The Impact of Credit on Village Economies

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This paper evaluates the short- and longer-term impactd of Thailand's 'Million Baht Village Fund' program, among the largest scale government microfinance iniative in the world, using pre- and post-program panel data and quasi-experimental cross-village variation in credit-per-household. We find that the village funds have increased total short-term credit, consumption, agricultural investment, income growth (from business and labor), but decreased overall asset growth. We also find a positive impact on wages, an important general equilibrium effect. The findings are broadly consistent qualitatively with models of credit-constrained household behavior and models of intermediation and growth.
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While the impacts of financial intermediation have been well studied at the macro-level, a criticism of some of this literature is that intermediation is endogenous.[1] We study a microfinance program that induced smaller though still substantial increases in intermediation with an important degree of exogeneity. This exogeneity makes the villages "test tube"-like experiments for studying the impacts of microcredit and phenomena important to macro-economies more broadly, including general equilibrium (GE) effects.

The program we examine is Thailand's Million Baht Village Fund Program, among the largest-scale government microfinance initiative of its kind. The intervention injected potential funds into 77,000 heterogeneous Thai villages[2] Each transfer of one million baht (about $24,000) was used to form an independent village bank for lending within

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[1] Earlier influential work by King and Levine (1993) establishes correlations between growth and private sector intermediation. Rajan and Zingales (1998) is an attempt to establish causality. Aghion, Howitt and Mayer-Foulkes (2005) models the non-linear relationship between financial intermediation on convergence. Townsend (2010) gives a very detailed analysis of the Thai experience of growth with increased financial intermediation.

[2] The Thai program involves approximately $1.8 billion in initial funds, or about 1.5 percent of Thai GDP in 2001. This injection of credit into the rural sector is much smaller than Brazilian experience in the 1970s, which saw a growth in credit from about $2 billion in 1970 to $20.5 billion in 1979. However, in terms of a government program implemented through village institutions and using micro-lending techniques, the only comparable government program in terms of scale would be Indonesia's KUPEDES village bank program, which was started in 1984 at a cost of $20 million and supplemented by an additional $107 million in 1987 (World Bank (2001)).

the village. Every village, whether poor or wealthy, urban[3] or rural, was eligible, and all villages in our data did indeed receive the funds. Across our sample, the transfers averaged twelve percent of total annual income in the village economies, and forty-one percent of total short term credit flows.

Two crucial elements of the structure of the Million Baht program gave the transfers a (plausible) degree of exogeneity. First, the program was a rapidly introduced "surprise" policy initiative. In November 2000, the Thai Parliament was dissolved, and by January 2001, the populist Prime Minister Thaksin Shinawatra was elected. The new policy was implemented quite rapidly with all our survey villages receiving the funds between the 2001 and 2002 survey rounds. Second, there is strong variation in the intensity of the credit injection in the cross-section of villages. Specifically, each village received the same amount – one million baht – regardless of the population of the village, so smaller village economies received a relatively more intense injection of credit. For example, the million baht transfer injection averaged 27 percent of income for the lowest quintile (i.e., smallest) village economies, and less than 2.5 percent for the top quintile (i.e., largest) village economies.

We therefore instrument for the amount of credit received using interactions of the program years and the number of households in a village as instruments, which we believe to be exogenous. *A priori* the variation in inverse number of villages in our data is among small villages, between 50 and 250 households (though our results are robust to including larger and smaller villages). Second, villages are geopolitical administrative units, and it is not uncommon for villages to be split for administrative purposes. Finally, while inverse village size is strongly related to outcomes in the years of the program, there is no significant pattern between inverse village size and either village fund credit or the outcome variables in the years before the program. That is, after controlling for household characteristics, villages look very similar until the program is instituted.

It is important to keep in mind that each village we consider is in many ways its own small economy, and so it matter where a person lives. Specifically, the village economies are open economies, but not identical and not entirely integrated with one another or the broader economy (nearby provinces, regions, etc.). There is substantial variation in institutional and market arrangements across villages (Townsend (1995)). Certainly informal borrowing and lending within the village is more common than across village lending, and there is cross village variation in interest rates and the amount of credit.[4] Even labor markets are not entirely integrated with local wages varying considerably across villages.[5] Finally, risk sharing may vary. The household-specific fixed effects we use attempt to control for much of this heterogeneity, but because village are small

[3]The village (*moo ban*) is an official political unit in Thailand, the smallest such unit, and is under the sub-district (*tambon*), district (*amphoe*), and province (*changwat*) levels, respectively. Thus, "villages" can be thought of as just small communities of households that exist in both urban and rural areas.

[4]The ratio of the number of loans to relatives within vs. outside of the village is 2:1, for non-relatives this ratio is 3:1 and interest rates are much lower on within-village loans. Small loans are less likely between households in different villages (Kaboski and Townsend (1998)).

[5]For each village in Thailand, we have a reported average wage in the village from the Thai Community Development Department. Among the four provinces (*changwats)* we examine, the within-province coefficient of variation in average daily wage across villages ranges between 23 and 41 percent.

(quasi-open) economies, we anticipate movements in quantities and prices that vary with the size of intermediation.

The Townsend Thai dataset we use has unique advantages. It contains eleven years (1997-2007) of panel data on 960 households in 64 rural and semi-urban villages across four provinces of Thailand. These data include information on: education; assets and investment; income; borrowing and saving through various forms; consumption; occupation; household composition; and other variables. The first five years of data give us a "before" picture of the environment, while the remaining years give us the ability to look at the effect of the program on levels and growth rates of relevant outcome variables. We use the first two relatively short "after" horizon gives us a window for examining the impacts of credit on villages, at a time when these impacts were still localized, as we verify. The full six years of post-program data are then used to discern long run impacts, and indeed this paper is the only study of the long run impacts of microfinance. Finally, a smaller monthly panel with only 16 villages has separate information on wage rates.

Methodologically, we run two-stage regressions using short-term village fund credit as a measure of treatment. The major impacts we examine are the effect of the new village institutions on (other and total) credit, saving and investment decisions, consumption, asset growth, income and income sources, wage rates, and business enterprise.

A. Findings in Light of Theory

Our analysis is motivated by two broad classes of theories on credit constrained environments: buffer stock models and entrepreneurship and growth models.

In the classic buffer stock savings model, households accrue buffer stocks of liquid assets in response to the borrowing constraints and income uncertainty they face. These theoretical features appear to characterize the data, but we also note that default is not uncommon (average credit in default is about 12 percent of average income), and households also make lumpy and illiquid physical investments that tend to pay higher returns than earned on liquid savings. In our companion paper, Kaboski and Townsend (2011a), we incorporate these features into an explicit structural model, which we then estimate and quantitatively simulate the Thai Million Baht intervention. Many of the findings here are broadly consistent with this class of model.

First, the availability of credit increased total borrowing, and so crowding out of or substitution away from other sources was not a major issue. Indeed, we cannot reject the null hypothesis that credit increased one-for-one with the injection of available credit. At the same time, average interest rates on short-term credit did not fall but may have actually risen slightly. This can be viewed as evidence that households were originally credit constrained, since credit increased even though interest rates did not fall. Thus, similar to Banerjee and Duflo (2008), households are not merely substituting toward lower cost credit or expanding borrowing in response to lower borrowing costs. Credit for the stated purpose of consumption is the primary type of borrowing that increased, however.

Second, and related, consumption increased substantially, perhaps one for one with credit, which indicates credit constraints are particularly binding in consumption deci-

sions. The surprising magnitude of such an increase in consumption is consistent with buffer stock models, where the ability to borrow has large effects on consumption by increasing consumption among both currently constrained borrowers but also the unconstrained, who are impacted by the potential to borrow in the future.[6] The composition of consumption increases is also of interest. Grain, clothes, tobacco, ceremony, and educational expenditures were stable, but credit increased expenditures on household and auto repair, meat, and alcohol. The more typically income elastic components of consumption or those with an intertemporal element (like repairs) responded the most to credit. The increase in fuel usage and auto repairs harmonizes with Karlan and Zinman (2010*a*)'s findings of increased transportation expenditures for consumer loans in South Africa.

The consumption and credit results are *not* consistent with an alternative story, in which households simply viewed the village fund transfers as a grant or aid program. For consumption, this story would predict that, absent credit constraints, households would only consume the return on this one-time, transitory income shock rather than the full amount of the grant. However, in the initial years, we observe consumption increasing more than one-for-one with the size of the credit injection. Moreover, the loans could only be a substantial gift if they were not repaid. Credit from the program persisted at or above initial rates throughout the six post-program years we examine, however, and the fraction of village fund credit in default was low: less than four percent each year.

Furthermore, looking at the longer run data, while village fund credit and short-term credit grew throughout the sample, the positive impacts of village fund credit on consumption and income growth were confined to the initial years of the program. These transitional impacts are qualitatively consistent with the dynamics in buffer stock savings model as in Fulford (2011) and our companion paper. Moreover, default (on all types of credit) did increase, but in a way consistent with the bufferstock story. Specifically, it did not increase in the first year, when more credit was available, but only in later years when loans need to be repaid.

The second broad class of models motivating our analysis are models of macro-intermediation, entrepreneurship and growth (e.g., Lloyd-Ellis and Bernhardt (2000), Greenwood and Jovanovic (1990), Banerjee and Newman (1993), Buera and Shin (2010), Buera, Kaboski and Shin (2011*a*), and Buera, Kaboski and Shin (2011*b*)). Such models have been shown to perform relatively well in fitting the long run Thai growth experience (see Felkner and Townsend (2011), Giné and Townsend (2004), Jeong and Townsend (2008), and Townsend and Ueda (2006),Townsend and Ueda (2011)). In these models, improvements in intermediation on the extensive and/or intensive margin can spur business or agricultural investments and growth in business income.

The implied connection between access to finance, entrepreneurship, and growth is often a central motivation for microfinance programs as poverty alleviation interventions. Microfinance programs typically cater to poor people who lack access to other forms of intermediation in the hope that the poor are financially constrained and have high returns to investment. Women, in particular, are often targeted under the belief that they have

[6]The fact that informal credit and household lending did not respond, however, indicates that relending to nonborrowers, as in Angelucci and De Giorgi (2009), is not a major issue.

less access to credit, lower outside options in the labor market, and therefore the highest returns to private entrepreneurship.

The results here under a quasi-experimental intervention are mixed with regards to the predictions of these models. On the one hand, we indeed measure significant increases in income growth and a change in the composition of income as a result of the intervention. As the models would predict, business and labor market income tended to increase, but agricultural income did not. On the other hand, business and labor income did not seem to be driven by the extensive margin of investment and business starts themselves. To the contrary, we find no change in business starts or business investment, and some evidence of an actual decline in assets in response to the program. We do see an increase in the frequency of agricultural investments, but a reduction in the use of fertilizer and, again, no increase in agricultural income.

