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Source: *Journal of the European Economic Association*, Vol. 3, No. 1 (Mar., 2005), pp. 1-50
Published by: The MIT Press on behalf of European Economic Association
Stable URL: http://www.jstor.org/stable/40004942
Accessed: 09/04/2010 16:04

POLICIES AND IMPACT: AN ANALYSIS OF VILLAGE-LEVEL MICROFINANCE INSTITUTIONS

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Abstract
This paper uses variation in policies and institutional characteristics to evaluate the impacts of village-level microfinance institutions in rural Thailand. To identify impacts, we use policies related to the successful/unsuccessful provision of services as exogenous variation in effective financial intermediation. We find that institutions, particularly those with good policies, can promote asset growth, consumption smoothing and occupational mobility, and can decrease moneylender reliance. Specifically, cash-lending institutions—production credit groups and especially women's groups—are successful in providing intermediation and its benefits to members, while buffalo banks and rice banks are not. The policies identified as important to intermediation and benefits: the provision of savings services, especially pledged savings accounts; emergency services; and training and advice. Surprisingly, much publicized policies such as joint liability, default consequences, or repayment frequency had no measured impacts. (JEL: 012, 016)

1. Introduction

Both macrotheory and macro-evidence point to the importance of financial intermediation on growth, especially in the context of developing economies. Given this evidence, one would expect to find access to financial intermediation playing important roles on the microlevel as well. Indeed, these expected micro-impacts are the justification for efforts by government and nongovernment organizations to improve access to financial intermediation, including the booming expansion of microfinance initiatives.[1] Despite the prevalence of such initiatives, there has

Acknowledgments: Kaboski gratefully acknowledges funding for this research from Ohio State University and the National Institute on Aging. Townsend thanks the National Science Foundation (Grant 318340) and the National Institute of Health (Grant R01 HD027638) for their financial support. We would like to thank Sombat Sakuntasathien for collaboration and making the data acquisition possible. We have benefited from helpful comments of Alberto Abadie, Aleena Adam, Joshua Angrist, Philip Bond, Esther Duflo, Tim Guinnane, Anna Paulson, Sergio Urzua Sosa, Sombat Sakuntasathien, and especially the editor and referees. Finally, we would like to thank John Felkner for assistance with the GIS calculations. All mistakes are our own.

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1. There are an estimated 9000 microfinance initiatives worldwide sponsored by a variety of organizations, including the World Bank, United Nations, USAID, and many charitable NGOs.

been little empirical examination of their impacts.[2] In contrast to previous work, this study examines a large set of heterogeneous village-level microfinance institutions, links impacts on households to variation in the characteristics and policies of these institutions, and evaluates whether the observed impacts of these types of intermediation are consistent with what theories predict.

The theories that motivate our analysis are two structural general equilibrium models of growth that make strong predictions on the ways in which financial intermediation can affect households with limited access to credit and/or savings services. The first model, due to Lloyd-Ellis and Bernhardt (2000), is a growth model with occupational choice, investment, and credit constraints.[3] Gine and Townsend (2003) get strong predictions from the exogenous introduction of a credit market into this model: intermediated households have higher asset growth rates and higher levels of entrepreneurship/occupational mobility. The second model is Greenwood and Jovanovic's (1990) model of endogenous financial intermediation, project investment, and growth, as generalized by Townsend and Ueda (2003). The model predicts higher (though time varying) asset growth rates and improved risk sharing for intermediated households. The two models jointly predict financial intermediation to have impacts on household's assets, risk sharing, occupations, entrepreneurship, and credit constraints. Furthermore, the two models together lead us to consider a broad definition of financial intermediation, including credit, savings, and informational advantages.

The villages we study are located in rural and semi-urban Thailand, a promising environment to look for the microimpacts of financial intermediation. That is, the Thai growth experience has been both qualitatively and quantitatively consistent with the above models of growth and financial intermediation (see Jeong and Townsend 2003). Despite this growth, there are still important segments of the population in the Thai data with limited access to formal financial intermediation. The fact that our institutions are operated at the village level is also a virtue. Since the institutions uncovered in the survey are promoted by a variety of agencies and ministries, our data shows a great deal of important variation in institutional types and policies. This variation is related to an institution's success in providing financial services (lending, savings, and membership). Essentially, we use this variation as an instrument that allows us to identify impacts (see Section 4).

2. The few serious efforts to evaluate the impacts of microfinance institutions (e.g., Pitt and Khandker 1998; Morduch 1998; Coleman 1999; Ravicz 2000; Aportela 1998) have produced mixed or contradictory results. These existing studies have focused on a single, or at most a handful, of larger organizations, such as the Grameen Bank, BRAC and BRBD in Bangladesh or BRI/BKK in Indonesia.

3. The works of Aghion and Bolton (1997), Banerjee and Newman (1993), Evans and Jovanovic (1989), Feder et al. (1991), Paulson and Townsend (2002), and Pikkety (1997) are important and related contributions.

The results of our analysis, highlighted here, are predominantly consistent with theory.

1) We find evidence in support of theory for positive impacts of village institutions on asset growth, especially among those institutions and policies that were associated with successful provision of intermediation services. That is, institutions that seem to succeed in membership, savings mobilization, and lending are institutions that have higher positive impact on households. In particular, cash loans are associated with the stability or expansion of services, while rice lending institutions and buffalo banks are associated with contraction or failure. PCGs and women's groups, institutions that typically lend cash, had positive impacts on asset growth, while buffalo banks and to a lesser extent rice banks appear to have had, if any, negative impacts. The results are significant only for the maximum likelihood estimation, and not for two-stage least squares regressions, however. Also, three specific policies associated with institutional success (offering training services, savings services, and pledged savings accounts) were each individually associated with faster asset growth rates. Institutions with these policies yielded 5–6% higher annual growth in assets to their villagers.
2) Institutions with certain policies can help to smooth responses to income shocks. These policies include offering emergency services, training services, and various savings-related policies. While both standard (i.e., flexible) and pledged (i.e., restrictive) savings accounts help with smoothing, flexible accounts appear more helpful. Households in villages with these beneficial policies were 10–29 percentage points less likely to reduce consumption/input use in a year with a bad income shock. Nevertheless, the average institution does not appear to alleviate risk and may increase the probability of having had to reduce consumption, buffalo banks and perhaps rice banks in particular. Though the overall lack of a positive impact on alleviating risk is troubling, the fact that institutions associated with diminishing services had perverse (if any) impacts,[4] and the polices correlated with successful intermediation had positive impacts is in line with what theory suggests.
3) We find some evidence in support of the theories of constrained occupational choice, but more so for job mobility per se than entering into business. Women's groups do seem to increase job mobility. Pledged savings accounts (associated with successful intermediation) appear to increase the probability

4. Though we do not wish to emphasize the perverse estimated impacts of rice banks and buffalo banks, the results are not implausible given the high failure and contraction rates of these institutions. Namely, buffalo bank loans seemed to be high risk (given the possibility that the buffalo either dies or does not produce offspring) and low return (given the high failure/contraction rates of the institutions), and so it is plausible that they prevented asset accumulation and consumption smoothing. Likewise, given the high failure rates of rice banks, the average member may have lost rice deposits that could have been saved privately to buffer income shocks.

of switching jobs, and possibly starting a business, while traditional savings accounts (associated with diminishing intermediation) seem to have the opposite impact. Nevertheless, the evidence is not fully in harmony with the theory, since PCGs decrease the probability of switching jobs and also perhaps the probability of starting a business, and emergency services also lower the probability of starting a business.

4) The most robust result is that institutions overall help reduce reliance on moneylenders, our indirect measure of the prevalence of formal credit constraints. The effect on the average villager is to reduce the probability of becoming a moneylender customer by 8 percentage points. Our interpretation is that village institutions loosen households' constraints on formal credit, at least to credit that could be acquired alternatively from moneylenders. Other than women's groups, there is no strong evidence of any particular institution or policy associated with this impact, however.

We emphasize that the results overall show that institutions and policies correlated with the success and stability of services are also significantly associated with positive impacts on households. This is our "smoking gun," as it were: If our data and statistical techniques allow us to gauge impact on client households and businesses, then we would expect institutions that eventually fail to have zero or perverse impacts. That such institutions continue to appear in our data, giving the needed exogenous variation, is a peculiarity of the Thai political environment.

The remainder of the paper is organized as follows. Section 2 gives a theoretical background for the study and places it within the research program on credit constraints, financial intermediation and growth, especially within the context of Thailand. Section 3 describes the data and the types of village institutions. In Section 4 we discuss the estimation equations and robustness checks. Section 5 organizes the results and main findings, while Section 6 summarizes and concludes.

2. Theoretical Background

An explicit theoretical model encompassing all of the outcomes and policy variations that we examine is beyond the scope of the paper. Nevertheless, the analysis is motivated by existing theory on the importance of credit markets and access to financial intermediation on household outcomes. Here we briefly discuss the two structural models that motivate our empirical work. The first is a theory of growth based on occupational transitions, particularly movement out of subsistence agriculture and into agribusiness and nonfarm business, as modeled by Lloyd-Ellis and Bernhardt, or LEB (2000).[5] The second model, Greenwood and Jovanovic,

5. Of course this is not the only possible model of credit-constrained occupation choice. The moral hazard models of Aghion and Bolton (1997) and the collateral constraints model of Banerjee and

or GJ (1990), could also be interpreted as a model of occupational choice, but emphasizes the risk sharing benefits and the endogenous participation decision in financial intermediation.

LEB model household occupational choice among subsistence agriculture, employed labor, and entrepreneurship. Each household has initial beginning-of-period wealth but no access to credit. The household can earn income during the period in agriculture or earn an equivalent income as an unskilled laborer for a firm, and save its beginning-of-period wealth in a backyard storage technology. Alternatively, the household can invest some or all of that wealth in covering fixed costs to start or maintain a business. These costs are inversely related to the level of talent. Residual wealth for those running business can be put into a neoclassical production technology with diminishing return to capital. For all households, end-of-period earnings on wealth and income from the choice of occupation can be either consumed or saved for the next period (at a fixed rate). Given the lack of a credit market, the model implies a positive relationship between initial wealth and business starts (transitions within the period from wage earnings and subsistence agriculture), a positive relationship between wealth and the level of investment in business or agribusiness, a negative relationship between wealth and marginal rates of return in business, and a negative relationship between wealth and those households who say they could make more profits in business or agriculture if they had more wealth (or could borrow).

A limitation of this analysis is the exogeneity of the intervention. The GJ model deals in a structured way with endogenous financial deepening. In this model, households (villages, regions) of varying initial wealth choose whether or not to join the financial system, and this comes at a cost, either paid directly or covered by fees. An advantage of the financial sector is its ability to reallocate the risk of idiosyncratic shocks and to provide information for the reallocation of capital toward optimal investments. In autarky, households (villages, regions) do not have these advantages and decide how much to save and how much to invest in nonfarm business or agribusiness, with a risky return, or in subsistence agriculture with a low but safe return. Financial intermediation leads to risk sharing, higher average returns on investment, and higher (though time varying) growth rates of wealth.

