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The impact of group lending in Northeast Thailand

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Abstract

Much of the literature on group lending focuses on its high repayment rates rather than its goal of promoting borrower welfare. Most studies that attempt to measure the impact of group lending neglect the issues of self-selection and endogenous program placement, thus leading to biased estimates of impact. One reason for this neglect is the lack of data that would allow for identification of impact. This paper surmounts these problems by using data from a quasi-experiment conducted in Northeast Thailand in 1995–1996. Program participants were identified in six control villages 1 year prior to receiving loans. Surveys were then conducted of these ''control'' members, ''treatment'' members in eight older program villages, and nonmembers in both types of village. This survey design allows for straightforward estimation of impact. The results indicate that program loans are having little impact although ''naive'' estimates of impact that fail to account for self-selection and endogenous program placement significantly overestimate impact.

JEL classification: O15; O16; O17; O19
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1. Introduction

One cause of poverty observed in less industrialized countries (LIC) may be the poor's lack of access to productive capital. The poor often find themselves in a vicious circle: producing at a subsistence level makes it difficult to accumulate

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savings or other assets, thus making it difficult either to invest in productive resources or to gain access to credit in formal capital markets, which leads to low productivity and continued poverty.

In most LICs, local moneylenders are the principal source of credit to peasant households. One advantage of the village moneylender is that he knows the reputations of his clients and can monitor their activities much more easily and cheaply than potential competitors. However, moneylenders often charge annual interest rates of more than 100%. Given that world interest rates in formal credit markets are in the range of 10 to 20%, many potentially profitable projects are not undertaken in rural areas of LICs. This inefficiency may have a greater impact on poor women (and the children under their care) than on men because women generally have even less access than men to formal credit markets. Hence, delivering sustainable, low-cost credit to the poor, especially to poor women, should lead to increases in efficiency and equity by increasing their income and expenditures on children.

Commercial banks generally do not cater to the needs of the rural poor. The projects that most peasant borrowers would undertake are small scale, requiring small loans; therefore, the costs of obtaining the information necessary to select borrowers, evaluate their creditworthiness, monitor the use of the loans, and enforce repayment outweigh the potential profits to most lending institutions. Hence, previous government-led efforts to deliver formal credit to rural areas have included setting up special agricultural banks or ordering commercial banks to loan a certain minimum percentage of their loan portfolio to rural borrowers. However, such efforts have generally failed because it is often politically difficult for governments to enforce repayment of loans, and because the below-market interest rates that have been charged induce non-price rationing of loans (e.g., loaning to those who can put up the most physical collateral), allowing the rural elites rather than the poor to receive the lion's share of the loans (Adams and Vogel, 1986; World Bank, 1989).

The failure of formal lending institutions and the apparent success of Bangladesh's Grameen Bank in reaching the rural poor have recently inspired numerous non-governmental organizations (NGOs) and LIC governments to establish group-lending schemes to deliver credit at low cost and reasonable interest rates to small-scale rural entrepreneurs. NGOs usually target women [1] in their

[1] In most village banks, including those surveyed in this research project, membership is open only to women. There are at least three reasons for this preference. First, because men more frequently have access to low-interest, formal credit, NGOs believe they can have a greater impact by loaning to women. Second, many NGOs consider women to be better credit risks than men. Third, women are perceived as spending their income more responsibly than men; e.g., women are thought to purchase food, medicine, and school supplies for the family whereas men are thought to have more of a tendency to spend money on themselves.

''village bank'' programs, which generally operate on a much smaller scale than government-led programs. In these programs, borrowers form their own peer groups of 20 to 60 members (sometimes further broken down into five-person ''solidarity groups''). The NGO lender grants a loan to each member, but group members co-guarantee each other's loans. Loans are generally due in about 6 months. If the group does not meet its collective responsibility to repay all of its members' loans, then all group members are denied future credit. The first loan is the same amount for all members (1500 baht in Thailand [2]). For each subsequent loan cycle, the member is entitled to borrow an amount equal to her previous loan plus her accumulated savings in the village bank, up to a fixed maximum (in Thailand, the maximum is 7500 baht). Moreover, the group also makes loans to its members (and sometimes to nonmembers) from its members' savings. Loans from the NGO lender are ''external account'' loans, and loans from members' savings are ''internal account'' loans. [3]

Hence, given the tightly knit communities that exist in many villages in LICs, members are well placed to judge the creditworthiness and to observe the actions of their peers, thus mitigating the problems of adverse selection and moral hazard (Stiglitz, 1990; Varian, 1990; Ghatak, 1999). Group lending also provides incentives for a member to avoid excessively risky projects (Stiglitz, 1990), to repay her loan in order to avoid the social sanction of her peer group (Besley and Coate, 1995), to seek assistance from other members if her project is performing poorly or provide similar assistance to other members (Varian, 1990), and to provide insurance to other members in the event that their projects fail (Coleman, 1998b). Given the trust that exists between group members who often have known each other all their lives, there may also be a strong inducement to self-monitor so that monitoring costs to other borrowers are close to zero. Hence, group lending provides a mechanism to overcome some of the informational disadvantages of commercial lenders. Indeed, Wydick (1995b) demonstrates that the social cohesion of groups in Guatemala, by mitigating adverse selection and moral hazard and by encouraging mutual insurance, is the primary determinant of group lending's high repayment rates. The repeated game theory, which states that borrowers repay simply to ensure the continuation of access to loans at favorable loan terms, is not supported.

Group lending is often viewed as a success, primarily because of its high repayment rates, usually over 90%, and low-cost delivery system. Indeed, most research to date focuses on reasons for the high repayment rates. The primary goal

[2] In 1995–1996, US$1 = 25 baht.

[3] This description of group lending is based on the ''village banking'' model developed by the Foundation for International Community Assistance (see Foundation for International Community Assistance, 1990 and Hatch, 1989 for a more detailed description of the model). This design, rather than the Grameen Bank design, is the most popular among international NGOs, having been adopted by Catholic Relief Services, CARE, Save the Children, and Freedom From Hunger among others.

of village banks, however, is to improve borrower welfare. It is usually assumed, based on the high repayment rates as well as numerous anecdotes of how individual members pulled themselves out of poverty, that village banks accomplish this goal (Remenyi, 1991). However, the hypothesis that village banks accomplish this goal has not been adequately tested. Indeed, leading members of the ''Ohio School'' of development finance have recently stated bluntly, in response to the mushrooming of group lending, that ''debt is not an effective tool for helping most poor people enhance their economic condition — be they operators of small farms or micro entrepreneurs, or poor women'' (Adams and von Pischke, 1992). They argue that access to credit is not a significant problem faced by small agricultural households and that factor and product prices, land tenure, technology, and risk are the factors limiting small farmer development. Thus, given that village banks aim to alleviate rural poverty, their success needs to be judged by other standards in addition to the lenders' costs, profits, and loan recovery rates. Moreover, village banks will not be sustainable unless the benefits to their members are sustainable. [4] The main purpose of this paper will be to evaluate the village banks against the primary goals for which they have been implemented.

The main problem plaguing attempts to evaluate the success of village banks in promoting borrower welfare is that village bank members self-select and are selected by their fellow members: a potential member must decide that she wants to participate in the program, and she must be accepted as a member by other villagers who have self-selected. Hence, it is likely that there are significant differences between village bank members and nonmembers. To the extent that such differences can be observed and measured (e.g., age, education), they can be controlled for when estimating village bank impact. To the extent that such differences cannot be observed (e.g., entrepreneurship, risk preferences, trustworthiness, attitudes regarding the role of women in the household, attitudes toward belonging to a poverty lending group), direct comparison of village bank members

[4] High observed repayment rates are not sufficient to guarantee program sustainability over the long run. In the program studied here, NGO lenders provide 6-month external account loans to village bank members over a period of 5 years, after which the group's internal account is expected to meet its credit needs. If the social sanctions of a group are strong enough, members may put a high priority on repaying village bank loans during this period of NGO intervention, even if they are made worse off ex post by borrowing. For example, they may borrow from a moneylender in order to repay their village bank loan, then use their next village bank loan to repay the moneylender, and get trapped in a vicious circle of debt. If this type of phenomenon is widespread and information is shared, then other villages may decide not to form a village bank at all, and the NGO will find itself with no clients. This phenomenon is not merely hypothetical. In a 4-year-old village bank sampled in the present study, approximately 20 of its 30 members regularly borrowed from a moneylender to repay their village bank loans, but the village bank had a 100% repayment rate to the NGO. Other village banks sampled also had members trapped in this vicious circle.

and nonmembers will yield biased estimates of village bank impact. This bias results because the same unobservable characteristics that lead some women to become village bank members will also affect impact measures such as income, accumulation of assets, and spending on education and health care. [5]

Most existing impact studies are nonacademic project evaluations that are of a descriptive nature or suffer from the selection bias problem. Chen (1992) reviews 11 studies of the Grameen Bank in Bangladesh, none of which make any correction for selection bias. Sebstad and Chen (1996) review 32 research and evaluation reports on the impact of micro-credit; none of these 32 studies account for section bias, with the exception of the study by Hulme and Mosley (discussed separately below). Hossain (1988) also presents descriptive and anecdotal evidence of the positive impact of the Grameen Bank. MkNelly and Watetip (1993) evaluate the impact of village banks in Thailand, but use as their control group women from villages that do not have a village bank and who, therefore, have not had the opportunity to self-select.

Among academic studies, Wydick (1995a,c) evaluates the impact of group lending in Guatemala on child labor and class mobility, but like MkNelly and Watetip (1993), he uses as his control group entrepreneurs who have never been given the opportunity to self-select, though he does attempt to match his control and treatment groups on observables.

