# Access to Credit and Productivity : Evidence from Thai Villages

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**Abstract**

Approaches to underdevelopment based on misallocation of resources have two premises. First, that there is huge heterogeneity in terms of underlying productivity among potential and actual entrepreneurs. Second, that the mechanisms that guide resource allocation do not necessarily result in the resources going to the most productive entrepreneurs. Using the Townsend Thai data and the Million Baht program studied by Kaboski and Townsend (2012), we show evidence for both these premises. First, using the fact that the Townsend Thai data include a long time series of pre-intervention information, we estimate TFP household by household. We then show that the effect of the Million Baht program, which was a source of additional short-term credit in the village, varies dramatically by pre-program TFP. There is no discernible effect in terms of income or business profits among low pre-program TFP households but the high TFP households show a large increase in profits (more than 1.5 THB increase in profits for 1 THB in loans). This effect doubles when we restrict to high TFP households that had a non-agricultural business before the intervention. On the other hand, program credit is not allocated based on baseline TFP. However market credit partly mitigates the disparity.

Keywords: Microcredit, entrepreneurship, productivity.

JEL: O16, G21, D21.

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# 1 Introduction

A large literature argues that factor misallocation can explain cross-country differences in output and income, and further, improving the allocation of resources within-country has the potential to unlock economic growth (see, for example, Banerjee and Duflo (2005); Restuccia and Rogerson (2008); Alfaro et al. (2008); Hsieh and Klenow (2009); Bartelsman et al. (2013); Foster et al. (2006) ). This argument relies on two pillars. First, there must be substantial heterogeneity in the productivity across firms and entrepreneurs. Second, market frictions must impede inputs from flowing to the most productive firms.

In this paper, we provide evidence for both ideas in the context of credit and small firms in Thailand. Namely, we ask whether cross-sectional heterogeneity in productivity predicts either the returns to credit or the allocation of credit to households.[1] We begin by developing a simple model of constrained households to show that the shadow price of capital is an increasing function of household productivity (TFP). We then apply our framework to the context of Thailand to assess the extent to which high-productivity households are more likely to increase profits and whether a local credit expansion directs more resources toward those high-productivity households.

Our analysis requires three crucial components. First, we require quasi-exogenous variation in access to credit. For this, we use exposure to the Million Baht Program, one of the largest credit-expansion programs of its kind, which began in Thailand in 2001.[2] We follow Kaboski and Townsend (2012), who exploit the fact that each program village received the same amount of funds from the central government to lend to local households, independent of village size. Thus we can compare villages before versus after the implementation of the program, by per-capita program resources. Second, we require a credible way to measure household TFP. One well-known problem when estimating production functions is that investment decisions may be endogenous to unobserved time-varying shocks to productivity (Olley and Pakes, 1996). We propose a novel method in which we use data on

[1] One key channel of misallocation operates through the credit market, which we study here (see Banerjee and Munshi (2004); Vera-Cossio (2018)).

[2] Concretely, the Thai government disbursed THB one million to each of the 77,000 participating villages (approximately USD 24,000 at 2001 exchange rates). The total program resources account for approximately USD 1.8 billion and reached over 95% of the total villages in Thailand.

household beliefs about future business conditions to proxy for household-specific productivity shocks. Our beliefs-based method is particularly attractive for settings like ours where households are likely credit constrained (Shenoy, 2017). Finally, we require detailed household panel data with enough pre-intervention observations to execute our method for estimating TFP. Here, we also follow Kaboski and Townsend (2012) and use the Townsend Thai Project panel (Townsend, 2007b,a), which follows 960 households from 64 villages. Importantly for our purposes, the panel is unusually long, with five pre-intervention observations per household from 1997-2001, and includes information on assets, inputs, revenues and profits for all household businesses.

Armed with these three tools, we first use the pre-program data to recover estimates of household productivity for all potential borrowers. We then combine the cross-household variation in productivity with the cross-village variation in the size and rollout of the program to test for productivity-based heterogeneity in the effects of the credit expansion.

Consistent with Kaboski and Townsend (2012), we find that indeed, villages with large inverse village sizes experience a large increase in short-term credit following the implementation of the program, relative to the baseline periods. We also find that the allocation of program credit is not detectably different for high- versus low- productivity households. In other words, for this community-driven credit product, credit was not disproportionately directed toward the more productive households.[3] One implication is that potential heterogeneity in downstream outcomes is unlikely to be explained by differential access to program credit.

While credit does not flow disproportionately to higher-productivity households, we nevertheless find strong patterns of heterogeneity by baseline productivity on business outcomes. First, we find no detectable impacts of the program on household income or business profits for low productivity households. However, the picture is quite different for high productivity households, which experience increases in total household income coming largely from household enterprise profits. Importantly, as in Banerjee and Duflo (2014), this is evidence that high-productivity households were indeed credit constrained

[3]This is consistent with Vera-Cossio (2018), who studies a different set of villages and argues that village fund credit was likely misallocated based on connections with local leaders.

before the program. Moreover, the increase in profitability comes almost entirely from non-agricultural businesses rather than farm-related activities. One interpretation is that in the Thai context, credit constraints aren't as binding for agricultural businesses, perhaps due to differences in collateralizability of farm versus non-farm assets or due to pre-existing targeted agricultural lending programs.[4]

Next, we show that household productivity predicts larger treatment effects, restricting to the subsample of households with preexisting non-agricultural businesses. We also find evidence that for high-productivity households, program credit crowds in other types of borrowing. While high and low productivity households obtain similar amounts of village fund credit, total short-term borrowing increases more for high-productivity households relative to low-productivity households. Consequently, we find that among owners of pre-existing non-agricultural businesses, high-productivity households are better able to use the village credit to increase profits. This increase in profitability appears to be driven by an immediate increase in assets, rather than increased inventories and wage expenses.