3. Description of Data and Institutions

The analysis here is based on household and institution level data from a survey conducted in May 1997 (before the financial crisis) in four provinces (*changwats*) of Thailand—the semi-urban changwats of Chachoengsao and Lopburi in the

Newman (1993) also deliver growth with increasing inequality. Paulson and Townsend (2002) and Karaivanov (2003) estimate various versions of these models with the Thai data.

Central region relatively near Bangkok, and the more rural Sisaket and Buriram in the poorer Northeast region. The survey design was based in part on the results of prior field research in the Northern region (see Townsend 1995). We utilize three subcomponents of this survey: the institutional module, the household module, and the key informant module. In the rest of this paper, we continue to refer to this collective data set simply as the Townsend Thai data (Townsend et al. 1997).

The institutional survey was given to all known microfinancing institutions that were encountered in the villages at the time of the household survey. In total, records for 161 institutions were obtained across 108 of the villages. Geographically, the institutions surveyed are well distributed across the 192 villages, although villages in the poorer, more rural Northeast region were about twice as likely to have institutions as those in the semi-urban Central region. The survey questions focused on both the individual policies and the experiences of the institutions, including their founding, membership, and saving and lending services.

The institutions are quasi-formal institutions. That is, they keep records and often have bank accounts, but do not in general have their own office, for example. Although administered at the local level, most have some relationship to the Thai government, most often the CDD (Community Development Department). Many institutions receive initial funding from these sources, and the government agencies also offer advice, training, and end-of-the-year accounting assistance.

As the word "microfinance" suggests, the institutions are fairly small. Funds typically started with between 30 and 40 members. (The median size of a survey village is about 500 people, with household size averaging 4.5.) The services offered are also small scale. For example, for lending services, the median "typical loan size" was 3500 baht ($140)[6] in 1997, while the median loan duration was one year. (For comparison, the median annual household income in the survey is 48,500 baht or $1940). Typical annual interest rates were 14–19%. Also, the institutions rarely require collateral on loans, but often use guarantors. For saving services, the median "typical annual deposits" was 500 baht ($20), and the return on these savings averaged 8%.

As stated earlier, village institutions operate at the local level. The vast majority (91%) operate at the village level, while the remainder operate at the next organizational level—the subdistrict (*tambon*), which typical contains 12 villages. Both the membership and administration is thus confined to the local level.[7] In different villages, and within the same village, institutions take different forms that are distinguished by their memberships, the services they offer, their purposes,

6. The precrisis (i.e., before July, 1997) fixed exchange rate was 25 baht/dollar.

7. Still, as noted, most of the institutions have some relationship with the Thai government, most often the CDD, or other institutions, such as Catholic Relief Services. Indeed, without being prompted, 84% of the institutions mentioned government involvement in their founding and 60% mentioned the CDD specifically. Many institutions receive funding from these sources, and, as noted, the government agencies also offer advice, training, and end-of-the-year accounting assistance.

and their level of funding. These include production credit groups (PCGs), rice banks, women's groups, and buffalo banks.

PCGs are the most common type of institution. Members of PCGs are relatively less likely to be the poorest in the village and are more likely to be mostly women. They typically lend cash. They are often promoted by the CDD, which calls them "village savings funds" because they aim to promote "good savings habits" within the village. Although more PCGs offered lending services than saving services, compared to other institutions, they were relatively more likely to offer saving services and less likely to offer loans. Given this dual nature and the fact that they lend cash, PCGs operate much like village savings and loan cooperatives, but are not linked into any larger intermediation network.

The second most common village institution is a rice bank, which usually makes small, short-term, emergency consumption loans intended primarily for consumption smoothing over time. These loans are rice and are made at higher interest rates than other institutions make. They are often promoted by the Ministry of Agriculture and are used as vehicles for introducing high yield varieties of seed. Members are generally required to deposit or donate a given amount of rice at the founding of the institution to build an initial (hopefully, self-sustaining) fund. Thus, compared to other institutions, rice banks are significantly more likely to lend, and less likely to accept ongoing savings. Their membership is relatively more likely to consist of primarily poor people and to be male. Rice banks are concentrated in the poorer, more rural provinces of Sisaket and Buriram.

As a category, women's groups are distinguished more by their female membership than by their financial activities or policies (see Kaboski and Townsend 2000). While women participate in PCGs and other groups (not only as members but also in leadership positions), the women's groups are groups that specifically target women for membership. Many of the groups are also linked with training and funding for occupational promotional activities that might allow women new ways of bringing income into their households. For example, in the Northeast women's groups have been founded in order to introduce silk production to the women in the village.

Buffalo banks are institutions that are formed to lend out buffalo or cattle. The loan is repaid when the initial buffalo gives birth and the young buffalo is returned to the fund. Once lent out, if the buffalo dies or does not give birth, no further loans can be made. One common problem is that the initial "fund" of buffalo may be beyond reproductive age. Thus, many buffalo banks made loans initially but were not (or no longer) lending at the time of the survey. Buffalo banks do not generally accept savings since their loans and repayments are in the form of livestock.

The form or type of the institution is not the lone dimension of variation among establishments in the institutional survey. The institutional survey also contains independent data on the services, policies and characteristics of the institutions,

which vary across the sample. Finally, the survey contains the historical experiences of the institution including membership, lending, and saving data drawn from the institutions' own record books. These experiences vary greatly across institutions. Some experience dramatic growth rates of membership and services, others maintain their levels, while still others experience sharp declines or even cease operation.

An analysis of the relationship between successful and unsuccessful experiences and the observable characteristics of the institutions (i.e., the type of institution, their membership, and the policies they choose) is given in an earlier working paper, Kaboski and Townsend (2000). The paper shows that a significant fraction of institutions failed in the first year or the first five years,[8] while others showed dramatic growth (over 10% annually) in membership, lending services, and saving services. Kaboski and Townsend use an indicator variable to distinguish institutions that showed declines from those that were either stable or showed growth along these dimensions and highlight policies and institutional types that were significantly correlated with growth experiences. Common policies associated with group lending such as individual/group liability, default consequences, payment frequency or monitoring frequency did not prove to be significant. However, many other individual policies and institutional characteristics were significantly correlated with growth or failure.

A summary of these significant relationships is reproduced in Table 1. Among these relationships, we note:

1) Buffalo banks tended to have negative growth in lending services.
2) Institutions that made rice loans were more likely to have negative growth in lending.[9] In contrast, cash loans were positively correlated with lending growth.
3) The provision of agricultural training was positively correlated with lending growth and the provision of non-agricultural advice/consultation was positively correlated with growth in savings.
4) In general, more stringent policies such as requiring minimum initial deposits and having pledged savings accounts were positively related to growth of membership and saving, while more flexible policies such as savings being optional for membership and having standard (save and withdraw as desired) accounts were negatively related to growth. One exception is that institutions with time deposit savings accounts—an inflexible account—were more likely to have negative savings growth.

8. Of those institutions founded in 1992 or before, about 25% stopped lending by five years after their founding, while about 10% had ceased saving and either failed completely or lost all membership. These members are likely lower bounds, as the survey certainly did not capture all defunct institutions.

9. Rice banks themselves were not significantly related to lending growth, however.

TABLE 1. Summary of significant correlations between relevant institution types policies and growth failure.

Correlations with membership growth		Correlations with savings growth		Correlations with lending growth	
Positive	Negative	Positive	Negative	Positive	Negative
Offer lending services	Saving is optional	Require minimum initial deposit	Standard savings accounts	Provide agricultural training	Institution is a buffalo bank
Require minimum initial deposit		Have membership application forms	Time deposit savings	Make cash loans	Make rice loans
		Pledged savings accounts	Only villagers can be members	Amount of savings used as evaluation criteria	
		Provide nonagricultural consultation or advice			
		Provide emergency assistance			

Note: Other policies that were tested include among others: collateral required, guarantors required, payment frequency of six months or less, monitoring frequency of six months or less, borrowers who default can't reborrow, and all borrowers are monitored. These did not have significant relationships with growth.

These policies[10] will be examined in Section 5 to see if institutions with successful (unsuccessful) policies had larger (negative) impacts on household/business outcomes. We do this using the institutional data by finding villages in which there is only one institution or in which every institution in the village has the same particular policy. We thus create village-level policy indicators that are linked to the household data in these villages.

The household survey was administered to 2,880 Thai households—15 households in each of 192 survey villages. The villages were divided evenly across the four provinces and selected in a stratified, clustered, random sample (see Binford, Lee, and Townsend 2004). Households provided an extensive array of demographic and socioeconomic information, including current data on income, borrowing, saving and lending, as well as retrospective histories of occupation, assets (divided among household, agricultural and business assets),[11] and organizational involvement/membership. Summary statistics for the household-level variables used in this study are presented in Table 2.

The study has several sources of village-level data. Several village-level variables (average wealth, average wealth squared, fraction of households in rice farming, and average years of education) were constructed by creating averages from the Townsend household data. These village levels vary slightly across individual households since each household's average excludes the household itself. In addition, the key informant survey, a survey of a key informant (generally, the headman) of each survey village, contains general data on the village and was used in this study to gain retrospective knowledge of the presence and absence of various types of institutions in the village during different years. Summary statistics for the village-level variables from the Townsend Thai data are given in Table 3.

Thailand's CDD data set provides a biannual census collected by Rural Development Committee (RDC) at the village level. The data are collected in two steps.

10. Three other policies that were significantly correlated with negative lending growth. Two of these policies (long loan periods, poverty eradication programs) involved institutions that were part of a government poverty program instituted in 1996. Since these poverty initiatives had much longer loan periods, most loans had not been paid at the time of survey, so lending had appeared to decrease for these institutions. Given this, and the fact that the poverty programs did not exist over most of the period of impact assessment, these relationships are not addressed in this study. Finally, the amount of collateral required was positively correlated with growth. Unfortunately, since most institutions did not require collateral, we had very little variation in this variable and could not use it in our assessment of impacts.

11. The past value of real assets is found by depreciating the purchase price of the asset (in 1997 baht) from the time of purchase to what it would have been worth six years ago. We assume that the depreciation rate for all household and agricultural assets is 10% per year. One exception is land, the value of which we do not depreciate over time.

The retrospective wealth levels are incomplete in (at least) two respects. The first issue is that we only have information on household and agricultural assets that the household still owns. The second concern is that we do not have any information on past financial assets and liabilities. Fortunately, financial assets and liabilities tend to make up a small fraction of current household wealth, and so were probably also a small fraction of past wealth.

TABLE 2. Summary statistics of relevant Townsend Thai household-level data.