Two exceptions to this lack of attention to selection bias exist. The study by Hulme and Mosley (1996) of micro-lending institutions in several countries included eight institutions that practice group lending, and in two of these, the authors identified a control group that had been accepted for a loan, but who had not yet received a loan. However, the authors present only the means of various outcome variables for both treatment and control groups. No statistical analyses of the differences are conducted. Moreover, their data would not allow them to control for the possibility of endogenous program placement, discussed in greater detail below.

The most thorough attempt to correct for selection bias and nonrandom program placement in group lending is the study by Pitt and Khandker (1998), who use data from a World Bank survey of the Grameen Bank and two other group lending programs in Bangladesh. Specifically, they use a quasi-experimental design in which they sample members and nonmembers from villages with a program, as well as randomly selected households from villages without a program. In this design, availability of a credit program is used as an identifying variable. However, they recognize that there will likely be systematic differences

[5] For a discussion of program evaluation, see Moffitt (1991), Grossman (1994) and Newman et al. (1994); for an informative debate on the advantages and disadvantages of conducting randomized field trials in economic research and evaluations, see Burtless (1995) and Heckman and Smith (1995).

between the two types of villages because program placement may be endogenous. Therefore, they use village fixed effects estimation to control for unobserved differences between villages. But, because the village-specific dummy variables that identify fixed effects would be collinear with the ''program availability'' variable, they also sample households in program villages which are, in principle, *exogenously excluded* from the programs — in the programs studied, households with more than a fixed amount of assets are theoretically excluded from membership consideration. [6]

Most group lending programs, however, do not impose such eligibility criteria. Rather, they attempt to attract the relatively poor and dissuade the relatively rich from participating by the small size of loans, the high frequency of meetings, and the stigma of belonging to a poor person's credit program. Hence, the method of Pitt and Khandker could not be implemented in most group lending programs. Moreover, even in the context of the three Bangladeshi programs they studied, their survey found that some 18–34% of program participants in fact had wealth that should have excluded them from participating. Hence, the use of this eligibility criterion as a key exclusion restriction may not be appropriate. [7]

The methods used in this paper do not require the existence and enforcement of exogenously imposed membership criteria to identify program impact. Instead, they rely on data collected using a unique survey that allows for the use of relatively straightforward estimation techniques. Member and nonmember households in 14 villages in Northeast Thailand were surveyed four times over the course of a year. At the time of the first survey, seven of the villages had a village bank for 2 to 4 years, and one village began its village bank immediately after the first survey. Six ''control'' villages were identified to receive NGO support for a village bank, such support to begin 1 year after they were identified. In these control villages, villagers were allowed to self-select to be village bank members or nonmembers. Hence, the ''old'' village bank members in the eight ''treatment'' villages can be compared with the ''new'' village bank members in the six control villages. Moreover, differences in the length of time that the program has been available to members in the treatment villages is taken into account to obtain more precise impact estimates. Inclusion of nonmembers in all villages allows for the

[6] In principle, to join the Grameen Bank, the value of a household's total assets cannot exceed the value of 1 acre of land; to join the other two programs they studied [Bangladesh Rural Advancement Committee (BRAC) and the Bangladesh Rural Development Board (BRDB)], a household cannot own more than 0.5 acre of land.

[7] Morduch (1998) discusses these issues in greater depth. Also, as he points out, Pitt and Khandker label as ''eligible'' any households in program villages that participate in the credit programs, including households that in principle should have been excluded. In non-program villages, however, the eligibility criteria were strictly followed in labeling households as eligible or not eligible. Hence, the ''treatment'' group does not conform to the ''control'' group, and the result is an overestimation of program impact.

use of village fixed effects estimation to control for the possibility that the order in which these 14 villages received program support is endogenous.

The potential impact of any credit program will depend largely on the context in which it is implemented. The main premise of group lending is that the rural poor in developing countries are credit constrained, having limited access to formal sector credit. In Thailand, however, the semi-statal Bank for Agriculture and Agricultural Cooperatives (BAAC) practices both individual and group lending and counts 84.5% of rural households as its clients. Hence, it is arguable that rural households in Thailand are considerably less credit constrained than their counterparts in other developing countries. The BAAC's outreach in the impoverished Northeast, however, is smaller than the rest of the country. In the 14 villages surveyed for this study, 63% of village households were BAAC members. Moreover, as is often the case in government-led credit programs, the BAAC's clientele is largely male. Although Thai women have traditionally been active participants in the market place and have enjoyed a certain amount of economic autonomy and power, only 29.5% of surveyed BAAC members were women. Hence, only 29.5% of 63%, or 18.6%, of surveyed households included women who had direct access to BAAC credit. However, 25.8% of surveyed households included women who were in debt to moneylenders. Hence, there is indirect evidence that women are credit constrained and may benefit from greater access to institutional credit. On the other hand, the village bank program studied here makes small loans, with a loan ceiling of 7500 baht (US$300). Average size of a BAAC loan in the survey region is 15,134 baht, with some loans as high as 100,000 baht. Average household wealth in the sample is 529,586 baht, which dwarfs village bank loan size.

The results from this research design indicate that village bank loans are having little impact on most household outcomes in Northeast Thailand, although ''naive'' estimates of impact that fail to account for selection bias and endogenous program placement significantly overestimate impact.

The remainder of the paper is organized as follows. Section 2 presents the empirical model and estimation strategy; Section 3 discusses the survey design in greater detail; Section 4 presents a brief description of the survey area; Section 5 presents the empirical results; and Section 6 summarizes the results and draws policy implications.

2. Empirical model and estimation strategy

This section consists of two parts. Part 1 presents the standard empirical specification usually encountered when measuring program impact, and briefly discusses the bias that can arise from self-selection and endogenous program placement. Part 2 presents the alternative specification used in this paper, which is permitted by the unique data set used.

2.1. The selection bias problem

To appreciate the bias potentially arising from self-selection and endogenous program placement, consider the following empirical specification:

$$B_{ij} = \boldsymbol{X}_{ij}\alpha_B + \boldsymbol{V}_j\beta_B + \varepsilon_{ij}, \tag{1}$$

$$Y_{ij} = \boldsymbol{X}_{ij}\alpha_Y + \boldsymbol{V}_j\beta_Y + B_{ij}\delta_Y + \mu_{ij}, \tag{2}$$

where B_{ij} is the amount borrowed from the village bank by household i in village j; $\boldsymbol{X}_{ij}$ is a vector of household characteristics; $\boldsymbol{V}_j$ is a vector of village characteristics; Y_{ij} is an outcome on which we want to measure impact; α_B, β_B, α_Y, β_Y, and δ_Y are parameters to be estimated; and ε_{ij} and μ_{ij} are errors representing unmeasured household and village characteristics that determine borrowing and outcomes, respectively. δ_Y is the primary parameter of interest as it measures the impact of village bank credit on the outcomes Y_{ij}.

Econometric estimation of this equation system will yield biased parameter estimates if ε_{ij} and μ_{ij} are correlated and this correlation is not taken into account. Correlation between ε_{ij} and μ_{ij} can arise from two different sources: (1) self-selection into the village bank (and subsequent decision on how much to borrow) and (2) nonrandom program placement.

To illustrate the first source of correlation, consider a sample of households drawn only from villages with a village bank: some households will have selected to be village bank members (and they will have further decided, within certain limits imposed by the bank, how much to borrow), [8] and others will have selected not to be members. In this sample, ε_{ij} and μ_{ij} will almost certainly be correlated. For instance, if the more entrepreneurial households join the village bank, then unmeasured ''entrepreneurship'' would influence both the decision to become a member (and, for members, how much to borrow) and impact measures such as income and assets. Or if households with attitudes favorable to gender equality joined the village bank more than gender-unequal households (because membership is open only to women), then unmeasured gender attitudes would influence both the decision to become a member and impact measures such as women's income and assets, and possibly spending on children's (especially girls') health care and education. Alternatively, the relatively poor may join the village bank more than the rich who might feel stigmatized in a group for poor people; in this

[8] It will be recalled that, after the first loan of 1500 baht, each subsequent loan ceiling is equal to the previous loan plus the member's village bank savings. Because savings are endogenous, the loan ceiling is endogenous, up to a maximum of 7500 baht. Moreover, a member may borrow less than her ceiling. So loan size largely is selected by the member.

case, ε_{ij} and μ_{ij} would be negatively correlated, and estimation of village bank impact would be biased downward.

To illustrate the second source of correlation, consider another commonly used sample, which includes households of village bank members from some villages and randomly selected households from villages without a village bank (e.g., MkNelly and Watetip, 1993; Wydick, 1995a,c). Now it is possible for ε_{ij} and μ_{ij} to be correlated across villages if village bank placement is not random. For example, if some villages are viewed as more entrepreneurial or better organized, have more dynamic leaders and such leadership spills over to affect others' behavior in the village, or are simply poorer (e.g., living in flood-prone or drought-prone areas), and if NGOs use such criteria to determine village bank placement, then ε_{ij} and μ_{ij} will be correlated.

There are three standard procedures used in a case where ε_{ij} and μ_{ij} are correlated (Moffitt, 1991). The first is to use instrumental variables. The identifying instruments would be variables that are included as regressors in Eq. (1) but not in Eq. (2); i.e., they would have to be determinants of joining the village bank and borrowing amount B_{ij}, but not be determinants of the impact measures Y_{ij}. It is usually, however, difficult to justify the use of any variables as determinants of B_{ij}, but not Y_{ij}.

The second method is to use panel data, so that differences in pretreatment outcome variables can be taken into account. The main problem with panel data is the practical difficulty and expense of collecting such a panel. Certainly, to date, no such data exists.

The third method is to assume an error distribution (almost always a normal distribution) of the outcome variable without treatment. The effect of treatment is then determined by measuring the deviations from normality of the outcome within the treatment group. Problems inherent in this method are: (1) we usually have no good basis on which to make an assumption about the error distribution, (2) the results are highly sensitive to the assumptions about the error distribution (Levy, 1996), and (3) in the case of censored dependent variables, identification of the treatment effect is sometimes still impossible (Maddala, 1983; Moffitt, 1991).