We show that these results are robust in three ways. First, we use an alternate fixed effects-based approach to estimate pre-program household productivity and show that the results are qualitatively quite similar. Second, we present several approaches to impute total labor inputs to use in our estimation of household TFP.[5] We show that our main findings are robust to estimating a production function in per-capita terms, and to the inclusion of the number of paid workers as a measure of labor in the production function estimation. Additionally, we show that the results are robust to using investment data to correct for potential measurement error in capital (Collard-Wexler and De Loecker, 2016).

In order to interpret the magnitude of these effects, we compute the effects of an additional THB of total short-term credit, induced by the introduction of the program, on household profits. We use the quasi-experimental variation in program exposure to instrument for total short-term credit. However, it is important to note that such approach

[4]Agriculture-oriented lenders are prominent in the context of rural Thailand. For instance, before the program's implementation, the Bank for Agriculture and Agricultural Cooperatives (BAAC) provided agricultural loans in all the sample villages.

[5]Unfortunately, the Townsend Thai Project annual data does not track total labor inputs in household businesses (i.e., time use by business activity). It only contains measures of the number of workers hired for non-agricultural businesses and the number of households members whose main occupation is to work in household businesses.

leans heavily on the exclusion restriction – that the program rollout only affected profits through the receipt of village fund credit.[6] With this caveat in mind, we find that, two years after the rollout of the program, profits increased by THB 1.47 per additional THB of short term credit in the case of high-productivity households, suggesting high returns to credit. We fail to find either significant or substantial positive returns to credit in the case of low-productivity households. Moreover, in the case of high-productivity households with preexisting non-agricultural businesses, we document annual returns to credit of the order of THB 2.9 per one additional THB of credit. Our estimates are consistent with evidence of high returns to credit in Morocco (Crepon et al., 2015), large annual returns to cash/asset grants in Mexico (McKenzie and Woodruff, 2008) and high returns to cash grants for entrepreneurs with high business growth potential in India (Hussam et al., 2017).

We also document increases in business assets for households with pre-existing businesses on the order of THB 4-8 per one additional THB of short-term credit. Such increases in fixed capital suggest that in addition to borrowing even more from other sources, households may also have used credit to complement savings for purchasing large assets. In order to quantify the returns to such investments--i.e., the increase in profits per an additional THB increase in assets--, we simply divide the treatment effect on profits by the treatment effect on assets, for the subsample of high-productivity owners of preexisting non-agricultural businesses. Back-of-the-envelope calculations yield estimates of the annual rate of return to fixed capital of 63% (5.25% monthly), way above the average annual interest rate associated with program loans (7%), and the average annual market interest rates for short term loans (9%). These returns are consistent with other estimates provided in the literature such as 39.6% annual for pre-existing firms winning a business plan competition in Nigeria (McKenzie, 2017), or estimates as large as 66-70% annual for the case of SMEs in Sri-Lanka (de Mel et al., 2008).[7]

We contribute to the literature measuring the effects of credit-supply expansions in developing countries and document a new empirical result.[8] In our setting, the most-

[6]To minimize potential violations to this assumption, we focus on estimates covering only the first and second years after the program rollout.

[7]We obtain these values by multiplying the monthly returns reported in the papers by 12.

[8]There is large body of research on the effects of micro-credit expansion programs in several settings ((Karlan and Zinman, 2010)- The Philippines, India (Banerjee et al., 2015), Morocco (Crepon et al., 2015),

productive households benefit the most from a credit expansion. While other studies provide evidence of heterogeneity in the tails of the profits distribution (Banerjee et al., 2015; Crepon et al., 2015) or based on observable characteristics such as pre-period business ownership (Banerjee et al., 2015), our results show that household TFP is predictive of larger treatment effects even within key subpopulations. However, we find no detectable correlation between program credit supply and household TFP in our setting, and our results are consistent with the ex ante credit constraints binding more for high productivity households. Interestingly, program credit does crowd in other sources of credit for high productivity entrepreneurs. One implication is that improved screening and targeting could magnify the impacts of credit expansions. This could potentially entail improvements in externally identifying entrepreneurs (see Fafchamps and Woodruff (2017), Hussam et al. (2017), and Mckenzie and Sansone (2017)). Alternatively, financial institutions could try to design better screening mechanisms for self-targeting (Beaman et al., 2014).

Finally, our paper is related to the large body of literature aiming to estimate production functions and TFP using optimal input decisions to overcome endogeneity issues (Olley and Pakes, 1996; Levinsohn and Petrin, 2003; Ackerberg et al., 2015). Shenoy (2017) argues that the assumptions typically made in the literature are likely unsuitable under credit constraints as frictions in credit markets may prevent households to adjust inputs in response to productivity shocks. We propose a novel implementation of the control-function approach using beliefs about future profits as a proxy variable, rather than intermediate inputs in order to appropriately proxy for productivity. By using beliefs, our approach does not assume away potential credit constraints or frictions in the market for inputs.