	No. of obs.	Mean or fraction	Stand. Dev.
Impact variables			
Asset growth, 1991–1997	2422	0.607	1.192
Reduced consumption in worst income year, 1992–1997*	2331	0.689	0.463
Became a moneylender customer, 1991–1997*	2725	0.148	0.355
Started a business, 1991–1997*	2874	0.128	0.334
Switched primary occupation, 1991–1997*	2480	0.188	0.391
Demographic variables			
Age of head	2841	51.4	13.6
Age of head squared	2841	2829.5	1466.0
Years of education—Head of household	2822	4.1	2.6
Male head of household	2841	0.77	0.42
Number of adult females in household	2870	1.59	0.85
Number of adult males in household	2870	1.44	0.90
Number of children (<18 years) in household	2870	1.54	1.25
Wealth variables			
Wealth†	2875	1.08	4.04
Wealth squared†	2875	17.51	215.2
Non business wealth†	2875	1.08	4.04
Non business wealth squared†	2875	17.45	215.0
Occupational dummy variables			
Business owner*	2875	0.078	0.269
Inactive no occupation*	2686	0.045	0.207
Rice farmer*	2686	0.481	0.500
Farmer, other crop*	2686	0.191	0.393
Shrimp farmer*	2686	0.034	0.180
Construction*	2686	0.034	0.181
Business/Skilled trade*	2686	0.068	0.251
Professional administrative*	2686	0.036	0.187
General worker, cleaner, janitor*	2686	0.084	0.278
Other*	2686	0.028	0.165
Member/Customer in organization/institution			
Formal financial institution‡	2875	0.176	0.381
Village institution/organization*	2875	0.123	0.328
Agricultural organization (BAAC or Agricultural cooperative)*	2875	0.270	0.444
Moneylender*	2875	0.040	0.196

Notes: * Binary variable.

† Wealth is made up of the value of household assets, business assets, agricultural assets, and land. Nonbusiness wealth excludes business assets. Wealth levels were divided by 1,000,000 to rescale estimates into convenient numbers. The sample excludes the top 1% of households by wealth.

‡ Formal financial institutions include commercial banks, the government savings bank, insurance companies, and finance companies.

All variables are for the year 1990 except for the impact variables (as noted) and the demographic variables, which are 1997.

TABLE 3. Summary statistics of relevant Townsend Thai village-level data.

	No of obs.	Mean or fraction	Stand dev.
Townsend village controls			
Average wealth†	2875	1.08	1.57
Average wealth squared†	2875	3.63	12.04
Fraction of households with rice farming as primary occupation	2686	0.481	0.201
Average years of schooling–head of household	2822	4.11	0.87
Townsend Thai data institutional presence			
Village has institution*	192	0.607	0.488
Village has rice bank*	192	0.151	0.358
Village has buffalo bank*	192	0.105	0.306
Village has PCG*	192	0.083	0.276
Village has women's group	192	0.231	0.421
Institutional data—All village institutions in village have specified policy			
Offer lending services*	49	0.837	0.373
Amount of savings used to evaluate loans*	51	0.314	0.469
Offer emergency services*	46	0.087	0.285
Offer training, advice, or consultation*	47	0.234	0.428
Offer savings services*	51	0.431	0.500
Offer pledged savings accounts*	48	0.229	0.425
Offer traditional (Deposit and withdraw as desired) savings accounts*	50	0.040	0.198
Saving is optional to members*	50	0.261	0.442
Saving requires minimum initial deposit*	49	0.306	0.466
Loans require collateral*	39	0.128	0.339
Loans require guarantors*	40	0.650	0.483
High loan repayment frequency (More than one payment per year)*	37	0.135	0.347
Frequent monitoring of loans (More than once per loan period)*	27	0.370	0.492
All borrowers are monitored*	26	0.577	0.503

Notes: * Binary variable.
† Wealth is made up of the value of household assets, business assets, agricultural assets, and land. Levels were divided by 1,000,000 to rescale estimates into convenient numbers. The sample excludes the top 1% of households by wealth. All variables are for the year 1990 except for average years of schooling–head of household. Given the average age of these heads of household (51.4), this 1997 schooling variable is likely quite close to its 1990 counterpart.

In the first step, members of the RDC fill in the questionnaire by themselves using the existing data from the Tambon office. After that, for each village, a meeting with the village headman and village committee is held and the missing information is collected.

The data include over 650 variables from which 19 were used as village controls in our robustness studies (see Table 4). The choice of these 19 variables was designed to capture the level of development, remoteness of the village along several dimensions, the occupational composition of the village, the financial institutions present in the village, and the role of government initiatives in the village. The variables are: (1) a dummy variable for municipal location; (2) typical travel

TABLE 4. Summary statistics of relevant CDD village-level data.

	No. of obs.	Mean or fraction	Stand. dev.
CDD village controls‡			
Municipal location*	174	0.017	0.131
Typical travel time to district office (in minutes)	172	38.67	22.82
Typical travel time to market (in minutes)	171	40.56	27.42
Number of households	176	121.7	146.7
Economic status of village relative to other villages in subdistrict (1,2,3)**	178	2.06	0.52
Development level of village relative to other villages in the district (1,2,3)**	177	2.08	0.518
Fraction of households with piped water supply*	176	0.049	0.179
Fraction of households with State-supplied electricity*	178	0.076	0.300
Fraction of households with members working in agriculture only	178	0.333	0.360
Fraction of households with members working in multiple occupations	178	0.504	0.367
Fraction of households engaged in cottage industries	178	0.001	0.012
Fraction of rice-farming households using government-promoted varieties	178	0.497	0.398
Households migrate of the village for labor*	175	0.943	0.233
Fraction of households with members working outside the subdistrict	173	0.290	0.237
Fraction of households that are members of an agricultural bank/cooperative	178	0.807	0.394
Use of a commercial Bank	178	0.236	0.423
Use of the agricultural Bank (BAAC)	178	0.865	0.343
Level of government aid relative to other villages in district (1,2,3)**	177	2.10	0.49
Village has assembly hall*	178	0.390	0.488
CDD data institutional presence			
Village has rice bank*	177	0.232	0.422
Village has buffalo bank*	178	0.146	0.353
Village has PCG*	178	0.112	0.316
GIS-predicted institutional presence			
Probability of village having rice bank	192	0.210	0.354
Probability of village having buffalo bank	192	0.134	0.299
Probability of village having PCG	192	0.125	0.281

Notes: * Binary variable.
** Qualitative variable with 1 = above average, 2 = average, and 3 = below average.
‡ From over 650 variables, these 19 village control variables were examined (see Section 4).
All variables are for the year 1990.

time to district office; (3) typical travel time to market; (4) number of households; (5) economic status of village relative to other villages in the subdistrict; (6) the development level of the village relative to other villages in the district; (7) fraction of households with piped water supply; (8) fraction of households with electricity; (9) fraction of households exclusively in agriculture; (10) fraction of households

engaged in multiple occupations; (11) fraction of households engaged in cottage industries; (12) fraction of rice farming households using promoted varieties; (13) a dummy variable for outmigration of labor from village; (14) fraction of households with members working outside the subdistict; (15) the fraction of households that are members of an agricultural cooperative; (16) use of commercial bank; (17) use of the BAAC (agricultural bank); (18) level of government aid relative to other village in subdistrict; and (19) a dummy variable for whether the village has an assembly hall. Of these, five are used in the results explicitly displayed in the tables.

Variables on institutional presence of village savings funds, livestock banks, and paddy banks are also used. These names correspond to PCGs (identical), buffalo banks (nearly identical), and rice banks (identical) in the Townsend data. Unfortunately, no corresponding variable for women's groups exists in the CDD data.

The census includes data for all villages in Thailand and not just the 192 villages included in the cross-sectional survey described above. We use the data on all villages in each of the four changwats in our analysis here coupled with positioning data from a GIS system in order to create spatially predicted probabilities of institutional presence in the Townsend survey villages. The methods used to construct these variables are explained in detail in Section 4.

4. Method

The focus of this study is whether microfinance produce the impacts of financial intermediation predicted by theory. Unfortunately, we have no complete measure of financial intermediation provided by the village institutions we study, certainly not at the household level. Even if we had such a measure, it would likely suffer from endogeneity problems. Our approach instead is to estimate impact using variables associated with financial intermediation, whose variation is either exogenous, or endogenous in ways that can be controlled.

The variables we use are the presence of (or membership in) the institution, the different types of institutions, and the different policies. We examine two sets of policies—the first set is the group of policies associated with successful financial intermediation in the data (recall Section 3), while the second set involve policies such as group liability, dynamic incentives, or better monitoring technologies, policies predicted to be important by theory (e.g., Ahlin and Townsend 2000; Alexander 2000; Banerjee, Besley, and Guinnane 1994; Besley and Coate 1995; Conning 1999; Ghatak 1999; Jain and Mansuri 2003; and Stiglitz 1990). For policies, we lack independent membership data, and so we can only look at the effect of institutions with these policies on outcomes of the average villager, not just members. For institutions overall, and each of the different types of institutions

(rice banks, buffalo banks, PCGs, and women's groups), we do have membership data and so can look at the impacts of institutions on members.

The focus on membership introduces the issue of household-level selection bias. Households that are members of village institutions (in villages with institutions) may differ systematically from nonmember households in the same villages. If these differences are the result of biased selection into the institutions (whether on the part of the household demand or the institution supply) they should not be attributed to the impact of the institution. Our use of the presence of the institution in the village (a village-level) variable as an instrument for membership addresses this problem in a simple intuitive way. Namely, we add to the outcome equation, a membership equation that includes the presence of an institution as an explanatory variable. We then use the presence of the institution (in 1990) to identify the impact of membership (in 1990). The years examined are not crucial to either the results or methods used. For alternative choices of timing see the robustness subsection later.

We introduce membership to the analysis, where possible, because theory suggests that most aspects of financial intermediation are linked to the direct use of services. If the institutions also have external positive (negative) impacts on nonmembers in the village, which is plausible,[12] our instruments would overestimate or underestimate the impact of membership. However, introducing the presence of the institution directly into the outcome equation would still yield good measures of they average impacts (including members and nonmembers) of the various institutions.[13]

12. LEB suggests, for example, general equilibrium wage effects from intermediation, and GJ suggest that savings rates of non-members may increase in anticipation of joining financial intermediation in the future. In addition, given the presence of informal loans among neighbors and family, intermediation may be passed on to nonmembers.

13. Using the notation below, we assume that institutional presence I effects membership D_M, which in turn effect financial intermediation F:

$$y = \beta F + \varepsilon_y$$
$$F = \alpha D_M + \varepsilon_F$$
$$D_M = \delta I + \varepsilon_F.$$

For simplicity we assume a linear relationship between membership and financial intermediation. The assumption here is that α is positive (for successful institutions), but unknown. That is, membership in a successful institution yields a positive, but unknown amount of financial intermediation. Given this model, instrumental variables (I instrumenting for D_M) gives a consistent estimate of $\beta\alpha$, the effect of membership on outcomes.