2.2. An alternative specification

Given this difficulty in identifying program impact econometrically, I conducted a unique survey which permits the estimation of an alternative, simpler specification that allows for the use of relatively straightforward econometric techniques to measure program impact.

A useful characteristic of most village bank programs is that they start small, often in only a half dozen villages to begin, and then gradually expand their operation into other villages. This characteristic can be exploited when measuring impact. In particular, a quasi-experiment was conducted in 1995–1996 in which

the NGOs preidentified six new villages that would soon receive program support and allowed households in these villages to self-select into the (as yet not functioning) village banks. At the same time, a random sample of eight existing program villages was selected. The ''control'' village bank households would presumably, on average, share the same unobservable characteristics (e.g., entrepreneurship, gender attitudes, etc.) as the ''treatment'' group of village bank members who had already benefited from loans. [9] In both control and treatment villages, both members and nonmembers were surveyed. With this survey design in mind, Eqs. (1) and (2) can be replaced by a single impact equation as follows:

$$Y_{ij} = \boldsymbol{X}_{ij}\alpha + \boldsymbol{V}_j\beta + M_{ij}\gamma + T_{ij}\delta + \upsilon_{ij}, \tag{3}$$

where Y_{ij}, $\boldsymbol{V}_j$, and $\boldsymbol{X}_{ij}$ are defined as before; M_{ij} is a membership dummy variable equal to 1 if household ij self-selects into the credit program, and 0 otherwise; and T_{ij} is a dummy variable equal to 1 if a self-selected member has already had access to program loans, and 0 otherwise. The membership dummy variable M_{ij} can be thought of as a proxy for the unobservable characteristics that lead households to self-select into the village bank — i.e., it captures the unobserved variables that caused ε_{ij} and μ_{ij} to be correlated across households. The variable T_{ij} measures availability of the program to members who have self-selected, which, unlike the amount borrowed, is exogenous to the household

[9] Although it is of course impossible to directly compare the two groups on unobservables, we can compare them on observables to see if they are similar. However, it is *not* appropriate to simply compare members in treatment and control villages to determine if they are similar on observables. In determining if the selection process is similar in the two types of village, one must also consider the characteristics of nonmembers. What one should consider is the following:

$(X_{\mathrm{MT}} - X_{\mathrm{NT}}) - (X_{\mathrm{MC}} - X_{\mathrm{NC}})$,

where X represents an exogenous variable, and the subscripts MT, NT, MC, and NC indicate member in treatment village, nonmember in treatment village, member in control village, and nonmember in control village, respectively. If this difference of differences is insignificantly different from zero, then we can conclude that the selection process (on X) is similar in both types of villages, even if X_{MT} and X_{MC} are different. An analysis of variance (ANOVA) can test this, as can the following equivalent regression:

$\boldsymbol{X}_{ij} = \mathrm{VILTYPE}_j\beta + M_{ij}\gamma + T_{ij}\delta + \eta_{ij}$,

where $\mathrm{VILTYPE}_j$ indicates if a village is a treatment or control village, M_{ij} is a member dummy variable, and T_{ij} is a treatment dummy variable. Hence, β captures differences in $\boldsymbol{X}_{ij}$ due to the type of village, γ captures differences in $\boldsymbol{X}_{ij}$ due to type of household, and δ captures any differences between treatment and control members *above and beyond* that captured by γ. In tests (not shown, but available upon request from the author), there is no significant difference between treatment and control members in 17 of the 21 exogenous household variables used beyond that already captured by $\mathrm{VILTYPE}_j$ and M_{ij}. The only exceptions are on land owned by men 5 years before our survey (marginally significant, with a coefficient of −63,585 and a p-value of 0.073), number of females age 5–9 (coef = 0.088, p-value = 0.050), number of males age 10–15 (coef = 0.106, p-value = 0.069), and number of males age 22–29 (coef = −0.080, p-value = 0.097).

(but which may not be exogenous with respect to the village, as discussed below). In this specification, δ measures the average impact of the program on Y_{ij}.

With this specification, the correlation between T_{ij} and μ_{ij} due to self-selection at the household level is eliminated because unobservable household characteristics are captured by M_{ij}. Moreover, if the order in which villages receive program support is random with respect to unobserved village characteristics, then efficient and unbiased estimates can be obtained with $\boldsymbol{V}_j$ as a vector of specific village characteristics affecting Y_{ij}. If, however, the order is not random with respect to unobservable village characteristics, then using specific village characteristics as regressors will lead to a biased estimate of impact. This is a slight variation on the second source of bias discussed above: because the control villages are also program villages, albeit program villages that have not yet started making loans, we no longer have to be concerned with nonrandom program placement. However, the order in which villages receive village banks may not be random. If, for example, the most ''dynamic'' villages receive a village bank before less dynamic villages, then T_{ij} and μ_{ij} will be positively correlated and δ will be biased. One method of eliminating this bias is through village fixed effects estimation. If the order of village bank placement is random, however, then village fixed effects estimation is still unbiased, but less efficient than using specific village characteristics as regressors. Therefore, Hausman specification tests can be conducted to determine the appropriate model to estimate.

The empirical model in Eq. (3) can be improved upon by recognizing that some treatment members have received program support longer than others. In the eight treatment villages, the age of the village banks varied from 0 to 4 years. Because the cumulative amount that a member can borrow grows over the life of the village bank, one would expect to see greater impact in villages with older village banks. Hence, the empirical model estimated in this paper is the following:

$$Y_{ij} = \boldsymbol{X}_{ij}\alpha + \boldsymbol{V}_j\beta + M_{ij}\gamma + \mathrm{VBMOS}_{ij}\delta + \mu_{ij}, \tag{4}$$

where the treatment dummy variable T_{ij} is replaced by VBMOS_{ij}, the number of months, for participants, that the village bank has been operating in the village. VBMOS_{ij} is thus zero for members in control villages and for nonmembers in control and treatment villages. VBMOS_{ij} is thus a more precise measure of program availability than T_{ij}, and δ now measures the impact *per month* of program availability. Similar to the case of specification (3), if the order of program placement is random with respect to unobservable village characteristics, then efficient and unbiased estimates can be obtained with $\boldsymbol{V}_j$ as a vector of specific village characteristics. If, however, program placement is not random with respect to unobservable village characteristics, then the correlation between VBMOS_{ij} and μ_{ij} can be eliminated with village fixed effects. Hausman tests again determine the appropriate specification.

The advantages of the specification in Eq. (4) compared to the specification in Eqs. (1) and (2) are twofold: (1) it arguably better captures the impact measure in

which project implementers are interested — the impact on households of making the program available to them (for an additional month), rather than the impact per baht borrowed; and (2) it is considerably easier to estimate — if Y_{ij} is uncensored, then OLS is appropriate; if Y_{ij} is censored, then tobit estimation is appropriate. [10]

Implicit in this approach is the assumption that there are no spillover effects to nonmembers in treatment villages. To the extent that such spillover effects exist, they are captured by the village fixed effects rather than by program effects. In other words, the coefficient on VBMOS_{ij} in Eq. (4) is in effect the following, conditional on the other regressors:

$$\delta = (Y_{\text{M}t+1} - Y_{\text{N}t+1}) - (Y_{\text{M}t} - Y_{\text{N}t}),$$

where $Y_{\text{M}t+1}$, $Y_{\text{M}t}$, $Y_{\text{N}t+1}$, and $Y_{\text{N}t}$ are the average Y for members in a village that has had a village bank for $t+1$ months, average Y for nonmembers in a village that has had a village bank for $t+1$ months, average Y for members in a village that has had a village bank for t months, and average Y for nonmembers in a village that has had a village bank for t months, respectively. To the extent that $Y_{\text{N}t+1}$ has been influenced by the village bank, δ will be biased. Unfortunately, the only way to correct for this bias requires the use of panel data. [11]

3. Survey design

A unique survey was conducted of 445 households in 14 villages in Northeast Thailand in 1995–1996. Eight of the villages are located in the province of Surin and are supported by the Rural Friends Association (RFA), and six are located in the adjacent province of Roi-Et and are supported by the Foundation for Integrated Agricultural Management (FIAM). RFA and FIAM are both Thai NGOs which have promoted village banks since 1988 and which receive financial and technical assistance from the American NGO Catholic Relief Services (CRS). The operation of both NGOs' village banks is virtually identical: both follow the FINCA village banking methodology, with 6-month loan cycles and identical beginning loans of 1500 baht and loan ceilings of 7500 baht.

[10] In principle, with VBMOS_{ij} as a valid exclusion restriction, appearing on the right-hand side of Eq. (1) but not Eq. (2), two-stage least squares could be applied to this two-equation system. However, because B_{ij} is censored and Y_{ij} is sometimes censored, Eqs. (1) and (2) would have to be estimated using a more complex maximum likelihood procedure (e.g., Rivers and Vuong, 1988).

[11] One should also recognize the possibility that the behavior of the members in the control villages could conceivably be affected by their knowledge that they will receive village bank loans in the future. For example, if households are concerned about potentially binding liquidity constraints in the future, then the knowledge that they will soon have access to program loans could change their behavior.

Of the 14 villages surveyed, six had never benefited from village bank support, and did not receive any village bank loans during the survey period. These control villages were identified as follows. RFA and FIAM have been establishing new village banks each year since 1988. This expansion, combined with a solid base of financial support from their own high repayment rate of 100% and from CRS, their primary US donor, enables the NGOs to forecast their expansion plans with a high degree of certainty. In January 1995, therefore, for the purposes of this study, RFA preidentified four villages and FIAM preidentified two villages that they would begin supporting with village bank loans in 1996. In February and March 1995, RFA and FIAM field staff organized the villagers into the new village banks, allowing them to self-select, according to the standard procedures normally used to organize new village banks, the only difference being that the villagers were told that loans would not begin for approximately 1 year. Hence, through this process, a control group of would-be village bank members was identified.