The body of the paper proceeds as follows. Section 2 details the empirical context and the data. Section 3 presents a simple framework of credit supply expansions under credit constraints and also outlines our production function estimation methodology. Section 5 documents the core first stage and reduced form results, while Section 6 provides IV estimates of the returns to credit and the returns to fixed capital. Finally, Section 7

Mongolia (Attanasio et al., 2015), Bosnia (Augsburg et al., 2015), Mexico (Angelucci et al., 2015), Ethiopia (Tarozzi et al., 2015)) as well as impact evaluations of alternative approaches to expand credit such as self-help groups Greaney et al. (2016) or governement-funded village fund programs (Kaboski and Townsend (2012) in Thailand and Cai et al. (2017) in China).

concludes.

## 2 Context and Data

We study the heterogeneous impacts of the Million Baht Program on household income and profits in the 64 villages of the Townsend Thai Project (Townsend, 2007b,a). Under the Million Baht program, the Thai government disbursed approximately USD 1.8 billion to 77,000 villages starting in 2001.[9] Our empirical strategy is based on the work of Kaboski and Townsend (2012), hereinafter KT, and exploits the unique implementation of the program to facilitate identification of its causal effects. Notably, the government disbursed exactly THB 1,000,000 to each village regardless of size, wealth or location (approximately USD 24,000 at 2001 exchange rates).[10] As such, inhabitants of small villages stood to receive more credit, on average, than residents of larger villages. In general, most of the credit was lent on a short-term (less than or equal to 12 months) basis, and because any funds repaid to the village fund committees were meant to be used to finance follow-on lending activities, the program could be viewed as a permanent supply shock to local short-term credit.[11]

Kaboski and Townsend (2012) provide quasi-experimental evidence of the effects of the program on household consumption and productive activities. Concretely, they document that short-term borrowing increased , crowding in credit from other lenders in the village, and leading to sizeable effects on consumption (increases of 1.7 THB per THB injected by the program). While most models of credit frictions would predict an increase in business investment and profits, the average effects of the program on productive activities are rather small.[12] In this paper, we build on this previous work by tackling the question of misallocation and asking whether the absence of any average effects on businesses are evidence for misallocation.

[9]See Kaboski and Townsend (2012) for a detailed description of the program.

[10]Subject to each village successfully forming a village fund committee, the body which would ultimately manage the funds and make credit decisions along with loan collections.

[11]However, by 2004 several village fund committees had gone bankrupt due to mismanagement or default outbreaks, spurred by powerful members of the village.

[12]The absence of large, detectible average effects on profits and incomes is consistent with the broader microfinance literature (Banerjee et al., 2015).

We focus on the Thai context for three reasons: First, the Thai Million Baht program provides quasi-experimental variation in the timing and size of the program to identify the effects of the program on household outcomes. Second, cross-village variation in the size of the program allows us to capture enough heterogeneity in household productive characteristics among program borrowers; in small villages which receive large per-capita program funds, both high and low productivity households may borrow. Third, the implementation of the program overlaps with the availability of a long-panel dataset, the Townsend Thai Project, which records extremely detailed household records for 960 households from 64 villages in 4 Thai provinces. The nature of the data is unique in its comprehensiveness and panel length, which allows us to exploit the detailed, repeated nature of the household observations to implement modern panel-data methods to characterize households in terms of pre-program productivity and other productive characteristics.

Table 1 presents summary statistics for the study sample. Two important characteristics are worth emphasizing. First, household economic performance involves a variety of economic activities. While on average, higher shares of household operating income correspond to farming and wage work outside the household, 35% of households have an off-farm business. Second, even before the program, access to credit was common. Over two-thirds of households borrowed either from institutional or informal lenders. Moreover, 50% of households report having an outstanding loan with institutional lenders such as the state-owned Bank of Agriculture and Agricultural Cooperatives (BAAC), commercial banks and other local cooperatives or village organizations.

## 3 A simple theoretical framework

In this section, we propose a simple theoretical framework to characterize the households who are best able to convert increased credit supply into business profits. We argue that in the presence of credit constraints, cross-household variation in the marginal return to capital - i.e., the shadow price from relaxing the budget constraint - is mainly driven by variation in total factor productivity (TFP). In order to illustrate this point, we start by analyzing a simple static profit-maximizing problem of a household or business facing a liquidity constraint.

Households are different in terms of total factor productivity ($TFP = A_i$), and combine $K$ and labor $L$ to produce output $Y$. Consider a Cobb-Douglas production function ($Y_i = A_i K_i^{\alpha_K} L_i^{\alpha_L}$),[13] then each household maximizes profits subject to a budget constraint:

$$\max_{K_i, L_i} A_i K_i^{\alpha_K} L_i^{\alpha_L} - p_K K_i - p_L L_i \tag{1}$$

subject to

$$p_K K_i + p_L L_i \leq B_i \tag{2}$$

Where $B_i$ denotes the total budget available to household $i$, and includes both wealth and credit. We allow heterogeneity in this dimension to capture differences in wealth as well as access to credit across households. Input prices $(p_K, p_L)$ are normalized with respect to the price of output. Let $\lambda_i$ denote the LaGrange multiplier associated with the budget constraint (2). Thus, $\lambda_i$ represents the shadow value of a marginal increase in household $i$'s budget ($B_i$): the marginal return to capital. Thus, if credit expansion programs effectively modify the availability of resources $B_i$, then heterogeneity in $\lambda_i$ captures heterogeneity in the ability of a household to benefit from increases in the supply of credit.