Theoretically, several potential explanations could reconcile these findings, but our ability to evaluate these empirically is unfortunately limited. First, we may simply have difficulty discerning investments given our sample size, since investment is highly variable and infrequent (e.g., business starts). In the simulations of the structural model in our companion paper, the actual positive impacts on investment cannot be typically discerned given our sample size. Second, households report both increased labor income and higher payments to outside laborers in response to the program. Perhaps credit was most useful as working capital, allowing businesses and farms to hire more laborers and potentially use more intermediate inputs. That is, perhaps it is the intensive margin, and access to working capital, rather than fixed entry costs that most constrain households in their business activities. McKenzie and Woodruff (2006) offer complementary evidence that fixed costs in Mexico are negligible, yet they find high average returns. Their experiments in Sri Lanka (McKenzie, de Mel and Woodruff (2008)) also find high returns to increases working capital among entrepreneurs. Our measures of inputs (fertilizer, wages paid) do not uncover this, but again data are limited here. A third possibility is that credit offers consumption-smoothing, cashflow management, and/or limited liability, which, for a given level of investment, can change the composition of investment and labor decisions toward higher risk but higher yield sources of income a la Greenwood and Jovanovic (1990) and Braverman and Stiglitz (1986). Indeed, the buffer stock model of our companion paper, predicts a decline in low return liquid assets (along with a move toward high return investment). Evaluating this conjecture on the composition of investment is difficult, however, since measuring second moments of returns on disaggregated investments is non-trivial.

A fourth potential explanation, which we can evaluate, is that the program caused a GE increase in wages, a common implication of many of the macro-intermediation, TFP, entrepreneurship, including the Thai research of Giné and Townsend (2004) and Jeong and Townsend (2007), and many of the other growth models above.[7] As an example, Buera, Kaboski and Shin (2011*b*) predict that microfinance will lead to a more efficient distribution of capital and entrepreneurs in the economy, and therefore an increased demand

[7] It can also lead to higher interest rates by expanding the demand for capital while reducing the capital stock. Our point estimates on interest rates are positive but insignificant.

for labor. Yet, the resulting higher wages greatly limits the aggregate increase in entry and investment. They further argue that the same increase in wages may lead to lower savings/higher consumption because it redistributes from households with high savings rates to those with low saving rates.

Thus hard-to-measure GE effects are central to theory, but here the sheer scale of the intervention and the partial segmentation of labor markets across villages allow us to discern impacts on wages. We find that wage rates increase overall with the point estimate implying an increase of seven percent in the median village during the first two years (the period for which we have wage data). Consistent with expectations from theory, the wages increase for general non-agricultural labor, construction in the village, but not for professional occupations or occupations outside of the village.

B. Existing Literature on Microfinance

A growing, yet still relatively small, literature has arisen to evaluate the booming field of microfinance. The advantages of this study relative to much previous work on microfinance interventions are essentially five-fold. First, the program is unique because of the size of the intervention and its consequent policy importance. A key policy question is the extent to which smaller programs can be scaled up for larger scale poverty reduction, or whether large scale increases in credit availability might hamper the programs (Duflo (2004), World Bank (2004), Buera, Kaboski and Shin (2011*b*)). Second, as stated earlier, the size of the intervention and the segmented credit and labor markets yielded GE effects both within the village economies.[8] Microevaluations have great difficulty identifying GE effects, since they require relatively large interventions and also because they impact the control group. Again, these impacts are important for scaling up and also give insights into the micro-mechanisms behind macro-theory. Third, we have data on households and small enterprises, and the relevant variables necessary to consider potential channels of impact in an environment of local, household-level investment and occupational choice decisions. Fourth, the program design produced a convincing, exogenous instrument for evaluation. Our exogeneity has both a cross-sectional and timing element, which is important since impacts may vary over time. Finally, and related, we have long run data extending six years after the program implementation which allows us to shed light on long run impacts.

This paper is closely related to our already mentioned companion paper, which presents an analysis of the short-run impact on four key outcomes (consumption, investment, income and default) using a partial equilibrium structural model. Methodologically, this paper is distinct in that we take a more reduced form approach here, which allows us to delve more deeply into the data. We also apply stronger tests of orthogonality of village size before the program and control for geographic spillovers. Topically, we evaluate a greater range of outcomes (including the credit market and subcomponents of consumption, income, and investment and productive activities) and assess the differential impact

[8] In principle, aggregate (economy-wide) general equilibrium effects would not be identified by our methods. However, since the general equilibrium impacts we find do not seem to extend to neighboring villages (see Section II.F), we do not think that general equilibrium impacts at an even wider scale are a major issue over the time span we examine.

on women. Moreover, our analyses of GE impacts on wages and long run impacts are also unique to this paper, and this is the only paper known to provide evidence of the impacts of microfinance along these two dimensions.

Of course, our paper contributes to an existing literature that includes many of five advantages above, though not simultaneously. Boonperm, Haughton and Khandker (2009) studies the same intervention with a larger data set, but they lack data prior to the intervention of the program. They confirm short-run increases in income and expenditures that we find. Karlan and Zinman (2010*a*), Karlan and Zinman (2010*b*) study true controlled experiments in which a financial institutions randomized loan decisions on consumer loans to wage-earners or microenterprise loans to entrepreneurs. Pitt and Khandker (1998) study the Grameen Bank, using cutoff participation requirements as an instrument, an instrument questioned by Morduch (1998). They have a cross-section, larger than ours, with four outcomes: labor supply, child schooling, female assets, and expenditure. The amount borrowed is quite large relative to expenditures per household. Pitt et al. (2003) studies the same program, but examines biometric health outcome measures. Burgess and Pande (2005) also study a big program, but it is an expansion of banks over twenty years differentially across regions in India. Their outcomes are macro-level poverty headcount and wage measures. Coleman (1999) studies much smaller NGO lending in Thailand using a smaller dataset of about 500 people, but with a great variety of variables. He has a set of villages with programs and a set that will receive them in the future. This is a fairly good control, but there is no exogeneity in the timing of how long the program has been used, and he examines only short-term effects. Gertler, Legine and Moretti (2009) study BRI in Indonesia to see if microfinance helps insure against shocks to health. They have an instrument with less clear exogeneity (proximity to financial institutions), but also a fairly large panel data set (the IFLS). Alem and Townsend (2010) use a similar instrumental approach to study the impact of financial institutions on risk-sharing. Banerjee and Duflo (2008) study firm's borrowing from banks but not household borrowing. Aportela (1999) looks at the expansion of bank branches and argues it is exogenous. In any event it is a smaller expansion, and he looks only at savings behavior. Finally, but not least, our results complement the results of Banerjee et al. (2009)), who use experimental data in India. They find higher entry into entrepreneurship and sizable income effects on owners of existing businesses but increases in consumption for households not in business.

Clearly, the exogeneity of our instrument (the inverse number of households in a village interacted with program years) is a critical argument in our analysis. We present a priori justification for its exogeneity in Section I, which also discusses the program and data in more detail. Section II lays out our methods, explicitly states our exogeneity assumption, and gives *empirical* support for the exogeneity of the instrument. Section III then presents the results, while Section IV concludes.

I. Description of Program and Data

We provide an overview of the Million Baht Village Fund, including its quasi-experimental implementation, and then describe the data.[9]

A. Overview of Million Baht Program

The fund was a key program in Prime Minister Thaksin's election platform. The primary hope was that the money would be a revolving, self-sustaining fund to be used for investments in occupational development, employment creation and income-generating activities. It was promoted as an attempt to reach the underprivileged, alleviate the dependence of villages on government aid, develop a decentralized grass roots approach to growth, and link communities with government agencies and the private sector.

The program was funded by the central government. While it is difficult to know precisely how the program was funded, it clearly entailed a substantial transfer from Bangkok to rural areas in line with the populist goals of the government. For example, the households in the rural areas pay little to no taxes.

The transfers were given to the villages with both carrot and stick provisions to encourage sound management and repayment of loans. The stick involved telling villages that if the funds were abused or the village institutions failed, they would be offered no further assistance, and even other sources of government funding would be cut off.[10] The carrots were the promises of additional loans and additional grants to village funds that receive their highest rating. In 2004, loans from the Bank of Agriculture and Agricultural Cooperatives (BAAC) were first available but take up rates were quite low. In 2005, funds with the highest rating were granted an additional 100,000 baht (de La Huerta (2011)). Thus, these subsequent injections, which took place after the focus of most of this study, were small relative to the initial injection but did provide incentives for responsible management.

ORGANIZATION AND FOUNDING. — The program was jointly administered by multiple government agencies. In the rural and semi-urban areas we study, the BAAC received the initial money transfer and held both the lending and savings accounts for the village funds.[11] Officers from the Community Development Department provided oversight and guidance, as they do with other village funds. Local teaching colleges were in charge of conducting audits of the village funds as well as an evaluation of the funds and member

[9] This overview is based on data from the institutional panel data set, as well government materials and informal interviews of village funds committee members, Community Development Department (CDD) officers, and Bank for Agriculture and Agricultural Cooperatives (BAAC) officers and administrators in March, 2002. BAAC administrators were interviewed in Bangkok, while three branch officers, a CDD officer, and six village fund committees were interviewed in Buriram, Chachoengsao and Chiangmai.

[10] This threat was not completely credible, which is especially clear since Thaksin is now deposed, but based on interviews it seemed to at least be an important issue to villagers.

[11] Each village fund holds two accounts, the first for receiving the million baht transfer and the second for holding member savings. When a loan is granted by the village fund, the member takes a form signed by committee members to the BAAC, and the loan amount is transferred from the fund account to the individual account.

households. These audits are in addition to the BAAC's own fund ratings mentioned above.[12]

In order to receive funds, villages needed to form committees, develop policies, submit an application/proposal for the village fund, and have the proposal evaluated and accepted.[13],[14] The vast majority of village households became members of the village funds and village funds averaged 94 members.[15] The committees were selected democratically by the villagers at a village meeting, with regulations set up to ensure fairness of these elections.[16]

Although a federal program, the village funds themselves are only quasi-formal, in the sense that they have no building or facility and no employees.[17] They are administered at the village level by a committee elected by the village and by occasional meetings of all villages.[18] Such quasi-formal village institutions are typical in Thailand (Kaboski and Townsend (2005)). One villager is appointed as an accountant/bookkeeper, and the accounting is fairly detailed, including dated records of all loans, payments, deposits and withdrawals.[19]

POLICIES. — Some savings and lending policies were stipulated, while others were set by the villages themselves, often based on the suggestions from printed materials or suggestions from CDD officers.

For lending, the fund was typically divided into two portions: 900,000 baht for standard lending, and 100,000 baht for emergency loans,which were typically smaller and

[12] We, the authors, tried to assist BAAC officials in the development of this rating system.

[13] Government agencies provided villagers with informal advice and manuals describing the goals, procedures and regulations of the village funds. In addition, the appendix contained an example of the policies of a village fund. Although these policies were shown as an example, from interviews, it appears that many committees felt that these suggested policies were fixed regulations for all funds, and also some policies were misinterpreted (de La Huerta (2011)).

[14] The applications in our survey villages were submitted to the BAAC and evaluated first by an district (*amphoe*) level sub-committee with final approval from the national fund committee. The evaluation criteria included: the selection of the fund committee; the qualification of the fund committee including its knowledge, experience and management ability; the policies and regulations of the fund; the extent of participation of villagers and members in the funds management; and the compliance with fund regulations.