Sampling of treatment villages was conducted as follows. From a list prepared by each NGO of their village banks (32 for RFA and 26 for FIAM), four villages were randomly sampled from each NGO. To ensure, however, that the treatment villages were similar to the control villages, I eliminated from consideration villages within 10 km of the provincial capital, villages with more than 175 households, and village banks established before 1990. From the remaining villages on the lists, I randomly selected seven villages that were at least 2 years old and 1 village that was just due to begin receiving loans because it could also serve as a control village for certain purposes [12] and because I wanted to observe the initial development of a newly created village bank.

A stratified random sample of households was then selected using lists of households in each village which indicated which households had a village bank member. In the treatment villages, 26 village bank members and 19 nonmembers were selected. In the control villages, 25 village bank members and 13 nonmembers were selected. [13] In total, 505 households were selected. Of these, 455 were located and agreed to be interviewed during the first survey, and 445 (88% of those initially selected) finished all four surveys.

[12] This village bank opened in April 1995 and is, depending on the empirical test being conducted, sometimes a treatment village and sometimes a control village. For example, when estimating the impact of village bank loans on various assets, as measured by the first survey, it is a control village since it had not yet benefitted from village bank loans; but when estimating the impact on income or expenditures over the 12-month period of the surveys, it is a treatment village since its first two 6-month loans would have impacted income during this period. For simplicity of prose, however, its is grouped with the treatment villages in the text.

[13] Some variation to this pattern was necessitated by the total number of village households or the number of village bank members in a given village. For example, one treatment village only had 20 village bank members; one treatment village only had 27 households, all of which were village bank members; and in one control village, all households were village bank members.

Each village was surveyed four times over the course of a year. The first survey, conducted in April 1995, collected data on household demographics, assets, and debts. The second, third, and fourth surveys were conducted in August 1995, October 1995, and February–March 1996, respectively. They collected data primarily on income, expenditures, and credit transactions (both borrowing and lending) during the dry season (February to May), the rainy season (June to September), and the harvest season (October to January), respectively. In addition to the household surveys, village surveys were also conducted to collect data on village infrastructure, prices, and other characteristics. The household surveys were administered by the staffs of RFA in Surin and FIAM in Roi-Et, under my supervision. [14] My research assistant and I conducted the village surveys as well as in-depth informal interviews with numerous villagers.

In all 14 villages, if a household had a village bank member, she was the primary interviewee. If a household did not have a village bank member, then at least one adult with knowledge of the household's economic activities was interviewed. In all households, other household members were encouraged to attend the interview.

4. Survey area

The provinces of Surin and Roi-Et are adjacent to each other and are located in Northeast Thailand. While enjoying some of the tremendous growth that Thailand has experienced in the last two decades, Northeast Thailand still lags far behind the rest of the country economically. It is the country's poorest region and is subject to frequent droughts. Most village households engage primarily in small-scale agriculture: 90.4% of the adult men and 91.3% of the adult women in the households surveyed listed farming as their primary or secondary occupation. In Surin, rain-fed rice is the primary (usually the only) crop grown, with planting around June and harvesting in November to January. During the off-season, a few households engage in small-scale irrigated gardening, but most either engage in nonagricultural income-generating activities or remain idle. The more common activities include pig raising, itinerant wage labor (especially construction) in the provincial capital or Bangkok, and small business activities such as petty trading, driving a motorcycle taxi, spinning and weaving silk and cotton, and operating small food stands. Some of the wealthier households buy and sell cattle and water buffalo. Agricultural and nonagricultural activities in Roi-Et are similar to those in Surin, with two differences. First, another important crop cultivated during the main growing season is sticky rice. Second, because of different soil quality,

[14] Questionnaires were checked for internal consistency in the villages immediately upon completion of an interview. Approximately 20% were subsequently checked for content in follow-up interviews.

tobacco is commonly grown as a cash crop during the off season from November to April, leading to less migrant labor compared to Surin.

As in many developing countries, moneylenders are an important source of credit for many villagers. Moneylender interest rates of 60% or 120% per year are the norm. However, many households also have access to low-interest institutional credit, the most frequently used source being the state-run Bank for Agriculture and Agricultural Cooperatives (BAAC), which serves 4 million Thai farm households (84.5% of all farm households in the country) with subsidized low-interest (3–12% per year) loans (BAAC, 1996). In 1996, the average BAAC loan size was 19,355 baht. The present survey sample in Surin and Roi-Et reflects the reduced outreach in the northeast: 63% of households surveyed were BAAC members, and the average loan size during the last 5 years was 15,134 baht. Although the BAAC had a respectably low 9.1% of its loans in arrears at the end of 1996, it has, like so many other state-led efforts, encountered some political difficulties enforcing repayment of its loans. During the 20 months that I was conducting field research in Thailand, northeastern farmers conducted three protest marches to Bangkok to demand, among other things, that some of their BAAC loans be forgiven. Perhaps most important from the perspective of the current research is that the BAAC loans mostly (but not exclusively) to men: only 18.6% of all households surveyed included female BAAC members. On the other hand, 25.8% of households surveyed included women who were in debt to moneylenders. Therefore, although low-cost credit is apparently available to men in most households in the survey area, there is evidence that many women are still denied access to institutional credit. [15]

Many people in Surin, which borders Cambodia, are culturally Khmer and primarily Khmer-speaking: 98% of households surveyed in Surin are ethnically Khmer. The people of Roi-Et, like those in most of Northeast Thailand, are ethnically Lao and the language spoken is a dialect of Laotian: 98% of households surveyed in Roi-Et are ethnically Lao. In both provinces, however, Thai is understood and spoken by all but the oldest villagers.

5. Results

This section discusses estimation results for village bank borrowing and for village bank impact. Results are presented as follows: Appendix A presents weighted [16] means and standard deviations of the variables used to obtain the

[15] For an overview of the Thai rural credit system, see Siamwalla et al. (1990) and Poapongsakorn and Netayarak (1989).

[16] Observations are weighted by the inverse of the probability of being sampled so that means are representative of the population sampled.

results; Table 1 presents tobit estimates of the village bank borrowing equation; Table 2 presents the complete output of a typical regression to measure impact; and Table 3 then presents estimated impact coefficients for the regressor of interest (months of village bank membership) in all the regressions.

5.1. Village bank borrowing

Table 1 presents tobit estimates of the village bank borrowing equation, estimated only on the subsample of households in treatment villages since borrowing by households in control villages is exogenously constrained to be zero. The dependent variable in this estimation is average external account outstanding loan size (in baht). [17] In principle, average loan size should increase as time of program availability increases. However, the coefficient on ''months as a VB member'', though positive, is not significant. One reason for the lack of significance could be that the NGOs reduce loan size during the last 2 years of external account loans in order to ''wean'' the village from external account loans. [18] Another reason for the lack of significance is that many women borrowed less than the maximum allowed, and several women used the village bank primarily as a saving facility rather than a borrowing facility.

One might expect the women who are household heads (and, therefore, presumably in strong bargaining positions within their households) to participate more actively in the village bank than women who are not household heads. However, the coefficient on this variable is not significant (0.42; $p = 0.999$). One explanation is that, in most cases where the household head was a woman, she was either single, widowed, divorced, or abandoned. While some of these women did indeed participate in the village bank and were actively recruited by their peers, others were viewed as uncreditworthy because their household was perceived as unstable, or because they would be unable to turn to their husband or his relatives for help in repaying village bank loans if the need arose.

Women's educational level is highly significant (226.79; $p = 0.006$). If years of education can be considered a proxy for women's human capital, then this result likely represents the complementarity of human capital and physical capital in

[17] Choice of the dependent variable is complicated by the fact that the older village banks started out on a 4-month loan cycle, then converted to a 6-month cycle. Newer village banks have been on a 6-month cycle since their start. Hence, summing loan volume would bias borrowing upward for older banks. An alternative would be to measure borrowing in ''baht-months'' (e.g., 2000 baht borrowed for 6 months is 12,000 baht-months). The results from using baht-months as the dependent variable are qualitatively similar to the results presented in this paper, using average outstanding loan size as the dependent variable.

[18] In this case, including VB months *squared* should capture this nonlinear path of loan size. However, including such a term added no explanatory value to the regression.

Table 1
Tobit estimates of average village bank external account outstanding loan size (in baht)

Independent variable	Dependent variable: village bank external account borrowing	
	Coefficient	S.E.
Months as VB member	10.23	8.35
Sex of household head (female = 1)	0.42	556.23
Education of most highly educated woman (years)	226.79	82.43***
Education of most highly educated man (years)	−76.65	59.06
Number of generations family in village	−283.59	138.00**
Number of blood relatives in village	71.42	20.80***
Is household member village chief or assistant? (0/1)	2587.55	725.90***
Are any females civil servants? (0/1)	−5013.10	1418.37***
Are any males civil servants? (0/1)	2979.91	909.72***
Value female-owned land 5 years ago	0.00038	0.00023*
Value male-owned land 5 years ago	0.000082	0.00053
Number females age 5 to 15	355.74	252.09
Number females age 16 to 21	−333.40	403.91
Number females age 22 to 39	702.27	385.86*
Number females age 40 to 59	791.17	422.17*
Number females age 60 and over	−966.40	473.87**
Number males age 5 to 15	435.44	215.12**
Number males age 16 to 21	−2.43	384.28
Number males age 22 to 39	−145.94	350.52
Number males age 40 to 59	−43.07	446.36
Number males age 60 and over	−382.75	547.01
Constant	−1219.82	801.62
Number of observations = 277		
$\chi^2(21) = 72.33$		
Prob > $\chi^2 = 0.0000$		
Pseudo $R^2 = 0.0230$		
Log likelihood = −1535.71		
117 censored observations		
160 uncensored observations		

* Significant at 0.00 level.
** Significant at 0.05 level.
*** Significant at 0.01 level.

production. It may also represent, at a given level of men's education, increased bargaining power of women in the household.