Combining the first order conditions corresponding to the choice of each input, it is possible to show that an optimal solution implies:

$$A_i B_i^{\alpha_K + \alpha_L - 1} \kappa = 1 + \lambda_i \tag{3}$$

As $\kappa$ is strictly positive, $\lambda_i$ is an increasing function of total factor productivity ($A_i$).[14] Moreover, with decreasing returns to scale ($\alpha_K + \alpha_L < 1$), $\lambda_i$ is decreasing in $B_i$. In words, households benefit more from relaxing the budget constraint if productivity is high and if wealth or credit availability are low. In the context of a technology with constant returns to scale, the budget constraint is irrelevant, and only heterogeneity in TFP drives

[13] The theoretical predictions highlighted in this sections do not depend on the number of inputs and hold for concave production functions.

[14] $\kappa = \left(\frac{1}{\alpha_K + \alpha_L}\right)^{\alpha_K + \alpha_L - 1} \left(\frac{\alpha_K}{p_K}\right)^{\alpha_K} \left(\frac{\alpha_L}{p_L}\right)^{\alpha_L}$

heterogeneity in the shadow value of capital.

# 4 Empirical strategy

Kaboski and Townsend (2012) exploit variation in the timing and size of the program to estimate its causal effects on productive outcomes. In particular, they compare changes in outcomes before and after 2001 corresponding to villages with high per-capita expected credit supply (or high inverse village size, $invHH$) to those with low per-capita expected credit supply (or low inverse village size). This approach would lead to the causal identification of the effects of the program under the assumption that there were not time-varying shocks that differently affected small and large villages, and could potentially be related to outcomes. The authors argue that the spatial distribution of village size is as if random and validate the identification assumptions with numerous robustness checks. We build on their empirical approach by analyzing the heterogeneous effects of the program motivated by our theoretical framework.

Our aim is to understand if households with higher $\lambda_i$ do in fact benefit more from the increase in the supply of credit induced by the Million Baht Village Fund program. Let $\lambda_{i,n}$ be the household's shadow value of capital for household $i$ in village $n$, corresponding to the baseline periods. While we do not observe $\lambda_{i,n}$, our theoretical framework suggests that baseline productivity $A_{i,n,t}$ captures important variation in the returns to capital. Thus, in our empirical analysis we aim to estimate the heterogeneous reduced-form effects of the program following:

$$\begin{aligned} y_{i,n,t} =& \delta_1 invHH_n \times Post_t + \delta_2 invHH_n \times Post_t \times \text{High A}_{i,n} + X_{i,n,t}\Gamma \\ &+ \delta_3 \text{High A}_{i,n} + \theta_t \times \text{High A}_{i,n} + \theta_t + \theta_n + e_{n,t} \end{aligned} \tag{4}$$

Here, n indexes the village, t indexes the year, and i indexes households. High $\text{A}_{i,n}$ is an indicator that identifies households in the top-third of the TFP distribution, within each village. We mainly focus on rankings rather than levels to attenuate potential measurement error as we estimate $A_{i,n}$ (see Section 4.1). $Post_t$ is an indicator that identifies post-program years (2002-2006). We allow for A-specific time trends and include a $1 \times I$ vector of covariates $X_{i,n,t}$ (including household composition, age, and education), village ($\theta_n$) and year fixed effects ($\theta_t$). The coefficients of interest are $\delta_1$, $\delta_1 + \delta_2$, and $\delta_2$; they represent the reduced-form effects of the program for households in the bottom-two thirds of the productivity distribution ($HighA = 0$), high-productivity households, and treatment effect heterogeneity between high and lower productivity households, respectively.

We are also interested in assessing the dynamics of the effects of the program. In section 5, we report estimates corresponding to the following flexible differences-in-differences specification in equation (5), which is separately estimated for high and low productivity households.

$$y_{i,n,t} = \sum_{\tau=1997,\tau\neq 2001}^{\tau=2006} \delta_\tau invHH_n \times \mathbf{I}[t=\tau] + X_{i,n,t}\Gamma + \theta_t + \theta_n + e_{n,t} \tag{5}$$

In this case, the parameters of interest are $\delta_\tau$. They denote differences in the outcome variable between villages with high and low per capita program resources in period $\tau$ relative to the same differences in 2001, the year preceding the full implementation of the program. This exercise is useful to graphically examine potential violations to the parallel trends assumption which is necessary for causal interpretation in differences-in-differences designs.

## 4.1 Production function estimation

Our analysis involves the measurement of baseline productivity $A_{i,n}$ for each potential borrower, which typically requires the estimation of a production function. We model log value added ($va_{i,t}$), aggregated across all household enterprises,[15] as a function of the stock of fixed capital $k_{i,t}$,[16] productivity shocks which are observed by the household but not by

[15] Enterprise activities include cultivation, livestock, production of livestock produce and off-farm family business. Value added is measured as total revenues net of the cost from input usage, other than capital and labor. For instance, we subtract the value of fertilizer, seeds, feed, merchandise and fuel (among others) from total gross household revenues.

