Microfinance and Vulnerability to Seasonal Famine in a Rural Economy: Evidence from Monga in Bangladesh

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ABSTRACT

This paper takes advantage of a unique data set on 143,000 poor households from northern Bangladesh to analyze the effects of microfinance membership on a household's ability to cope with seasonal famine known as Monga. We develop an instrumental variables strategy that exploits a jump and a kink at the 10 decimal (0.1 acre) land ownership threshold driven by MFI screening process to ensure repayment by excluding the ultra-poor. Evidence from the local 2SLS estimator (Dong, 2017) shows that microfinance membership improves food security during the hungry season, especially for the poorest households who struggle to survive at the margin of 1 and 2 meals a day. Microfinance membership also reduces the probability of short-term migration for work during Monga, but is ineffective in reducing the incidence of advance sale of labor at low wages. These conclusions are also supported by estimates from minimum-biased IPW estimator of Millimet and Tchernis (2013) that reduces bias without imposing exclusion restrictions.

Keywords: Microfinance, Ultra-Poor, Aggregate Anticipated Shock, Seasonal Famine, Monga, Coping Mechanisms, Food Security, Distress Sale of Labor, Short-term Migration, Local 2SLS,

JEL Codes: O1, I3

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(1) Introduction

Microfinance programs have become an integral part of the anti-poverty strategies in many developing countries over the last four decades. While proponents argue that microfinance has improved the lives of millions of poor people (especially women) by providing access to credit without collateral, the critics have raised serious doubts about its efficacy as a poverty alleviation tool and as a broader development strategy (for recent surveys of the literature, see Armendáriz and Morduch (2010), Banerjee et al. (2015), Mahmud and Osmani (2017)).[2]

Although the initial focus of the literature on microcredit had been on the implications of missing credit market and the potential effects of relaxing binding credit constraint on household outcomes, it is now well appreciated that microcredit may at least partially be filling in for other missing markets.[3] Microcredit in many cases works as an imperfect substitute for missing insurance and consumer credit markets.[4] That the credit market can play an insurance role, especially when the credit contract is renegotiable, has been well-understood in the literature on consumption smoothing in developing countries (Udry (1990, 1994), Besley (1995), Morduch (1995, 2011), Carter and Lybbert (2012)).[5] Taking advantage of a unique data set on 143,000 ultra-poor households and using the seasonal famine in Northern Bangladesh known as Monga as a case study, this paper provides evidence on the effectiveness of microfinance programs in coping with anticipated seasonal shocks in rural economy. Seasonality and seasonal hunger are not unique to Bangladesh, it is a feature of many poor agrarian economies (see Chambers et al. (1981), Devereux et al. (2012)).

Almost every year during the months of September-November (before the Aman rice harvest), a large part of rural Northern Bangladesh, especially in the Greater Rangpur region, becomes vulnerable to seasonal famine or near famine situation. During this lean season, a large number of poor and extreme poor (ultra-poor) households cannot find any employment. The poor

[2] Recent empirical analysis of the effects of microfinance includes six papers based on RCT in the special issue of American Economic Journal Applied Economics (2015). For papers using nonexperimental data, see Berhane and Gardebroek (2011), and Islam (2011), Morduch and Roodman (2014), Menon (2006), among others.
[3] For example, Emran, Morshed and Stiglitz (2011) show that microcredit addresses simultaneously two missing markets: credit and labor.
[4] There is evidence that many households use microcredit for consumption smoothing (an insurance market role), and for financing indivisible consumer durables such as bicycle and television (missing market for consumer credit), or other lumpy expenditures (such as financing migration and education).
[5] Microfinance NGOs are also increasingly offering explicit savings and insurance products to poor households (see Karlan and Morduch (2009) and Banerjee and Duflo (2011) for discussions).

and ultra-poor households thus turn to desperate measures to cope with the seasonal famine such as reducing daily food intake and distress sale of assets (Rahman (1995)). Since many of them own few assets other than their labor endowment, selling labor in advance during Monga is among the few options they might have to avoid starvation. When households own land, they can also sell their crops in advance.[6] Such 'distress sale' of labor or crops implies that the households receive very unfavorable prices, and thus may be trapped in a vicious cycle of poverty. The average wage received for advance labor sale is about 50 percent of the wage in the spot market (Rahman (1995)). Access to credit in such a situation can potentially make a big difference. Microcredit is likely to yield higher income when used in productive activities, and fungibility of credit means that loans taken ostensibly for investment can be used for consumption smoothing. Most of the microcredit projects are non-farm activities, and thus are not subject to seasonality or weather shocks, unlike agriculture. Moreover, microcredit may be effective in mitigating the effects of low demand for labor in agriculture during the lean season, because it creates employment opportunities within the household.

One can, however, easily find reasons to suspect that microcredit may not be an effective policy tool for addressing seasonal famine. First, microcredit loans are not short-term consumption loans, and thus a household may not be able to get a new loan when they need it most, i.e. during the Monga period. In fact, the rigid loan repayment schedule might make it difficult to buy enough food, and repay the loans at the same time. Second, although microfinance groups may be helpful in the face of idiosyncratic shocks at the household level (such as health shocks as found by Gertler et al. (2009)), Monga is a more aggregate shock (at the village or sub-district level) that affects many (or even most of the) households in a village at the same time. This is likely to make the 'solidarity group' aspect of microcredit programs such as Grameen Bank and BRAC less effective in coping with Monga and other aggregate shocks, both anticipated and unanticipated.[7] Thus, it is by no means obvious that access to microcredit necessarily makes it easier for a household to cope with seasonal adversity. Whether microfinance helps a household cope with Monga is thus an empirical issue.

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[6] In our data set the incidence of advance crop sale is very low, less than 1 percent. So, we do not use it as an indicator of a household's ability to cope with seasonal famine.

[7] The income level of most of the households in the affected regions suffers during Monga and the resulting fall in local demand can make nonfarm activities financed by microcredit unprofitable.

This paper analyzes the impact of microfinance membership on a set of economic indicators that are informative about a household's ability to cope with famine. They are: indicators of food security (number of meals a day during Monga), advance sale of labor as an indicator of distress sale, and short-term migration for work during Monga. An important advantage of our study is that we have an exceptionally large data set on 143,000 poor and ultra-poor households (monthly household income less than Tk. 1,500 which was approximately $21 in 2006) which was collected by the Institute of Microfinance (InM), Dhaka, through a household census of the Monga-prone districts in Northern Bangladesh.[8] A second important advantage is that our sample covers many different microfinance NGOs working in the Northwestern Bangladesh including Grameen Bank, BRAC, ASA and BRDB. Our estimates give average effects of the different programs, and thus can lay claim to a higher level of generality compared to most of studies that focus on one specific microfinance program with a relatively small sample size of ultra-poor households.

Identification and estimation of the effects of microfinance membership is difficult because of selection biases arising from a household's participation decision and the screening by the MFIs (for a discussion, see Armendáriz and Morduch (2010)). Our main empirical strategy is an instrumental variables approach motivated by the screening process of MFIs in Bangladesh, especially as it relates to the ultra-poor households. While MFIs have in general been successful in reaching the moderate poor, the ultra-poor are more often excluded from microcredit services (see the discussion in Matin (1998), and Emran et al (2014)). The focus on moderate poor reflects an uneasy tradeoff between the poverty alleviation and repayment objectives of MFIs (Salim (2013), Emran et al. (2014)). Land ownership has been used by most of the MFIs in Bangladesh as a screening device. The microfinance programs are supposed to use half acre (50 decimal) land ownership as a cut-off, but the evidence in the literature shows that the half-acre rule is often violated (Morduch and Roodman (2014)). Consistent with the literature, we find that the half-acre landownership has little power in explaining the microfinance membership in our data set and thus cannot be the basis for an instrumental variables approach. Our approach instead exploits the fact that, to improve the repayment rate, MFIs try to exclude the ultra-poor, defined by BRAC and others as households owning less than 10 decimal land. We show that there is evidence of both a

[8] We are not aware of any other work that utilizes such a large data set to analyze the effects of microfinance on ultra-poor households.

jump and a kink in the probability of MFI membership at the 10 decimal land ownership threshold: the households with less than 10 decimal land are less likely to be included in the MFI program (after controlling for land owned and land squared). We implement the local 2SLS (henceforth L2SLS) suggested by Dong (2017) which is appropriate in our case with binary treatment and both a jump and kink in participation probability at the 10 decimal threshold. Since our outcome variables are binary, we also report estimates using a local bivariate probit estimator (LBiprobit) with the same instruments.[9]

The results from our empirical analysis provide robust evidence in favor of a beneficial effect of microfinance membership on a household's ability to cope with hunger during the seasonal famine (Monga), but microfinance seems to be ineffective in tackling the seasonal labor market failure. The probability that a household survives on 1 meal a day during the hungry season declines by 19 percentage points for the microfinance members according to the local Biprobit estimate.[10] This suggests that the poorest of the poor who struggle at the margin of 1 and 2 meals a day benefit substantially from microfinance. The relatively moderate poor for whom the relevant margin is 2 and 3 meals a day also seem to benefit, but to a much lesser extent. Microfinance membership improves the probability of having 3 meals a day during Monga by only 1.5 percentage points. The probability that a household resorts to short-term migration to cope with seasonal famine is 16 percentage points lower among the microfinance households. The magnitudes of the effects on these three outcome variables are larger according to the local 2SLS estimator.[11] In contrast, there is no evidence of a reduction in the probability of distress sale of labor in advance at low wages. The evidence thus suggests that microfinance is ineffective in addressing the labor market failure during the seasonal famine.