It is not surprising that the influence of the number of relatives in the village on borrowing is positive and significant (71.42; $p = 0.001$). The number of relatives would represent village relationships that would both increase the trustworthiness

necessary to be selected for membership by one's peers and would facilitate obtaining support and insurance if the borrower had difficulty repaying her loan. [19] One might expect a similar effect of the number of generations the family has been in the village. The negative and significant coefficient on this variable (-283.59; $p = 0.041$) is therefore somewhat anomalous and difficult to explain.

Having a village chief or assistant chief in the household has a large and significant impact on borrowing (2587.55; $p = 0.000$). In fact, every household with a chief or assistant chief in the eight treatment villages participated in the village bank, and quite often their wives were officers (e.g., president, vice president, treasurer) in the village bank and participated very actively. To a certain extent, this fact could be explained by the chief's desire to support the program, but it can also be explained by the ability of powerful households in the village to gain unequal access to program loans. [20]

Civil servants in Thailand generally have access to large, low-interest loans from their ministry's credit union, and several households sampled were teachers (who belonged to the teachers' credit union) or health care workers (who belonged to their own credit union). A typical loan would be on the order of 75,000 baht at 12% annual interest. Given this access to credit at more attractive terms than the village bank, the influence on village bank borrowing of a woman's being a civil servant is large, negative, and significant (-5013.10; $p = 0.000$). [21] The influence of a man's being a civil servant, however, is to increase village bank borrowing (2979.91; $p = 0.001$). Most civil servant households are also agricultural producers. To the extent that a man's being a civil servant (controlling for whether his wife is also a civil servant) shifts farm-management responsibilities to his wife and reduces his own labor input in farming activities, his wife's credit needs will increase as she has to purchase input substitutes. Of course, this begs the question why the male civil servant would not borrow at better terms from the credit union to finance his wife's domestic production. The most plausible explanation would be that resources within the household are not frictionlessly allocated; i.e., that it does make a difference whether the man or the woman has access to credit.

[19] It might also represent alternative sources of credit that might reduce the level of desired participation in the village bank. This effect, however, seems to be overwhelmed by the other effects.

[20] These differences are explored more fully by Coleman (1998a).

[21] It might also be tempting to conclude that civil servants, who have secure and relatively well-paying jobs, would also have lower credit demand in general. However, the influence of this variable on ''low-interest credit'' in Section 5.2 below is extremely large and significant, on the order of 200,000 baht with a p-value of 0.000. Most civil servants surveyed had very high levels of debt, not just from their credit unions, but also from merchants (especially motorcycle and, to a lesser extent, automobile dealerships) who arrange for automatic, electronic monthly payments to be deducted from the civil servants' paychecks.

Similarly, the influence of female-owned land and male-owned land is different. The influence of the value of female-owned land is positive and marginally significant (0.00038; $p = 0.098$), and the influence of the value of male-owned land is not significant (0.000082; $p = 0.878$). Again, if resources are not allocated frictionlessly within the household, then increased women's land holdings will increase women's need for credit from the village bank, whereas increased men's land holdings will not increase women's need for credit. It is also likely that women's ownership of land is taken into account by other villagers when screening potential village bank members. The more land a woman owns, the more guarantee she can provide to the village bank that she will repay her loan: she will presumably have higher agricultural production, and she will have greater access to other credit sources in case she needs to borrow to repay her village bank loan.

Finally, an examination of the various age–sex categories reveals that more will be borrowed by households with women age 22 to 39 (702.27; $p = 0.070$) and women age 40 to 59 (791.17; $p = 0.062$), whereas households with women age 60 and over will borrow less (-966.40; $p = 0.042$), and households with boys age 5 to 15 will borrow more (435.44; $p = 0.044$). These results are not unexpected. Younger adult women, in their economically most productive years, could expect to have the greatest credit needs to finance their activities, while older, less active women would have less need for credit (and may be seen as less productive and thus less creditworthy than younger women). The positive influence of school-age boys may be an indication of the favorable position that boys retain vis-a-vis girls in the Thai household.

5.2. Village bank impact

Table 2 presents the complete output of a typical estimation of Eq. (4) (for female-owned wealth in the household); and Table 3 presents the estimated coefficients and associated standard errors for the regressor of interest (months of village bank membership) in all the regressions. All told, regression results for 72 outcome variables are presented, though these 72 dependent variables are not necessarily independent of each other: impact on large household variables (e.g., total wealth) is estimated, then these variables are broken down by gender (e.g., female-owned wealth and male-owned wealth) and by type (e.g., land and nonland assets, by gender, and nonland assets are broken down further); similarly, total household income and expenditures are subsequently broken down by gender and type.

This study involves at least four innovations not found in most previous studies: (1) it allows for the use of village fixed effects (used by Pitt and Khandker only) to control for the possibility of endogenous program placement; (2) it identifies a control group of would-be borrowers, and then surveys ''treatment'' members, ''control'' members, and nonmembers, thus allowing for the use of a member

Table 2
Impact of village bank on women's wealth (fixed effects, nonfixed effects, naive, and super-naive models). Standard errors in parentheses

Independent variable	Fixed effects coefficient	Nonfixed effects coefficient	''Naive'' model coefficient	''Super-naive'' model coefficient
Months as VB member	240 (475)	77.7 (339)	154 (282)	3247 (1622)**
Sex of household head (female = 1)	67,010 (17,595)***	64,470 (17,444)***	63,993 (17,387)***	236,614 (102,825)**
Education of highest educated female (years)	−1798 (2565)	−2216 (2514)	−2216 (2512)	58,263 (14,362)***
Education of highest educated male (years)	1846 (1985)	2194 (1954)	2159 (1950)	38,208 (11,436)***
Number generations family in village	−5528 (4829)	−2772 (4538)	−2717 (4531)	−33,085 (26,870)
Number relatives in village	−323 (749)	−402 (729)	−395 (728)	52.5 (4326)
Household member village chief or assistant? (0/1)	43,926 (24,243)*	43,558 (24,119)*	44,632 (23,949)*	−17,164 (141,865)
Is female in hh a civil servant? (0/1)	82,021 (42,821)*	87,364 (41,804)**	87,302 (41,762)**	−656,226 (245,219)***
Is male in hh a civil servant? (0/1)	−9037 (30,151)	−12,094 (29,633)	−11,535 (29,571)	−459,662 (174,036)***
Female-owned land value 5 years ago (baht)	1.008 (0.00861)***	1.008 (0.00855)***	1.01 (0.0085)***	
Male-owned land value 5 years ago (baht)	−0.00511 (0.0155)	−0.00680 (0.0153)	−0.0067 (0.015)	

Does hh have a village bank member? (0/1)	2245 (14,778)	5116 (12,649)		
Number females age 5–15	−1347 (7891)	−1345 (7757)	−1009 (7705)	−13,497 (45,755)
Number females age 16–21	−10,571 (13,625)	−10,734 (13,325)	−10,738 (13,311)	−76,949 (78,957)
Number females age 22–39	4723 (12,227)	3116 (12,050)	3140 (12,038)	63,038 (70,832)
Number females age 40–59	−2843 (13,899)	−1048 (13,730)	−995(13,715)	163,754 (81,022)**
Number females age 60 and over	1829 (14,388)	6295 (14,194)	6208 (14,178)	250,536 (83,328)***
Number males age 5–15	−9977 (6892)	−9205 (6815)	−9232 (6808)	−36,702 (40,394)
Number males age 16–21	−10,273 (11,473)	−11,8198 (11,266)	−11,977 (11,248)	−37,167 (66,778)
Number males age 22–39	−9225 (11,173)	−11,535 (11,034)	−11,649 (11,019)	−16,486 (65,439)
Number males age 40–59	3734 (14,582)	2201 (14,495)	2528 (14,458)	−65,159 (84,968)
Number males age 60 and over	−13,039 (17144)	−14,134 (16,908)	−14,463 (16,871)	−130,666 (98,682)
	$F(35,408) = 537$	$F(27,416) = 699$	$F(26,417) = 727$	$F(24,419) = 5.36$
	Prob > $F = 0.000$	Prob > $F = 0.000$	Prob > $F = 0.000$	Prob > $F = 0.000$
	$R^2 = 0.9787$	R-sq. = 0.9784	R-sq. = 0.9784	R-sq. = 0.2350

* Significant at 0.10 level.
** Significant at 0.05 level.
*** Significant at 0.01 level.

Table 3
Impact of village bank on household outcomes
Only coefficients on ''months of village bank membership'' are shown and examined; for a list of other regressors included, see Table 2.
Nonfixed effects model includes five village characteristics.
''Naive'' model excludes member dummy variable; includes five village characteristics in the nonfixed effects model.
''Super-naive'' model excludes member dummy variable and variables for land value owned 5 years before surveys; includes five village characteristics in the nonfixed effects model.
(T) indicates tobit estimates.
Numbers in parentheses are standard errors.