[15] The primary membership criteria for most institutions was to live in the village. Non-member households typically did not want to borrow, and two reasons were often given: either the households were wealthy and did not need the money or wanted to leave the funds for poorer households, or the households were poor and did not want to get into more debt.

[16] The village meeting required 75 percent of households in the village for a quorum. By regulation, the committee needs to consist of 9 to 15 villagers, with half of them women. Requirements were that committee members be at least 20 years old, have lived in the village for at least two years, be a person of good character (e.g. no gamblers or drug users), not be bankrupt, never have been imprisoned or have violated position or property, not have been evicted from the government or a state enterprise, have maintained the right to vote, and never have been evicted from the fund committee. Committee members can serve a maximum of two years with half of the committee members being replaced each year.

[17] According to the sample regulations, committee members were by regulation allowed to divide ten percent of the fund profits among themselves as compensation for their work. Few of the funds surveyed compensated committee members, however.

[18] While a general meeting of fund members is required to take place at least once a year, only 85 percent of the funds interviewed reported having these general meetings. The committee plays the primary administrative role in the fund and typically reported meeting one to two times a year to evaluate loan applications.

[19] Instruction manuals of accounting procedures were provided by various government agencies. These manuals were roughly 50 pages, and while groups noted that the accounting was tedious, complicated and difficult, none claimed that it was unmanageable.

shorter term.[20] According to the institutional survey, village funds lent out on 950,000 baht in the first year, and according to the household data lending increased about 22 percent from the first to the second year. In order to ensure equal access to the funds, regulations stipulated a maximum loan size of 20,000 baht.[21] Loans above this amount require approval by all members of the fund, but loans were not supposed to exceed 50,000 baht (about $1100) regardless. Less than five percent of loans exceeded 20,000 baht, but we do observe four households with loans exceeding 50,000). The repayment period could not be set longer than one year. In addition, villagers claim that they were required to charge a positive rate of interest on loans. Village funds set a standard rate to all borrowers, but these interest rates varied from two to twelve percent across funds, with an average nominal interest rate of seven percent. Another suggested policy that was generally adopted was the use of two guarantors for loans, though the number of guarantors required ranged from one to eight across the sixty-four institutions.[22] Only eleven of these institutions required collateral, and only three had fully collateralized loans. Repayment was quite high. According to the household data, using a 90-day definition, default rates to the village funds were quite low (see Table 13).

Committee members typically were to decide who receives loans. The evaluation of the loans included the members' ability to repay, the appropriateness of the investment, and the amount requested. Given the small loan sizes, institutions made a large number of loans, and a large fraction of households received loans. In the eleven-year balanced panel, 76 percent of households received loans at some point and the median number of years with village fund loans is four.

Seventy percent of the village funds also offered savings services, with most of these requiring that members save and make pledged deposits into their accounts. Members' savings are jointly held in a separate (individual) BAAC savings account. One suggested set of savings regulations that was often followed was that all members must pay an application fee, and buy at least one, but not over 20 percent of shares in the fund. Another suggestion was pledged savings funds with the following policies: deposits are made on a given date, pledged amounts varying from 10 to 500 baht across members, and pledge amounts able to be changed once a year. The average nominal interest rate on savings was just 0.5 percent, that is, a negative real interest rate. The total stock of initial savings averaged about 4000 baht across funds. Some funds lent out member savings, while others limited the loans to the initial transfer.

B. Quasi-Experimental Design of the Program

As described in the introduction, the program design was beneficial for research in two ways. First, it arose from a quick election, after the Thai parliament was dissolved

[20] Many funds claimed this was a requirement of the program, but again it appeared to only have been an element of the sample village fund regulations.

[21] About 35 percent of all loans are of this maximum size.

[22] Other suggested policies that were often adopted: a late payment penalty of 0.5 percent per day, a duration for emergency loans that was less than one year, and no future loans in the event of default. de La Huerta (2011) finds that the latter policy was associated with lending growth and repayment.

in November, 2000, and was rapidly implemented in 2001. None of the funds had been founded by our 2001 (May) survey date, but by our 2002 survey, each of our 64 village had received and lent funds, lending 950,000 baht on average. Households would not have anticipated the program in earlier years.[23] Second, the same amount was given to each village, regardless of the size, so villages with fewer households received more funding per household. Regressions below report a highly significant relationship between household's credit from a village fund and inverse village size in 2002 after the program.

There are strong *a priori* reasons for expecting this variation in inverse village size in the years of the program to be exogenous with respect to important variables of interest.

First, villages are geopolitical units, and villages are divided and redistricted for administrative purposes. These decisions are fairly arbitrary and unpredictable, since the decision processes are driven by conflicting goals of multiple government agencies. (See, for example, Puginier (2001) and Arghiros (2001)). Data for the relevant period are unavailable, but between 2002 and 2007 the number of villages increased by three percent, while since 1960 the number of villages increased by roughly 50 percent.

Second, because inverse village size is the variable of interest, the most important variation comes from a comparison among small villages (e.g., between 50 and 250 households). Indeed, we focus our baseline estimates on these villages, but show that results are quite robust to including the whole sample. That is, our analysis is not based on comparing urban areas with rural areas, and we are not picking up the effects of other policies biased toward rural areas and against Bangkok.

Third, village size is neither spatially autocorrelated, nor correlated with underlying geographic features like roads or rivers. Figure 1 shows the random geographical distribution of villages by decile of village size over the four provinces (Chachoengsao, Lopburi, Buriram and Sisaket) in the year 2001. The Moran spatial autocorrelation statistics in these provinces are 0.019 (standard error of 0.013), 0.001 (0.014), 0.002 (0.003), and 0.016 (0.003), respectively.[24] Only the Sisaket autocorrelation is statistically significant, and the magnitudes of all of them are quite small. For comparison, the spatial autocorrelation of the daily wage in villages ranges from 0.12 to 0.21. We also checked whether village size was correlated to other underlying geographic features by running separate regressions of village size on distance to nearest two-lane road or river (conditioning on changwat dummies). The estimated coefficients were 0.26 (standard error of 0.32) and -0.25 (0.24), so neither was statistically significant. Small villages did tend to be located closer to forest areas however, where the coefficient of 0.35 (0.03) was highly significant,

[23] Although villages did received the funds in different months of the year, the precise month that funds were received is uncorrelated with the amount of credit per household after controlling for village size.

[24] The general formula for Moran's statistic is:

$$I = \frac{n}{\sum_{i=1}^{n}\sum_{j=1}^{n} w_{ij}} \left(\frac{\sum_{i=1}^{n}\sum_{j=1}^{n} w_{ij} \left(z_i - \bar{z}\right)\left(z_j - \bar{z}\right)}{\sum_{i=1}^{n}\sum_{j=1}^{n} \left(z_i - \bar{z}\right)^2} \right)$$

where n is the number of observations (villages), z_i is the statistic for observation i (village size of village i), and w_{ij} is the weight given villages depending on their spatial distance. Here we use inverse cartesian distance between villages.

indicating that forest area may limit the size of villages.[25] Nonetheless, these regressions explain at most five percent of the variation in village size, so the variation is not well explained by geographic features. We have included roads, rivers, and forest in Figure 1.

Finally, since we control for household level fixed effects, any contamination would need to result from village size capturing changes in the outcome variables over time, which is doubtful. We verify in Section II.D that village size is unrelated to the variables we examine in the years prior to the program.

C. Data

As stated in the introduction, our data are panel survey data from the Townsend Thai dataset.[26] We utilize five years (1997-2001) of data before the onset of the program and six years (2002-2007) of post-program data. We focus on two components of the survey (the household data and the institutional data), and supplement the data with information gathered in informal interviews conducted in the field. For our analysis of wages, we use a parallel monthly longitudinal survey, August, 1998 through December, 2003. Both surveys are part of an on-going project. That is, they have no specific relationship with the village fund program, which limits incentives to misreport regarding the program.

The household panel data set is a stratified, clustered, random sample, including 15 households in each of 64 villages distributed across four provinces (*changwats*) of Thailand - the changwats of Chachoengsao and Lopburi in the Central region relatively near Bangkok, and Sisaket and Buriram in the poorer Northeast region.[27] The attrition rate from year to year averaged only three percent annually so that, of the 960 households surveyed annually, 800 of them were followed for the seven years, while 655 were followed for all eleven years. Attrition was largely due to migration. We use a balanced panel in our regressions, though with the larger sample for the seven year analyses.

The household data set has several strengths. First, it is the only panel data from Thailand that spans across the pre- and post-program years. Second, the data is exceptional in its breadth and level of detail. These data include information on education, assets[28] and investment, income and expenditures in production, borrowing and saving through various forms, consumption,[29] occupation, businesses operated, and household compo-

[25] Forest conservation efforts have driven some redistricting decisions but these decisions have been largely haphazard and unsystematic. For discussions, see Puginier (2001) and Giné (2005).

[26] See Townsend et al. (1997).

[27] The survey design was based in part on the results of prior field research in the Northern region (Townsend (1995)).

[28] The initial 1997 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 percent 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.

Subsequent asset levels were found using current investment data and a depreciation rate of ten percent.

[29] Consumption is non-durable in that it excludes household asset expenditures, and includes only food, drink, fuel, clothing and services. Consumption is measured by a solicitation of 13 disaggregate items that best predict aggregated non-durable consumption expenditure in the larger more comprehensive SES survey. In practice 50-80% of the variation can be explained by these 13 items. A price index for each of the four provinces was created by the average price of the

sition, for example. Using credit as an example of the detail in the data, for every year we have a record of all loans, both formal and informal, that a household has taken. The lending environment in these villages is very nuanced, with the BAAC, commercial banks, family, relatives, money lenders, and other quasi-formal village institutions in addition to the village funds all playing significant roles.[30] These household level loan data include the amount of the loan, date of the loan, duration, amount to be repaid, interest rate, lender, stated reason for borrowing[31], collateral used, value of collateral, whether the loan has been repaid, and the consequences of defaulting on the loan. We measure default as loans that are 90 days past due using current data on repayment and terms but also linking loans across years to uncover default (e.g., we do not allow the term of the loan to be extended after it was taken.) We then record the amount of village fund credit in default and whether a household has any loan either short- and long-term in default.[32]

Table 1 gives summary statistics for the relevant variables of the annual household data used in this paper. The exchange rate of baht to dollars in this period is roughly 40 to 1. Importantly, we see that after the introduction of the program, 54 percent of households borrow per year with average borrowing of 9000 baht. The median level of village fund credit is 16,000 baht, with a mean of 16,700. Loan sizes vary, but the middle 90 percent of loans are between 5000 and 30,000 baht. For reference, household income averages 108,000 baht with a median of 64,000 baht (per capita numbers are 24,000 and 15,200 baht, respectively).