The rest of this paper is organized as follows. Section (2) provides a brief background of the persistent seasonal famine (Monga) in Bangladesh. The next section discusses our empirical

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[9] In the empirical results section, we also discuss and report estimates from the recently proposed minimum biased inverse probability weighted (MB-IPW) estimator by Millimet and Tchernis (2013). The estimates from MB-IPW are useful in two ways: (1) a comparison of OLS, MB-IPW, and L2SLS estimates help us better understand the direction of omitted variables bias, and (2) they provide a robustness check without imposing exclusion restrictions.
[10] We use three meals a day as an indicator of food security, as it is the norm in rural Bangladesh to have three full meals when a household has enough wealth. It is important to appreciate the fact that even the three meals are not likely to supply enough nutrition (especially protein) nor calories during Monga for most of the poor households.
[11] It is important to appreciate that the estimates from local Biprobit are average treatment effect on treated, while the estimates from local 2SLS are local average treatment effect. For an excellent discussion on this point, see Chiburis et al. (2012).

strategy. Section (4) is devoted to a discussion of the data and main variables. The empirical results are reported in section (5). The paper concludes with a summary of the empirical findings.

(2) Monga: The Season of Deprivation and Starvation

Despite the enormous social and economic progress made in last few decades, Bangladesh remains a poor country. While poverty fell nationwide from 49 to 40 percent between 2000 and 2005 (World Bank (2008)), the rural poverty rate in Rangpur district and surrounding areas in Northern Bangladesh was 56 percent in 2005 (Khandker et al. (2012)). Five million of Bangladesh's poor live in the 'Greater Rangpur' region, an area plagued almost every year by Monga, a season of near famine (World Bank (2008)).

An agricultural phenomenon, Monga is a season of severe food deprivation that strikes parts of Bangladesh with disturbing regularity. Locally known as Mora Kartik, or October the month of deprivation, it is "the bane of the rural poor, the season of half-meals and debt bondage" (Rahman (1995), p. 234). Every mid-September through mid-November there is a negative shock to income for the poor landless households primarily because of a lack of employment, and consequently households find it difficult to buy enough food for three meals a day. It is important to note that seasonal hunger is the consequence of seasonal income fluctuations among the ultra-poor whose income is barely enough to survive during regular times. It is not primarily the result of a shortage of food, the real problem is one of seasonal 'entitlement failure' in the sense of Sen (1981).

In rain-fed areas, the agricultural calendar in Bangladesh is divided into two main growing seasons. Aus, the wet season first paddy crop is grown when enough rainfall occurs (April to August). Aman, the wet season second rice crop lasts from July to December. In addition, if irrigation is available, a third rice crop, called Boro, can be planted after Aman. The hunger months occur between the planting and harvest of Aman, the autumn rice crop, when there is a shortage of employment and wages are very low but grain prices are high (Rahman (1995)). Thus, Monga repeats itself almost every year around September to November after the previous season's food has run out, before the transplanted rice is harvested in December. These months, and especially October, give rural people a harder time than usual because of extremely limited job opportunities (Muqtada (1975), Hossain and Bayes (2009)). Evidence shows that this variation in income is more pronounced in Greater Rangpur area which is the focus on our study than it is in other parts of the country (Khandker (2009)).

The Monga-affected region of Greater Rangpur, encompasses the districts of Gaibandha, Kurigram, Lalmonirhat, Nilphamari, and Rangpur. In the Monga of October 1991, Greater Rangpur experienced an average 50 percent drop in the daily wage rate compared to the rest of the year (Rahman (1995)). The existing evidence shows that the inhabitants of Greater Rangpur are poorer than the rest of the country in terms of a variety of indicators including income, expenditure, and poverty level. Extreme poverty in Greater Rangpur is 48 percent compared to the national rural average of 31 percent in 2005 (Khandker et al. (2012). Also, the daily wage rate in the same year was for male agricultural workers in Greater Rangpur is 46 taka compared to 64 taka found elsewhere (Khandker et al. (2009)). During Monga, the landless ultra-poor are especially hard hit as they cannot find employment.

Seasonal variation in income is, however, not unique to Bangladesh, it is common to agrarian economies. In addition to Bangladesh, Devereux et al (2012) discuss seasonality in China, Ethiopia, India, Malawi, Niger, Peru, and Sub-Saharan Africa. With monsoon-dependent crops and a lack of irrigation, agricultural households in Indian villages receive on average 75 percent of their annual income in the span of a three-month period (Chauduri and Paxson (2001)). When combined with extreme poverty, seasonal variation in income translates to seasonal hunger. Dercon and Krishnan (2000) found consumption of rural households in Ethiopia to vary greatly over a short period of time. Malawi has its own hungry season, as one of its citizen recounts: “Come January, most people are forced to tighten their belts and wait until harvest…We call this period ‘the hungry season’. In the countryside, people are working the hardest … but doing so with the least amount of food. Understandably, they grow thin, slow, and weak” (Kamkwamba and Mealer, (2009, p. 71)). Seasonal hunger in turn is the “father of famine” (Devereux et al (2008)). The distinction between seasonal hunger and famine is one of severity. Periodically, an annual shortfall in income will be so severe as to cause a famine.

(3) Empirical Strategy

To understand the issues involved in identification and estimation of the effects of microfinance on a household’s ability to cope with seasonal famine, consider the following triangular model:

$$C_i = \beta_0 + \beta_1 M_i + X_i^{'}\Gamma + \xi_i \quad (1)$$

$$M_i = \alpha_0 + X_i^{\prime}\Pi + \varepsilon_i \quad (2)$$

Where (1) is the outcome equation and (2) is the selection equation. The outcome equation shows household i's ability to cope during Monga (C_i), as a function of membership in any Microfinance Institution (M_i), household and village characteristics (X_i), and an error term (ξ_i). As noted earlier, the outcome variables we focus on are indicators of household food security (number of meals a day), incidence of advance labor sale, and migration in search of work during Monga.

The parameter of interest is β_1 in equation (1) which indicates the effect that microfinance membership has on a household's ability to cope during Monga. OLS and Probit estimation of equation (1) is likely to yield biased results. The endogeneity arises from the fact that there are individual, household and village level unobserved factors that may affect both the outcome and the selection equations, and thus the correlation between the error terms is not zero, i.e., $\rho = Cov(\xi_i, \varepsilon_i) \neq 0$. The most obvious individual-level common unobserved heterogeneity in the context of microfinance is ability (or entrepreneurial capability). An individual with higher ability would expect higher net return from participation in the credit program, and thus would self-select into the program. But higher ability may also mean that she will have better economic outcomes in the absence of credit availability. This creates a positive selection effect, and $\rho > 0$ on this account. However, it is also possible that the outside option for a high ability entrepreneur is much better (shadow price of time is higher), and thus she might not be interested in high interest rate microloans with its web of restrictions (such as group liability, and substantial time commitments for regular group meetings). If this is the case, MFI would attract relatively low ability micro-entrepreneurs and thus the selection would be negative implying $\rho < 0$.

The correlation between ξ_i and ε_i can also arise because of nonrandom placement of MFI programs (and branches) across different villages. For example, to ensure high repayment rates, the MFIs have incentives to select villages with concentration of moderate poor and eschew the most vulnerable villages with concentration of ultra-poor. The MFI can also use information about a village's economic potential to pick better endowed villages. This would result in a positive correlation between the error terms in equations (1) and (2) above implying $\rho > 0$. But if the MFIs are true to their objective of poverty alleviation, then they will target relatively unfavorable villages, thus making $\rho < 0$ The available evidence shows that the objective function of microfinance NGOs is a convex combination of poverty objective and repayment objective (Salim (2013)).

Tackling Omitted Variables Bias: Alternative Approaches

When the outcome variable is binary, as is the case in our application, one can estimate the triangular model using maximum likelihood under the assumption that ξ_i and ε_i are approximately bivariate normal without imposing any exclusion restrictions. However, such identification relies on the nonlinearity of Normal CDF, and is regarded as non-robust (Altonji et al, (2005)). In the absence of any exclusion restrictions, one can also use various matching methods (Heckman and Navarro-Lozano (2004), Heckman et al. (1998), Hirano and Imbens (2001)). However, the matching methods rely on the assumption that there is no significant selection on unobservables conditional on matching on the observable characteristics. This is a strong assumption, unlikely to hold in our context.

Recent literature has developed ways to minimize and correct for the bias that results from the failure of the conditional independence assumption (CIA). We use the Minimum Biased Inverse Probability Weighted IPW (MB-IPW) estimator proposed by Millimet and Tchernis (2013) which provides a way to reduce the bias. The Minimum-Biased IPW estimator adapts the normalized Inverse Probability Weighting (IPW) estimator of Hirano and Imbens (2001) by exploiting a result due to Black and Smith (2004) that the bias minimizing propensity score is equal to 0.50 when estimating the average treatment effect on the treated. The MB-IPW estimator restricts the sample to the observations with a propensity score in the "neighborhood" around the Bias Minimizing Propensity Score (BMPS). This can be thought of as a formalization of the increasingly common practice of reducing bias in matching and propensity score weighted estimates by excluding observations from the tails of propensity score distribution (see, for example, Moretti and Kline (2014)). Millimet and Tchernis define the neighborhood around the 'BMPS' as observations with a propensity score within a radius θ, such that at least θ percent of both treatment and control groups are included (after trimming observations with a BMPS below 0.02 and above 0.98). They set this radius at 0.05 and 0.25. Motivated by the Monte Carlo evidence, we implement the MB-IPW estimator with a radius of 0.25.

