | -0.288$^{***}$ | -0.302$^{**}$ | -0.441$^{***}$ | 132.6$^{***}$ | 72.40 | -0.161$^{**}$ |
| General growth effects ($t$) × Interaction (formal×informal) | 0.0793$^{**}$ | 0.133$^{*}$ | 0.0668$^{++}$ | -3.974 | 15.88$^{++}$ | 0.0140$^{*}$ |
| Constant | 0.733 | 1.799 | -5.126$^{***}$ | 178.4 | -2007.8 | 6.695$^{***}$ |
| Observations | 2205 | 2205 | 2205 | 2144 | 2205 | 2205 |
| $R^2$ | 0.120 | 0.094 | 0.161 | 0.286 | 0.041 | 0.289 |

*Notes:* Same as in Table 8.
Source: Authors' calculations.

Table 10: Impacts of exiting credit borrowing programme (DID)

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| General growth effects (t) | -0.0254 | 0.0896 | -0.322$^{++}$ | 77.41$^{***}$ | 311.7$^{***}$ | -0.0643 |
| General growth effects ($t$) × Treatment exiting | -0.620$^{++}$ | -1.121$^{**}$ | -0.194$^{**}$ | 17.76$^{*}$ | -36.38 | -0.269$^{***}$ |
| Constant | 5.400$^{*}$ | 9.289$^{*}$ | -0.764 | 1444.700$^{**}$ | 1646.700 | 7.613$^{***}$ |
| Observations | 1185 | 1185 | 1185 | 1162 | 1185 | 1185 |
| $R^2$ | 0.140 | 0.119 | 0.202 | 0.293 | 0.139 | 0.349 |

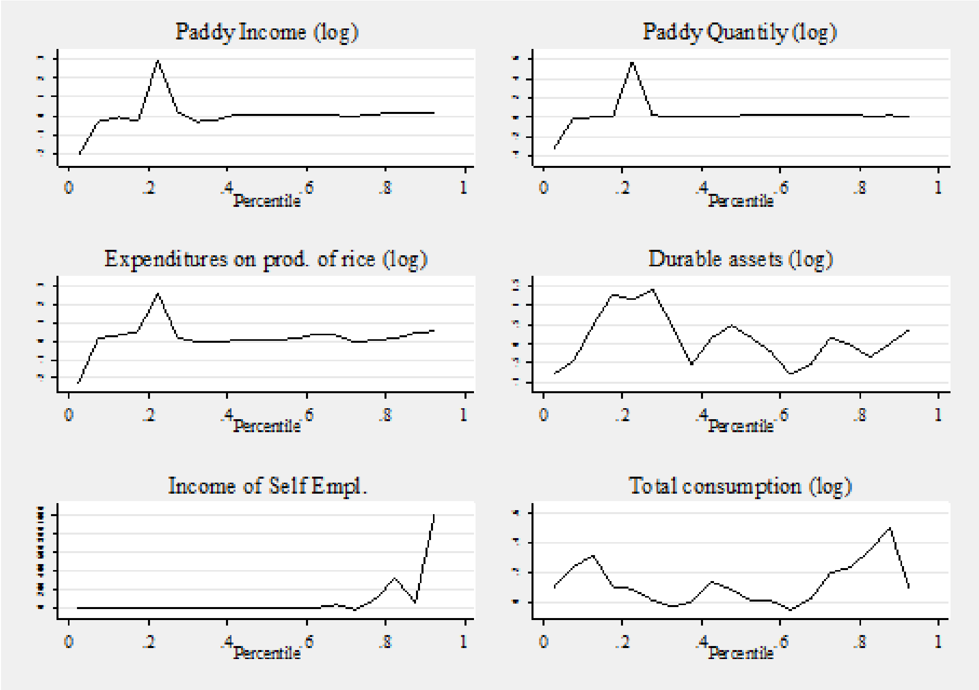
*Notes:* Same as in Table 8.
Source: Authors' calculations.

Table 11: Impacts of credit borrowing and being female (DDD)

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| - Treat | -0.162 | -0.357$^{++}$ | -0.161$^{+}$ | -39.82$^{++}$ | -187.8$^{***}$ | -0.0781$^{++}$ |
| - Female-headed | -0.849$^{**}$ | -1.550$^{***}$ | -0.665$^{**}$ | 44.06 | 283.3$^{*}$ | -0.0521 |
| General growth effects (t) | 0.553$^{**}$ | 1.077$^{**}$ | 0.666$^{***}$ | 192.9$^{***}$ | 30.02 | 0.0205 |
| - Treat×Female-headed | -0.450$^{+}$ | -0.564 | 0.545$^{++}$ | 97.18 | -92.55 | 0.207$^{**}$ |
| - Female-headed×Time | -0.728$^{++}$ | -1.071$^{++}$ | -0.572$^{++}$ | -3.991 | -333.7 | -0.0279 |
| - Treat×Time | -0.747$^{***}$ | -1.136$^{***}$ | -0.693$^{***}$ | 53.60 | -190.6 | -0.0168 |
| Interaction (Treat×Female-headed×Time) | -0.534$^{+}$ | -0.984$^{++}$ | -0.0656 | 12.80 | 307.7$^{++}$ | 0.0182 |
| Constant | 0.818 | 3.135 | -1.316 | 579.7$^{++}$ | -598.9 | 5.097$^{***}$ |
| Observations | 1619 | 1619 | 1619 | 1586 | 1619 | 1619 |
| $R^2$ | 0.258 | 0.239 | 0.353 | 0.211 | 0.100 | 0.328 |