Dependent variable (measured in baht unless stated otherwise)	Coefficient on months of village bank program availability			
	Fixed effects model	Nonfixed effects model	Naive model	Super-naive model
Physical assets				
Household wealth	442 (730)	431 (524)	178 (437)	1349 (1880)
Women's wealth	240 (475)	77.7 (339)	154 (282)	3247 (1662)**
Men's wealth	229 (548)	311 (395)	−4.81 (330)	−1939 (977)**
Household land value	−50.1 (102)	−48.3 (72.6)	−18.0 (60.4)	1095 (1761)
Women's land value (T)	42.5 (93.3)	87.5 (65.3)	121 (54.6)**	6916 (1974)***
Men's land value (T)	−27.0 (113)	−194 (84.5)**	−219 (70.0)***	−5882 (1521)***
Household nonland assets	617 (660)	305 (474)	41.4 (395)	7.15(405)
Women's nonland assets	159 (0.712)	−51.9 (308)	12.1 (256)	78.5 (254)
Men's nonland assets	538 (570)	358 (409)	22.4 (341)	−92.2 (356)
Household productive assets	182 (261)	197 (186)	152 (154)	115 (156)
Women's productive assets (T)	245 (187)	79.5 (131)	217 (111)**	241 (111)**
Men's productive assets (T)	−73.6 (285)	30.5 (204)	−48.8 (170)	−104 (172)
Household nonland farm assets	37.6 (90.8)	84.9 (66.6)	51.6 (55.5)	55.7 (56.3)
Women's nonland farm assets (T)	127 (100)	86.8 (72.8)	104 (−63.2)	122 (62.4)**
Men's nonland farm assets (T)	−96.5 (107)	−46.9 (78.4)	−63.2 (65.4)	−75.1 (65.9)
Household livestock	87.7 (65.3)	25.1 (47.0)	14.9 (39.15)	−0.55 (39.5)
Women's livestock (T)	89.0 (80.8)	−20.4 (58.7)	32.6 (49.5)	38.4 (49.4)
Men's livestock (T)	−31.5 (93.8)	−35.2 (67.6)	−52.0 (56.4)	−75.9 (56.7)
Household business assets (T)	17.8 (543)	−64.4 (369)	181 (307)	136 (307)
Women's business assets (T)	101 (393)	−141 (249)	213 (211)	212 (210)
Men's business assets (T)	303 (941)	222 (657)	36.3 (538)	−117 (541)
Household consumer durables	−51.1 (255)	13.7 (185)	−123 (154)	−96.5 (157)
Women's consumer durables (T)	−59.3 (171)	−130 (123)	−110 (103)	−84.4 (103)
Men's consumer durables (T)	−76.4 (330)	−200 (237)	−366 (199)	−382 (203)*
House value	506 (415)	153 (298)	29.8 (248)	39.9 (247)
Savings, debt, lending				
Household savings (cash, bank deposits, etc.)	−18.6 (150)	−57.0 (107)	−17.2 (89.2)	−51.5 (90.2)
Women's savings (T)	−15.1 (69.6)	41.0 (49.7)	61.4 (41.7)	49.8 (41.9)
Men's savings (T)	−7.78 (154)	−156 (110)	−71.4 (92.2)	−94.2 (92.7)
Household low interest debt (interest rate $\leq$ 2%/month) (T)	445 (435)	−308 (311)	−184 (260)	−255 (267)

Table 3 (continued)

Dependent variable (measured in baht unless stated otherwise)	Coefficient on months of village bank program availability			
	Fixed effects model	Nonfixed effects model	Naive model	Super-naive model
Savings, debt, lending				
Women's low interest debt (T)	136 (398)	−639 (270)**	−265 (230)	−214 (231)
Men's low interest debt (T)	748 (462)	−5.07 (334)	−149 (277)	−269 (294)
Household high interest debt (interest rate $>2\%$/month) (T)	338 (269)	35.2 (206)	−5.74 (167)	−42.3 (171)
Women's high interest debt (T)	538 (273)**	291 (218)	91.0 (171)	87.1 (170)
Men's high interest debt (T)	−126 (395)	−499 (304)	−260 (243)	−310 (261)
Household lending out at positive interest (T)	1393 (608)**	47.7 (420)	105 (347)	123 (356)
Women's lending out at positive interest (T)	1399 (715)**	9.68 (478)	236 (396)	274 (411)
Production, sales, expenses, and labor				
Household self-employment production (sales and own consumption)	25.5 (5386)	3013 (3843)	2173 (3098)	1592 (3067)
Women's self-employment sales (T)	−10.7 (504)	174 (364)	542(296)*	545 (295)*
Men's self-employment sales (T)	−1556 (6985)	−4.02 (5074)	−1350 (4123)	−2377 (4093)
Household agricultural production	32.5 (78.4)	30.3 (57.5)	14.5 (46.4)	16.7 (47.0)
Women's agricultural sales (T)	76.5 (101)	162 (73.9)	101 (59.5)*	113 (59.9)*
Men's agricultural sales (T)	−101 (106)	−126 (79.0)	−143 (65.1)**	−154 (65.1)**
Household animal production (sales and own consumption)	−59.5 (41.9)	−47.0 (30.2)	−23.8 (24.4)	−25.6 (24.1)
Women's animal sales (T)	−39.4 (54.5)	−23.9 (39.8)	−4.39 (32.4)	−1.45 (32.1)
Men's animal sales (T)	−95.2 (79.3)	−123 (56.9)	−86.4 (47.2)*	−95.8 (46.9)**
Household business sales (T)	−3166 (9408)	−231 (6672)	2542 (5357)	1711 (5330)
Women's business sales (T)	212 (1228)	−774 (830)	504 (681)	490 (676)
Men's business sales (T)	1947 (14,034)	7571 (10,342)	2893 (8176)	2245 (8168)
Household self-employment expenses	−49.3 (5081)	2673 (3625)	1858 (2922)	1352 (2891)
Women's self-employment expenses (T)	−202 (402)	−89.4 (289)	355 (234)	337 (233)
Men's self-employment expenses (T)	3323 (5789)	−52.3 (4246)	−2342 (3445)	−2938 (3426)
Household farming expenses	−43.9 (42.5)	−41.0 (30.3)	−58.3 (24.4)**	−61.2 (24.5)**
Women's farming expenses (T)	0.621 (39.1)	−16.8 (27.6)	24.2(22.4)	23.1 (22.3)
Men's farming expenses (T)	−71.4 (50.2)	−59.4 (37.4)	−188 (30.4)***	−122 (30.5)***
Household animal-raising expenses	−14.9 (36.5)	−68.1 (26.2)***	−34.0 (21.2)	−32.1 (21.0)

Table 3 (continued)

Dependent variable (measured in baht unless stated otherwise)	Coefficient on months of village bank program availability			
	Fixed effects model	Nonfixed effects model	Naive model	Super-naive model
Production, sales, expenses, and labor				
Women's animal-raising expenses (T)	−7.70 (34.7)	−27.1 (24.3)	6.76 (19.8)	10.0 (19.6)
Men's animal-raising expenses (T)	−37.1 (42.7)	−80.0 (30.9)***	−58.4 (25.3)**	−58.9 (25.1)**
Household business expenses (T)	−3268 (9046)	259 (6391)	2444 (5139)	1704 (5112)
Women's business expenses (T)	93.2 (1085)	−631 (732)	485 (601)	489 (596)
Men's business expenses (T)	869 (13,686)	7429 (10,067)	1014(7986)	382 (7979)
Household self-employment labor hours	1.33 (6.70)	−2.50 (4.99)	2.70 (4.04)	2.20 (4.00)
Women's self-employment labor hours	1.81 (4.09)	−0.635 (3.01)	2.15 (2.43)	2.01 (2.40)
Men's self-employment labor hours	−0.482 (4.17)	−1.86 (3.06)	0.55 (2.47)	0.19 (2.45)
Health care, education				
Household medical expenses (T)	−55.4 (27.9)**	−16.4 (20.3)	−17.1 (16.4)	−21.1 (16.2)
Medical expenses made for women (T)	−7.85 (17.6)	−7.51 (12.7)	−6.97 (10.2)	−8.75 (10.2)
Medical expenses made for men (T)	−62.3 (33.0)*	−26.0 (24.0)	−20.58 (19.4)	−25.9 (19.3)
Medical expenses made for children (T)	1.11 (9.95)	−4.16 (7.31)	−0.000021 (0.00020)	−3.61 (5.82)
Medical expenses made for girls (T)	3.38 (11.3)	−1.38 (8.00)	1.19 (6.44)	1.09 (6.36)
Medical expenses made for boys (T)	−2.46 (11.3)	−4.47 (8.30)	−5.52 (6.66)	−5.22 (6.64)
School expenses made for children in household	9.67 (16.4)	−7.42 (11.4)	0.14 (9.12)	0.57 (8.97)
School expenses made for girls	6.22 (16.8)	−0.293 (10.9)	−0.78 (8.79)	−1.11 (8.63)
School expenses made for boys	9.65 (15.8)	−13.4 (11.2)	−5.14 (8.94)	−3.67 (8.77)

* Significant at the 0.10 level.
** Significant at the 0.05 level.
*** Significant at the 0.01 level.

dummy variable to proxy for unobservable differences between members and nonmembers; (3) it uses the value of land owned by men and women in the

household 5 years before our surveys to proxy for initial household wealth [22]; and (4) it uses the length of time the program has been in a village to obtain more precise impact measures. [23]

To illustrate the importance of these innovations in correcting for the bias resulting from self-selection and endogenous program placement, four sets of regression results, corresponding to four specifications, are presented in Tables 2 and 3. The first is the ''correct'' specification, using village fixed effects and including a member dummy variable and variables for land value owned (by men and by women) 5 years before our surveys. As explained in Section 2.2 above, fixed effects estimates will be consistent (and unbiased if the dependent variable is uncensored) but possibly inefficient.

The second specification is identical to the first, except that it uses a vector of specific village characteristics (a provincial dummy variable, the number of paved roads in the village, a school dummy variable, the daily harvest wage, and the distance to the provincial capital) that one might expect to influence the dependent variables. If the order of program placement is random, and we are fortunate enough to choose all the relevant observable village characteristics, then estimates from this nonfixed effects (''village characteristics'' or VC) model will be efficient and consistent (and unbiased if the dependent variable is uncensored). If the order of program placement is not random, or we choose the wrong village characteristics, then estimates from this model will be biased and inconsistent.