The monthly dataset is a smaller panel of 400 households in 16 villages over 65 months from late-1998 through 2003. The villages differ from the annual panel data, but they are in the same changwats and both were drawn from a common survey in 1997. The monthly dataset has strengths that complement the annual data. In particular, it includes not only income, but separate records for labor supply (measured in days), which allow for daily wage rates by activity to be calculated.

Finally, we use data from the Community Development Department (CDD), which includes all villages in our provinces, for our geographic analysis.

II. Methods

We focus on the effects of village funds on short-term credit (defined as loans of one year or less). The vast majority of village fund credit was short-term, and so we want to see its impact on the short-term credit market and abstract away from other credit markets.

The dependent variables we focus on are divided into four categories:

inter-quartile, 25-75% range of purchases and sales of the key consumption items for which both quantities and values were recorded. Given the weights on each component, impacts on the components of consumption do not simply sum to the total impact (see Table 6).

[30] See Kaboski and Townsend (1998)

[31] Variables measuring the amount of credit borrowed for different purposes are based on these reported reasons for borrowing.

[32] The panel data also include an institutional component surveying all of the quasi-formal micro-financing institutions encountered in the survey villages, which we use as the source of many of the descriptive statistics given above.

- First, we measure the impact of the village fund credit on the short-term credit market, including: its effects on total short-term credit; borrowing from other formal sources (i.e., the BAAC and commercial banks); the stated reasons for borrowing (i.e., business investment, agricultural investment, fertilizer/pesticides, and consumption); and measures of the tightness of credit markets (interest rates, default and informal borrowing).

- Second, we measure the effect of village fund credit on consumption and its different components. Specific components include grains, dairy, meat, fuel, clothes, home repair, vehicle repair, eating out, tobacco, alcohol (consumed both in and out of the home), ceremonies, and education.

- Third, we assess the impact on the income and productive decisions of households. In particular, we look at overall asset and income growth, as well as components of net income (agriculture by component, business, and wages/salaries), investment (agricultural and business), and input use (wages paid and fertilizer/pesticides). We also look at wages (calculated as the ratio of income over work days) by type of activity.

- Fourth, we look at differential impacts on the above variables in female-headed households. Microcredit is often targeted toward women, and theory (e.g., Bourgingon et al. (1994), Browning and Chiappori (1998)) and evidence (e.g., Pitt and Khandker (1998), Kaboski and Townsend (2005)) suggest that impacts may differ across men and women.

We propose the following specification for the impact of short-term village fund credit ($VFCR_{n,t}$) of household n at time t on outcome measure $y_{n,t}$:

$$y_{n,t} = \alpha VFCR_{n,t} + \sum_{i=1}^{I} \beta_i X_{i,n,t} + \phi_t + \phi_n + \varepsilon_{n,t} \quad (1)$$

$VFCR_{n,t}$ is a measure of the amount (stock) of credit with less than twelve month duration that household n borrowed from a village fund in year t. The X_i are a set of household control variables including number of adult males, number of adult females, number of children, a dummy for male head of household, age of household head, age of head squared, years of schooling of head. In addition, we allow for a time-specific fixed-effect ϕ_t, and a household-specific fixed-effect ϕ_n.

Equation (1) has strengths and disadvantages. On the one hand, by not adhering to one particular theoretical model, it allows us to look at a wide range of outcomes that go beyond the predictions of an explicit theory. On the other hand, equation (1) is at best a reduced form attempt to approximate a more explicit behavioral model.[33] In Kaboski and

[33]We also used the differenced version of equation (1). This specification had advantage of allowing for fixed effects

Townsend (2011a), our structural model implies that credit interventions ought to affect the growth rate of income and asset accumulation, while affecting the level of choice variables such as consumption and investment. (When we focus on specific components of income, we look only at levels, since these measures are noisy, and differencing appears to eliminate most of the signal in the data.) Similarly, for the three outcome variables that may proxy borrower's *ex post* ability to repay loans, default, interest rates and borrowing from informal sources, we run alternative regressions using either current village fund credit $VFCR_{n,t}$ or the lagged value of village fund credit, $VFCR_{n,t-1}$.

A. *Instrumenting*

In addition to running OLS on equation (1), we use a two-stage approach to instrument for village fund credit. The instrument used is the interaction between the inverse number of households in the village and the post-program year dummies, χ. That is, we control for variation across households correlated with the inverse of village size, but use the additional effect of village size in post-program years ($invHH_n * \chi_{t=t^*}$, where t^* is the relevant program year) as our instrument. This first-stage regression is therefore[34]:

$$\begin{aligned} VFCR_{n,t} &= \lambda_2 invHH_n \chi_{t=2002} + \lambda_3 invHH_n \chi_{t=2003} + \\ &\quad \sum_{i=1}^{I} \delta_i X_{i,n,t} + \phi_t + \phi_n + e_{n,t}. \end{aligned} \tag{2}$$

The sufficient assumptions for ensuring consistency refer to the error terms in the second-stage (outcome $y_{n,t}$) equations, and are given below:

$$\begin{aligned} &\text{Orthogonality Assumption:} \\ &\varepsilon_{n,t}, u_{n,t} \perp invHH_n * \chi_{t=2002} | X_{i,n,t}, \phi_t, \phi_n \\ &\varepsilon_{n,t}, u_{n,t} \perp invHH_n * \chi_{t=2003} | X_{i,n,t}, \phi_t, \phi_n \end{aligned} \tag{3}$$

In the discussion of impacts, we will primarily focus on significance of estimates $\hat{\alpha}$ in equations (1), respectively, at the five-percent level, but also point out significance at the ten-percent level, when those results are supported by multiple regressions.

Table 2 gives a sample of the first- and second-stage estimation results from the 2SLS procedure on equations (2) and (1), respectively. The variables of greatest interest are

on not only levels, but also changes. The specification produced broadly consistent results, but for the components of consumption and income where measurement error is greater, results were often no longer significant.

[34] The corresponding equation for when lagged credit is used in the outcome equation is:

$$\begin{aligned} VFCR_{n,t-1} &= \lambda_2 invHH_{t,n} \chi_{t=2002} + \\ &\quad \sum_{i=1}^{I} \delta_i X_{i,n,t} + \theta_t + \theta_n + e_{n,t}. \end{aligned}$$

italicized. We cluster by village-year combination and report robust standard errors throughout the paper. We multiply the coefficient on village fund credit by 10,000, so that the coefficient represents roughly an average treatment effect, since the program led to an average of 9000 baht village fund credit per household.

In the first stage estimates on the top of the table one can see that the instrument, inverse village size, is strongly predictive of village fund credit in the years of the Million Baht Program, but not otherwise. The z-statistics are 2.4 and 8.7 in 2002 and 2003, respectively. The magnitude of the interacted instrument in 2002 of 464,000 is nearly 50 percent of the 950,000 (an accumulated flow) that village funds claimed to have lent out on average. The higher coefficient of 853,700 in 2003 reflects the higher total household borrowing from village funds in 2003. So the coefficients are both statistically significant and economically meaningful.

The second stage shows that total (i.e., from all sources) short-term credit increased in response to village fund credit, since the $\hat{\alpha}$ estimate is 19,200.

B. Outlier Robustness

The data show a great deal of variability, and so the results can be very sensitive to a single or handful of observations. For example, the vast majority of investments and loans are small, so that one major investment or loan in the regressions can swamp all the activity happening at a smaller scale.

We run several different regressions in order to deal with this problem.

- Our baseline instrumental variable regression is a standard two-stage fixed-effect least squares regression omitting households in villages with greater than 250 households and fewer than 50 households. This excludes nine of 64 villages. In 2002, the two very small villages had 30 and 34 households, while the large villages had 268, 297, 305, 314, 400, 900, and 3194 households.
- The second regression includes outlier villages. It is identical to the baseline regression above except that it uses all 64 villages.
- The third regression excludes outlier observations of the dependent variable. Specifically, we drop the top and bottom one percent of non-zero values of the dependent variable. If one of the endpoints of the distribution has a mass point greater than one percent, we do not drop any observations from that end.

C. Heterogeneity of Impacts

In the theories that motivate our study, unobserved heterogeneity (i.e., ability, project size, permanent income) is important and leads to heterogeneous impacts of exogenous shifts in intermediation (see Kaboski and Townsend (2011a), Giné and Townsend (2004), Townsend and Ueda (2006), for example). Also, impacts can be non-linear and time-varying. Moreover, GE impacts may play a role, and so a precise policy-relevant interpretation of α is limited, and we will not assign one. We view estimates of α as rough

but nonetheless informative measures of an average linearized impact of the program on village households, scaled into per baht of credit injected terms.

Still, we are interested in potentially observable heterogeneity in impacts. If women are indeed more constrained, female headed households may be differentially impacted by the program. When estimating the differential impacts of female-headed households, we use an additional interaction term of village fund credit with a dummy variable for female headed households:

$$y_{n,t} = \alpha_1 VFCR_{n,t} + \alpha_2 VFCR_{n,t} * \chi_{female,n} + \sum_{i=1}^{I} \beta_i X_{i,n,t} + \phi_t + \phi_n + \varepsilon_{n,t} \quad (4)$$

where $\hat{\alpha}_2$ is the differential impact of credit on female-headed households. Our second instrument comes from letting the the impact of inverse village size vary by female headed households in the first-stage.

We also looked at impacts based on two other potential proxies for the degree a household is constrained: tercile of time-averaged income and land-ownership. Households with higher income tend to borrow more (see Kaboski and Townsend (2011a)), so we conjectured that they may be less constrained by the availability of credit. Similarly, land is necessary to collateralize loans (from commercial banks and also the BAAC), and so landowners may have been less constrained. We found no evidence of differential impacts along either of these dimensions, however, and so we do not report the results.[35]

D. *Exogeneity of Village Size*

Here we focus on evidence of whether inverse village size is plausibly exogenous during the program years. We do so by introducing interactions of the inverse village size variable with the pre-program years, i.e., $invHH_n\chi_{t=j}$ for all $j < 2002$. Here we divide the coefficients by 100 (dividing by 1,000,000 to put them them in roughly per baht terms, since that was the total injection in the village and multiplied by 10,000 as we did with the treatment variables). We then run a series of F-tests to evaluate the joint significance of these variables. The actual values of the coefficients for four different interactions and our 41 different dependent variables are not reproduced, but they are available in our on-line appendix.

The major point here is that these year-specific village size interactions do not significantly predict outcomes before the program. Of the 41 outcome regressions, only one yielded jointly significant dummies at a five-percent level of significance. The exception is wage income which had a p-value of 0.03. In terms of the individual dummies, income from wage labor is significantly lower in small villages in the year prior to the program, with a coefficient on $invHH_n\chi_{t=2001}$ of -5200 (standard error: 2001). At a ten percent

[35] Using a similar village-size identification strategy to evaluate an Indonesian grant program, Yamauchi (2008) finds heterogeneity in impacts across underlying village features. Namely, impacts on labor supply, income and expenditures were greater in villages with local markets and in villages accessible by land.

level, one additional variable is significant, log asset growth with a p-value of 0.09. Asset growth tends to be somewhat smaller in small villages, especially in the year after the crisis, but none of the individual coefficients are significant. The largest is the coefficient on $invHH_n\chi_{t=1998}$ of -3300 (standard error: 1900). Even at a much more stringent fifteen-percent level of significance, the dummies were jointly significant for only a third variable: income from crops other than rice. In the case of crop income, none of the individual dummies are significant, but the largest coefficient is again on $invHH_n\chi_{t=2001}$. This value is 4600 (standard error: 5100). The signs on the coefficients on wage and crop income change from year to year. Moreover, the frequency of significance is well within the expected rate of type I-errors.