Instead we might propose that the presence of the institution I itself influences the financial intermediation along with membership D_M:

$$y = \beta F + \varepsilon_y$$
$$F = \alpha_1 D_M + \alpha_2 I + \varepsilon_F$$
$$D_M = \delta I + \varepsilon_F.$$

In sum, we are not able to independently quantify impacts of membership from external impacts (without assuming away external impacts). We are also not able separately identify the two stages of the channel: the impact of policies on financial intermediation versus the impact of financial intermediation on outcomes. Consequently, we limit our quantitative interpretation to the average effects of institutional presence or policies on villagers. Nevertheless, several caveats should be noted. First, interpreting the magnitudes of coefficients in the linear probability models is problematic. Second, the policies and institutions we analyze are correlated with one another, so we do not have enough data to truly quantify the independent effects of these policies. Third, the confidence intervals of the results are typically wide.

The second form of selection bias involves the possible endogeneity of the presence of the institution in the village. That is, programs may exist in a biased sample of villages, and therefore a biased sample of households, because of either biased program placement or possibly biased program survival. We address this village-level selection using a wide range of village-level controls from the Townsend Thai and CDD data sets and using GIS spatial techniques that isolate "surprise" (i.e., exogenous) innovations in program placement. The robustness of our results give us confidence that our village controls adequately account for village-level selection.

An additional way that we account for unobserved heterogeneity is by focusing on changes over time, after 1990 to the date of interview, all of which can be interpreted as allowing for household fixed effects. Our five outcome variables for measuring impact are: (1) growth in assets (1991–1997);[14] (2) the probability of reducing consumption or input in a year with a bad income shock (1992–1997);[15] (3) probability of starting a business (1991–1997);[16] (4) the probability

A simple regression of y on I would yield a consistent estimate of $\delta\beta\alpha_1 + \beta\alpha_2$. This is simply the effect of the institution on villagers. The first term is the effect of membership ($\beta\alpha_1$) times the probability of being a member given an institution δ, and the second term is the external effect of the institution on all villagers.

14. The growth in assets variable is calculated using households current (1997) surveyed levels of business, agricultural and household assets and by constructing retrospective past (1991 and 1990) asset stocks.

15. That is, we do not look simply at the cross-sectional variability of consumption relative to income but examine this impact over time. Household respondents were asked which year (if any) of the past five (i.e., 1992–1997) had been the worst in terms of household income. Those who gave a specific year were then asked a series of possible responses to this shock, including (among others) whether or not they had reduced consumption or the use of inputs.

16. We have retrospective knowledge of the date businesses were started only for businesses that still existed at the time of the survey. Thus, our data may omit businesses that were started but failed before the time of survey.

of switching primary occupation (1991–1997);[17] and (5) the probability of becoming a moneylender customer (1991–1997).[18]

Each of these can be thought of as allowing for underlying unobserved heterogeneity on a primary "level" variable (i.e., level of assets, level of consumption/input use, probability of being a business owner, probability of being a rice-farmer, and probability of borrowing from a moneylender), whose value depends on past access to financial intermediation F. For assets, business ownership, occupation, and moneylender reliance, the equations in the primary variables of household n would take the form:

$$\tilde{y}_{n,t} = \sum_{j=1}^{\infty} \beta F_{n,t-j} + \theta_n + \tilde{\varepsilon}_{y,n,t}. \tag{1}$$

Here, $\tilde{y}_{n,t}$ represents the "level" variable of household n at time t, β, the effect of intermediation,[19] θ_n a household-specific fixed effect, and $\tilde{\varepsilon}_{y,n,t}$, the error term. Time differencing eliminates θ_n and yields:

$$y_n = \beta F_{n,t-1} + \varepsilon_{y,n} \tag{2}$$

where we have defined new notation $y_n \equiv \tilde{y}_{n,t} - \tilde{y}_{n,t-1}$ and $\varepsilon_{y,n} \equiv \tilde{\varepsilon}_{y,n,t} - \tilde{\varepsilon}_{y,n,t-1}$. In our study, t is considered 1997, while $t-1$ is 1991.

The equation for consumption/input use assumes no change in access to financial intermediation between the years of interest[20] ($F_{n,t-1} = F_{n,t-2} \equiv F_n$) and postulates an interaction effect between current income ($Y_{n,t}$) and membership:

$$\tilde{y}_{n,t} = \alpha \tilde{Y}_{n,t} + \beta \tilde{Y}_{n,t} F_{n,t-1} + \theta_n + \tilde{\varepsilon}_{y,n,t}.$$

Again, time differencing yields:

$$y_n = \alpha Y_n + \beta F_n Y_n + \varepsilon_{y,n} \tag{3}$$

17. We have full retrospective histories of primary and secondary occupations for each member of the household over age 10. Here we use the primary occupation of the head-of-household. The majority of job changes indicate upward mobility. The most common job change was out of rice farming and into a different type of farming (e.g., livestock, corn, orchards). Aggregated tables of these job changes are given in Appendix A. Table A.1 contains all of the job changes, while Table A.2 contains only those of member households of institutions.

18. Again, this is constructed using retrospective data from the household survey. Households that were already money lender customers in 1991 were excluded from the sample.

19. Although the theories in GJ and LEB impacts would vary across households depending on observables, we simply do not have enough data to estimate interaction effects. We simplify the empirics by assuming that β is common to all households, and that selection biases result from other sources. In this case, the "treatment" effect of the institutions is common to all agents and the standard parameters of interest (average treatment effect, treatment on the treated, local average treatment effect, marginal treatment effect) are all equal (Heckman, Lalonde, and Smith 1999).

20. In GJ, which motivates the risk-sharing outcome measure, the decision to enter the intermediated sector is once-and-for-all.

using the additional notation, $Y_n \equiv \tilde{Y}_{n,t} - \tilde{Y}_{n,t-1}$. As explained later for consumption/input use, t is a year (between 1992 and 1997) with low income, while $t-1$ is the previous year. Thus, with $\beta < 0$, past financial intermediation lowers the coefficient on (idiosyncratic) income variation.

Below, we add household and village control variables, X_n and Z_n, respectively, and interpret each of our outcome regressions in light of the preceding equations:

1) The asset growth interpretation is a straightforward analysis of the differenced variable.
2) The starting a business and becoming a moneylender customer variables are analyses of the differenced variables, conditional on the initial value. That is, we include only households for which $\tilde{y}_{n,t-1} = 0$.
3) For occupations, instead of using the probability of switching out of a low-income occupation like rice farming (i.e., the change in the probability of working in rice farming conditional on working in rice farming at $t-1$), we measure the probability of switching occupations overall, and show that these changes are overwhelmingly toward higher-income jobs.
4) Unfortunately, we do not have panel income and consumption data in differences to measure the response of consumption/input use to current cash flow (conditional on other controls for lifetime wealth and the consumption needs, such as household demographics and education).
   Instead, we measure this using data on whether a household reduced consumption/input use in a year of relatively low income. That is, for one year, we have an indicator variable $\chi_{y_n} < 0 | Y_n < 0$.[21]

Differencing eliminates household heterogeneity θ_n, but we do not argue that differencing is our fundamental way of accounting for selection, nor that it precludes the use of an instrument/exclusion restriction. If we used the primary variable $\tilde{y}_{n,t}$ directly, our regressions would still appropriately account for individual-level selection as long as our instruments I_n were independent of the idiosyncratic component of $\tilde{y}_{n,t}$ after controlling for observable heterogeneity using controls X_n and Z_n, i.e., $I_n \perp (\theta_n + \tilde{\varepsilon}_{y,n,t} | X_n, Z_n)$. We instead use the differenced variables y_n because it seems more plausible that the instrument is independent of changes in the idiosyncratic component of the underlying variables, i.e., $(I_n \perp \varepsilon_{n,t} | X_n, Z_n)$. We return to the discussion of instruments and controls momentarily.

21. Imperfect consumption smoothing implies $\alpha > 0$. Financial intermediation assisting in consumption smoothing would imply that intermediation would reduce the response, $\beta < 0$, and $\beta F_n \in (-\alpha, 0)$. Assuming orthogonality of the error term $\varepsilon_{y,n}$ to income shocks, the probability that a household reduces consumption in a bad income year $P(y_n < 0 | Y_n < 0) = P[\varepsilon_{y,n} < -(\alpha + \beta F_n) Y_n < 0]$ would be decreasing in financial intermediation F_n, if an only if $\beta < 0$.

Impact estimation involved several different regressions, each of which is explicitly discussed below. We begin with the overall impact regression, next discuss regressions that incorporate GIS variables, then explain the impact estimation using specific policies, and close by a discussion of our robustness checks.

4.1. Impact by Type of Institution

We start with a simple model where the presence of an institution (of a given type) in a village influences whether a household is a member of such as institution, and membership m_n is a proxy for the access to financial intermediation F_n that influences outcomes. We uses two different sets of regression equations to try and model this. The first is a two-stage least squares approach that assumes linear membership and outcome equations. The second is a simultaneous equation, maximum likelihood approach that accounts for the binary nature of the membership variable and each of the outcome variables except asset growth. It also uses the possible correlation of error terms between the membership and outcome equations in the estimation.

4.1.1. Two-stage least squares. Again, let y_n be the outcome variable and M_n the membership variable for household n:

$$y_n = \sum_{i=1}^{I} \alpha_i X_{i,n} + \sum_{j=1}^{J} \tau_j Z_{j,n} + \beta M_n + u_{y,n} \tag{4}$$

$$M_n = \sum_{i=1}^{I} \gamma_i X_{i,n} + \sum_{j=1}^{J} \phi_j Z_{j,n} + \delta I_n + u_{m,n}. \tag{5}$$

Again, membership M_n affects outcomes y_n additively and the presence of the institution in the village I_n affects membership additively. The $X_{i,n}$ are sets of household-specific variables and $Z_{j,n}$ are sets of village specific-variables for household n.

We assume that $u_{y,n}$ and $u_{m,n}$ are independent of $X_{i,n}$ for all i. We are interested in the parameter β in equation (4) as our measure of membership impact.[22]

Since membership M_n may be potentially endogenous (i.e., correlated with $u_{y,n}$), we use the presence of an institution as an instrument for membership via the membership equation. Although, institutions may also be present in a based set of villages, we assume that our observable village characteristics $Z_{j,n}$ control for this village selection bias. That is, given the village-level observables, we assume I_n is uncorrelated with $u_{y,n}$ and is therefore a valid instrument for two-stage least squares estimation.

22. Here β denotes the impact of the *proxy* for financial intermediation, not the impact of financial intermediation F itself as in (2) and (3).

4.1.2. Simultaneous equation MLE. One problem with two-stage least squares is that it assumes linearity of relationships that are clearly nonlinear. For example, the membership variable M_n is binary, but first stage estimation will give us not only intermediate values, but also values less than zero and greater than one. Similarly, for all but asset growth, the outcome variables are binary as well. Given this, we use a second model specification that allows us to account for these nonlinearities, though it requires us to assume a (normal) distribution for the errors terms.