The third and fourth specifications provide (to co-opt Pitt and Khandker's terminology) ''naive'' estimates. The first set of naive estimates measure impact ignoring the first two innovations; i.e., they use specific village characteristics rather than fixed effects, and they leave out the member dummy variable. The second set of naive estimates also ignores the third innovation by leaving out the variables for land value owned 5 years before our surveys. As such, the naive models correspond to the models most commonly used to measure impact, which ignore selection bias, endogenous program placement, and prior wealth. Because we are interested in the coefficient on ''village bank months'', the fourth innovation is retained in the naive estimates so that the results of the four specifications can be compared.

[22] Land value makes up 73.7% of household wealth in the sample and is therefore an excellent proxy for wealth. Collecting data on land owned 5 years before the surveys is relatively easy. Because land transactions tend to be large and important for a household, yet relatively infrequent, households can easily recall land transactions made in the previous 5 years, and land owned 5 years earlier (and its value) can be deduced. Of course, collecting similar data on other assets is not feasible without surveying over several years.

[23] All of the results and tests in this section are robust to using ''village bank months'' or a treatment dummy variable as the regressor of interest. That is, we obtain qualitatively similar results in estimating Eq. (3) as in estimating Eq. (4).

Hausman tests were attempted on the consistency of the village characteristics (VC) model and the naive models vis-a-vis the fixed effects model. The Hausman test statistic for the VC model, distributed as χ^2, is:

$$(\beta_{FE} - \beta_{VC})'(\Sigma_{FE} - \Sigma_{VC})^{-1}(\beta_{FE} - \beta_{VC}),$$

where β_{FE} and Σ_{FE} are the fixed effects estimates of the regression coefficients and variance–covariance matrix, respectively, and β_{VC} and Σ_{VC} are the corresponding estimates from the village characteristics model. The Hausman test statistic for the naive models are defined analogously. If the Hausman test statistic is large, with a correspondingly low p-value, then using the VC model (or naive model) will yield inconsistent estimates.

Unfortunately, as is often the case with sample data, the difference of the covariance matrices was not positive definite in most regressions, and hence the Hausman test could not be applied. Because the source of bias in the nonfixed effects and naive models is correlation between the village fixed effects and the household regressors, controlling for effects captured by specific village characteristics, the following alternative test (similar to that used by Pitt and Khandker) is applied to the nonfixed effects model. Analogous tests are applied to the naive models. First, the fixed effects model is estimated. The estimated village fixed effects are then regressed on the full set of regressors in the nonfixed effects model, including the vector of specific village characteristics. An F-test on the joint significance of the household regressors in this auxiliary regression is then performed. If the specific village characteristics do not adequately explain the village fixed effects (i.e., if the village fixed effects include any unobservable effects), then the F-statistic will be large and we will reject the null hypothesis that there is zero correlation between the household regressors and the village fixed effects.

Results of this test demonstrate the importance of the first three innovations in eliminating bias from measured impact. The null hypothesis of zero correlation between village fixed effects and household regressors is rejected at the 5% level in 67 of 72 nonfixed effects regressions, 64 of 72 regressions of the first naive model, and 71 of 72 regressions of the second naive model. Especially telling are the differences in estimated impact on the largest wealth measures. The naive results would lead us to believe that the village bank was having a significant and positive impact on women's accumulated wealth, especially landed wealth. However, the truth is that village bank members tend to be wealthier in the first place, especially in terms of women's land value [24], and the first three innovations are crucial to controlling for this fact.

A more detailed examination of the coefficients on the member dummy variable and the land wealth variables in the fixed effects model indicates how

[24] See Coleman (1998a).

their omission biases the naive estimates. The member dummy variable, it will be recalled, proxies for unmeasured differences between members and nonmembers (e.g., entrepreneurship, preferences, etc.). Its coefficient can thus be interpreted as the impact on outcomes due to these unobservables. This coefficient is in fact significant in only eight of the 72 regressions, indicating that for many outcomes, unobservable differences between members and nonmembers are of little consequence. However, some of the regressions in which they are significant are informative. For example, the member dummy coefficient in the female-owned business assets equation is large and significant (24,246; $p = 0.050$). Likewise, the dummy coefficients on women's business revenue (87,209; $p = 0.056$) and women's business expenses (81,641; $p = 0.045$) are large and significant. Previous studies that have failed to account for selection bias have concluded that village bank impact on women's business activities are especially large (greatly pleasing project implementers since these ''microenterprise lending'' programs specifically aim to promote women's small business activities); yet evidence from Northeast Thailand indicates that unobservable characteristics, such as entrepreneurship, may be biasing those estimates. The member dummy variable is also a significant determinant of women's high-interest debt ($-18{,}048$; $p = 0.056$; possibly indicating different risk preferences between members and nonmembers), women's self-employment revenue (36,456; $p = 0.057$), women's self-employment expenses (39,618; $p = 0.011$), and men's farming expenses (-3565; $p = 0.060$). Likewise, the coefficients on women's and men's land value 5 years before our surveys were jointly significant in 24 of 72 fixed effects regressions, and the coefficients on all three variables (member dummy, women's land, and men's land) are jointly significant in 26 regressions.

Estimated impact is presented for the 72 dependent variables in Table 3 under the headings of physical assets; savings, debt, and lending; production, sales, expenses, and labor time; and health care and education. What is immediately noticeable is the virtual absence of any significant impact of the village bank as measured by the fixed effects estimation model. No dependent variables measuring physical assets, broken down by type and by gender of owner, show a village bank impact significant at even the 0.10 level. Impact on all variables measuring production, sales, expenses, and labor time are similarly insignificantly different from zero. Impact on variables measuring expenses on health care and education are either insignificant or, in the case of medical expenditures for men, significant and negative (-55.4; $p = 0.048$).

An important and significant impact of the village bank is seen on women's debt and lending. Paradoxically, village bank loans appear to both increase women's high-interest debt [25] (fixed effects coefficient 538 and $p = 0.049$; but consistent nonfixed effects coefficient 291 and $p = 0.183$) and women's lending

[25] High-interest debt is debt that charges more than 2% interest per month, the village bank rate.

out at positive interest (1399; $p = 0.051$). This apparent contradiction is explained by first-hand knowledge of the different experiences of borrowers. As mentioned in [4], several women reported falling into a vicious circle of debt: they borrowed from a moneylender, who charged 3% for a one-day loan, to repay their village bank loan, and when they received their next village bank loan, they had to use it to repay the moneylender so they had little left to invest. Indeed, many village bank members reported in informal conversations that they joined the village bank largely for social reasons (e.g., to ''be a part of the group'') or because they assumed that any NGO program would be beneficial to them. Such women often borrowed without having identified any project to invest in, and simply spent their loans on consumption. Therefore, they often had no funds to repay their loans 6 months later. On the other hand, some village bank members, especially some committee members, engaged in moneylending, borrowing from the village bank at its relatively low interest rates and loaning out at higher interest rates. Hence, the results showing an increase in high-interest debt and in moneylending are actually measuring impact on different groups within the village bank.

6. Summary and policy conclusions

Using a unique survey designed to overcome the selection bias problems that have plagued previous studies, this paper has presented results on the impact of a women's village bank group-lending program in Northeast Thailand. The survey sample included village bank members from eight treatment villages which had already received village bank support, village bank members from six control villages which had not yet received village bank support, and nonmembers from both types of village. The importance of these innovations is illustrated by comparing results from the ''correct'' empirical specification to three other specifications that fail to correct for some or all of the bias resulting from self-selection and endogenous program placement. The more ''naive'' specifications significantly overestimate program impact.

Estimates of impact that correct for self-selection and endogenous program placement are disappointing. Impact is insignificant on physical assets, savings, production, sales, productive expenses, labor time, and most measures of expenditure on health care and education. And the only dependent variables on which impact is significant are not cause for rejoicing. Impact is significant and negative on expenditure for men's health care. Perhaps more importantly, impact is significant and positive on women's high-interest debt because a number of members had fallen into a vicious circle of debt from moneylenders in order to repay their village bank loans. And impact is significant and positive on women's lending out with interest because some members engaged in arbitrage, borrowing from the village bank at its relatively low interest rate and then lending money out

at a mark-up. There is no evidence in these results that village bank loans are being directly invested in productive activities with a positive return.

These results are consistent with Adams and von Pischke's assertion that ''debt is not an effective tool for helping most poor people enhance their economic condition'' and that the poor are poor because of reasons other than lack of access to credit. However, caution is in order before extrapolating these results to other contexts, and such caution also points the way to certain policy implications and to further research. First, Thailand (the recent monetary crisis not withstanding) is a relatively wealthy developing country, with annual GDP growth at close to 10% for the past two decades, and many villagers already have access to low-interest credit from financial institutions such as the BAAC. In fact, the average wealth of survey households was 529,586 baht, and average household low-interest debt, excluding village bank debt, was 31,330 baht, of which 9342 baht was held by women. In such an environment, it should not be surprising that loans of 1500 to 7500 baht would have a negligible impact. Indeed, a common complaint of women surveyed was that the size of village bank loans was far too small for them to be productive. The village bank model of group lending, as practiced throughout the world, sometimes takes a ''one size fits all'' approach, based on a first loan of US$50 and a maximum of US$300. These limits are in fact commonly used in much poorer countries in Africa, where they represent a lot of money to a village household. They are also the limits used in Northeast Thai villages, and they are arguably too low. On the other hand, the significant number of members who had worked themselves into debt by borrowing without having identified a productive activity to invest in points to the need for project field staff to redouble efforts to stress the need to invest the loans productively.

The paradoxical result of positive impact on both high-interest debt and moneylending, and its explanation, also points toward trying to measure and explain differential impact on different classes of borrowers. It could well be that the village bank is having a positive impact on some subgroups of borrowers. Coleman (1998a) in fact identifies two classes of village bank members and measures the differential impact of the village bank on them. It further explores the determinants of member selection and class-based determinants of access to village bank loans.

It is also possible that these loans, being too small to invest productively because of economies of scale, serve primarily as consumption loans. Hence, research should be undertaken to determine if access to village bank credit allows households to smooth consumption across seasons despite large seasonal fluctuations in income. In fact, such research will be undertaken with the current data set, which is a seasonal panel.