E. Multiple Inference

Type I-errors are also a potential issue in our impact estimates, especially given the large number of outcomes we evaluate.Kling, Liebman and Katz (2007) and Karlan and Zinman (2010*a*) address these problems in two ways: (1) reducing the number of outcomes by creating indexes, and (2) using family-wise adjusted p-values. Creating indices is less necessary in our analysis since the four main components (credit, consumption, income, and assets) are essentially natural indexes, while the other variables are generally subcomponents of these four. In our tables, we report significance based on individual p-values, but in the text we also note family-wise significance, first for the four main components jointly where a z-statistic of at least 2.23 would lead to a five percent significance level, and next for the subcomponents of credit (13 subcomponents, z-statistic$\geq$2.66), consumption (12, 2.63), income (5, 2.32) and assets/investment (7, 2.44).

F. GIS Robustness

Another question of interest is to what extent the impacts of credit spillover to non-borrower households. One interpretation of the above specifications assumes that the effects are only on the borrowing household. Of course, viewing each village as a small (open) economy, we might presume that credit injections could affect even non-borrowing villagers, through internal GE effects, in particular. In this case, a second interpretation of the $\hat{\alpha}$ estimates in (1) would be the impact of an additional dollar of credit in the village on the outcome, rather than the impact of directly borrowing an additional dollar on the household's outcome. What is important for this interpretation is that households only benefit from credit injection into its own village. That is, any impacts of credit on non-borrowers must be local to the village.

We test whether it is the local injection of credit into the village that drives our results, or whether neighboring village also has important effects. That is, we construct a GIS control variable for the size of neighboring villages. The control variable is a spatial kernel estimate of the inverse village size (number of households) of neighboring villages (e.g., all villages in a 5 kilometer radius). The second-stage regressions are therefore of

the form:

$$
\begin{aligned}
y_{n,t} &= \alpha VFCR_{n,t} + \sum_{i=1}^{I} \beta_i X_{i,n,t} + \mu inv HH_{n,t,neighborhood} * \chi_{t>=2002} + \\
&\quad \phi_t + \phi_n + \gamma\, inv HH_{t,n} + \varepsilon_{n,t}
\end{aligned} \tag{5}
$$

The results we present are overwhelmingly robust to the inclusion of such a neighborhood control variable. The $\hat{\alpha}$ estimates from regressions of equation (5) are nearly identical to those of equation (1). As above, we again scale the coefficients by 100 to assist in comparability. All significant coefficients are significant in both direction and of very similar magnitude. Even the insignificant estimates are of the same sign in 49 of the 50 estimates again with very similar magnitudes. Finally, the $\hat{\mu}$ estimate was not a strong predictor of outcomes and was significant in only two of the regressions. Villages surrounded by smaller villages are associated with less income from rice farming (coefficient: -11,000; standard error: 5500) and more from other crops (24,600; 13,100). Neither of these coefficients are significant using the family-wise p-values, however. Again, these results are available in our on-line appendix.

Together, the robustness of our results to the GIS variable support the claim that in the two years after the program's founding, which we study, impacts remained local to the village in the short run, and our view of the experiment on separate village economies appears justified. We note, however, that our GIS variable does pick up significant variation in the longer run estimation described below.

G. Long Run Impacts

In the long run, village funds likely have spillovers onto other villages, through migration or wider GE effects, for example. Given this caveat, we examine the long run data. To our knowledge, the results we present, however imperfect, are the only estimates of the long run impact microfinance over five years. For these results, in order to see trends in the overall impact of the program, we present reduced form results rather than two stage estimates. For the same reason, for log assets and net income, we use levels rather than growth as the dependent variable. That is, we use the following equation:

$$
\begin{aligned}
y_{n,t} &= \sum_{\tau=1}^{T} \omega_\tau inv HH_n \chi_{t=\tau} + \sum_{i=1}^{I} \beta_i X_{i,n,t} + \\
&\quad \phi_t + \phi_n + \varepsilon_{n,t}
\end{aligned} \tag{6}
$$

We scale the estimates $\hat{\omega}_\tau$ by the one million baht injection so that the coefficient are in terms of per baht injected. We interpret the series of $\hat{\omega}_\tau$ as reflecting the changing impact of the program over time. The caveat is that it may confound changing impacts with the changing predictive power of initial village size and/or the changing importance of spillovers. Indeed, the addition of year-specific GIS controls (as in equation (5))

after 2003 into (6) yields jointly significant estimates as well as significant estimates for individual years, generally in the last two years. These estimates were significant for village fund credit, consumption and income, but ω_τ estimates do not appear to be significantly affected by inclusion of the controls, as we note in Section III.E below.

III. Results

Table 3 presents estimates of the program's short-term impacts on four key summary variables: credit, consumption, asset growth and income growth. The table reports estimates of α along with standard errors, and significance at the five and ten percent levels is noted. Again, we have scaled the coefficients by 10,000 so that the estimates are roughly average treatment effects for the program, given the average of 9000 baht of village fund credit per household. Each of the columns corresponds to a different outcome variable, while the rows correspond to OLS (at the top), the baseline regression, and the regressions with alternative treatment of outliers.

The first column indicates that the flow of total new short-term credit increased. That is, the program was successful in increasing overall credit and did not simply crowd out other sources of credit. There actually is some evidence from the levels regression that the credit injection may have had a multiplier effect (i.e., a baht of credit injected by the village fund led to more than one baht of additional total credit), though none are significantly greater than one at the five percent level.

Similarly, the second column of IV estimates shows substantial and significant increase in consumption levels. Indeed, the estimates suggest that the increased value of consumption is of the same order of magnitude as the credit injection, or even larger with the baseline estimate of an additional 17,100 baht of consumption for every 10,000 baht of village fund credit injected. The estimate that drops outliers also indicate a large number (14,700). The consumption impacts is not seen in the OLS regression, perhaps because those with lower than typical consumption are more likely to borrow.

The third column indicates some evidence that credit lowered the log growth of assets. Recall assets includes the value of physical assets and financial assets (net of loans). The point estimates are all negative and of substantial magnitude, but only the regression that includes all the villages is statistically significant.

The fourth column indicates that households had higher income growth, significant in three of the regressions. The impact is quite large, but recall that the fund injection was large, averaging twelve percent of village income, and this lead to an even greater increase in overall credit. The impact on income growth was short-lived as we discuss in Section III.E.

To summarize, we see a substantial increase in credit on the order of the size of the injection, a comparable, perhaps larger, increase in consumption, and a higher preponderance of low asset growth, and high income growth. Of these IV impacts, only the impact on consumption (and only in the baseline regression) drops to a ten percent significance level, when the family-wise p-levels are applied.

The large increase in credit may be evidence of credit constraints. The large increase in consumption – of similar magnitude, if not larger, than the increase in credit – is a striking

finding. A major argument in favor of credit interventions like the Million Baht Program is that the poor in non-intermediated sectors actually have returns to investment that exceed market interest rates and the returns to investment in the financially-intermediated sector.

The observed large increase in consumption might indicate that the returns are actually highest in consumption. Such behavior is quantitatively consistent with Kaboski and Townsend (2011a)'s structural buffer stock savings model. In this model, two groups increase consumption: consumption-constrained households with short-term liquidity needs, and households with buffer stocks that are larger than necessary after the credit constraint has been relaxed. The second group can make consumption growth exceed credit growth, since they increase consumption without actually borrowing.[36] The intermediation and growth explanation is that constraints are binding on investment and input use and the observed income growth may reflect this. The asset growth might then be a result of households with higher future income intertemporally substituting toward present consumption (as in the intermediation and growth models). Finally, even though we focus on non-durable consumption, the increase in consumption may have an investment aspect to it.

To gain more insight into these issues, we analyze each of the impacts (credit, consumption, and income/assets) more closely below.

A. Impact on the Credit Market

In Tables 4, we delve more deeply into the impacts of the program on other borrowing and reasons for borrowing. For the purpose of comparison, the first column reproduces the results for the impact on total new short-term credit of Table 3. The most salient finding is that credit for consumption increased significantly, and this is robust across all four regressions. (This is the only additional IV impact in Table 3 that remains significant when the family-wise p-level is applied, and this is only at the ten percent level for the baseline.) These consumption loan estimates are substantially less than the total increase in short-term borrowing, and the positive point estimates on credit for other reasons may also be contributing to this total. The increase in credit for fertilizer and pesticides are also sizable, though this increase is only statistically significant in the regression using all villages (and the OLS regression).

Clearly, the reason for borrowing should be ambiguous, since money is fungible across uses. We will see, however, that the consumption (and to some extent investment) borrowing patterns are reflected by actual levels of consumption (investment), while fertilizer usage is not. Fertilizer and pesticide usage may simply be a fallback reason that households give for borrowing; in the past, a large share of loans from the BAAC in the past were given for such use, for example. Related, there is some evidence in Table 4 that borrowing from the BAAC increased as a result of the program.

[36] Another potential way that the program could impact non-borrowers consumption is through relending to non-borrowers as in Angelucci and De Giorgi (2009). We do not view such indirect borrowing as an important channel in the Thai context, since we found no substantial or significant increase in household lending to others, whether inside or outside of the village.

Table 5 shows the effect of the program on other aspects of the credit market: interest rates, default, and informal borrowing. We distinguish between the impact on the credit market in the year the loans were taken, and the impact on the credit market in the year the loans were due. The results indicate that the injection did not appear to have large effects on these aspects of the credit market. First, short-term interest rates did not fall. The baseline impact is insignificant and small, and the point estimates for the other IV regressions are actually positive. The fact that short-term interest rates did not fall is supporting evidence that households were credit constrained. The taking of loans seems to have little effect on default and the use of informal credit. The results for the impact on the credit market in the year of repayment provide some evidence of tighter credit markets, however. Looking at the point estimates, there is some evidence that more households are in default, and face higher interests rates after borrowing, but they do not appear to be resorting more to informal lenders in the year of repayment. Only one lone positive estimate on the probability of default has any level of significance, and this is just at a ten percent level.

B. Impact on Consumption

Table 3 showed a substantial impact on consumption, and Table 4 showed that stated borrowing for consumption increased in a similar fashion. We analyze here the impacts on different components of nondurable consumption in Table 6. Durable consumption showed no significant impacts and are therefore not presented.[37] A first observation from Table 6 is that the consumption of several components of nondurables are unaffected by the credit program. The fact that grain, a "necessity" does not increase is perhaps not surprising, but other components such as ceremonies, clothes, and educational expenditures are also not significantly affected. Our result of no measured impact on educational expenditures should not be construed as evidence against credit constraints in educational investment, since an increase in the opportunity cost of going to school may have offset the reduced cost from credit constraints.