Let the binary variables $D_{y,n}$ and $D_{M,n}$ be determined by continuous latent indexes y_n^* and M_n^*, respectively:

$$\begin{aligned} y_n &= 1, \text{for } y_n^* > 0 \\ y_n &= 0, \text{for } y_n^* \leq 0 \end{aligned} \tag{6}$$

and

$$\begin{aligned} M_n &= 1, \text{for } M_n^* > 0 \\ M_n &= 0, \text{for } M_n^* \leq 0. \end{aligned} \tag{7}$$

Now, we assume linear empirical relationships for these two latent unobserved indexes, and avoid imposing linear relationships for the binary outcome variable and membership variable themselves:

$$y_n^* = \sum_{i=1}^{I} \alpha_i X_{i,n} + \sum_{j=1}^{J} \tau_j Z_{j,n} + \beta M_n + u_{y,n} \tag{8}$$

$$M_n^* = \sum_{i=1}^{I} \gamma_i X_{i,n} + \sum_{j=1}^{J} \phi_j Z_{j,n} + \delta I_n + u_{m,n}. \tag{9}$$

We again assume that both $u_{y,n}$ and $u_{m,n}$ are independent of the $X_{i,n}$ and $Z_{j,n}$. However, we explicitly model the dependence of membership M_n and $u_{y,n}$ through the correlation between $u_{m,n}$ and $u_{y,n}$. That is, we assume a joint normal distribution of $u_{m,n}$ and $u_{y,n}$ with a correlation of ρ:

$$(u_{m,n}, u_{y,n}) \sim \text{Bivariate Standard Normal}(0, 0; \rho). \tag{10}$$

The normalization of variances to unity is possible since y_n^* and M_n^* are unobserved indexes, with zero being the only critical value.

Equations (6)–(10) can be estimated as a system of simultaneous equations with the village presence variable I_n playing the role of an exclusion restriction, instead of an instrument as in the 2SLS. Given the assumption of normality, we

write down the joint likelihood equations and estimate the parameters by maximizing the likelihood. The actual likelihood equations are given in Appendix B.[23]

Again, the advantage of the simultaneous MLE approach is that we account explicitly for the bounded, non-linear conditional expectation of the binary outcome and membership variables. The weakness of the approach is its reliance on the assumed joint normality that cannot be justified a priori. The strength of the 2SLS approach is that we avoid making distributional assumptions. Its weakness is that we propose a linear fit to a conditional expectation function that is clearly nonlinear. Both approaches are used, since neither approach clearly dominates and we want to make sure our assessment of impact is not peculiar to a particular technique.

4.1.3. Direct impact of institution. A third approach is to introduce the presence of the institution I_n directly into the outcome equation. That is, instead of measuring the effect of intermediation on members only, we estimate its average effect on all sampled households in the village, or more succinctly on an average villager as discussed earlier. This approach would in theory capture any external effect that the institution might have on nonmembers. The equation used is:

$$y_n = \sum_{i=1}^{I} \alpha_i X_{i,n} + \sum_{j=1}^{J} \tau_j Z_{j,n} + \beta I_n + u_{y,n}. \tag{11}$$

Again, β here represents the direct impact of institutional presence on the outcomes of households in the village, not the impact of membership. These regressions produced results that are generally smaller, less significant or insignificant, but not strikingly different than the membership impacts using the above methods. We therefore omit the detailed results but note the exceptions where these estimates were highly significant.

4.1.4. Actual estimation. The household-level independent variables ($X_{i,n}$) used in the regressions are: age of head, age of head squared, years of education of head, male head (dummy), number of adult males in household, number of adult females, number of children (under 18 years), total wealth, wealth squared, customer of formal financial institution (dummy), and member of agricultural organization (dummy). In addition to these controls, dummy variables for occupation

23. The simultaneous equation, maximum likelihood approach to the estimation of the asset growth equation differs slightly from the above equations, since the outcome variable itself is continuous. We instead replace the latent index variable y_n^* in the equation above with the actual observed outcome (asset growth). The standard deviation of $u_{y,n}$ must then be estimated instead of normalized, since asset growth is directly observed. The resulting likelihood equations for asset growth are also presented in Appendix B.

of the household head are used for the "asset growth," "becoming money lender customer," and "reduce consumption in a bad year" regressions. The village-level controls from the Townsend Thai data include average household wealth in the village, average wealth squared, fraction of village households in rice farming, and average years of schooling of household heads. Those from the CDD data are the fraction of households with members working in agriculture, the fraction of households in multiple occupation, presence of a village assembly hall (dummy), village economic status relative to other villages in the subdistrict, and the level of government aid relative to other villages in the district. This list of village-level controls was chosen since these variables were most often significant in regressions with larger sets of controls (see Table 4 and the following robustness section).

The impact is measured by the coefficient $\hat{\beta}$ on institutional presence or membership in 1990. Our measure of I_n, the impact variable itself or as instrument for membership, is the presence of an institution in the household's village in 1990 as indicated in the retrospective key informant survey. Since the linear probability model produces heteroskedastic error terms, we report White–Huber robust standard errors.

Examples of the regression equations (4) and (5) are presented in Table 5 using "becoming a moneylender customer" as an outcome variable. This outcome is shown since it proved to have the most robust impact using the full-sample of institutions.

The instrument (i.e., village institutional presence) is shown to be sizable and extremely significant in the membership equation. The results are fairly comparable using either the 2SLS or maximum likelihood estimation. Maximum likelihood produces a significant correlation $\hat{\rho}$ between the error terms in the outcome and membership equations, which is reported at the bottom of the table. The membership impact variable is negative (reduces moneylender reliance) according to both estimates.

In the 2SLS, the first-stage membership regression is the same membership regression used for each outcome as in Table 5. The instrument is strongly significant with a t-statistic of 10.2, and the first-stage regression has an R^2 of 0.08. The first stage regressions for individual group membership (e.g., rice banks, PCGs) are similar with t-statistics on institutional presence ranging from 3.0 (buffalo banks) to 8.9 (rice banks), and R^2 ranging from 0.03 (buffalo bank) to 0.13 (rice banks). The one exception is women's groups, which had a relatively weak relationship in the first-stage of the 2SLS (t-statistic $= 1.1$ and $R^2 = 0.01$).

Table 8 presents only the impact estimate $\hat{\beta}$ results for all five outcome equations using both estimation techniques. This table is discussed in Section 5.

4.2. Membership Impact Estimation Using GIS

In the previous subsection, we accounted for village-level selection by the use of controls of observable village-level characteristics $Z_{j,n}$. In this section, we utilize an additional method by controlling for the probability of a particular type of institution, given its geographic location. The general robustness of our results to the inclusion of these controls, even when significant, gives us added confidence in the reliability of estimates using only the earlier sets of controls. These results can therefore be thought of as a robustness check.

We posit that the presence of an institution I_n consists of a predictable component $\bar{I}_n$ and an exogenous error component or "surprise" e_n. The predictable component is allowed to influence household outcomes y_n. Modifying the linear probability model equations presented previously, we have:

$$y_n = \sum_{i=1}^{I} \alpha_i X_{i,n} + \sum_{j=1}^{J} \tau_j Z_{j,n} + \eta \bar{I}_n + \beta M_n + \varepsilon_{y,n} \tag{12}$$

$$M_n = \sum_{i=1}^{I} \gamma_i X_{i,n} + \sum_{j=1}^{J} \phi_j Z_{j,n} + \delta I_n + u_{m,n} \tag{13}$$

$$= \sum_{i=1}^{I} \gamma_i X_{i,n} + \sum_{j=1}^{J} \phi_j Z_{j,n} + \delta \bar{I}_n + \delta e_n + u_{m,n}. \quad (14)$$

From the preceding equations, we see that our identifying instrument (or excluded variable) is no longer the institutional presence I_n, but the "surprise" component e_n.[24] We are now able to weaken our identifying assumption, allowing I_n to be correlated with $u_{y,n}$ (i.e., $u_{y,n} = \eta \bar{I}_n + \varepsilon_{y,n}$) as long as this is through the predictable component $\bar{I}_n$. We do assume, however, that the surprise component e_n is not correlated with $\varepsilon_{y,n}$. In words, we argue that after controlling for observable village characteristics, variation in villages' institutional presence (either with or without institutions) that differs from the institutional presence of surrounding villages, is exogenous variation that is unrelated to the future outcomes of the village households except through the impact of the institutions themselves.

To develop this control variable, we utilize a Geographic Information System (GIS) using the CDD census data. For virtually every village in Thailand, we have biannual data of whether PCGs, rice banks, or buffalo banks were present in the village in 1990 (or other even numbered years). (Women's groups were not included in the census data.) Given this data, for every village in the Townsend Thai survey, we create the predicted probabilities of it having each of these types of institutions, one at a time. These probabilities are created nonparametrically, by applying a geographical kernel smoother on CDD records of the institutional presence of surrounding villages (whether included in the Townsend Thai data or not).

The results presented use a neighborhood defined as the nearest 12 villages, where neighboring villages are weighted in proportion to their inverse distance to the village in question. Knowing that too small a neighborhood or too much damping would simply reproduce the original data, while too large of a neighborhood or too little damping would remove important variation, a scheme was chosen that yielded strong variation in probabilities (i.e., intermediate probabilities that differ from zero or one). Nevertheless, estimation was remarkably robust to changes in the size of the neighborhood and damping weights of the GIS variable. The robustness of the weighting scheme was examined by changing the power (0.1, 0.5, 2, and 3) on the inverse distance in constructing the weights.[25] We also examined changing the neighborhood definition from "nearest 12 villages"; we examined the "nearest 5" and "nearest 20" villages as neighborhoods. We also defined the neighborhood as "all villages (minimum of two) within a given radius." The different radii that were examined were 2, 5, 10 and 20 kilometers.

24. Note that the identical coefficient δ multiplies both $\bar{I}_n$ and e_n, so that only the actual I_n need be entered into equation (14).

25. The probabilities used are given for geographic pixels representing 500 meter by 500 meter squares of land. Automated programs in ArcInfo search outward under the designated criteria from the center of each pixel and thus assign a probability value to the entire pixel. Thus, the odds of an infinite weight—where the village lies at the center of a pixel—are negligible.

The radii of 10 and 20 kilometers usually contained more than 12 villages, while less than 12 villages usually existed within a 2-kilometer radius.