An Instrumental Variables Approach

Our main identification approach is an instrumental variables strategy inspired by the recent literature on Regression Jump and Kink Design (Nielson et al. (2010), Card et al (2012), Dong (2017)). While it has long been known that a jump (or a discontinuity) in the participation probability at a policy threshold generates credible identifying variation, the recent advances in the literature has clarified the advantages of such a design for nonexperimental data (for a survey, see Lee and Lemieux (2010)). The general idea is that observations within a close neighborhood around the threshold are similar except for the fact that some received treatment while others did not because of idiosyncratic factors beyond their control, thus mimicking a local randomization. Differences in their outcomes can then be attributed to the effect of the treatment. An interesting recent extension called regression kink design (RKD) shows that, in the absence of a discontinuity, a kink (i.e., a change in the slope) in the probability of participation at a threshold created by program design or policy rule can provide the basis for identification. The approach is implemented as an instrumental variable estimator when compliance is imperfect.[12] In our context, while the MFI loan officers tried to exclude high repayment risk ultra-poor households with less than 10 decimal land, this screening process was less than perfect. In a recent paper, Dong (2017) proposes a local 2SLS estimator for the case when there is evidence of both kink and discontinuity at a threshold and the treatment is binary as is the case in our application. We thus implement the L2SLS approach suggested by Dong (2017) which applies the 2SLS estimator to a subsample of observations around the threshold.

In the context of microfinance in Bangladesh, it is widely discussed in the literature that MFIs use land ownership of potential borrowers as a screening mechanism (for a discussion, see Mahmud and Osmani (2017)). Following Grameen Bank, most MFIs in Bangladesh, at least in principle, use less than half an acre (50 decimal) of land ownership as an eligibility criterion, ostensibly to target the poor. This suggests half an acre of land as a possible cutoff, as was used in Pitt and Khandker (1998), Morduch (1998), and Menon (2006), among others. However, there is substantial evidence that most of the MFI programs, in many cases, fail to adhere to the half-acre rule. Moreover, it might be subject to manipulation by credit constrained households, a

[12] Nielson et al (2010) first coined the term "RKD" and demonstrated that it is enough that there is a kink in the treatment probability to achieve identification. Building on the Nielson et al (2010) paper, Card et al (2012) present a fuzzy RKD for the case when treatment is continuous.

possibility especially important in our context given the weakness of land administration and record keeping in Bangladesh. In our data set, there is no evidence that the half-acre rule is an important factor in determining which households get microcredit and which do not. As shown in Table 1, the half-acre land dummy is not statistically significant at the 10 percent level in the microcredit membership equation.

We instead rely on the widely-documented fact that the standard microcredit programs systematically exclude the ultra-poor, motivated by the goal of ensuring high repayment rates (see, e.g. Noor et al. (2004), Rabbani et al. (2006), Emran et al. (2014), Mahmud and Osmani (2017)). This suggests a different land ownership cutoff that is potentially important for explaining microcredit membership because most of the MFIs in Bangladesh define a household as ultra-poor if it owns less than 10 decimal land. BRAC's specialized CFPR-TUP asset transfer program uses 10 decimal land as an eligibility criterion for ultra-poor (see Emran et al. (2014), Matin et al. (2008)). The households with less than 10 decimal land belong to "landless I" (with no land) and "landless II" (own only homestead land) categories according to the landlessness definitions used by Land Occupancy Survey of 1977-78 done by Bangladesh Bureau of Statistics in collaboration with USAID.[13] The importance of 10 decimal land cut-off for MFI membership is confirmed by the regression evidence presented in column 2 of Table 1; the coefficient on the dummy for less than 10 decimal land-ownership is -0.056 and is statistically significant at the 1 percent level (standard error is 0.01), after controlling for land owned and its square term along with village level controls and district fixed effects. The ultra-poor households are thus less likely to receive microcredit. Figure 1 shows that there is a visible jump in the probability of MFI membership around the 10-decimal threshold using the 20 percent sample around the threshold (10 percent above and 10 percent below). Figure 2 shows the corresponding graph for the 10 percent sample (5 percent above and 5 percent below). The graphs also suggest that the slopes of the probability function may be different below and above the 10-decimal threshold. To check if the slope of the propensity score function changes at the 10-decimal threshold giving rise to a kink, we regress the dummy for MFI membership on the interaction of the dummy for 10 decimal land with rescaled land (=land owned – 10). The coefficient on this variable is -0.036 with a standard error of 0.012 (significant at the 1 percent level). This suggests that there is a kink in the probability of MFI

[13] The households with half-acre land are classified as "landless III". The half-acre eligibility criterion used by the standard microfinance programs is based on this definition of landlessness.

membership at the 10-decimal land ownership threshold. We thus use the interaction as an additional instrument.

A key issue for identification using 10 decimal land ownership cut-off is whether it is manipulated by the households. The worry is that the poorest of the poor households with less than 10 decimal land might misreport higher than 10 decimal land to increase their odds of getting credit. This assumes that the practice of excluding ultra-poor with less than 10 decimal land was widely known to the households. However, unlike the 50-decimal (half-acre) eligibility criterion, the 10-decimal cut-off used by the loan officers was not widely publicized. So, it is likely that many of the households were not aware of the 10 decimal cut-off, and the possibility of manipulation is much less in this case. Second, even if some ultra-poor households over-report land to increase their probability of getting loans, the fuzzy jump and kink design remains valid as long as they cannot precisely manipulate the land ownership information to meet the de facto exclusion rule used by the loan officers. Third, if the households are successful in manipulating the selection process by overstating land ownership, we should observe a lump in the land distribution just above the 10-decimal threshold. However, a McCrary (2008) test does not show a lump above the 10-decimal land ownership (see Figure 3). A test of the null hypothesis that the households over-report land ownership to increase their odds of getting credit is soundly rejected at the 1 percent level.

As additional evidence, we check whether the households in the sample around the 10-decimal land threshold are sufficiently similar in terms of observables. Figure 4 displays Quantile-Quantile Plots comparing the control variables for microfinance members with those of non-members using the 10 percent sample around a tenth of an acre.[14] Except for distance to secondary school, all the other controls appear balanced on either side of the 45-degree line.

An important advantage of our data for local 2SLS and local Biprobit estimation is that it is exceptionally large. This allows us to trim the data around the threshold considerably while still retaining enough power to yield credible estimates. Our main IV estimates use a 10 percent sub-sample consisting of 24,132 households, which is much larger than the other household data sets used for microfinance evaluation in Bangladesh. The 10 percent sample consists of households with land ownership in a very small interval: [6 decimal, 16 decimal] (1 acre = 100 decimal). This considerably strengthens the credibility of the local 2SLS and local Biprobit estimates.

[14] See Calel and Dechezleprêtre (2016) for recent use of quantile-quantile plots.

(4) Data

The data used in this paper were collected by the Institute of Microfinance (InM), a non-profit research institution in Dhaka, Bangladesh. The InM and Palli Karma Shahayak Foundation (PKSF), non-profit government organizations, jointly conducted a census of poor households in the district of Lalmonirhat in 2006 and in the districts of Gaibandha, Kurigram, Nilphamari, and Ranpgur in 2007. The sample was limited to the poor households meeting at least one of the following three criteria: less than 1,500 taka of monthly income, dependent on day labor, or having less than 50 decimals (half an acre) of land (Uddin (2008)).

The questionnaire used to collect the data covered a broad range of topics including employment, family size, assets, migration for work during Monga, food security, and membership in any microfinance institution. The census was conducted during normal (non-Monga) time and Monga-values refer to the seasonal famine during the previous lean season. An example of a question asked of households is "During the last Monga how many times a day did you eat (how many meals)?" This provided information on whether a household could consume three meals or was facing starvation with only one or two meals a day during Monga.

The outcome variables we focus on in this paper are indicators of food security, distress sale of labor, and short-term migration during the seasonal famine. We use two indicators of food security: binary variables for ability to have three meals and one meal a day during last Monga. We use a dummy for advanced labor sale during last Monga as an indicator of distress sale. Village-level variables were collected in a separate questionnaire and included information on the moneylender interest rate; agricultural wages; electricity use in the village; and distances to the nearest bank, market, and secondary school.

This paper focuses on the Districts of Gaibandha, Lalmonirhat, and Nilphamari as they had the most complete data set (Kurigram and Rangpur were lacking village-level variables). For the basic OLS and Probit estimates, we use a sample of 143,346 households. Although the household census covered a much larger sample of 280,000 households, only a subset of villages was covered by the village survey. Our sample consists of only those households where the village survey was carried out. More important, to implement the local 2SLS estimator we need to restrict the sample by excluding the households far away from the 10 decimal land threshold. After dropping the households with missing variables, and restricting to the observations in the 10 percent of the sample around the 10 decimal land threshold, we focus on 24,132 households for the main empirical analysis. Table A1 in the appendix reports summary statistics of the main variables used in our analysis. We check the robustness of the conclusions using alternative samples.

(5) Empirical Results

Preliminary Results

We begin our analysis with OLS and Probit estimates. Given the binary-binary structure of our empirical model, Probit estimates may be more efficient, but the OLS may be more robust, as it does not rely on distributional assumptions. The estimates from OLS and Probit are, however, very similar, and we thus focus on the OLS results in the main text (see Table 2), and report the Probit results in the online appendix. The set of controls used in different specifications include household level controls (age and age squared, land owned and land squared), and village level controls (electricity use in the village, and distances to the bank, market, and secondary school). The controls are selected to reduce biases due to self-selection by households and MFI program placement. For an extended discussion on the choice of control variables, please see the online appendix.

The broad picture that emerges from the OLS results for the full sample (143,346 households) reported in Table 2 panel A is that microfinance membership seems to have a small positive effect on a household's ability to cope with Monga. The OLS estimates suggest that microfinance membership increases the probability that a household can have three meals a day, and reduces the probability that a household survives only on one meal a day, and thus help avoid hunger during the seasonal famine. The estimated effects from the most complete specification including household and village characteristics are statistically significant at the 1 percent level. In terms of magnitude, microfinance membership raises the probability of consuming three meals a day by about 1 percentage point, and reduces the probability of surviving on one meal a day by about 3 percentage points, which are not substantial.[15] The estimated impacts on three meals and one meal a day are robust across the samples: Panel B reports the results for the 20 percent sample and panel C for the 10 percent sample.

[15] Note, however, that the OLS results may be significantly biased downward due to negative selection and attenuation bias. The results reported later that correct for the bias seem to confirm this suspicion.