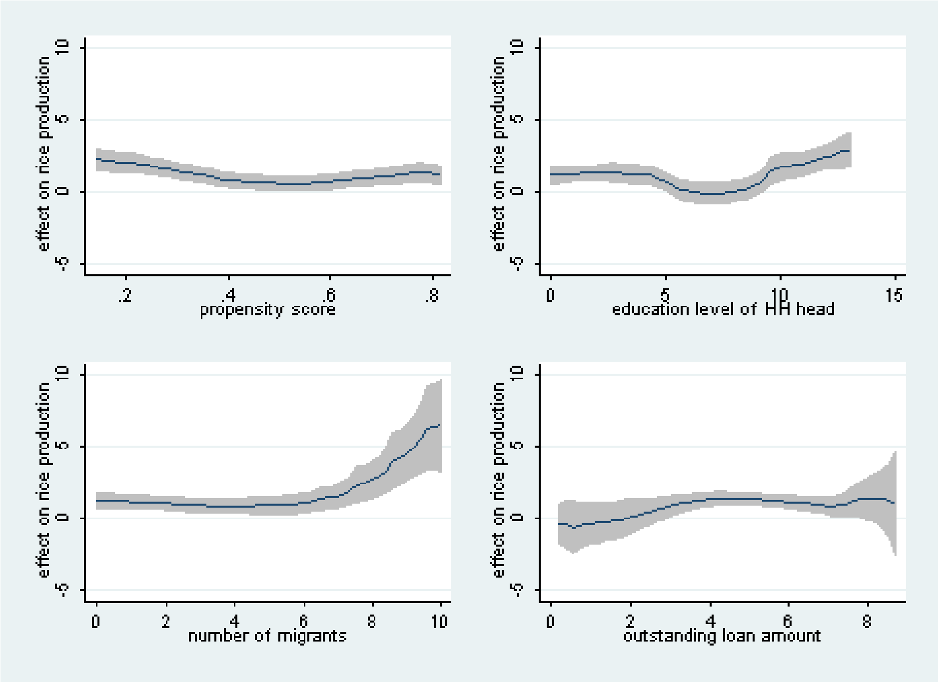
*Notes:* Same as in Table 8.
Source: Authors' calculations.

Figure 2: Outcome results by quintile for treatment group




Source: Authors' preparation.

Figure 3: Heterogeneous effects of credit access on paddy quantity




*Notes:* Treatment ($t_{2011} = 0, t_{2014} = 1$) and controls ($t_{2011} = 0, t_{2014} = 0$). The average treatment effects on the treated ($ATT$) are estimated using nearest-neighbour matching estimator with 4 matches. Similar results are obtained with 1-to-1 match. Given space limitation, we do not report the heterogeneous effects of credit participation on other outcomes. The estimates, as well as do files, are available upon request.

Source: Authors' calculations.

# A Definitions of outcome, treatment and other control variables

| Variable | Definition |
|---|---|
| **Dependent** | |
| Gross income from paddy (*log*) | Total value of produced crops adjusted for 2004 village prices. |
| Quantity of paddy produced per harvested area (*log*) | Total quantity of crops produced divided by harvested area. |
| Total expenditure on paddy production (*log*) | Households' expenses on organic and chemical fertilizer, pesticide, hired labour and other inputs adjusted for 2004 village prices. |
| Values of non-land assets (*level*) | Total values of durable non-land assets adjusted adjusted for 2004 village prices. |
| Self-employment income (*level*) | Off-farm income adjusted for 2004 village prices. The income is mainly originated from small businesses/petty trade from all members of the household. The amount is an annual total. |
| Daily per capita consumption (*log*) | Food and non-food consumption calculated as daily per capita equivalent scale, adjusted for 2004 village prices. |
| **Treatment** | |
| Loan access | 1 if the households have outstanding loans for the last six months and 0 otherwise. |
| Loan amount | Total amounts of outstanding loan for the last six months. |
| **Controls** | |
| *Household characteristics* | |
| Average years of schooling of HH | Average of the highest level of education members have completed. |
| Years of schooling of HHH | Highest level of education household head has completed. |
| Sex of HHH | 1 male and 0 female. |
| Marital status of HHH | 1 married and 0 otherwise. |
| Age of HHH | Age of household head. |
| Household size | Total number of household members (without migrants, but include return members). |
| Female-headed HH | 1 if household head is female and 0 otherwise. |
| Average age of household members | Average age of all household members. |
| # of migrants | Total number of migrants (both internal and international). |

| | |
|---|---|
| Participation in development programme | 1 if households at least participate in certain development programmes sponsored by government and/or development partners and 0 otherwise. |
| Negative shocks | 1 if households at least face with certain negative shocks (for example, lose of household members, crop failure and theft) and 0 otherwise. |
| Access to information | 1 if households can access information for improving farming and other income generation activities and 0 otherwise. |
| *Village characteristics* | |
| Total population | # of people in the village as of survey date. |
| Area prepared for paddy | Total area prepared for paddy cultivation in the village (*ha*). |
| Access to electricity (public and private) | % of households in the village that have electricity connection. |
| Disaster | 1 if the village experiences any disaster in the past five years and 0 otherwise. |
| Wages for male | Average daily wages in the village for male. |
| Wages for female | Average daily wages in the village for female. |

Source: Authors' preparation.