Visual examples of the GIS output for PCG presence are given in Figures 1 and 2. Figure 1 presents the data for the changwat of Sisaket in the Northeast. The actual CDD village data are represented by the points (dark for no reported village fund access in 1990 and light for access), while the shading shows the predicted probabilities (where, for contrast, the lighter shading represents the lower probabilities of access). Figure 2 gives a more detailed view of the northern portion of Sisaket. Here we highlight the villages of the Townsend Thai survey data, while still presenting CDD data (dark dots again represent no access in 1990, while dark squares now represent access) and the GIS output, that is, the probability surface. The numerical values are the actual probabilities $\bar{I}_n$ used in the impact estimation. One can see that the probability surface retains many of the features of the underlying data. White areas with low probability have many villages reporting no access. On the other hand, "surprise" villages do exist. For example, the dark square labeled 0.444 in the east is a village that actually did have an institution located in an area that gave it just a 0.444 probability of having an institution. Conversely, the black dot labeled 0.702 in the upper north had a 0.702 probability of having a village institution, but did not actually have one.

We replicate the results from the previous section using this GIS control variable for the institutions for which we have CDD data. For the MLE, the distributional assumption is now made over $\varepsilon_{y,n}$ and $u_{m,n}$, instead of $u_{y,n}$ and $u_{m,n}$. An example of the regression where "becoming a money lender customer" is the outcome variable and membership in a rice bank is the treatment is presented in Table 6. The results are very similar to those in the earlier section. Rice bank presence remains significant in the membership equation and highly predictive (the presence of an institution increases the probability of being a member by 13%.)[26] Indeed, the GIS variable, predicted presence of a rice bank, is actually insignificant in this impact regression indicating that the GIS control is not doing additional work beyond the other village level controls. Still, in some regressions, the GIS variable $\bar{I}_n$ is significant. For PCGs, the variable is significant in the equations for asset growth, reducing consumption in a bad year, starting a business, and changing jobs. For buffalo banks, the GIS variable $\bar{I}_n$ is significant in the regressions for starting a business and reducing consumption. For rice banks, the GIS variable is never significant. The impact results for all of these regressions are presented in Table 9 and discussed in Section 5. Comparing with the corresponding results in Table 8, the GIS results are strongly consistent with the asset growth results, ambiguous in other cases, and only contradict the earlier result of PCGs impact on starting a business.

26. The presence of buffalo banks and PCGS are also significant in their respective membership equations.

FIGURE 1. 1990 CDD villages, grey-scaled by those reporting access to village saving funds overlaid on top of interpolated probability surface.

FIGURE 2. Northern Sisaket close-up view. Distribution of villages in Townsend survey. Symbol-coded by 1990 reported access/no access to village savings funds. Also labelled with their probability of access values, (obtained from interpolated access probability surface). Overlaid on access probability surface.

4.3. Impact by Policy

We do not have direct evidence of membership of households in institutions with different policies because policy information is taken form the institutional survey and the household survey only records membership in an institution, not its policy. So, instead of using the presence of an institution as an instrument for membership, we again use the direct impact equation (11).

Our proxy for intermediation, I_n, is now a dummy variable for whether all the institutions in a village had a particular policy or whether no institution in the village had a particular policy. The coefficient β again represents our parameter of impact and is an estimate of the average impact of the intermediation on members and nonmembers.[27]

Though we also ran probits for the binary outcome variables, we present here the linear regressions which allowed for a fuller use of the sample and clearer results (see footnote 34). Here X_i and Z_j are again the household- and village-level controls, respectively. Households in villages that had multiple institutions

27. See again footnote 13.

that differed in the relevant policy or had an institution for which the relevant policy was unclear were not used in the regression.[28]

Since membership is no longer used, we do not have the issue of household-level selection in these policy impact regressions. As long as I_n is independent of $u_{y,n}$ in (11), after controlling for village observables Z_n, we do not have a problem with village-selection either. We have many reasons for believing this a justified assumption and that policy variation is primarily exogenous, as discussed earlier in the introduction.

An example of the regression for becoming a money lender customer on the policy of offering pledged savings accounts is given in Table 7. Again, in all tables we report White–Huber robust standard errors to account for the heteroskedasticity of linear probability models. The full results are presented in Tables 10 and 11. Table 10 shows the impacts of the policies in Table 1 associated with institutional growth or failure, while Table 11 shows the impact of the policies traditionally mentioned in the microfinance literature. Since the sample sizes for the policy estimation are somewhat smaller, we also report significance at a 10% level, in addition to the 10% standard used in the previous tables.

4.4. Robustness

Beyond the use of three different estimators (2SLS, MLE, direct impact estimator) and the aforementioned regressions using GIS, many more robustness checks were run. We discuss these in turn.

First, we checked the results to see if the specific year chosen was unusual. In the regressions presented we focus on six-year changes (1991–1997), using 1990 membership as a treatment. We also looked at five-year impacts (1992–1997) using 1990 membership as a treatment, and four-year impacts (1993–1997) using 1992 as a treatment. (GIS estimates require use of the biannual CDD census data). The results were robust. Four-year impacts were slightly less significant, but this might be expected if impacts grow over times as in (1).

Second, geographic fixed effects were added.[29] Dummy variables for the more rural Northeast region (*Sisaket* and *Buriram*) versus the Central region

28. This makes sample sizes markedly smaller. The major problem with probits and small sample sizes occurs when a given value of a binary independent variable perfectly predicts the value of the regressor. Using a probit estimate, the coefficient on this independent variable tends toward positive or negative infinity (in order to increase the conditional probability of the event to one or zero). Given the lack of an internal solution to this likelihood problem, the probit subroutines drop the independent variable and the relevant observations form the estimation. To preserve the valuable information of these regressors, and maintain comparability across estimations, we present the OLS estimates.

29. We could not add village fixed effects to the regression since the identifying variable (I_n) is a village-level variable. A linear combination of village dummies would be perfectly collinear with our identifying instrument/excluded variable.

(*Lop Buri* and *Chachoengsao*) did not greatly affect the results. Nor did inclusion of province (*changwat*)-specific fixed effects, except for lower levels of significance. Using subdistrict (*tambon*)-specific fixed effects, results were also consistent, but again occasionally lost a measure of significance.

Third, we ran regressions with additional village controls 19 in total), as well as a subset of these controls altogether different form the ones presented. The original list of 19 CDD control variables (see Section 2 for the list) were selected to capture the level of development, remoteness of the village along several dimensions, the occupational composition of the village, the financial institutions present in the village, and the role of government initiatives in the village. Unfortunately, many of these variables are highly collinear. Regressions with all

19 variables produced estimated with consistent signs on the impact coefficient $\hat{\beta}$, but the precision of estimates was greatly lowered and many lost significance. We present the results for the subset of these controls that were often significant in the regressions with all 19 control variables. (They were also frequently significant in the regressions presented.) A second, alternative subset of controls (i.e., level of development relative to the district, the fraction of households with piped water supply, the time to the district office, the use of the agricultural bank (BAAC) or an agricultural cooperative in the village, the fraction of rice farmers using government-promoted varieties, and the fraction of households with members who migrate for labor) also gave extremely consistent results in terms of both sign and significance.

Fourth, we added cubic and quadratic age, wealth, and average village wealth terms to the regressions. These terms were not significant and did not effect the results.

Fifth, we ran policy impact regressions for the binary outcome variables using probit regressions instead of linear regressions as mentioned in footnote 34. The results were robust. However, in a few cases, one-sided correlation of a binary independent variable with the dependent variable forced its omission form the regression. To keep the list of dependent variables consistent across regressions, we decided to report the linear results.

Sixth, we attempted a semi parametric approach to estimation suggested by Abadie (2003), which allows for covariate controls. Unfortunately, the predictive power of the first stage of this two-stage approach was very weak. Not a single variable of any kind showed significance in the impact equation using this estimator, so the semi-parametric approach was abandoned.

Finally, we ran regressions using the growth of institutions (membership, lending services, or saving services) directly in regressions of impact. The effect of institutional growth did not show up as significant, though the samples were sometimes greatly reduced since villages with multiple institutions occasionally had conflicting measures of success. We view these regression results as confirmation of the endogeneity of more direct measures of financial intermediation and the importance of our policy and institution type variation in estimation impacts.

In the next section, we highlight the most salient results included in Tables 8, 9, 10, and 11. Again, these results are robust to the above checks except where noted.

5. Results and Findings

In this section we highlight the significant impacts of interest and evaluate them in terms of the predictions of the LEB and GJ models. We measure significance of relationships at the 5% level. The results are organized by the respective outcome measures (asset growth; consumption/input use smoothing; entrepreneurship and job mobility; and money lender reliance).

5.1. Asset Growth

Both the LEB and GJ theories discussed in Section 2 predict that increased financial intermediation leads to higher asset growth rates. In support of these theories, there is some evidence that institutions, especially those institutions with stability or expansion of services, promote asset growth among members.

In general, the 2SLS and MLE results are consistent in sign, but only the MLE results are significant. For institutions overall, we focus on the first two rows of Table 8. Both the 2SLS and MLE estimate positive impacts of membership on asset growth, but only the MLE is significant.

Only those institutions that did not tend to diminish services have positive impacts; the institutions associated with declining services have negative impacts on asset growth. Specifically, Table 8 shows that rice banks and buffalo banks tend to have negative impacts on asset growth, while PCGs and women's groups have positive impacts. Again, the results are only significant using the MLE, however. Looking at Table 9 to see the results for the regressions using the GIS variable, we see a similar pattern with MLE estimates: a significant positive effect of PCGs and negative effect of rice banks and buffalo banks.

The sign of the 2SLS estimate is consistent with this result for PCGs, but not for rice banks and buffalo banks. The negative affect of rice banks was less strongly supported in the robustness checks. Indeed, OLS regressions of the direct effect of institutional presence on asset growth of members and nonmembers yielded a small, but significant, positive effect of rice banks. Thus, the positive impact of

PCGs is perhaps the strongest result, while the impact of rice banks is perhaps the most ambiguous.

The divergence between the 2SLS and MLE estimates is a bit troubling, especially since the linear model should be consistent despite the fact that membership is binary. It could be that these results would indeed turn significant given more data, however, and the MLE incorporates more information (i.e., the correlation of error terms in the membership and outcome equations) into its estimation. For the results in Table 8 and Table 9, these estimated correlations are both sizable and significant). Nevertheless, these MLE results also rely on the distributional assumption of normality.

Tables 10 and 11 show that the policies correlated with growth have positive impacts on asset growth, but the policies traditionally mentioned in the literature

as important to successful microfinance intermediation do not. Providing training or advice, offering savings services, and offering pledged savings accounts in particular are associated with significant positive impacts on households.

Quantitatively, these impacts are sizable. Ceteris paribus, households in villages with institutions that offered savings services had 26% higher growth in assets over six years (about 4% per year) than households in villages that did not (see Table 10). Institutions that offered savings services yielded 25% higher growth (again, about 4% per year), and institutions offering pledged savings accounts in particular yielded 32% higher growth (5% per year).