The impact of microfinance on no labor sale is consistently positive and significant at the 1 percent level. In terms of magnitude, microfinance membership increases the probability that a household does not need to sell labor in advance by about 1.5 percent. As was the case for three meals, the coefficient for no labor sale is robust to different control variables. Unlike three meals, though, as the sample is reduced the magnitude falls, suggesting that the effect may not be robust.

The OLS results do not indicate any impact of microfinance on the probability of migration for the full sample. Since microfinance uses family labor, one might have expected a negative effect on migration for short-term work. When we look at the 20 and 10 percent samples, we find that microfinance has a negative effect on migration when including only household characteristics. However, the finding is not robust, as it becomes insignificant with the inclusion of village characteristics.

These preliminary results are interesting, but are subject to potentially serious endogeneity bias due to omitted heterogeneity that affects both the selection into the program, and the outcomes. In addition, measurement error may partly be responsible for very small numerical magnitudes of the estimates in Table 2. To address the potential bias in the OLS estimates, we report estimates from two approaches below: (i) minimum bias (MB) estimator due to Millimet and Tchernis (2013) that reduces the omitted variables bias without any exclusion restrictions; and (ii) the instrumental variables approach developed above in section (3).

Estimates from Minimum Biased IPW Estimator

In this sub-section, we discuss the results from the Minimum Biased IPW (MB-IPW) estimator reported in Table 3. All the estimates reported in Table 3 use the full specification with household and village controls used in column (3) of Table 2. Column (1) runs the MB-IPW estimator on the full 143,346 sample of households. All of the MB-IPW point estimates are larger as compared to OLS, suggesting that the bias in the OLS estimates is negative. In terms of magnitude, microfinance increases the probability of three meals by 1 percent, decreases the probability of only one meal by 5 percent, and increases the probability of not selling labor in advance by 2 percent, and increases the probability of migration by 2 percent. The 90 percent confidence interval shows that the effect of microfinance membership is statistically significant for all four outcomes.

Columns (2) and (3) focus on the 20 and 10 percent sub-samples, respectively. These are included as a benchmark, since we will focus on the 10 percent sample for our main IV estimation, because the local 2SLS and local Biprobit estimators use only a subsample of observations from the neighborhood of a threshold (Dong, 2017). It is reassuring to note that the point estimates are robust in these subsamples for three of the four outcome variables. The exception is migration, which becomes insignificant in smaller samples. Note that the 20 percent and 10 percent samples are symmetric around the threshold in that they include one half of the households from above and the other half from below.

Evidence from Instrumental Variables Approach

Our main IV results are reported in Tables 4 (local 2SLS) and 5 (local Biprobit). Following Dong (2017), we use a subset of the observations in the neighborhood of the threshold for estimation. The results reported in Tables 4 are based on the 10 percent sample: 5 percent from above the 10-decimal threshold and 5 percent from below. We later check the sensitivity of these estimates with alternative samples around the threshold (12 percent and 8 percent samples).

We control for the direct effect of land ownership on the outcome variables in the regressions. One would expect that the effect of land on the outcome variables is concave because of diminishing returns. However, the sample covers a narrow interval of land ownership, with a minimum of 6 decimal and a maximum of 16 decimal (1 acre = 100 decimal). Since the households vary little in terms of land ownership, the direct effect is likely to be well approximated by a linear control term. We take a conservative approach and report the estimated effects with quadratic controls for land ownership. The results with a linear land ownership control are similar and reported in the online appendix, Table B.2.

The first two columns report the estimates when a dummy for less than 10 decimal land is used as the instrument, i.e., the "jump instrument"; and the second and third columns contain the results for the "kink instrument", i.e., the 10 decimal land dummy interacted with (land owned – 10), while the last two columns report the estimates where both the "jump" and "kink" instruments are used together. The odd numbered columns report estimates when we control for possible direct effect of land ownership by including land owned and its square in the regressions, but no other controls are included. It is important to appreciate that our main sample (i.e., the 10 percent sample) include a very small interval of land distribution, which implies that land ownership is not a major determinant of the observed variations in the outcomes.[16] The even columns in addition include household and village characteristics described in detail in the online appendix A.

The results in Table 4 are from L2SLS estimator and those in Table 5 are marginal effects from LBiprobit using the same set of instruments. We report estimates from both L2SLS and LBiprobit because our outcome variables are binary, and thus we have a binary-binary set-up. The linear IV estimator may yield implausible estimates in this case, for example, predict probability of an outcome that falls outside the [0,1] bound. It is, however, important to emphasize that the estimates from local 2SLS and local Biprobit are not comparable. As discussed by Chiburis et. al. (2012) in detail, the instrumental variables estimates from Biprobit provide average treatment effect on treated (ATET), while the estimates from 2SLS provide local average treatment effect (LATE). It is extensively discussed in the literature that 2SLS estimates differ substantially (and usually larger in magnitude) when compared to the corresponding Biprobit estimates (see the discussion in Altonji et al. (2005)).

The first stage results reported in the lower panel of Table 4 show that the dummy for less than 10 decimal land ownership has a negative effect on the probability of microfinance membership, and is statistically significant at the 1 percent level. The lower probability of microcredit membership found among the ultra-poor is consistent with substantial evidence accumulated over last four decades that the microcredit programs in Bangladesh consider them repayment risk and thus systematically exclude them (see the discussion in Emran et al. (2014), and Matin et al. (2008)). The “kink instrument”, i.e., the interaction of 10 decimal dummy with rescaled land (land owned - 10) also bears a negative sign across specifications, and is significant at the 1 percent level. When both instruments are used, the kink instrument loses its significance and identification in this case seems to be primarily driven by the 10 decimal dummy. The Angrist-Pischke F statistics for the jump and kink instruments reported in first four columns show that the IV estimates are not subject to weak instrument bias: in only one case the A-P F statistics is marginally lower than 10 (9.61). The A-P F statistic falls when we use two instruments together because the kink instrument loses significance and A-P F incorporates penalty for more instruments.

[16] Compare them to the minimum=0 and maximum=363 decimal in our “full” sample with 143,346 households. I acre = 100 decimal.

The instruments based on the 10 decimal land provide us the local average treatment effect for those households which have more than 10 decimal land and thus were selected into the program. As noted before, the 10 percent sample used for the IV estimation includes households with land in the interval of [6 decimal, 16 decimal]. The treatment households captured by the IV approach thus belong to the right of 10 decimal in the above interval.

Estimated Effects of Microfinance Membership

The local 2SLS (L2SLS) estimates in Table 4 show that microcredit membership consistently has a beneficial effect on a household's food security during the seasonal famine. Microcredit-member households were more likely to have 3 meals a day and to avoid surviving at the brink of starvation with only one meal a day (estimates are significant at the 1 percent level in 10 out of 12 cases in Table 4, and at the 5 percent level in 1 case, and at the 10 percent level in 1 case).[17] The magnitudes of the effect vary across different instruments: the estimated effect is larger when we use the interaction instrument compared to the corresponding estimates for 3 and 1 meal a day based on 10 decimal dummy as the instrument. Since the effects are likely to be heterogeneous, the two instruments seem to refer to different subpopulations (i.e., different LATEs).[18] The L2SLS estimates for the interaction (i.e., kink) instrument for the outcome one meal a day are, however, implausibly large; the estimates are larger than 1 in both specifications, a limitation of the linear IV estimates noted before. A plausible interpretation of this evidence is that there is a subset of poor households for whom the probability of having one meal a day during Monga becomes zero once they become microfinance member.

The most conservative local 2SLS estimates come from the specification where both instruments are used; but even those estimates, reported in the last two columns of Table 4, are larger than the corresponding OLS and MB-IPW estimates. The relative magnitudes of the OLS, MB-IPW and Local 2SLS estimates taken together, i.e., OLS < MB-IPW < L2SLS, tells a consistent story about the direction of omitted variables bias: the estimates become progressively larger as more effective method to address the bias is used. This suggests significant downward bias in the OLS estimates owing to negative selection on unobservables and possibly attenuation

[17] Chiburis et al. (2012) recommend that the standard errors are bootstrapped when sample size is less than 10,000. Our main sample consists of 24,132 households.
[18] This also implies that it is not meaningful to use Hansen's J statistic to test for instrument validity.

bias caused by measurement error. A comparison of the first two rows show a consistent pattern: the effect is systematically larger in the case of 1 meal a day, suggesting that microfinance is especially helpful for the poorest of the treatment households struggling at the margin of starvation during seasonal famine. The relatively food secure households for which the relevant margin is between 2 and 3 meals a day also benefit from microfinance membership, but to a much lesser extent.

In contrast to the strong positive effects on food security, the evidence from L2SLS estimates suggest that microcredit membership has no perceptible effect on a household's propensity to sell labor in advance; the MFI membership dummy is not significant at the 10 percent level in any of the six columns reported in Table 4.

The estimates in the fourth row of Table 4 refer to the probability that a household had to resort to short-term migration to cope with seasonal famine. The evidence is strong that microfinance membership reduces the probability of short-term migration during the season of starvation. All six estimates, however, are larger than 1 in absolute value. One way to interpret this evidence is that microfinance membership reduces the probability of short-term migration during Monga season virtually to zero for a subset of households.

The LBiprobit estimates in Table 5 support the qualitative conclusions based on the L2SLS results in Table 4: (1) microfinance membership improves food security of a household during seasonal famine, (2) the poorest households at the margin of starvation benefit more (i.e., the effect is larger in magnitude for one meal a day), (3) microfinance is ineffective in reducing a household's propensity to sell labor in advance at lower wages, and (4) microfinance helps avoid short-term migration to cope with Monga. However, the point estimates from LBiprobit are strikingly different from the corresponding L2SLS estimates; they are much smaller in magnitude. This pattern is not surprising, given a wealth of evidence in the literature that the linear IV estimates almost always are much larger than the corresponding Biprobit estimates in a binary-binary model (see Altonji et al (2005) and Chiburis et al. (2012)). More important is the fact that the Biprobit estimates provide us ATET and thus refer to the average of heterogeneous treatment effects over the whole sample (i.e., the 10 percent sample). In contrast, the 2SLS estimates are LATEs, and thus refer to some subset of the households in the 10 percent sample.