The components with the largest responses to the credit programs are housing repair and vehicle repair, which are investment-like in the sense that they have a durable aspect to them. Housing repair expenditures are sizable but infrequent, and so do not show up in the regression using dummy variables. The baseline estimates indicate that 10,000 baht of village fund credit led to 13,300 baht of expenditures on household repair and 1800 baht on vehicle repair. To the extent that vehicles are necessary inputs into production or transportation to jobs, such repairs may be investments with high returns rather than consumption. Karlan and Zinman (2010*a*) make a similar argument in their assessment of transportation expenditures.

The other components with statistically significant increases are spending on alcohol consumed at home (800 baht per 10,000 baht of credit). The positive impacts on tobacco (600 baht) and meat consumption (300 baht) are only marginally significant in the baseline, and the alternative specifications find some evidence of significant increases on

[37] This differs in an important way from the results of Banerjee et al. (2009) for microfinance in India.

dairy and eating out. However, none of these are significant with family-wise p-values. Indeed only the impact on vehicle repair in the bottom row is significant at a five percent level using these p-values, while the other impacts on vehicle repair as well as the impact on alcohol in the home and home repair drop to a ten percent significance level.

We find the breakout of consumption of great interest, since the components that policy makers might particularly associate with waste (e.g. alcohol, tobacco, clothing) show relatively small increases, while again the repair services, which have an aspect of investment to them, show the largest response.

C. Impact on Productive Activities

Recall that in Table 3, we saw that income growth increased as a result of the village fund credit. Table 7 examines this in more detail by showing the effect of village fund credit on income generated from the most important sources of earned income: business profits, wage/salary labor income, and agricultural income from rice, other crops and livestock.

There is some evidence that wage income, and perhaps business profits, increased in response to the program. The marginally significant point estimate on wage income indicates an increase of 12,500 baht in wage income for every 10,000 baht of village fund credit. The estimate on business profits is of similar magnitude, but it is only in the regression using all villages. (This impact is the only additional IV impact in Table 3 that remains significant under the family-wise p-values, and it remains at the five percent level.) We see no significant increase in income from rice and other crops, and indeed in alternative regressions that look at the fraction of income, these sources show a statistically significant decline. The increase in business and wage income relative to agriculture is broadly consistent with the models of intermediation, entrepreneurship, and growth, and the stated aims of the program.

On the other hand, the results in Table 8, which shows the impact on measures of investment and input use, do not support a story in which credit is needed for either start-up costs or business investment. Specifically, the last five columns focus on this investment behavior and the use of inputs. We see no significant impact on business starts. The lack of significance may simply be due to a lack of power. Less than five percent of the sample start new businesses. The point estimates are all positive, and the baseline would imply three higher percentage points for the average household. The coefficient on business investment is actually negative, however, and we do not find a large effect on the probability of investing, or even wages paid. The evidence of an increase in wages earned, and some evidence of an increase in business profits, is puzzling since no measures of investment, intermediates, or payments to labor appear to have increased.

The increase in income, and large increase in consumption, despite few measured impacts on investments is potentially puzzling. Karlan and Zinman (2010*b*) find a similar result. At least two potential explanations exist, though there are doubtless others. First, our companion paper shows that such the large increase in consumption can be quantitatively explainable through buffer stock dynamics, and that investment increases are

difficult to discern in our sample size because of the noisiness of the data. Second, Buera, Kaboski and Shin (2011*b*) show that GE increases in wages from improved allocative efficiency can lead to redistribution from high- to low-saving households. That is, an increase in consumption can increase without aggregate changes in investment.[38]

An increase in the actual wage rate is a strong prediction of models of intermediation, entrepreneurship and growth, however, and we therefore examine the evidence for wage rate increases a little more directly. Although the annual data does not have separate data on wages, the monthly panel provides direct evidence of a GE effect on prices (i.e., wages) from the program. The monthly data distinguish between days of labor supply and daily wages by activity, but it is a smaller sample of (16) villages, and the very high frequency of the data creates timing issues (e.g., should credit affect outcomes in the month it is disbursed, some period after disbursement, or for the loan period, or after it is repaid?). Using regressions that best replicate the annual data, the monthly data corroborates the significant positive impact we found on income growth.[39] These results are available in our on-line appendix. The main point is that we view these data as informative.

Analogous regressions with the level of log wages as the dependent variable of interest yield quite interesting results as shown in Tables 9 and 10. In Table 9, we find a robust impact on the overall level of wages across occupations. The baseline estimate is an increase of roughly 7 percentage points for the average household. This is both qualitatively and quantitatively consistent with the comparably-sized hypothetical microfinance simulations of Buera, Kaboski and Shin (2011*b*) which yield wage increases of 5-10 percent.

ITable 10 delves into which occupations or types of labor experienced wage increases. Agricultural wages decline substantially, which is somewhat surprising, but the other impacts are all consistent with expectations from theory. We find no impacts in government or professional work, construction outside of the village, and factory work. White-collar employers and factories are unlikely to be financed by small microfinance loans, and all three are likely to be performed outside of the village. In contrast, there are significant positive impacts on wages in general-non agricultural work, construction in the village, and "other". The impact on construction wages is particularly interesting because it is only evident for local wages. Wages for construction work in other counties (including Bangkok) do not increase. This is consistent with the idea of village economies, with (partially) segmented labor markets, and also with the increases in the consumption of household repairs found above.

[38] Studying the same program, Boonperm, Haughton and Khandker (2009) find increases in consumption only using log consumption, which they interpret as evidence that consumption growth is concentrated among the poor.

[39] The credit variable is a point in time stock of outstanding short-term credit, while the outcome variables are the twelve month growth in total income and income by source twelve months later. We include household and time fixed effect, but, lacking data on time-varying data on head of household characteristics and household composition in these data, we instead add a quadratic in assets as a substitute control for these changes.

D. Differential Impact on Women

We examined whether the impacts of credit were significantly different for female-headed households using all of the outcome measures. Overall, perhaps the most surprising result that female-headed households behave similarly to households headed by males. We found no significant differential impacts of the village fund on female headed household with respect to credit or agricultural income. The only significant differential impacts were on the sources of income, and the distribution of consumption. Tables 11 and 12 summarize these impact results, i.e., estimates of $\hat{\alpha}_2$ in equation (4).

Table 11 shows a subset of the sources of income; the only significant difference between male- and female-headed households is that credit causes a relatively larger positive impact on business income for female-headed households, but this just at a ten percent level in the full sample of villages.

Table 12 shows that there are also significant responses of female-headed households is in their consumption patterns, but not in the ways typically argued in the literature. In other countries, the literature (e.g., Pitt and Khandker (1998)) has found that men tend to spend money on things such as alcohol, while women's spending patterns are directed toward children. Our results in Thailand differ. For example, there is no difference in expenditures on children's education in response to credit. There is also some evidence that female-headed households shift consumption toward clothing and especially meat, and less on home repairs. Finally, we do find that female-headed households shift consumption less toward alcohol consumed outside of the home, but this is balanced by their increased consumption of alcohol in the home, where it is more culturally acceptable.

E. Long Run Impact

Tables 13 and 14 present the long run results, which incorporate a balanced panel on all eleven years of data. Village funds were relatively successful in lending over time, as evidenced by Table 13, which shows the average levels of village fund credit and the fraction of village fund credit in default over the six years of post-program data. The average amount of village fund credit grows over time, and the amount of village fund credit in default as a fraction of total village fund credit is relatively low and stable, less than 0.04 each year. Thus, our assumption that households viewed this as a lasting credit program rather than a short-lived gift is not unfounded.

Table 14 shows the responses of outcome variable to village size (scaled by 100 to yield roughly a per household treatment effect). The first column corroborates the results of Table 13, since village fund credit increases over time.

Two other interesting facts emerge. First, the program led to an even larger long term expansion of overall credit, though default also became more prevalent. The second column shows the significant increase in overall short-term credit. The ratios of the impacts on overall credit to village fund credit fluctuate between 1.6 and 2.7. The prevalence of default on any credit decreases in the first year, and the increases thereafter, with significantly higher default in alternating years.

Third, the increase in consumption is short lived, lasting only the first four years, and it also show an alternating pattern, where consumption is higher in years where default is higher. The increase in consumption is not significant under this specification, however. In the two-stage specification, the response of consumption to village fund credit is significantly positive as in Table 3 but only in the first two years. A transitory increase in consumption is consistent with bufferstock savings dynamics in response to a relaxed borrowing constraint. Finally, the point estimates on log assets is positive in all years but insignificant, while the impact on net income appears to follow the alternating years pattern, where high income coincides with high consumption and default. Nevertheless, only the initial impact on income is significant. In sum, the program seemed to have large persistent impacts on credit, but transient impacts on consumption and income. Finally, we note the drop in credit, consumption, and income and the dramatic increase in default during the last year. This increase in default amounts to almost a doubling of default rates. This was the year of unrest following the coup and ousting of Thaksin, which appears to have affected repayment.

In sum, the increase in credit appears to have been persistent (at least until the coup), but the impacts on consumption and income were short-lived. These results are robust to the inclusion of GIS controls for average village size in surrounding villages, although these controls do yields significant estimates in later years. Specifically, villages surrounded by large villages showed an increase in income and consumption. While these controls tended to lower standard errors, the point estimates were quite similar and not statistically distinguishable. Results are available in the on-line appendix.

IV. Conclusions

The Million Baht Village Fund injection of microcredit in villages had the desired effect of increasing overall credit in the economy. Households responded by borrowing more and consuming more, yet earning more as well. The village fund credit had a short-term effect of increasing future incomes, and making business and market labor more important sources of income. The increased borrowing and short-lived consumption response, despite no decline in interest rates, point to a relaxation of credit constraints. The increased labor income and especially wage rates indicate important spillover effects that may have also affected non-borrowers.

The large increase in borrowing and consumption are broadly consistent with buffer stock models of credit constrained households. Our companion paper develops this link more explicitly and in a quantitative fashion, but the reduced form analysis of this paper shows that the composition of consumption increases is not only toward luxury goods but also repairs. Similarly, the increase in income, and the increasing importance of business and labor income are consistent with models of intermediation and growth. The GE impact on wages that we discover offers more credence to these models, where rising wages play an important role.