5.2. Consumption Smoothing

Recall that the measure of consumption smoothing is whether or not households were forced to reduce consumption or input use in a bad year. The GJ model predicts that financial intermediation will reduce idiosyncratic risk through risk sharing and aggregate risk through the better use of information. We find that some policies associated with the growth of intermediation services, especially savings growth, can reduce risk, though institutions on average, especially buffalo

banks and rice banks, which are associated with declining services, can lead to higher risk.

The positive policy results are highlighted in Table 10. Not surprisingly, institutions that offer emergency services significantly reduce the probability of reducing consumption/input use in a bad year. Other things equal, households in villages with these institutions were 20 percentage points less likely to have to reduce consumption/input use in a bad year. Savings policies appear to play a particular role in consumption smoothing as well. Institutions that offered savings services, required minimum initial deposits to start saving, and used savings to evaluate loan applications lower the probability of having to lower consumption/input use in response to a bad shock by 12–18 percentage points. Perhaps these savings policies induce or enable households to build up a buffer stock to protect against bad shocks.

Within types of savings services, pledged savings accounts, associated with the growth in savings services, had significant positive benefits to risk sharing, making households 12 percentage points less likely to have to reduce consumption in a bad year. Still, from the more flexible traditional savings accounts (which are associated with declines in saving services) the benefit is at least as strong and more significant. We posit that the rigidness of pledged accounts may lead to increased savings (one aspect of intermediation) that has its own benefits, at the cost of easy liquidity (another type of intermediation) that aids in consumption smoothing. Savings being optional to members, another policy associated with declining services, also may allow for more flexibility/liquidity, since it too improved consumption smoothing.

Finally, the provision of training or advice is marginally significant (at the 10% level).

Table 8 shows that the impact for institutions overall (any village institution) is perverse according to both estimates, but only significant in the MLE. This result is only true for institutions associated with declining services, however; in Table 8, buffalo banks and rice banks follow the pattern of institutions overall.

The evidence on rice banks is less strong though, given the GIS results in Table 9. The MLE estimate for rice banks is no longer significant, and the 2SLS impact estimate is actually of the opposite sign.[30]

The evidence that buffalo banks increase the likelihood of reducing consumption or input use in a bad year are bit stronger.[31] The MLE results for the regression without the GIS variable (Table 8) is significant while the 2SLS estimate is nearly

30. The OLS result of the direct effect of rice bank presence using the GIS variable is also the opposite sign (negative), and significant at a 10% level.

31. Indeed, the direct effect OLS regressions also yielded a significant, though small perverse effect of institutional presence on the likelihood of smoothing consumption and input use.

significant (significant at a 10% level), and the estimates for regressions with the GIS variable, though insignificant, agree in sign.[32]

The traditional policy variable results are not generally significant, but there is an important exception. The policy of monitoring all borrowers shows a positive effect on consumption/input use smoothing significant at the 10% level.

5.3. Starting a Business and Changing Jobs

The LEB model (and a particular interpretation of the GJ model) also predict that intermediation, or its introduction, should increase occupational mobility and entrepreneurship. The results for changing jobs are more consistent with these theories than are the results for entrepreneurship.

For occupational mobility, we find some evidence that women's groups increase job mobility among member households (Table 8), as do pledged savings accounts (Table 10). In contrast, institutions with the flexible savings accounts (associated with contraction of services) decreased occupational mobility.

In Table 8, the measured impact of women's groups on changing jobs is positive in both the 2SLS and MLE, but significant only in the MLE. The results using the GIS variable in Table 9, however, show a significant perverse impact of PCGs on changing jobs using both the 2SLS and MLE. The signs of these impacts are consistent with the results without the GIS control in Table 8.

Table 10 shows that the pledged savings accounts had significant positive effects on job mobility, while those with the less successful traditional savings accounts have negative impacts on mobility. Pledged savings accounts made households 13 percentage points more likely to change jobs, while traditional savings accounts made them 26 percentage points less likely. Since, these policies were also correlated (positively and negatively, respectively) with the growth in savings services, we interpret this as evidence that successful provision of savings services is important for job mobility.

The results for starting a business are weak and not consistent with theory. Offering emergency services in Table 10 lowered the probability of starting a business by 10 percentage points, though emergency services were associated with success. The lone significant relationship from the institutional membership regressions is in Table 9; the 2SLS estimate using the GIS control indicates that PCG membership reduces the probability of starting a business.[33] While the sign is

32. Though the 2SLS estimates with the Townsend Thai key informant data were not significant, the direct effect of the institutional presence in an OLS regression yielded a significant, though small perverse effect.

33. In addition, the direct effect regressions produced a positive significant impact of buffalo bank presence using the GIS variable. This contrasted the results for the impacts on members.

confirmed by the MLE estimate in Table 9, this sign is dependent on the inclusion of the GIS control (compare Table 8). Thus, the result is fairly weak.

The distinction between the impacts on occupational mobility and entrepreneurship is somewhat problematic. Since self-employment is quite common in the data, it is often difficult to distinguish households who have switched occupations from households who have started a business. The only agricultural enterprises that we designate as business are shrimp or fish farms, while raising new crops or livestock as the primary source of income is viewed as an occupational shift. We also have no clear explanation for the different impacts of women's groups and PCGs, except that women's groups are often geared toward teaching new occupational skills or promoting certain trades, while PCGs may be used to support more traditional agriculture.

5.4. Moneylender Reliance

As mentioned in the introduction, the most robust and salient result is that membership in institutions reduce moneylender reliance, that is, the probability of households becoming moneylender customers. We interpret this as evidence that institutions improve access to formal credit, allowing households to avoid costly borrowing from moneylenders. Table 8 shows that this negative relationship for (any) village institution is significant using either the 2SLS or MLE estimates. Table 10 also shows that the baseline effect of institutions (regardless of policy) was to lower the probability that the average household in the village became a moneylender customer by 8 percentage points.

The results on the impact on money lender reliance by institution, or by policy type, are much weaker. According to both the estimates with (Table 9) and without (Table 8) the GIS controls, PCGs and buffalo banks have no significant impacts on moneylender use.[34] The MLE estimation without the GIS controls (Table 8) showed that rice banks increased the probability of moneylender use. The significance disappeared after the GIS control was used (Table 9) and was not present in the 2SLS results.[35] Women's groups, on the other hand, do have negative impacts on the use of moneylenders according to Table 8. Both the 2SLS and MLE estimates are negative, and the MLE result is significant. The results by policy yield that pledged savings accounts decrease the probability of becoming a moneylender customer.

34. The OLS estimate of the direct effect of buffalo bank presence using the GIS data was significant and perverse.

35. The OLS estimate of the direct effect of institutional presence using the Townsend Thai data was negative (lowering moneylender reliance), though insignificant.

It should be noted that moneylender reliance was the one outcome variable for which the direct impact regressions of the form (11), which omit membership, also produced strongly significant results, significant at levels comparable to the estimates that included membership. Indeed, OLS estimates using the GIS data for buffalo banks yielded a significant direct effect of institutional presence, while the 2SLS using membership were insignificant. If these direct effect regressions are indeed picking up external effects of the institutions on the moneylender reliance of nonmembers, then our instrument for membership would be invalid and the estimated impacts of membership would be inconsistent. That credit offered to members could reduce the moneylender reliance of nonmembers is a distinct possibility, since loans to neighbors and especially relatives are not uncommon. That is, as noted earlier, we may not be measuring the effect of institutions directly on members, but rather some combination of their effects on members and nonmembers.

6. Conclusions

Our analysis of the impacts of different institutions and policies produced evidence of the micro-impacts consistent with Thailand's experience of growth and financial intermediation and in harmony with the models of occupational choice of Lloyd-Ellis and Bernhardt (2000) and financial deepening of Greenwood and Jovanovic (1990). Several of the key findings uncovered also lead to interesting considerations or areas for future research. Specifically, we have used the policies and institutions associated with the successful provision of intermediation services to identify impacts on households, but the question remains as to why certain types of institutions or institutions with certain policies are successful and others not.

For example, cash lending institutions were associated with stable or expanding provision of services. Women's groups and PCGs, the institutions that lend cash, had positive impacts on asset growth, while women's groups also promoted job mobility. The particularly strong impact of women's institutions is of considerable interest. As mentioned in Section 3, the only (observed) way that women's groups differ significantly from the other groups is their female membership. The impact findings would seem to indicate that there may be something special about gender. The finding of greater impacts for women's groups is consistent with Pitt and Khandker's (1998) study of Grameen Bank lending which found higher impacts on households for loans issued to women, than impacts for loans issued to men, and leads one to consider theories in which households do not operate as unitary families or single-agent optimizers (see Becker 1981; Bourgignon et al. (1994); and Browning and Chiappori 1998).

In contrast, rice lending institutions and buffalo banks were more likely to see reductions in services, and also had smaller, in fact, perverse impacts. The open ended answers in the institutional survey indicate that the high risk of buffalo banks, and the indivisibility of a relatively large investment relative to the scale of the institution, play a role in their unsuccessful experiences. For example, if a buffalo dies or is infertile, this greatly reduces lending, even causing failure in the case of a one-buffalo fund. Similarly, some rice banks indicated that aggregate village shocks played a role in their decline or failure.

Of the policies, providing training and advice and emergency services are most closely associated with growth of intermediation and beneficial impacts. Perhaps these are crucial auxiliary services in financial intermediation.

The special role of some savings policies is also of great interest. Using savings to evaluate loans, offering savings accounts, and savings requiring a minimum deposit are each associated with both institutional success and better consumption smoothing.

Offering pledged savings accounts (associated with growth) versus more flexible savings policies (associated with contraction) is perhaps the most important policy distinction measured. Offering pledged savings accounts[36] was the single policy associated with the largest, most significant, and most positive impacts. Institutions with pledged savings accounts promote higher asset growth rates and more job mobility. There is also evidence that they may promote business starts and reduce moneylender reliance. On the other hand, the one outcome for which the flexible policies (traditional accounts and optional savings) produced better results was in smoothing bad shocks. Though the impact of pledged accounts is still positive, the effect is smaller and much less significant than the impact of institutions with the flexible policies, where consumers could access funds more easily and presumably decide whether or not and how much of a buffer stock of savings to maintain. But again, except in the area of smoothing shocks, it appears that the more flexible policies have less impact on households than the restrictive policies, such as tying loans to savings, requiring minimum initial deposits, and, most especially, pledged savings accounts.