Given the differences in the estimates from L2SLS and LBiprobit, one can adopt two alternative strategies to interpret the evidence. The first, and most conservative, is to settle on the robust qualitative conclusions without endorsing any preferred point estimate. This is perhaps the most widely acceptable interpretation. The second approach is to focus on the ATET estimates from LBiprobit, as we do in the introduction of this paper. This approach affects the conclusion regarding the effects of microfinance membership on the probability of 3 meals a day most dramatically; the effect is numerically ignorable (about 1.5 percentage points) according to LBiprobit estimates, but the lowest L2SLS estimate indicates a substantial impact (26 percentage points). This suggests that while there are some households who benefit a lot when it comes to having three meals a day during seasonal famine, on an average the effects of microfinance membership are negligible in the 10 percent sample used for local IV estimates.

We have so far focused on the IV estimates from the 10 percent sample around the 10 decimal. To check robustness of the results, we report the L2SLS and LBiprobit estimates for 8 percent and 12 percent samples (see Table 6). The strength of the instruments in explaining microfinance membership varies across samples and the instrument used. In both 12 percent and 8 percent samples, the dummy for less than 10 decimal land provides enough power for identifying the effects, but the A-P F statistics are low when the interaction instrument alone or two instruments are used together. So, we focus only on those cases where the dummy for less than 10 decimal is the only identifying instrument. The results are consistent with the evidence in Table 4 and support the conclusions reached above regarding the effects of microfinance on a household's ability to cope with seasonal famine in Bangladesh.

(6) Conclusions

This paper provides evidence on the effects of microfinance membership in Bangladesh on the ability of a poor household to cope with anticipated seasonal adversity using the seasonal famine in North-west Bangladesh as a case study. We take advantage of an exceptionally large households survey data set with more than 143,000 households for the empirical analysis.

We implement an instrumental variables approach that is motivated by the observation that MFIs exclude the ultra-poor households (with less than 10 decimal land) to ensure repayment rate. We show that a household with less than 10 decimal land is significantly less likely to get microcredit. The evidence suggests that there are both a jump and a kink in the probability of MFI membership at the 10 decimal threshold. We implement the local 2SLS estimator (Dong, 2017) and a local Biprobit to estimate the effects of MFI membership on indicators of food security (number of meals a day), labor market vulnerability (advance sale of labor at low wages), and seasonal migration for job during the season of hunger (Monga). We also report estimates from the minimum biased IPW estimator due to Millimet and Tchernis (2013) that reduces bias in the estimates without imposing any exclusion restrictions. Our estimates from alternative samples, estimators, and control variables yield robust conclusions: (1) MFI membership improves food security during seasonal famine, especially for the poorest households who struggle at the margin of 1 or 2 meals a day; (2) MFI members are significantly less likely to be forced to short-term migration for jobs during the hungry season, (3) MFI membership is ineffective in reducing the labor market vulnerability of ultra-poor: the probability of advance sale of labor during Monga is not affected by MFI membership.

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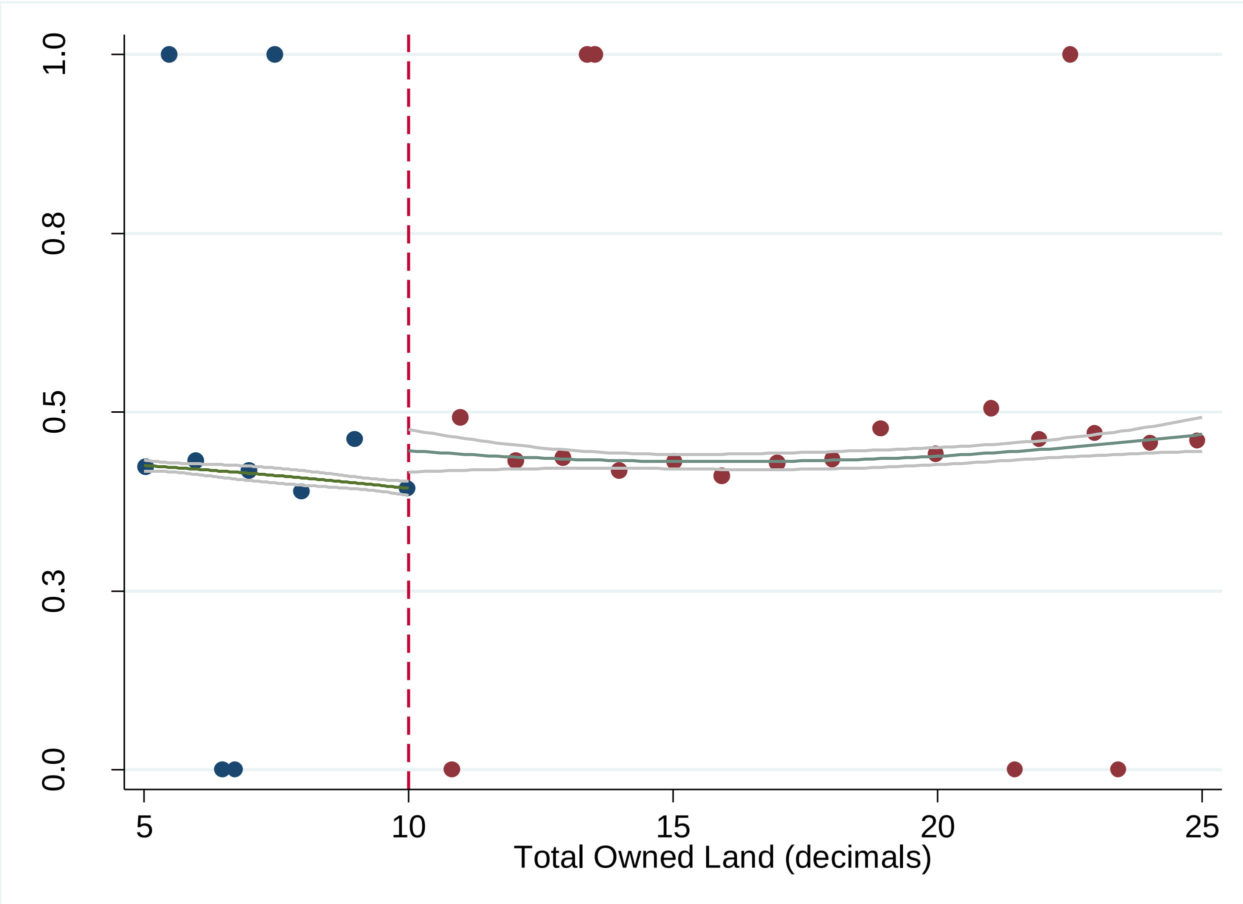
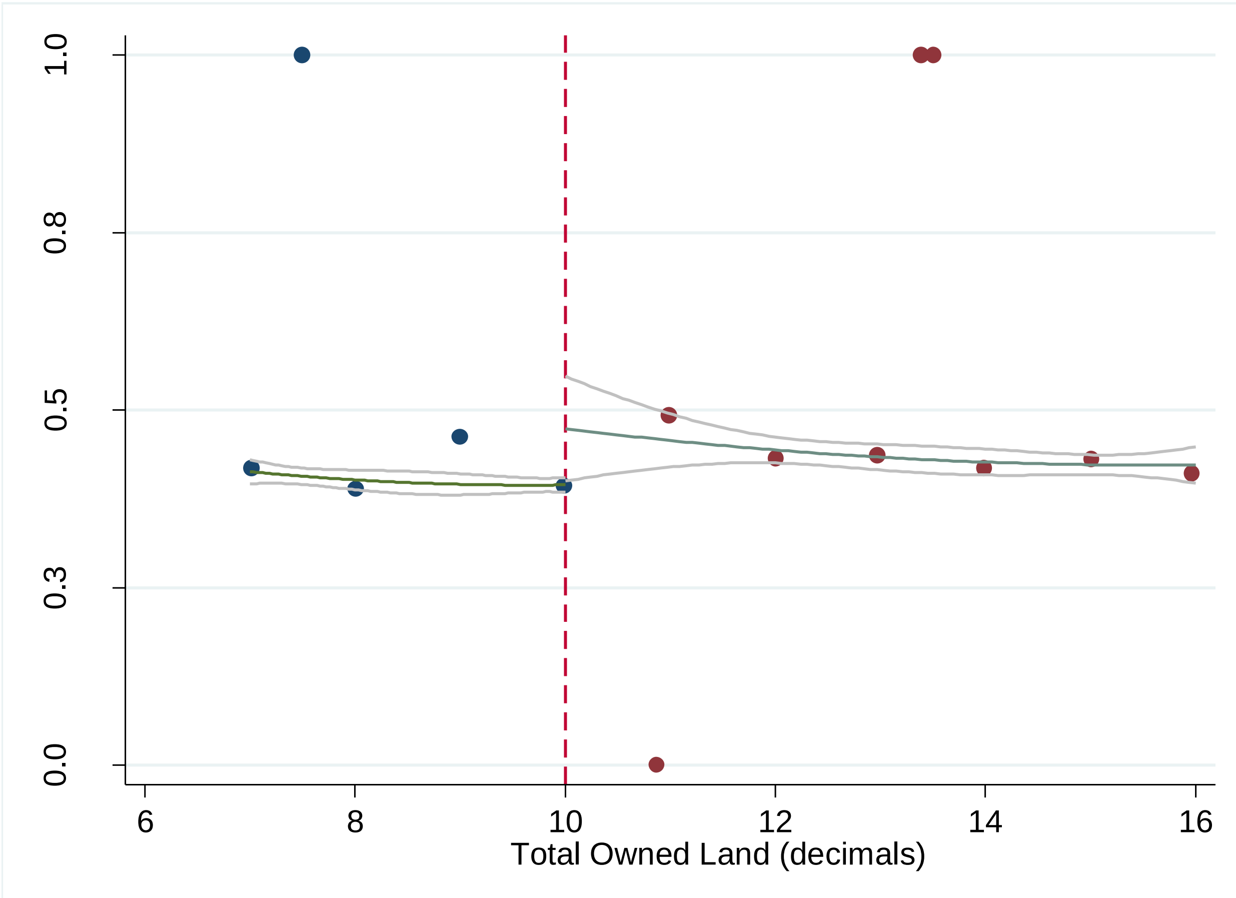
Figure 1: Probability of MFI Membership and Land Ownership (20% Subsample)