[16] The stock of capital is measured as the stock of fixed assets corresponding to farm and non-farm businesses.

the researcher $\omega_{it} = log(A_{it})$, and unexpected shocks to production ($\epsilon_{i,t}$) which are neither known by the household nor by the researcher.[17]

$$va_{i,t} = \beta_0 + \beta_k k_{i,t} + \omega_{it} + \epsilon_{i,t} \tag{6}$$

We are interested in estimating $\omega_{it}$ for each household, which represents variation in value-added conditional on capital.[18] That is, we aim to capture differences across households in their ability to generate value added, holding constant their capital endowments. We note that ideally, Equation (6) would also include labor inputs on the right hand side in addition to capital. However, unfortunately, we do not have detailed data regarding labor hours. In our main analysis, we estimate $\omega_{it}$ using Equation (6), considering only capital inputs. However, we present robustness checks based on estimates that use the number of workers hired for off-farm businesses and the number of adults in the households as a proxy for labor inputs (see Section 5.4).

We allow productivity to evolve following two sources of variation: foreseen variation based on previous realizations (e.g., $\omega_{i,t-1}$) and unforeseen shocks to productivity $\zeta_{i,t}$. The empirical challenge is to consistently estimate $\beta_k$, which is essential to back out $\omega_{it}$. In order to do so, we need to tackle two potential problems. First, households may adjust capital to respond to unforeseen shocks to production $\epsilon_{i,t}$ –i.e., spoilage– and shocks to productivity $\zeta_{i,t}$ such as unexpected favorable business opportunities. Second, households may optimally decide their investment decisions in order to accommodate foreseen variation in productivity $\omega_{it}$ (Olley and Pakes, 1996). Both sources of endogeneity may lead to biased OLS estimates of $\beta_k$. Ideally, we would rely on household-level experimental variation in the stock of fixed capital to compute $\beta_k$. While such a source of variation is not available in our context, the richness and length of our panel dataset allow us to go a long way in reducing these concerns.

[17]We restrict the analysis to a value-added function for ease of exposition of our method. This is an advantage with regard to home-produced goods, which may serve as inputs for the production of other goods. A value added approach prevents double counting.

[18]We use a value-added function over a gross revenue function as households may have different sources of income and use output from one occupation as inputs for another. For instance, a farmer may produce some crops for sale but may use part of the harvest for feed for its livestock. Without a systematic accounting process, a gross revenue approach could lead to double accounting.

In order to tackle the first problem, we define the stock of capital available at the beginning of period $t$ as the stock of capital reported in the survey wave $t-1$. By doing so, we focus on a predetermined measure of capital such that $\mathbf{E}[k_{i,t}\epsilon_{i,t}] = 0$ and $\mathbf{E}[k_{i,t}\zeta_{i,t}] = 0$. This approach is consistent with models in which there is time to build related to productive capital (Kydland and Prescott, 1982) and with evidence of lumpy investments in Thai villages (Samphantharak and Townsend, 2010). Tackling the second problem requires controlling for unobserved variation in $\omega_{i,t}$ which is correlated with capital choices. We propose two approaches that rely on different identification assumptions to overcome this issue.

#### 4.1.1 Fixed-effects approach

In the fixed effects approach, we assume that variation in productivity is explained by a time-invariant component which is correlated with capital decisions, year-specific aggregate shocks, and a time-variant unforeseen shock which is experienced after households choose capital –i.e., $\omega_{it} = \bar{\omega}_i + \omega_t + \zeta_{it}$, with $\mathbf{E}[\bar{\omega}_i k_{it}] \neq 0$ and $\mathbf{E}[\zeta_{it} k_{it}] = 0$. This specification allows us to estimate (6) through a fixed-effects approach using the 5 years preceding the program (1997-2001)[19] and use within-village rankings of the estimated $\hat{\bar{\omega}}_i$ to estimate equations (4) and (5).

While simple, this approach has two limitations. First, by not allowing the foreseen part of productivity to evolve over time, the fixed-effects approach rules out models in which households may accumulate knowledge or develop abilities which may allow them to more efficiently use capital in future periods. If the latter models are the main drivers of households behavior, then the fixed-effects approach may fail to fully account for the relation between capital and productivity. Second, even if a fixed-effects model is a good description of the true data-generating process, the identification of $\beta_k$ will rely on within household-variation in capital, which may be troublesome in contexts in which investment is lumpy and there is measurement error in capital. In such cases, fixed-effects estimates of productivity may end up absorbing most of the variation in the stock of capital.

[19] Concretely, we estimate the following specification through OLS: $va_{it} = \bar{\omega}_i + \beta_k k_{it} + \delta_t + u_{it}$. $\bar{\omega}_i$ represents household-specific indicators and $\delta_t$represents year fixed effects. We then use the OLS coefficients associated to the household-specific indicators as estimates of productivity: $\hat{\bar{\omega}}_i$.