What might explain the importance of and different effects of these savings policies? One possible explanation comes from the behavioral economics literature (see Benartzi and Thaler 2004 and Laibson 1996, for example), where

36. It may appear puzzling that savings that cannot be accessed, given the restrictive nature of pledged savings accounts, could have strong effects on outcomes. The limited access to pledged savings, however, is somewhat overstated. First, loans are often linked to the amount of savings (and this policy is associated with positive impacts as we have seen). That is, the funds can be effectively accessed through loans. (Lending itself was not associated with impacts, however, but this may be due too little variation in this policy since most institutions made loans.) Secondly, savings might be used as collateral for loans from other sources, since virtually all of the survey villages use multiple sources of credit. Finally, savings can be used as collateral for others via cosigned loans.

it has been argued that internal conflict and time inconsistencies cause people to save less than they would like to, and savers would actually like to bind themselves to higher forced savings rates. A related explanation is that these conflicts or time inconsistencies may be internal to the household, but not the individual. Pledged savings accounts may then be a commitment technology in household bargaining between members who differ in their level of impatience or desired savings. A second possible explanation is that the growth of the institution drives the impacts on households. Pledged savings plans seem to have several organizational and accounting advantages over standard savings accounts (Kaboski and Townsend 2000). Organizationally, infrequent deposit and withdrawal times allow funds to avoid either the (crime and interest) costs of holding large amounts of money in the village or the transportation costs of repeated trips to the formal, outside bank that holds the savings. In addition, pledged savings accounts (often only allowing a standard pledge rate) allow for very simple accounting procedures and so self-managed funds may be easier to maintain.[37]

The paucity of results on the impacts of policies traditionally mentioned in the literature also leave open paths for future research. We do not view our findings as strong evidence against the importance of these policies. Indeed monitoring may facilitate risk sharing. More generally, there are several caveats. Our data showed little variation in these policies, especially in the policies of providing lending services, requiring frequent payments, monitoring frequently, and monitoring every loan. Furthermore, there may have been a great deal of measurement error in all of these policies.[38] While we do not view the negative results on these traditional microfinance policies as strong evidence against the importance of these policies, they do help to highlight the potential importance of the policies that do show strong results.

We hope that these findings can lead to not only future research, but specific recommendations to Thai policymakers and microfinance practitioners more generally.

37. A further possible explanation is that benefits may not be altogether positive—institutions may be forcing households to save at a higher-than-desired rate. Of course, in the case of requiring an initial deposit, the policy was positively correlated with membership growth, so one might wonder why people are joining if the institutions are welfare reducing.

38. Three examples of possible measurement issues are: (1) lending services is a simple binary variable and allows for no measure of the intensity of credit provision; (2) loans that do not require collateral but link loans to savings are (at least partially) collateralized in effect, but we designate them as not requiring collateral; and (3) the frequent payments dummy variable was constructed using one payment as a cutoff. That is, any loan that required a payment before the loan was due in full was considered to require frequent payments. The same is true for loan monitoring. These weak conditions were necessary in order to get any meaningful variation in the data but do not perfectly match the ideas of frequent payment and monitoring that have been the focus of the literature.

Appendix A

TABLE A.1. Percentage distribution of occupational changes over past six years.

New occupation of household head / Old occupation of household head	Rice farmer	Farmer, other crop	Shrimp farmer	Construction	Business/ Skilled trade	Professional/ Administrative	General worker, Cleaner/Janitor	Other	Total
Inactive/No occupation	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.2	0.2
Rice farmer	0.2	7.1	7.3	6.7	7.9	0.8	4.7	2.8	37.5
Farmer, other crop	1.4	15.4	2.8	1.0	3.9	0.2	1.8	1.4	27.8
Shrimp farmer	0.0	1.2	2.0	0.2	0.6	0.0	0.0	0.2	4.1
Construction	0.2	0.6	0.4	0.2	1.0	0.2	0.4	0.4	3.4
Business/Skilled trade	1.4	1.2	2.0	0.8	4.9	0.4	1.2	0.6	12.4
Professional/Administrative	0.0	0.6	0.0	0.0	0.6	0.4	0.4	0.4	2.4
General worker, Cleaner/Janitor	0.8	1.8	1.0	1.8	1.6	0.0	0.8	0.6	8.3
Other	0.8	1.0	0.0	0.2	1.2	0.0	0.4	0.4	3.9
Total	4.7	29.0	15.4	10.9	21.7	2.0	9.7	6.9	100.0

Note: Positive entries in diagonal elements indicate within category occupational transitions or multiple temporal transitions.

TABLE A.2. Percentage distribution of occupational changes over past six years, only institution member households in 1990.

New occupation of household head Old occupation of household head	Rice farmer	Farmer, other crop	Shrimp farmer	Construction	Business/ Skilled trade	Professional/ Administrative	General worker, Cleaner/Janitor	Other	Total
Inactive/No occupation	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0
Rice farmer	0.0	17.0	6.4	2.1	12.8	4.3	4.3	2.1	48.9
Farmer, other corp	0.0	6.4	4.3	0.0	2.1	2.1	0.0	2.1	17.0
Shrimp farmer	0.0	0.0	4.3	0.0	2.1	0.0	0.0	0.0	6.4
Construction	0.0	2.1	0.0	0.0	0.0	0.0	0.0	0.0	2.1
Business/Skilled trade	0.0	0.0	0.0	0.0	6.4	0.0	0.0	0.0	6.4
Professional/Administrative	0.0	2.1	0.0	0.0	2.1	2.1	0.0	4.3	10.6
General worker, Cleaner/Janitor	0.0	0.0	0.0	4.3	0.0	0.0	0.0	0.0	4.3
Other	2.1	0.0	0.0	0.0	0.0	0.0	2.1	0.0	4.3
Total	2.1	27.7	14.9	6.4	25.5	8.5	6.4	8.5	100.0

Note: Positive entries in diagonal elements indicate within category occupational transitions or multiple temporal transitions.

Appendix B

Here we present the log-likelihood functions used in the MLE. We look first at those used for the regressions with binary outcomes (reducing consumption/input use in a bad year, becoming moneylender customer, starting a business, and changing jobs). The continuous outcome variable for asset growth is developed last.

B.1. Binomial Evaluation Criteria

Recall that y_n and M_n are indicator functions for the binary impact and membership, respectively. The observations for impact and membership are binary events, and so there are thus four combinations of possible observations. Denoting the CDF of the bivariate standard normal as $\Phi_2(.,.;.)$, the log-likelihood function for the entire population (for all combinations) can be succinctly written: from equations (9), (8), and (10):

$$\ln \pounds = \sum_{n=1}^{N} \ln \Phi_2 \left(\begin{array}{c} (2y_n - 1)\left\{ \sum_i \alpha_i X_{i,n} + \sum_j \tau_j Z_{j,n} + \beta M_n \right\}, \\ (2M_n - 1)\left\{ \sum_j \gamma_n X_{j,n} + \delta I_n \right\}; \rho \end{array} \right). \quad (15)$$

Given this model, the log-likelihood function is now correctly specified. Hence, maximum likelihood estimation is consistent and efficient. We present the estimates of the coefficients themselves, since measures of marginal probability were often problematic when evaluated at the sample means. The actual regressions were performed using the *biprobit* subroutine in Stata 6.0.

B.2. Continuous Evaluation Criterion

For assets, the evaluation criterion is not binary, but continuous. In this case, we interpret the equation for y_n to be the actual criterion (asset growth) instead of merely an index. The stochastic component of this equation, $u_{y,n}$ can no longer be simply normalized to have a variance of one. We therefore consider a general bivariate normal function:

$$(u_{y,n}, u_{m,n}) \sim \text{Bivariate Normal}(0, 0, \sigma_y, \sigma_m, \sigma_{ym}). \quad (16)$$

Since the membership equation is still just an index based on whether the index is greater than zero or not, we are still free to normalize $\sigma_m = 1$. We thus write

(16) without loss of generality:

$$(u_{y,n}, u_{m,n}) \sim \text{Bivariate Normal}(0, 0, \sigma_y, 1, \rho). \tag{17}$$

The likelihood function for assets can be written:

$$\mathcal{L} = \left[\prod_{n=1}^{N} P\left(M_n^* > 0 | y_n^*\right) P\left(y_n^*\right)\right]^{M_n} \left[\prod_{n=1}^{N} P\left(M_n^* \leq 0 | y_n^*\right) P\left(y_n^*\right)\right]^{1-M_n}. \tag{18}$$

In words, the joint probability of the survey results is the probability of all members being members given their asset growth levels y_n^* times the probability of their asset growth rates, while the second product is the counterpart for nonmembers. The log-likelihood is naturally:

$$\log \mathcal{L} = \sum_{n=1}^{N} \left\{ \begin{array}{l} M_n[\ln P\left(M_n^* > 0 | y_n^*\right) + \ln P\left(y_n^*\right)] + \\ (1 - M_n) \ln P\left(M_n^* \leq 0 | y_n^*\right) + \ln P\left(y_n^*\right) \end{array} \right\} \tag{19}$$

$$= \sum_{n=1}^{N} \ln P\left(y_n^*\right) + M_n \ln P\left(M_n^* > 0 | y_n^*\right) + (1 - M_n) \ln P\left(M_n^* \leq 0 | y_n^*\right). \tag{20}$$

The unconditional density of y_n^* is simply a normal density function with standard deviation σ_y. Given equation (8) this is just:

$$P\left(y_n^*\right) = \phi\left(y_n^* - \left[\sum_i \alpha_i X_{i,n} + \sum_j \tau_j Z_{j,n} + \beta M_n\right]; \sigma_y\right). \tag{21}$$

With a bivariate normal where $\sigma_m = 1$, the conditional distribution of $u_{m,n}$ (given $u_{y,n}$), is normal with mean:

$$\frac{\rho}{\sigma_y}\left(y_n^* - \left(\sum_i \alpha_i X_{i,n} + \sum_j \tau_j Z_{j,n} + \beta M_n\right)\right)$$

and variance $1-\rho^2$. Using this distribution, along with equations (21), (8), and (9), yields the final log likelihood function:

$$\ln \mathcal{L} = \sum_{n=1}^{N} \ln \phi \left(y_n^* - \left[\sum_i \alpha_i X_{i,n} + \sum_j \tau_j Z_{j,n} + \beta M_n \right] ; \sigma_y \right)$$

$$+ \sum_{n=1}^{N} M_n \ln \Phi \left(\frac{\sum_j \gamma_n X_{j,n} + \sum_j \phi_j Z_{j,n} + \delta I_n + \frac{\rho}{\sigma_y} \left(y_n^* - \left(\sum_i \alpha_i X_{i,n} + \sum_j \tau_j Z_{j,n} + \beta M_n \right) \right)}{1-\rho^2} \right)$$

$$+ \sum_{n=1}^{N} (1-M_n) \ln \Phi \left(\frac{\sum_j \gamma_n X_{j,n} + \sum_j \phi_j Z_{j,n} + \delta I_n + \frac{\rho}{\sigma_y} \left(y_n^* - \sum_i \alpha_i X_{i,n} - \sum_j \tau_j Z_{j,n} - \beta M_n \right)}{-(1-\rho^2)} \right).$$

The first summation is the log-likelihood of observed sample of asset growths, the second summation is the log-likelihood of observing members given asset growth, and the final summation is the log-likelihood of observing nonmembers given asset growth. The actual estimation is carried out using the *treatreg* command in Stata 6.0. □

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