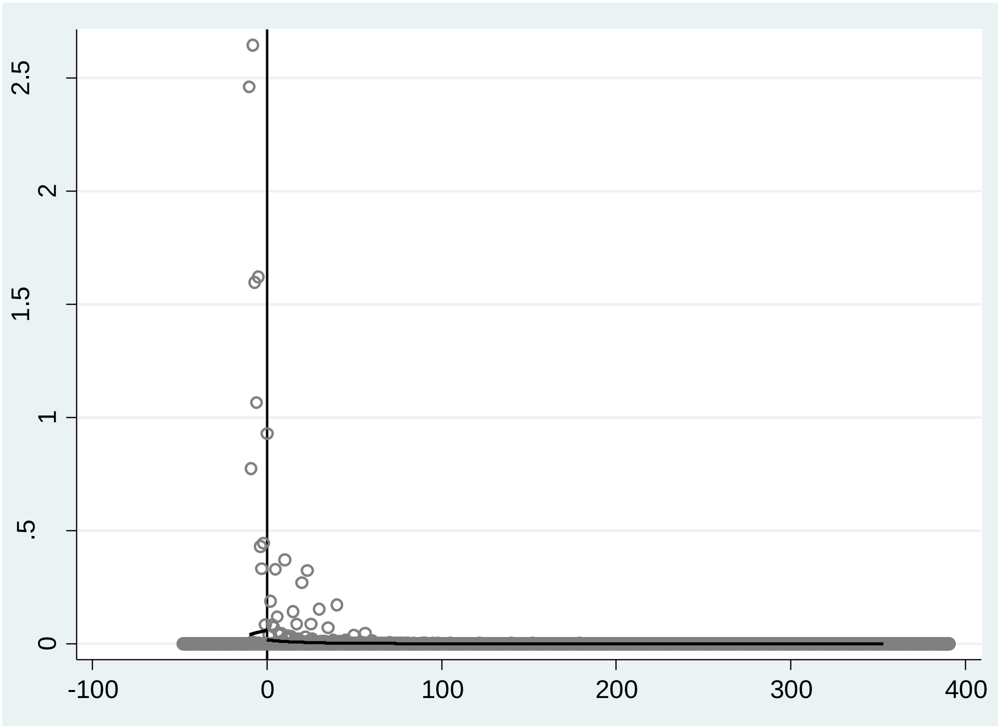
Notes: Discontinuity in probability of MFI membership with quadratic fit and 95% confidence intervals reported. The left hand side indicates household owning less than 10 decimals of land and the right hand side indicates those owning more. The graph shows that owning more land increases the probability of MFI membership discontinuously. The graph is calculated from the 20% subsample of households (49,753). (Graph generated using Stata Code cmogram.)

Figure 2: Probability of MFI membership and Land Ownership (10% Subsample)



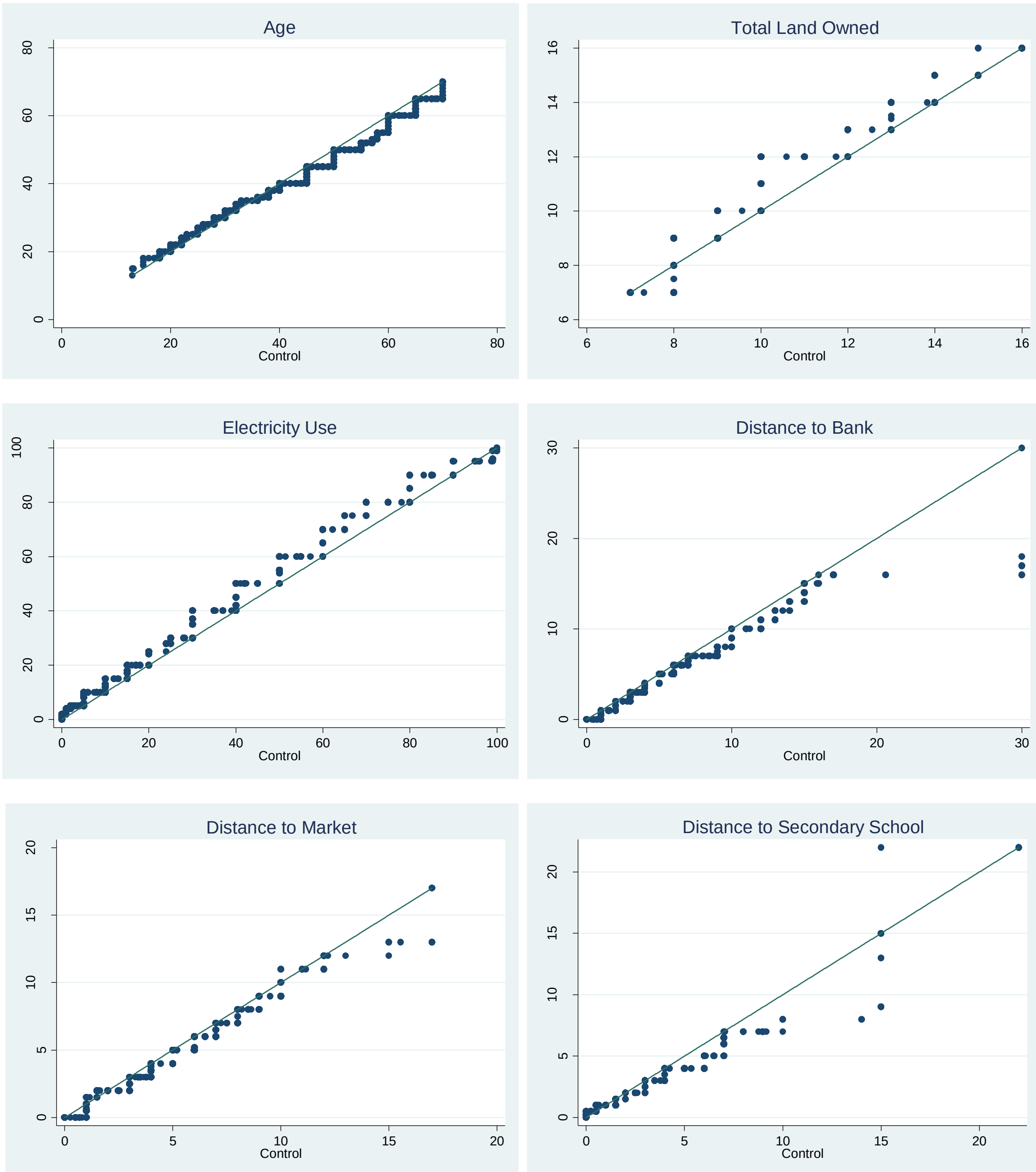
Notes: Discontinuity in probability of MFI membership with quadratic fit and 95% confidence intervals reported. The left hand side indicates household owning less than 10 decimals of land and the right hand side indicates those owning more. The graph shows that owning more land increases the probability of MFI membership. The graph is calculated from the 10% subsample of households (24,132). (Graph generated using Stata Code cmogram.)

Figure 3: McCrary Density Test



Notes : McCrary Density Test on total land owned by the household. (Graph generated using Stata Code DCdensity, McCrary (2008)).

Figure 4: Quantile-Quantile Plots of Control Variables



Note: The above empirical Quantile-Quantile Plots are calculated from the 10% subsample and display the value of the control variables for the treated (y-axis) and untreated (x-axis) units. The closer the observations fall relative to the 45 degree line, the more similar the treatment and control groups are.

Table 1: Determinants of MFI Membership: The Role of 10 Decimal (0.1 acre) and 50 Decimal (Half-acre) Thresholds

Dependent Variable: MFI Member (yes = 1)	**(1)**	**(2)**	**(3)**	**(4)**
Owns less than 10 decimals of land (yes = 1)	**-0.060*** **	**-0.056*** **		
	(0.01)	(0.01)		
Owns less than 50 decimals of land (yes = 1)			**-0.013**	**-0.012**
			(0.02)	(0.02)
Land	-0.005	-0.005	-0.003***	-0.003***
	(0.01)	(0.01)	(0.00)	(0.00)
Land squared	-0.000	0.000	0.000*	0.000*
	(0.00)	(0.00)	(0.00)	(0.00)
Age of household head		0.020***		0.017***
		(0.00)		(0.00)
Age squared		-0.000***		-0.000***
		(0.00)		(0.00)
Electricity use in village (%)		0.001***		0.002***
		(0.00)		(0.00)
Distance from bank		-0.010***		-0.008***
		(0.00)		(0.00)
Distance from market		0.002		0.001
		(0.00)		(0.00)
Distance from secondary school		-0.003*		0.003
		(0.00)		(0.00)
Constant	0.509***	0.140**	0.568***	0.234***
	(0.06)	(0.07)	(0.05)	(0.07)
Observations	24,132	24,132	10,276	10,276
R-squared	0.001	0.019	0.002	0.020

Note: columns (1) and (2) use the 10% subsample of observations around 10 decimals of land ownership. Columns (3) and (4) use the 10% subsample of observations around 50 decimals of land ownership.
Robust standard errors in parenthesis: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2: OLS Results