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Number of Households per Villages, Four Provinces, Thailand

Si Sa Ket

Buri Ram

Chachoengsao

Lop Buri

Table 1. Summary Statistics of Relevant Household Level Data, 1997-2003

	No of Obs.	Mean	Std Dev	Cross-Sectional Std. Dev.
Short-Term Credit Variables				
New Short-Term Credit (Total)	5,831	20,900	50,600	34,200
Village Fund Credit, Post-program	*1,666*	*9,000*	*10,300*	*8,800*
Vill. Fund Loan Received Dummy, Post-progran	*1,666*	*0.54*	*0.50*	*0.43*
BAAC/Ag Coop Credit	5,831	11,000	30,900	18,900
Commercial Bank Credit	5,831	300	7,000	2,900
Informal Credit	5,831	5,600	31,800	21,700
Credit for Agricultural Investment	5,831	1,400	10,000	4,500
Credit for Business Investment	5,831	3,600	31,900	23,000
Credit for Fertilizer, Pesticides, etc.	5,831	10,100	33,200	21,600
Credit for Consumption	5,831	8,300	24,600	13,500
Credit Market Indicators				
Average Short-Term Credit Interest Rate	2,982	0.095	0.139	0.104
Dummy for Credit in Default	5,831	0.23	0.42	0.19
Consumption Variables				
Total Consumption	5,767	75,300	101,500	68,300
Education	5,784	5,200	11,000	8,300
Grain	5,767	8,900	11,300	5,200
Dairy	5,767	2,100	4,400	2,600
Meat	5,767	4,100	4,700	2,900
Alcohol at Home	5,767	1,900	4,800	3,200
Alcohol Out of home	5,767	900	3,600	2,200
Fuel	5,767	5,000	11,400	7,500
Tobacco	5,767	1,100	3,000	2,100
Ceremony	5,767	5,200	13,000	5,400
House Repair	5,784	6,300	37.000	15,300
Vehicle Repair	5,784	2,100	8,100	4,300
Clothes	5,784	1,500	2,500	1,700
Eating Out	5,784	1,900	5,400	3,100
Income and Asset Variables				
(Total) Net Income	5,825	96,900	193,500	144,400
Business Income	5,825	16,500	148,600	97,200
Wage and Salary Income	5,808	31,500	65,000	57,900
Gross Income from Rice Farming	5,808	20,800	37,000	31,100
Gross Income from Other Crops	5,808	21,200	95,100	60,200
Gross Income from Livestock	5,808	6,956	50,600	36,400
Gross Assets (incl. savings)	5,614	1,577,000	4,108,000	2,774,500
Investment and Input Uses Variables				
Number of New Businesses	5,823	0.05	0.24	0.10
Business Investment	5,831	3,400	48,400	29,600
Agricultural Investment	5,824	3,300	28,600	13,300
Expenditure on Fertilizer, Pesticides, etc.	5,825	9,100	20,700	14,500
Total Wages Paid	5,825	8,400	32,900	22,600
Other Control Variables				
Male Head of Household Dummy	5,790	0.73	0.44	0.42
Age of Head	5,790	53.7	13.4	12.9
Years of Education of Head	5,679	6.15	3.17	2.99
Number of Male Adults in Household	5,790	1.45	0.90	0.75
Number of Female Adults in Household	5,790	1.56	0.76	0.62
Number of Kids in Household	5,790	1.54	1.20	1.03
Farming Dummy for Household Head's Primary Occupation	5,831	0.61	0.49	0.38
Instrument				
Inverse Village Size	5,831	0.010	0.006	0.006

Table 2. Sample Regression – Two-Stage Household Fixed-Effect Estimate of the Impact of Current Level of Village Fund Credit on New Short-Term Credit Level

First Stage: Village Fund Credit on Instruments	**Coeff.**	**Std. Err.**	**z-statistc**
Year=1998 Dummy	40	210	0.18
Year=1999 Dummy	110	240	0.48
Year=2000 Dummy	60	240	0.25
Year=2001 Dummy	120	240	0.49
Year=2002 Dummy	4,020**	1680	2.40
Year=2003 Dummy	1,450	1040	1.40
Number of Adult Males in Household	-90	160	-0.59
Number of Adult Females in Household	610**	210	2.90
Number of Children (< 18 years) in Household	180	150	1.19
Male Head of Household	1040*	570	1.84
Head of Household's Primary Occupation is Farming	20	280	0.06
Age of Head	260**	130	2.01
Age of Head Squared	-2.55**	1.10	-2.33
Years of Education – Head of Household	-2.64	70	0.04
Interaction of Inverse Village Size and Year=2002 Dummy	463,900**	192,500	2.4
Interaction of Inverse Village Size and Year=2003 Dummy	853,700**	98,300	8.7
Number of Observations/Groups		4,960 / 715	
Second Stage: New Short-Term Credit on Predicted Village Fund Credit			
Year=1998 Dummy	7,300**	2,190	3.33
Year=1999 Dummy	8,660**	2,700	3.21
Year=2000 Dummy	6,180**	3,110	1.99
Year=2001 Dummy	7,960**	3,620	2.20
Year=2002 Dummy	-3,000	6,280	-0.48
Year=2003 Dummy	-4,580	7,020	-0.65
Number of Adult Males in Household	2,420**	1,590	1.93
Number of Adult Females in Household	1670	1,030	1.05
Number of Children (< 18 years) in Household	550	880	0.53
Male Head of Household	12,010**	5,740	2.09
Head of Household's Primary Occupation is Farming	-3530	2,090	-1.69
Age of Head	100	1,320	0.02
Age of Head Squared	-0.32	10.00	-0.01
Years of Education – Head of Household	-350	500	-0.82
Village Fund Credi (predicted)/10,000	19,200**	6700	2.85
Number of Observations/Groups		4,960 / 715	

Note: ** indicates significance at 5%, * indicates significance at 10%

Table 3. Summary: The Impact of Village Fund Credit

Technique \ Response Variable	New Short-Term Credit Level	Consumption Level	Asset Growth Rate	Net Income Growth Rate
OLS Regression	12,800** (1300)	2200 (2000)	-0.108 (0.277)	0.116** (0.38)
Baseline IV Regression: Only Villages With 50-200 Households	19,200** (6700)	17,100** (0.88)	-0.073 (0.163)	0.737** (0.33)
IV Regression using All Villages	13,800** (3700)	24,000** (0.63)	-0.210** (0.099)	0.211 (1.32)
IV Regression without 1% Outliers	13,900** (4600)	14,700** (5700)	-0.013 (0.014)	0.699** (0.304)

** Significant at 5% level * Significant at 10% level

The treatment variable is the level of short-term village fund credit. Coefficients are multiplied by 10,000, representing roughly a treatment effect, since the program led to an average of 9000 baht village fund credit per household. The independent variables are year dummies, household fixed effect dummies, male head of household dummy, number of adult males, number of adult females, number of kids, age of head and age of head squared, and years of schooling of head. The additional instruments in the first-stage are the inverse village size interacted with a dummy variable for year=2002 and year=2003. Standard errors for are robust standard errors clustered by village-year.

Table 4. Impact of Village Fund Credit on Other Credit and Reasons for Borrowing

Response Variable / Technique	New Short-Term Credit	Other Formal Credit		Stated Reasons for Borrowing			
		BAAC/Ag. Coop Credit	Commercial Bank Credit	Credit for Agricultural Investment	Credit for Business Investment	Credit for Fert., Pest., etc.	Credit for Consumption
OLS Regression	12,800** (1300)	2500** (1000)	-10 (100)	800 (500)	1800* (900)	5200** (1300)	5600** (1100)
Baseline IV Regression: Only Villages With 50-200 Households	19,200** (6700)	8000 (6900)	800 (700)	20 (1500)	2700 (2600)	8000 (6600)	8000** (0.38)
IV Regression using All Villages	13,800** (3700)	5100* (3100)	100 (500)	300 (700)	1500 (1400)	6300** (3000)	7000** (2200)
IV Regression without 1% Outliers	13,900** (4600)	3700 (4900)	800 (700)	200 (1200)	1600 (2300)	1700 (3400)	7200** (2800)

** Significant at 5% level * Significant at 10% level

The treatment variable is the level of short-term village fund credit. Coefficients are multiplied by 10,000, representing roughly a treatment effect, since the program led to an average of 9000 baht village fund credit per household. The independent variables are year dummies, household fixed effect dummies, male head of household dummy, number of adult males, number of adult females, number of kids, age of head and age of head squared, and years of schooling of head. The additional instruments in the first-stage are the inverse village size interacted with a dummy variable for year=2002 and year=2003. Standard errors for are robust standard errors clustered by village-year.

Table 5. Impact of Village Fund Credit on Interest Rates, Default and Informal Borrowing

Technique \ Response Variable	Credit Market Indicators					
	Year Borrowing			Year After Borrowing		
	Avg. Short-Term Credit Interest Rate	Probability of Short-Term Credit in Default	Informal Credit	Avg. Short-Term Credit Interest Rate†	Probability of Short-Term Credit in Default†	Informal Credit†
OLS Regression	-0.006 (0.004)	-2.5e-4 (0.011)	-100 (700)	1.37e-5 (0.004)	0.011 (0.018)	100 (900)
Baseline IV Regression: Only Villages With 50-200 Households	-0.008 (0.023)	0.637 (0.531)	-2200 (2800)	0.002 (0.056)	0.138 (0.101)	-4700 (5900)
IV Regression using All Villages	0.011 (0.011)	0.103 (0.329)	-2700 (1800)	0.025 (0.018)	0.067* (0.034)	-2000 (2200)
IV Regression without 1% Outliers	0.057 (0.184)	††	-1100 (2600)	0.021 (0.048)	††	-4000 (5900)

** Significant at 5% level * Significant at 10% level

The treatment variable is the level of short-term village fund credit. Coefficients are multiplied by 10,000, representing roughly a treatment effect, since the program led to an average of 9000 baht village fund credit per household. The independent variables are year dummies, household fixed effect dummies, male head of household dummy, number of adult males, number of adult females, number of kids, age of head and age of head squared, and years of schooling of head. The additional instruments in the first-stage are the inverse village size interacted with a dummy variable for year=2002 and year=2003. Standard errors for are robust standard errors clustered by village-year.

† Regressions are based on specification (3), where the treatment variable is the level of *lagged* village credit.

Table 6. Impact of Village Fund Credit on Consumption and its Components

Technique \ Response Variable	Total	Components of Consumption					
		Education	Grain	Dairy	Meat	Alcohol Home	Alcohol Out
OLS Regression	2200 (2000)	200 (200)	-200 (200)	100 (100)	100 (100)	100 (100)	20 (100)
Baseline IV Regression: Only Villages With 50-200 Households	17,100** (8800)	1100 (900)	400 (900)	500 (400)	600* (400)	800** (300)	200 (400)
IV Regression using All Villages	24,000** (6300)	700 (600)	500 (500)	600* (300)	400 (300)	300 (400)	300 (300)
IV Regression without 1% Outliers	14,700** (5700)	400 (700)	-300 (500)	300 (400)	300 (300)	600** (300)	300 (300)

Technique \ Response Variable	Total	Components of Consumption						
		Fuel	Tobacco	Ceremony	House Repair	Vehicle Repair	Clothes	Eating Out
OLS Regression	2200 (2000)	600** (200)	-40 (100)	-100 (200)	-200 (1300)	300 (300)	40 (40)	-100 (100)
Baseline IV Regression: Only Villages With 50-200 Households	17,100** (8800)	-700 (1200)	300* (200)	-200 (1200)	13,300** (6200)	1800** (700)	-50 (200)	500 (300)
IV Regression using All Villages	24,000** (6300)	1100 (900)	300 (200)	-400 (700)	7000* (3600)	1400** (700)	200 (100)	-10 (300)
IV Regression without 1% Outliers	14,700** (5700)	300 (700)	300* (100)	-400 (400)	5600** (2600)	600** (200)	100 (100)	500** (200)

** Significant at 5% level * Significant at 10% level

The treatment variable is the level of short-term village fund credit. Coefficients are multiplied by 10,000, representing roughly a treatment effect, since the program led to an average of 9000 baht village fund credit per household. The independent variables are year dummies, household fixed effect dummies, male head of household dummy, number of adult males, number of adult females, number of kids, age of head and age of head squared, and years of schooling of head. The additional instruments in the first-stage are the inverse village size interacted with a dummy variable for year=2002 and year=2003. Standard errors for are robust standard errors clustered by village-year.