Further research should also be conducted on village banks in other areas of the world to determine if these results are unique to Northeast Thailand or are representative of village banks in general. The research methods and survey design used here should be easily implemented elsewhere. Although we identified control

Appendix A. List of variables

Variable name	Weighted mean ($n = 445$)	S.D.	Means by group			
			Members in treatment villages ($n = 181$)	Members in control villages ($n = 113$)	Nonmembers in treatment villages ($n = 97$)	Nonmembers in control villages ($n = 54$)
Independent variables						
Months as VB member	10.3	17.1	41.93	0	0	0
VB external account borrowing (baht-months)	21,260	67,538	86,097	0	0	0
Value male-owned land 5 years ago	169,763	340,890	120,182	174,936	184,325	111,907
Value female-owned land 5 years ago	217,333	628,873	360,420	254,474	133,618	134,583
Sex of household head (female = 1)	0.212	0.41	0.20	0.16	0.25	0.30
Education of male (years)	5.16	3.27	5.64	5.34	5.01	5.11
Education of female (years)	4.81	2.59	5.15	5.12	4.46	5.00
Generations family in village	3.44	1.13	3.29	3.68	3.34	3.48
Number relatives in village	8.05	6.84	9.25	7.90	7.44	7.56
Village chief or assistant? (0/1)	0.0323	0.21	0.07	0.06	0.00	0.02
Is female a civil servant? (0/1)	0.0207	0.15	0.03	0.01	0.03	0.02
Is male a civil servant? (0/1)	0.0383	0.20	0.06	0.04	0.02	0.04
Village bank member dummy	0.463	0.47	1.00	1.00	0.00	0.00

Number females age 5–9	0.205	0.44	0.20	0.30	0.19	0.11
Number females age 10–15	0.237	0.46	0.24	0.32	0.16	0.22
Number females age 16–21	0.164	0.40	0.19	0.15	0.14	0.15
Number females age 22–29	0.265	0.46	0.20	0.29	0.25	0.33
Number females age 30–39	0.338	0.49	0.35	0.35	0.30	0.31
Number females age 40–49	0.248	0.47	0.35	0.35	0.19	0.22
Number females age 50–59	0.275	0.44	0.24	0.25	0.28	0.31
Number females age 60 and over	0.264	0.46	0.20	0.19	0.36	0.24
Number males age 5–9	0.252	0.46	0.22	0.24	0.26	0.26
Number males age 10–15	0.287	0.56	0.34	0.34	0.20	0.41
Number males age 16–21	0.187	0.46	0.20	0.23	0.10	0.26
Number males age 22–29	0.234	0.46	0.20	0.23	0.32	0.19
Number males age 30–39	0.359	0.48	0.34	0.35	0.32	0.37
Number males age 40–49	0.229	0.45	0.28	0.30	0.20	0.19
Number males age 50–59	0.209	0.41	0.22	0.25	0.13	0.20
Number males age 60 and over	0.206	0.38	0.14	0.13	0.28	0.22

Appendix A (continued)

Variable name	Weighted mean ($n = 445$)	S.D.	Means by group			
			Members in treatment villages ($n = 181$)	Members in control villages ($n = 113$)	Nonmembers in treatment villages ($n = 97$)	Nonmembers in control villages ($n = 54$)
Independent variables						
Prov. dummy (Roi-Et = 0, Surin = 1)	0.61	0.50	0.45	0.60	0.49	0.54
Village has paved road (0/1)	0.465	0.62	0.48	0.35	0.53	0.28
Village has school (0/1)	0.579	0.50	0.71	0.20	0.82	0.19
Distance to market (km)	16.16	8.4	16.22	20.50	14.92	16.99
Daily planting wage	96.5	25.3	106.08	95.49	103.14	97.22
Dependent variables						
Household wealth	529,586	742,452	641,116	558,305	473,285	386,825
Female-owned wealth	267,272	654,629	415,920	306,918	200,669	185,112
Male-owned wealth	256,640	389,261	218,527	245,666	268,944	195,641
Household land value	390,330	686,081	485,442	432,295	321,849	248,065
Female-owned land value	218,379	628,373	363,768	256,023	134,401	134,306
Male-owned land value	171,951	338,945	121,674	176,272	187,448	113,759
Household nonland assets	171,581	199,883	197,080	155,991	184,384	166,285
Female nonland assets	58,050	123,455	62,384	57,836	72,843	62,264
Male nonland assets	107,713	159,159	127,432	92,435	107,870	97,949
Household productive assets	43,052	56,071	49,394	43,154	42,630	43,011
Female productive assets	10,459	27,582	12,586	14,268	8303	13,169
Male productive assets	31,064	50,504	34,518	28,490	33,778	28,388

Household nonland farm assets	15,934	20,749	17,748	15,588	14,710	16,270
Female nonland farm assets	3485	10,509	4113	3116	3096	4668
Male nonland farm assets	11,676	18,735	12,268	12,292	11,615	10,580
Household livestock	15,205	14,366	14,312	16,199	13,720	17,815
Female-owned livestock	4391	10,147	3693	6506	2983	7829
Male-owned livestock	10,068	13,693	9697	9478	10,219	9554
Household business assets	11,913	45,793	17,333	11,367	14,200	8926
Female business assets	2583	20,890	4780	4647	2225	672
Male business assets	9320	40,735	12,554	6720	11,944	8254
Household consumer durables	32,340	83,611	41,898	24,028	50,541	23,700
Female-owned consumer durables	14,584	56,455	14,337	12,046	27,544	10,712
Male-owned consumer durables	15,585	60,394	23,321	10,196	22,620	12,259
Value of house	82,551	109,238	88,321	73,823	81,242	86,333
Household cash savings	13,574	37,730	17,365	14,845	9953	13,203
Female cash savings	6383	16,369	6613	5418	6988	6757
Male cash savings	7191	29,499	10,752	9427	2961	6446
Household low-interest debt ($\leq 2\%$/month)	31,330	103,351	41,087	25,918	36,049	31,064
Female low-interest debt ($\leq 2\%$/month)	9342	46,252	12,228	5585	9608	11,983
Male low-interest debt ($\leq 2\%$/month)	21,775	73,630	27,988	20,332	26,441	19,081
Household high-interest debt ($> 2\%$/month)	7386	22,842	5986	9915	4610	7939
Female high-interest debt ($> 2\%$/month)	3928	14,810	2374	3816	1804	6661
Male high-interest debt ($> 2\%$/month)	3458	16,577	3612	6099	2805	1278

Appendix A (continued)

Variable name	Weighted mean ($n = 445$)	S.D.	Means by group			
			Members in treatment villages ($n = 181$)	Members in control villages ($n = 113$)	Nonmembers in treatment villages ($n = 97$)	Nonmembers in control villages ($n = 54$)
Dependent variables						
Household loaning out at interest	3823	27,027	3725	3127	5246	6590
Female loaning out at interest	3104	25,950	3441	1925	4514	5354
Male loaning out at interest	649	5773	8	1202	732	1236
Household self-employment production	135,215	1,273,136	248,363	115,232	173,436	135,763
Female self-employment sales	29,852	101,596	57,722	22,998	20,042	22,808
Male self-employment sales	93,825	1,269,232	178,772	79,686	143,396	102,149
Household agricultural production	24,254	21,974	23,271	28,527	19,729	30,610
Female agricultural sales	6160	13,145	7682	4919	4687	9435
Male agricultural sales	8163	14,513	6482	12,451	6193	11,469
Household livestock production	6171	10,219	5882	8432	5737	5620
Female animal sales	2839	7332	2600	3661	2323	2856
Male animal sales	2195	6602	2042	3380	2575	1665
Household business sales	104,791	1,271,791	219,210	78,274	147,970	99,533
Female business sales	20,853	100,446	47,441	14,419	13,031	10,518
Male business sales	83,466	1,267,907	170,248	63,855	134,628	89,015
Household self-employment expenses	108,963	1,200,117	211,575	88,209	152,892	109,734

Female self-employment expenses	23,540	90,119	47,149	19,257	14,044	16,451
Male self-employment expenses	84,182	1,195,831	157,695	68,554	138,595	93,283
Household agricultural expenses	12,044	10,974	9922	13,908	10,304	15,382
Female agricultural expenses	4634	7026	5817	4814	3346	5429
Male agricultural expenses	7408	9780	4094	9094	6958	9952
Household animal-raising expenses	3759	8783	1351	2488	1057	1454
Female animal-raising expenses	1985	6131	812	1414	522	806
Male animal-raising expenses	1653	5531	540	1074	535	648
Household business expenses	92,715	1,196,996	192,740	68,809	140,077	91,466
Female business expenses	17,279	88,911	39,859	11,532	9907	9209
Male business expenses	75,121	1,194,188	151,923	57,277	129,916	82,257
Household self-employment labor hours	3487	2120	3570	4004	2926	3790
Female self-employment labor hours	1695	1223	1808	1950	1342	1880
Male self-employment labor hours	1792	1352	1761	2054	1584	1909
Household medical expenses	2606	6100	2065	1786	3010	1987
Medical expenses for females	1281	3137	1152	1051	1286	1292
Medical expenses for males	1325	5303	913	736	1723	695
Medical expenses for children	573	1478	522	597	543	632
Medical expenses for girls	284	1015	260	264	233	307
Medical expenses for boys	289	299	262	333	311	325
School expenses for children	2430	3918	2891	3411	1512	2318
School expenses for girls	1079	2250	1258	1484	843	809
School expenses for boys	1351	1483	1633	1927	670	1510

villages 1 year before they received village bank support, this early identification is not necessary for the impact measures discussed in this paper. It would suffice to survey treatment and newly identified control villages just before the new villages receive village bank support. Since most village bank programs regularly open new village banks, this type of survey could be widely undertaken.

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