A: Full Sample

	(1)	(2)	(3)
Has 3 full meals per day during Monga (yes = 1)	0.011***	0.010***	0.009***
	(11.84)	(10.45)	(9.31)
Has 1 full meal per day during Monga (yes = 1)	-0.043***	-0.031***	-0.027***
	(-15.88)	(-11.60)	(-9.98)
Does not sell labor in advance (yes = 1)	0.016***	0.015***	0.015***
	(14.22)	(13.34)	(13.17)
Migrates in search of work (yes = 1)	0.004	-0.004	0.009***
	(1.37)	(-1.38)	(3.18)
Number of observations	143,346	143,346	143,346

B: 20% Sample

Has 3 full meals per day during Monga (yes = 1)	0.012***	0.012***	0.010***
	(7.72)	(7.45)	(6.58)
Has 1 full meal per day during Monga (yes = 1)	-0.047***	-0.043***	-0.036***
	(-10.31)	(-9.43)	(-8.05)
Does not sell labor in advance (yes = 1)	0.009***	0.009***	0.009***
	(5.22)	(5.01)	(4.88)
Migrates in search of work (yes = 1)	-0.007	-0.012***	-0.001
	(-1.54)	(-2.74)	(-0.22)
Number of observations	49,753	49,753	49,753

C: 10% Sample

Has 3 full meals per day during monga (yes = 1)	0.015***	0.014***	0.013***
	(6.34)	(6.24)	(5.53)
Has 1 full meal per day during monga (yes = 1)	-0.056***	-0.053***	-0.044***
	(-8.66)	(-8.14)	(-6.78)
Does not sell labor in advance (yes = 1)	0.008***	0.008***	0.008***
	(2.95)	(2.86)	(2.93)
Migrates in search of work (yes = 1)	-0.013**	-0.017***	-0.005
	(-1.97)	(-2.66)	(-0.81)
Number of observations	24,132	24,132	24,132

Notes: Panel A uses the full sample, Panel B focuses on 20% of the sample around 0.1 acres of land ownership, and Panel C focuses on 10% of the sample around 0.1 acres of land ownership.
Robust t-statistics in parenthesis: * significant at 10%; ** significant at 5%; *** significant at 1%.
Model (1) regresses the outcome variable against microfinance membership. Model (2) adds household control variables to Model (1). Model (3) adds village controls to Model (2). The set of household controls includes: age of household head, age squared, land owned, and land squared. The village controls include: electricity use in the village, and distances to the bank, market, and secondary school.

Table 3: Estimates from Minimum Biased-IPW

	(1) Full Sample	(2) 20% Sample	(3) 10% Sample
Has 3 full meals per day during Monga (yes = 1)	0.010 [0.007, 0.014]	0.010 [0.005, 0.016]	0.016 [0.009, 0.024]
Has 1 full meal per day during Monga (yes = 1)	-0.045 [-0.053, -0.037]	-0.048 [-0.064, -0.036]	-0.055 [-0.075, -0.037]
Does not sell labor in advance (yes = 1)	0.020 [0.017, 0.023]	0.014 [0.009, 0.020]	0.013 [0.004, 0.020]
Migrates in search of work (yes = 1)	0.018 [0.010, 0.025]	0.006 [-0.008, 0.018]	-0.009 [-0.025, 0.015]
Household Controls	Yes	Yes	Yes
Village Controls	Yes	Yes	Yes
Number of observations	143,346	49,753	24,132

Notes: MB-IPW stands for Minimum Biased Inverse Probability Weighted estimator due to Millimet and Tchernis (2013) and is estimated using a 0.25 radius. The Average Treatment Effect on the Treated are reported. The 90% confidence intervals in brackets are calculated by bootstrapping 250 replications. All models include controls: age of household head, age squared, land owned, land squared, electricity use in the village, and distances to the bank, market, and secondary school. Column (1) uses the full sample of households, column (2) focuses on 20% of the sample around 0.1 acre land owned, and column (3) focuses on the 10% sample.

Table 4: Local 2SLS Results

	(1)	(2)	(3)	(4)	(5)	(6)
Has 3 full meals per day during monga (yes = 1)	0.349***	0.351***	0.538***	0.568***	0.287***	0.265**
	(0.12)	(0.12)	(0.19)	(0.21)	(0.10)	(0.11)
Has 1 full meal per day during monga (yes = 1)	-0.799***	-0.782**	-1.517***	-1.566***	-0.564**	-0.471*
	(0.30)	(0.32)	(0.53)	(0.59)	(0.27)	(0.28)
Does not sell advanced labor (yes = 1)	0.049	0.051	-0.121	-0.125	0.104	0.121
	(0.09)	(0.10)	(0.12)	(0.13)	(0.09)	(0.10)
Migrates in search of work (yes = 1)	-1.704***	-1.604***	-1.927***	-1.826***	-1.631***	-1.515***
	(0.47)	(0.49)	(0.64)	(0.67)	(0.45)	(0.46)
First Stage Results						
Land < 10 (yes=1)	-0.060***	-0.056***			-0.083***	-0.083***
	(0.01)	(0.01)			(0.03)	(0.03)
Land < 10 (yes=1) x (Land - 10)			-0.040***	-0.036***	0.021	0.025
			(0.01)	(0.01)	(0.03)	(0.03)
Angrist-Pischke F Statistic	17.54	15.48	11.60	9.61	9.05	8.18
Prob > F	0.000	0.000	0.001	0.002	0.000	0.000
Control Variables	No	Yes	No	Yes	No	Yes
Observations	24,132	24,132	24,132	24,132	24,132	24,132

Notes: The above table reports results from local 2SLS, estimated from the 10% subsample around the 10 decimal of land threshold. Columns (1) and (2) use as an IV a dummy indicating that the household owns less than 10 decimals of land. Columns (3) and (4) use the interaction of this land dummy and rescaled land (land – 10). Columns (5) and (6) include both IVs in the first stage. Odd numbered columns do not include any additional controls beyond land and land squared. Even numbered columns include for household and village characteristics: age of household head, age squared, electricity use in the village, and distances to the bank, market, and secondary school. Robust standard errors in parenthesis: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5: Local Biprobit Results

	(1)	(2)	(3)	(4)	(5)	(6)
Has 3 full meals per day during monga (yes = 1)	0.015***	0.013***	0.015***	0.013***	0.015***	0.013***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Land < 10 (yes=1)	0.000	-0.001**			-0.000	-0.000
	(0.00)	(0.00)			(0.00)	0.00055
Land < 10 (yes=1) x (Land - 10)			-0.000	-0.000**	-0.000	-0.000
			(-0.06)	(0.00)	(0.00)	(0.00)
Has 1 full meal per day during monga (yes = 1)	-0.210***	-0.193***	-0.228***	-0.216***	-0.228***	-0.215***
	(0.03)	(0.03)	(0.00)	(0.01)	(0.00)	(0.02)
Land < 10 (yes=1)	-0.036***	-0.034***			0.003	-0.003
	(0.01)	(0.01)			(0.02)	(0.02)
Land < 10 (yes=1) x (Land - 10)			-0.052***	-0.031***	-0.044	-0.029*
			(0.01)	(0.01)	(0.02)	(0.02)
Does not sell advanced labor (yes = 1)	0.022	0.022	-0.010	0.009	0.067	0.035
	(0.04)	(0.01)	(0.01)	(0.01)	(0.09)	(0.02)
Land < 10 (yes=1)	-0.058***	-0.054***			-0.093***	-0.089***
	(0.01)	(0.01)			(0.04)	(0.03)
Land < 10 (yes=1) x (Land - 10)			-0.039***	-0.034***	0.033	0.032
			(0.01)	(0.01)	(0.03)	(0.03)
Migrates in search of work (yes=1)	-0.205***	-0.168***	-0.205***	-0.157***	-0.205***	-0.168***
	(0.00)	(0.02)	(0.00)	(0.02)	(0.00)	(0.002)
Land < 10 (yes=1)	-0.059***	-0.047***			-0.067***	-0.059***
	(0.01)	(0.01)			(0.02)	(0.02)
Land < 10 (yes=1) x (Land - 10)			-0.043***	-0.032***	0.007	0.011
			(0.01)	(0.01)	(0.02)	(0.01)
Control Variables	No	Yes	No	Yes	No	Yes
Number of observations	24,132	24,132	24,132	24,132	24,132	24,132

Notes: The above table reports results from local Biprobit, estimated from the 10% subsample around the 10 decimal of land threshold. Columns (1) and (2) use as an IV a dummy indicating that the household owns less than 10 decimals of land. Columns (3) and (4) use the interaction of this land dummy and rescaled land (land – 10). Columns (5) and (6) include both IVs in the first stage. Odd numbered columns do not include any additional controls beyond land and land squared. Even numbered columns include for household and village characteristics: age of household head, age squared, electricity use in the village, and distances to the bank, market, and secondary school. Robust standard errors in parenthesis: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6: Local 2SLS Robustness Checks

	8% subsample			12% subsample		
	(1)	(2)	(3)	(4)	(5)	(6)
Has 3 full meals per day during monga (yes = 1)	0.289*	0.563**	0.120	0.322***	0.468*	0.262**
	(0.15)	(0.26)	(0.11)	(0.12)	(0.25)	(0.10)
Has 1 full meal per day during monga (yes = 1)	-0.875*	-1.833**	-0.280	-0.605**	-0.939	-0.468*
	(0.45)	(0.81)	(0.35)	(0.31)	(0.58)	(0.27)
Does not sell advanced labor (yes = 1)	0.282*	0.396*	0.211	-0.075	-0.477*	0.088
	(0.17)	(0.22)	(0.15)	(0.11)	(0.27)	(0.10)
Migrates in search of work (yes = 1)	-1.956***	-2.984**	-1.318**	-1.420***	-1.355*	-1.447***
	(0.75)	(1.24)	(0.55)	(0.46)	(0.73)	(0.43)
First Stage Results						
Land < 10 (yes=1)	-0.046***		-0.079*	-0.050***		-0.083***
	(0.02)		(0.05)	(0.01)		(0.02)
Land < 10 (yes=1) x (Land - 10)		-0.035***	0.031		-0.019**	0.024
		(0.01)	(0.04)		(0.01)	(0.01)
Angrist-Pischke F Statistic	8.72	6.48	4.63	14.37	5.43	8.44
Prob > F	0.0032	0.0109	0.0098	0.0002	0.0198	0.002
Observations	19,990	19,990	19,990	25,004	25,004	25,004

Notes: The above table reports results from local 2SLS estimated using alternative subsamples. Columns (1)-(3) are estimated from the 8% subsample around the 10 decimal of land threshold. Columns (4)-(6) are estimated from the 12% subsample around the 10 decimal land threshold. Each model controls for household and village characteristics: age of household head, age squared, land owned, land squared, electricity use in the village, and distances to the bank, market, and secondary school. Robust standard errors in parenthesis: * significant at 10%; ** significant at 5%; *** significant at 1%.