### 4.1.2 Control function approach

A less restrictive approach for estimating $\beta_k$ relies on the use of proxy variables in order to control for variation in productivity (Olley and Pakes, 1996; Levinsohn and Petrin, 2003; Ackerberg et al., 2015). By doing so, this approach allows productivity $\omega_{it}$ to vary over time and across households. Typically, the control-function approach uses variation in the demand for intermediate inputs to proxy for variation in productivity, which involves assuming that there is a strict monotonic relation between the demand of intermediate inputs and productivity. Thus, as long as firms can afford to modify intermediate inputs to accommodate productivity shocks, the control-function approach would yield consistent estimates of $\beta_k$. While appealing, the approach is not well-suited to settings of limited access to credit: liquidity-constrained households/firms may not be able to freely adjust intermediate inputs in order to accommodate productivity shocks, and thus variation in intermediate inputs may not fully capture variation in productivity (Shenoy, 2017).[20]

In this paper, we propose a simple modification to the control-function approach that overcomes some of the problems highlighted by Shenoy (2017) while taking advantage of the benefits of the control function approach. Namely, we use household beliefs about future business conditions to proxy for variation in foreseen productivity. While credit constraints may prevent households from adjusting intermediate inputs to accommodate productivity shocks, they are less likely to prevent household from adjusting their beliefs. We view this insight as a contribution to the literature in cases where credit constraints are likely to bind.

While households observe productivity and we don't, the Townsend Thai survey includes questions about household forecasts of future profits. We postulate that household's beliefs about business conditions in period $t$ ($b_{i,t}$) are a function of capital (observable to the researcher) and productivity (unobservable to the researcher): $b_{i,t} = b(k_{i,t}, \omega_{i,t})$. Thus, our ability to effectively use variation in $b_{i,t}$ to proxy for variation in $\omega_{i,t}$ relies on the idea that

[20] Shenoy (2017) proposes the use of dynamic panel methods that would be based on weaker assumptions regarding optimal firm behavior. However, relaxing such assumptions as in Blundell and Bond (2000), comes at the cost of imposing functional forms to the productivity process (typically, assuming that productivity follows an AR(1) process). Moreover, the implementation of such models requires long time series in order to avoid problems with precision.

if we observed different beliefs across households with similar stocks of capital, it should be the case that households with more positive beliefs are also households with higher productivity. If households fully incorporate variation in productivity into their beliefs in a frictionless way, then beliefs are a strict monotonic function of productivity. Under this assumption, it is thus possible to invert the relation between beliefs and productivity and write down $\omega$ as a function of household beliefs and capital ($\omega_{i,t} = b^{-1}(k_{i,t}, b_{i,t})$).

Under these assumptions, our estimation procedure is similar to the two-stage approach proposed by Levinsohn and Petrin (2003) and can be easily extended to Ackerberg et al. (2015)'s approach to allow frictions in other inputs. We focus on the former for simplicity. First, we use third-order polynomials of $k_{i,t}$ and $b_{i,t}$ to semi-parametrically recover variation in value added that is explained by capital and household beliefs:

$$\hat{va}_{i,t} = \sum_{j=0}^{3}\sum_{l=0}^{3} \hat{\delta}_{jl} k_{i,t}^{j} b_{i,t}^{l} \tag{7}$$

Second, for a given initial value of $\beta_k$, we can recover estimates of productivity shocks:

$$\hat{\omega}_{i,t}(\beta_k) = \hat{va}_{i,t} - \beta_k k_{i,t} \tag{8}$$

Next, we allow non-parametric persistence in productivity by assuming $\omega$ follows a first-order Markov process ($\omega_{it} = \mathbf{E}[\omega_{i,t}|\omega_{i,t-1}] + \zeta_{i,t}$), and estimate $\mathbf{E}[\hat{\omega}_{i,t}|\hat{\omega}_{i,t-1}]$ by regressing $\hat{\omega}_{i,t}(\beta_k)$ on a third-order polynomial of the previous realization of the shock ($\hat{\omega}_{i,t-1}(\beta_k)$). Finally, $\beta_k^*$ is is chosen to minimize the sum of squared residuals:

$$\min_{\beta_k^*} \sum_t \sum_i \left(va_{it} - \beta_k^* k_{it} - E\left[\hat{\omega}_{i,t}(\beta_k)|\hat{\omega}_{i,t-1}(\beta_k)\right]\right)^2 \tag{9}$$

We implement this procedure using the pre-program sample only. A more formal discussion of the identification assumptions and the estimation process are detailed in Appendix Section (C.1). At the end of the procedure, we average the estimates $\hat{\omega}_{i,t}(\beta_k^*)$ over the pre-intervention periods and generate within-village rankings of household productivity. We then use these rankings to analyze heterogeneity in the effects of the program.

Note that our approach relies on the same moment conditions corresponding to Levinsohn and Petrin (2003)– i.e.,$\mathbf{E}[\hat{\zeta}_{i,t}|k_{i,t}] = 0$,[21] but it uses a different source of variation to compute the sample analog of such conditions. Based on the idea that agents smoothly respond to productivity shocks by modifying the demand of intermediate inputs, the traditional control-function approach uses variation in intermediate inputs and capital to recover foreseen productivity $(E[\hat{\omega}_{it}|\hat{\omega}_{it-1}])$.[22]

In contrast, our approach uses variation in household beliefs and capital to proxy for the foreseen part of productivity and makes no assumption regarding the existence or not of liquidity constraints.