Table 7. Impact of Village Fund Credit on Sources of Income

Technique \ Response Variable	Sources of Income				
	Business Profits	Wage and Salary	Rice Farming	Other Crops	Livestock
OLS Regression	6900 (4600)	1800** (0.09)	1900* (1000)	4000 (3900)	1600 (1700)
Baseline IV Regression: Only Villages With 50-200 Households	10,700 (16,100)	12,500* (6600)	2100 (5600)	10,300 (11,400)	18,900 (20,900)
IV Regression using All Villages	16,400** (7000)	6600* (3900)	-1000 (2400)	-200 (6300)	6700 (8300)
IV Regression without 1% Outliers	9700 (13,200)	12,600** (6500)	3600 (4000)	-9800 (12,800)	8800 (6000)

** Significant at 5% level * Significant at 10% level

The treatment variable is the level of short-term village fund credit. Coefficients are multiplied by 10,000, representing roughly a treatment effect, since the program led to an average of 9000 baht village fund credit per household. The independent variables are year dummies, household fixed effect dummies, male head of household dummy, number of adult males, number of adult females, number of kids, age of head and age of head squared, and years of schooling of head. The fertilizer expenditure regressions also contain the area of cultivated land as an explanatory variable. The additional instruments in the first-stage are the inverse village size interacted with a dummy variable for year=2002 and year=2003. Standard errors for are robust standard errors clustered by village-year.

Table 8. Impact of Village Fund Credit on Investment and Input Uses

Technique \ Response Variable	Investment and Input Uses						
	Number of New Businesses	Amount of Business Investment	Probability of Business Investment	Amount of Agric. Investment	Probability of Agric. Investment	Total Wages Paid	Fert., Pest., etc. Expenditures
OLS Regression	-0.011* (0.006)	100 (1000)	-0.001 (0.006)	-1000 (1000)	0.006 (0.007)	400 (800)	1000 (600)
Baseline IV Regression: Only Villages With 50-200 Households	0.037 (0.031)	-3300 (4000)	-0.007 (0.029)	-400 (3800)	0.019 (0.032)	-2400 (3100)	-1300 (3100)
IV Regression using All Villages	0.008 (0.022)	-1200 (1900)	-3.2e-4 (0.021)	-1500 (1800)	0.043* (0.027)	-2200 (1600)	-3000 (2400)
IV Regression without 1% Outliers	0.037 (0.031)	-100 (1700)	--	2500 (2500)	--	1100 (1600)	-1100 (1500)

** Significant at 5% level * Significant at 10% level

The treatment variable is the level of short-term village fund credit. Coefficients are multiplied by 10,000, representing roughly a treatment effect, since the program led to an average of 9000 baht village fund credit per household. The independent variables are year dummies, household fixed effect dummies, male head of household dummy, number of adult males, number of adult females, number of kids, age of head and age of head squared, and years of schooling of head. The fertilizer expenditure regressions also contain the area of cultivated land as an explanatory variable. The additional instruments in the first-stage are the inverse village size interacted with a dummy variable for year=2002 and year=2003. Standard errors for are robust standard errors clustered by village-year.

Table 9. Impact on Log Wages in the Monthly Panel

Technique \ Response Variable	Overall Log Wage Rate
Number of Observations	12,283
OLS Regression	0.007 (0.014)
Baseline IV Regression	0.074** (0.026)
IV Regression without 1% Outliers	0.092** (0.025)

** Significant at 5% level * Significant at 10% level

The treatment variable is the 12-month-lagged stock of short-term village fund credit. Coefficients are multiplied by 10,000, representing roughly a treatment effect, since the program led to an average of 9000 baht village fund credit per household in the annual data. The independent variables are time dummies, household fixed effect dummies, and assets and assets squared. The latter is a substitute for the lack of time-varying data on household composition and head-of-household characteristics, which we lack in these data. The additional instruments in the first-stage are the inverse village size interacted with dummy variables for months after the fund was started. Standard errors for are robust standard errors clustered by village.

Table 10. Impact on Log Wages by Occupation

Response Variable / Technique	Log Wage Rates by Occupation							
	Agriculture	**Factory**	**Merchant**	**Govt. and Prof.**	**General Non-Agric.**	**Constr. Within Village**	**Constr. Outside County**	**Other**
Number of Observations	2123	2055	108	3073	913	295	119	2514
OLS Regression	0.011 (0.032)	-0.037* (0.021)	-0.061 (0.053)	-0.005 (0.015)	0.015 (0.054)	0.031 (0.034)	-0.119 (0.156)	0.047* (0.026)
Baseline IV Regression	-0.108* (0.058)	-0.038** (0.019)	-0.117 (0.098)	0.067 (0.066)	0.171** (0.073)	0.287** (0.139)	-0.070 (0.020)	-0.004 (0.0480)
IV Regression without 1% Outliers	-0.146** (0.049)	-0.036** (0.018)	0.064 (0.066)	0.077 (0.067)	0.157** (0.068)	0.116 (0.089)	-0.055 (0.196)	0.019 (0.043)

** Significant at 5% level * Significant at 10% level

The treatment variable is the 12-month-lagged stock of short-term village fund credit. Coefficients are multiplied by 10,000, representing roughly a treatment effect, since the program led to an average of 9000 baht village fund credit per household in the annual data. The independent variables are time dummies, household fixed effect dummies, and assets and assets squared. The latter is a substitute for the lack of time-varying data on household composition and head-of-household characteristics, which we lack in these data. The additional instruments in the first-stage are the inverse village size interacted with dummy variables for months after the fund was started. Standard errors for are robust standard errors clustered by village.

Table 11. Differential Impact of Village Fund Credit on Income Sources of Female-Head Household

Technique \ Response Variable	Income	
	Business Profits	**Wage and Salary**
OLS Regression: Only Villages With 50-200 Households	-9000 (6900)	800 (1800)
Baseline IV Regression: Only Villages With 50-200 Households	-7700 (6100)	3100 (4000)
IV Regression using All Villages	-9000* (5200)	4000 (3100)
IV Regression without 1% Outliers	-6100 (3800)	3900 (4000)

** Significant at 5% level * Significant at 10% level

The treatment variable is the level of short-term village fund credit interacted with a female-headed household dummy. Coefficients are multiplied by 10,000, representing roughly a treatment effect, since the program led to an average of 9000 baht village fund credit per household. The independent variables are the short-term village fund credit (direct effect), year dummies, household fixed effect dummies, male head of household dummy, number of adult males, number of adult females, number of kids, age of head and age of head squared, years of schooling of head, and inverse number of households in village. The additional instruments in the first-stage are the inverse village size interacted with a dummy variable for year=2002 and year=2003, and both of these dummies interacted with a female-headed household dummy.

Table 12. Differential Impact of Village Fund Credit on Consumption Components of Female-Head Household

Response Variable / Technique	Components of Consumption						
	Education	**Meat**	**Alcohol Home**	**Alcohol Out**	**House Repair**	**Vehicle Repair**	**Clothes**
OLS Regression: Only Villages With 50-200 Households	-100 (300)	200 (200)	30 (100)	-300* (100)	-3700** (1400)	20 (500)	100 (100)
Baseline IV Regression: Only Villages With 50-200 Households	-100 (600)	700* (300)	400* (200)	-500** (200)	-100 (3800)	100 (700)	200 (100)
IV Regression using All Villages	200 (600)	700** (200)	400 (200)	-400** (200)	-1400 (3200)	500 (700)	300** (100)
IV Regression without 1% Outliers	-200 (400)	400** (200)	300* (200)	-300** (100)	-2500** (1200)	100 (200)	50 (100)

** Significant at 5% level * Significant at 10% level

The treatment variable is the level of short-term village fund credit interacted with a female-headed household dummy. Coefficients are multiplied by 10,000, representing roughly a treatment effect, since the program led to an average of 9000 baht village fund credit per household. The independent variables are the short-term village fund credit (direct effect), year dummies, household fixed effect dummies, male head of household dummy, number of adult males, number of adult females, number of kids, age of head and age of head squared, years of schooling of head, and inverse number of households in village. The additional instruments in the first-stage are the inverse village size interacted with a dummy variable for year=2002 and year=2003, and both of these dummies interacted with a female-headed household dummy.

Table 13. Average Village Fund Credit and Default Over Time

Credit Indicators / Year	Amount of Village Fund Credit per Household	Fraction of Village Fund Credit in Default
Year 1 (2002)	810	0.01
Year 2 (2003)	990	0.03
Year 3 (2004)	1600	0.02
Year 4 (2005)	1840	0.02
Year 5 (2006)	1910	0.01
Year 6 (2007)	1140	0.02

Table 14. Long Run Impacts

Year \ Response Variable	Response Variables					
	Village Fund Credit	**New Short-Term Credit Level**	**Probability in Default**	**Consumption Level**	**Log Assets**	**Level of Net Income**
Year 1 (2002)	4900** (2000)	12,500** (0.55)	-0.075 (0.050)	9300 (7600)	--	--
Year 2 (2003)	9200** (1100)	15,100** (6700)	0.096* (0.052)	14,700 (9100)	0.043 (0.078)	36,100** (13,700)
Year 3 (2004)	13,800** (2700)	29,200** (11,000)	0.014 (0.065)	3900 (10,000)	0.123 (0.083)	-21,200 (17,400)
Year 4 (2005)	16,700** (2300)	40,800** (12,100)	0.179** (0.061)	15,400 (10,500)	0.027 (0.076)	23,500 (23,900)
Year 5 (2006)	16,900** (2000)	46,000** (14,400)	0.076 (0.058)	-20 (9200)	0.016 (0.088)	9600 (13,200)
Year 6 (2007)	9200** (1500)	17,400** (6900)	0.184** (0.060)	-5300 (7200)	0.095 (0.104)	-14,300 (17,700)

** Significant at 5% level * Significant at 10% level

The treatment variables are inverse village size interacted with year dummies. Coefficients are divided by 1,000,000 (putting them in roughly per baht terms, since that was the total injection in the village) and multiplied by 10,000. Thus, they are roughly a treatment effect, since the program led to an average of 9000 baht village fund credit per household in the first two years. The independent variables are year dummies, household fixed effect dummies, male head of household dummy, number of adult males, number of adult females, number of kids, age of head and age of head squared, years of schooling of head, gross assets and gross assets squared, income, and inverse number of households in village.