ONLINE APPENDIX: NOT FOR PUBLICATION

Appendix A. Choice of Control Variables ($\boldsymbol{X_i}$)

The choice of the control variables is motivated by the objective of minimizing omitted variables in the regressions. Since omitted variables bias arises only when a variable affects both the selection and outcome equations, we include observable characteristics that belong to the intersection set of the determinants of both these equations. Following Angrist and Pischke (2009), we leave out potentially 'bad controls', i.e. those variables which may themselves be outcome variables.

The household variables we control for include age of the household head, age squared, total owned land, and land squared. Age and its square are included as an indicator of social and human capital (experience and wisdom). The social and human capital might be important for access to informal finance in the absence of microcredit, and thus may affect the opportunity cost of not joining an MFI program. Also, MFIs may target households with relatively young and energetic borrowers, and thus too young or too old age will be less likely to get selected into the program. We include land owned as an indicator of a household's endowment. Land obviously affects the outcome variables, such as the ability to consume three meals per day during Monga. As discussed previously, land also serves as a screening mechanism for MFIs. Following the RD/RKD literature, we include a polynomial of land in the regression.

The village-level control variables include percent of electricity usage in a village, the average agricultural wage for men and women, distance to the nearest bank, distance to the market or business center, distance to the secondary school. All of these variables are indicators of the level of development of a village. Percent of electricity usage is a good measure of wealth that is not determined by microfinance. The average male and female wages in agriculture reflect unobserved land productivity, and are important as indicators of labor market opportunities in a village. The distance to bank captures access to formal credit market, and distance to business center captures access to urban market. A better access to urban market increases the profitability of non-farm activities financed by microcredit. Since the data lack information concerning the education level of the household head, we include distance to secondary school as an indicator of the availability of educational opportunities in the village.

Table A1. Summary Statistics

	(1) Full Sample	(2) 20% Sample	(3) 10% Sample
Has 3 full meals per day during Monga (yes = 1)	0.028	0.029	0.030
	(0.166)	(0.167)	(0.169)
Has 1 full meal per day during Monga (yes = 1)	0.487	0.467	0.478
	(0.500)	(0.499)	(0.500)
Does not sell labor in advance (yes = 1)	0.950	0.957	0.956
	(0.218)	(0.203)	(0.206)
Migrates in search of work (yes = 1)	0.493	0.501	0.508
	(0.500)	(0.500)	(0.500)
Age of the household head (years)	39.041	39.635	40.047
	(11.945)	(11.702)	(11.794)
Total owned land (decimals)	8.356	9.886	10.511
	(12.245)	(5.437)	(2.717)
Electricity use in the village (percent)	31.894	31.205	29.691
	(25.782)	(25.537)	(25.165)
Distance to bank (km)	4.315	4.157	4.237
	(3.517)	(3.224)	(3.271)
Distance to market (km)	3.462	3.505	3.641
	(2.878)	(2.845)	(2.931)
Distance to secondary school (km)	1.482	1.431	1.485
	(2.087)	(2.021)	(2.060)
Number of Observations	143,346	49,753	24,132

Notes: Mean values for the main variables used in the analysis, with standard deviations in parenthesis. Column (1) reports the summary statistics for the main sample, column (2) uses the 20% sample of households around 0.1 acre of land ownership, and column (3) focuses on 10% of the households around 0.1 acre of land ownership.

Appendix B. Further Robustness Checks

Table B1: Probit Results

A: Full Sample

	(1)	(2)	(3)
Has 3 full meals per day during Monga (yes = 1)	0.011***	0.010***	0.008***
	(11.84)	(10.77)	(9.98)
Has 1 full meal per day during Monga (yes = 1)	-0.043***	-0.032***	-0.027***
	(-15.88)	(-11.48)	(-9.83)
Does not sell labor in advance (yes = 1)	0.016***	0.015***	0.015***
	(14.22)	(13.16)	(13.03)
Migrates in search of work (yes = 1)	0.004	-0.004	0.008***
	(1.37)	(-1.49)	(3.02)
Number of observations	143,346	143,346	143,346

B: 20% Sample

Has 3 full meals per day during Monga (yes = 1)	0.012***	0.012***	0.009***
	(7.72)	(7.45)	(6.70)
Has 1 full meal per day during Monga (yes = 1)	-0.047***	-0.043***	-0.037***
	(-10.31)	(-9.42)	(-8.06)
Does not sell labor in advance (yes = 1)	0.009***	0.009***	0.009***
	(5.22)	(5.01)	(4.83)
Migrates in search of work (yes = 1)	-0.007	-0.012***	-0.001
	(-1.54)	(-2.73)	(-0.27)
Number of observations	49,753	49,753	49,753

C: 10% Sample

Has 3 full meals per day during monga (yes = 1)	0.015***	0.014***	0.012***
	(6.34)	(6.24)	(5.60)
Has 1 full meal per day during monga (yes = 1)	-0.056***	-0.053***	-0.045***
	(-8.66)	(-8.14)	(-6.79)
Does not sell labor in advance (yes = 1)	0.008***	0.008***	0.008***
	(2.95)	(2.86)	(2.89)
Migrates in search of work (yes = 1)	-0.013**	-0.018***	-0.006
	(-1.97)	(-2.66)	(-0.84)
Number of observations	24,132	24,132	24,132

Notes: See Table 2.

Table B.2: Local 2SLS Results
(Linear control for land ownership)

	(1)	(2)	(3)
Has 3 full meals per day during monga	0.352***	0.470*	0.349***
(yes = 1)	(0.12)	(0.28)	(0.12)
Has 1 full meal per day during monga	-0.689**	-0.506	-0.693**
(yes = 1)	(0.30)	(0.54)	(0.30)
Does not sell advanced labor (yes = 1)	0.024	-0.237	0.031
	(0.10)	(0.23)	(0.10)
Migrates in search of work (yes = 1)	-1.504***	-0.993	-1.517***
	(0.45)	(0.69)	(0.45)
First Stage Results			
Land < 10 (yes=1)	-0.056***		-0.058***
	(0.01)		(0.02)
Land < 10 (yes=1) x		-0.008**	0.001
(Land - 10)		(0.00)	(0.00)
Angrist-Pischke F Statistic	16.62	4.23	8.32
Prob > F	0.0000	0.0397	0.0002
Control Variables	No	Yes	No
Observations	24,132	24,132	24,132

Notes: The above table reports results from local 2SLS, estimated from the 10% subsample around the 10 decimal of land threshold. Columns (1) and (2) use as an IV a dummy indicating that the household owns less than 10 decimals of land. Columns (3) and (4) use the interaction of this land dummy and rescaled land (land – 10). Columns (5) and (6) include both IVs in the first stage. Each model controls for household and village characteristics: age of household head, age squared, land owned, electricity use in the village, and distances to the bank, market, and secondary school. Robust standard errors in parenthesis: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table B.3: Local Biprobit Results
(Linear control for land ownership)

	(1)	(2)	(3)
Has 3 full meals per day during monga	0.013***	0.013***	0.013***
(yes = 1)	(0.00)	(0.00)	(0.00)
Land < 10 (yes=1)	-0.001**		-0.001**
	(0.00)		(0.00)
Land < 10 (yes=1) x (Land - 10)		-0.000*	0.000
		(0.00)	(0.00)
Has 1 full meal per day during monga	-0.187***	-0.174***	-0.188***
(yes = 1)	(0.03)	(0.06)	(0.03)
Land < 10 (yes=1)	-0.033***		-0.034***
	(0.01)		(0.01)
Land < 10 (yes=1) x (Land - 10)		-0.005**	0.001
		(0.00)	(0.00)
Does not sell advanced labor (yes = 1)	0.019	0.011	0.020
	(0.01)	(0.01)	(0.01)
Land < 10 (yes=1)	-0.054***		-0.056***
	(0.01)		(0.02)
Land < 10 (yes=1) x (Land - 10)		-0.008*	0.001
		(0.00)	(0.00)
Migrates in search of work (yes=1)	-0.166***	-0.029	-0.168***
	(0.02)	(0.07)	(0.02)
Land < 10 (yes=1)	-0.046***		-0.050***
	(0.01)		(0.01)
Land < 10 (yes=1) x (Land - 10)		-0.005*	0.002
		(0.00)	(0.00)
Control Variables	Yes	Yes	Yes
Number of observations	24,132	24,132	24,132

Notes: The above table reports results from local Biprobit, estimated from the 10% subsample around the 10 decimal of land threshold. Each model controls for household and village characteristics: age of household head, age squared, land owned, electricity use in the village, and distances to the bank, market, and secondary school. Robust standard errors in parenthesis: * significant at 10%; ** significant at 5%; *** significant at 1%.