Our approach is not free of assumptions. First, it requires that household beliefs capture meaningful variation in value added, conditional on capital. Appendix Table A1 reports within-village correlations between household value added and income forecasts, with and without including the stock of capital as a predictor. Reassuringly, household forecasts are significant predictors of value added. Second, our approach assumes that there is a strict monotonic relation between household beliefs and productivity. This requires that households adjust their beliefs in the same direction of foreseen productivity shocks.[23]

We implement our empirical strategy using household projections of profits at time $t$, which were measured at the end of period $t-1$.[24] In order to account for differences across households in the way in which they form beliefs as well as scale and volatility, we follow Ahlin and Townsend (2007) and use household forecasts under different scenarios to recover subjective beliefs about a successful business. Concretely, we exploit the fact that the survey collects information regarding income projections in *a)* a regular scenario, *b)* an adverse scenario and *c)* a good scenario. We normalize household beliefs by dividing

[21] Because we assume that capital is predetermined with respect to production shocks $(\epsilon_{i,t})$ and to unforeseen innovations in productivity $\zeta_{i,t}$, identification is achieved under the following moment condition $\mathbf{E}[\zeta_{i,t}|k_{i,t}] = 0$ in which $\zeta_{i,t} = va_{i,t} - \mathbf{E}[\omega_{i,t}|\omega_{i,t-1}] - \beta_k k_{i,t}$

[22] Traditional proxy variables are materials or electricity. The control function approach observes, that demand for intermediate goods, $m_{it}$, can be expressed as a function of the current capital stock and productivity, $m_{it} = m_{it}(k_{it}, \omega_{it})$. Under some assumptions, mainly a strictly monotonic relation between $m$ and $\omega$, the demand function can be inverted yielding $\omega_{it} = m^{-1}(K_{i,t}, m_{i,t})$

[23] One clear limitation of this assumption is that it rules out models of cognitive rigidities in the formation in beliefs (Handel and Schwartzstein, 2018).

[24] Note that because we measure beliefs about $t+1$ at the end of period $t$, we assume that such beliefs fully capture the part of productivity in $t+1$ that is correlated with input use. If this assumption fails then beliefs only capture the foreseen part of productivity $(E[\omega_{i,t+1}|\omega_{i,t}])$ instead of the actual realization $(\omega_{i,t+1})$. However, Appendix Section C.1 shows that identification is not compromised when capital is predetermined, which is the case here.

the difference in projected income between a regular and a bad scenario by the difference in projected income between the good and bad scenario.[25]

Table 2 reports estimates of $\beta_k$ under different methods and provides summary statistics of $\hat{\omega}$, averaged across the 5 pre-program periods, which is our main measurement of household productivity $A_i$. While the fixed-effects approach achieves low estimates of $\beta_k$ and larger estimates of productivity than the control-function approach, the implied within-village productivity rankings are similar across both methods: Appendix Table A2 reports correlations between percentile rankings of control-function estimates of productivity (dependent variables) and percentile rankings of fixed-effects estimates of productivity with and without fixed effects (regressor). It shows that both productivity measures are highly correlated. Importantly, Appendix Table A3 shows that our productivity estimates are correlated with household characteristics that are usually associated with higher productivity, such as education. This pattern holds for both estimates of productivity: (the fixed-effects and proxy-variable approach) and suggest that our productivity estimates capture meaningful economic attributes. Throughout the rest of the paper, we present evidence based on estimates from both approaches and rely on results that are robust across both measurement strategies.

# 5 Reduced-form results

## 5.1 First Stage: Effects on program and total short-term credit

We begin by asking whether baseline productivity captures heterogeneity in program borrowing and total short-term borrowing. For the sake of consistency with previous studies (KT), we focus our analysis on observations from 1997 to 2006, covering 5 years of pre and post-program data. Two notes regarding estimation and inference are worth discussing. First, because household outcomes such as income, profits and earnings are likely to exhibit outliers, we winsorize each outcome to the top 1% of the full sample distribution. However, we do report results using untrimmed raw data as robustness in the appendix.

[25] More formally, we define beliefs as the probability of observing high profits as: $b_{i,t+1|t} = \frac{\pi_{\{i,t+1|t\}}}{\pi^g_{\{i,t+1|t\}} - \pi^a_{i,t+1|t}}$. Where $\pi$ denotes profits.

Second, we conduct inference based on block-bootstrapped standard errors at the village level that incorporate both the estimation of the within-village productivity rankings and the estimation of equation (4) in each bootstrap replication. [26]

Figure 1 presents flexible difference-in-differences estimates of the effect of the rollout of the program on program credit (Panel A) and total short-term credit (Panel B), for high and low productivity households (based on the proxy-variable approach). Consistent with Kaboski and Townsend (2012), we find that indeed, villages with large inverse village sizes experience a large increase in short-term program credit following the implementation of the program, relative to the baseline years. These increases are associated with an average loan size of THB 16,000 for compliers (USD 360 at 2001 exchange rate).

We also find that baseline productivity is not predictive of program borrowing. Panel A from Figure 1 shows that differences in program participation in small villages (more per capita resources) with respect to large villages (less per capita program resources) are orthogonal to productivity. This pattern suggests that there was some degree of misallocation as resources were not systematically delivered to the most productive households, and is consistent with evidence of allocative frictions in the program (Vera-Cossio, 2018). Interestingly, this strong "first-stage" for both high and lower productivity households suggests that potential heterogeneity in downstream outcomes is unlikely to be driven by differences in access to program credit.

To analyze whether the program crowded in or crowded out other sources of credit, Panel B from Figure 1 shows the reduced-form effects of the program for high and low productivity households. Instead of crowding out other sources of credit, the program appears to have crowded-in other types of credit. The figure shows point estimates that are larger than those associated with program credit (Panel A). While this result holds for high and low productivity households pooled together, the point estimates are larger in the case of high-productivity households, suggesting that the rollout of the program caused them to borrow more from other lenders. One implication is that the failure of the program to provide more credit to high-productivity households is unlikely to be driven by lack of demand. High-productivity households also borrowed from other sources, likely

[26] Regression tables also present village-clustered standard errors for reference.

at higher rates of interest, as program credit was subsidized. However, these differences are not significant on average (see Column 2 from Panel A in Table 3).

## 5.2 Effects on household income

While there are not heterogeneous effects in program credit, we document strong heterogeneity in the reduced-form effects of the program on household income. The top-left panel of Figure 2 shows that total income did not change significantly for lower-productivity households but increased for high-productivity households due to the program. To complement the graphical evidence, Panel A from Table 3 presents reduced-form estimates corresponding to the specification in equation (4), which capture the effect of an extra per-capita THB of credit in a given village on household outcomes.[27] Column (3) documents statistically significant differences in the effects of the program on income between high and low productivity households ($p < 0.05$). Columns (4) and (5) show that while there is not substantial heterogeneity in the effects on wage income, productivity-based heterogeneity is large in the case of the effects of the program on profits from household enterprises ($p < 0.1$). These patterns are reassuring because household productivity is expected to be more predictive of higher effects in sectors that are more likely to face liquidity constraints. Panel B from Table 3 show qualitatively similar results based on fixed-effects estimates of baseline productivity. [28]

To analyze the main source of this increase, we also look at the effects of the program on farm profits and non-agricultural business profits. We find no evidence of heterogeneous effects on farm profits (see bottom-left panel of Figure 2). However, we observe quite a different pattern in the case of non-agricultural businesses. The top-right panel of Figure 2 shows that non-agricultural business profits increased for high-productivity households and not so in the case of lower-productivity households. Note that although the differences in the effects between high and low productivity households seem to slowly decay over time, they are stronger and more precisely estimated during the first two years of the program.

[27] We divided the point estimates from equation (4) by 1,000,000 to provide a THB-to-THB interpretation.

[28] Interestingly, Panel B shows significant increases in wage income for high-A households and not so for lower-A households. This result is consistent with evidence of increases in wages due to the program (Kaboski and Townsend, 2012).

This pattern is not surprising since baseline productivity may have higher predictive power in earlier periods. Column (8) from Panel A on Table 3 shows that the differences in the reduced-form treatment effects are also statistically significant ($p < 0.10$). Appendix Figure A1 shows that these patterns are qualitatively similar if we use the fixed-effects approach to recover baseline household productivity, and Panel B in Table 3 shows that this alternative approach yields magnitudes that are similar though less precisely estimated than those from our benchmark specification.

These results show that non-agricultural family businesses drive the effects of the program on household profits. One interpretation is that in the rural Thai context, credit constraints are not as binding for agricultural businesses, perhaps due to differences in collateralizability of farm versus non-farm assets or due to preexisting credit options targeting agricultural businesses. Indeed, over one-half of the households in the sample had access to institutional credit at baseline (see Table 1), mainly through the Bank of Agriculture and Agricultural Cooperatives (BAAC) but also through other agriculture-oriented lenders such as production credit groups (PCGs), and cooperatives. The results suggest that in the Thai context, a targeted policy oriented at alleviating constraints for non-agricultural businesses would have complemented the preexisting government-led agricultural programs.

We also analyze whether there is productivity-based heterogeneity in the effects of the program on consumption. Column (3) from Appendix Table (A4) shows that total consumption increases similarly in the case of both high-productivity and low productivity households. We also fail to detect significant heterogeneity on food spending and spending on durables (vehicle and dwelling repairs). One explanation is that while low-productivity households may have borrowed to finance consumption, high-productivity households may have borrowed to generate income and used part of the increases in income to finance consumption. The results are similar using the fixed-effects approach.

### 5.3 Effects on non-agricultural businesses

Next, we explore the extent to which the evidence of higher effects for high-productivity households are driven by the creation or expansion of non-agricultural businesses. Appendix Table (A5) shows that there is not significant heterogeneity in the reduced-form effects of the program on the number of non-agricultural businesses and the number of non-agricultural businesses that were started less than one year prior to the survey. These results suggest that increased credit might have been used to boost preexisting, well-established businesses.

While our results are consistent with previous evidence showing that pre-program business ownership is a relevant source of heterogeneity (Banerjee et al., 2015), it is not clear whether there is an remaining heterogeneity in the effects of credit expansion programs within well-established, preexisting non-agricultural businesses.[29] We begin by visually analyzing the heterogenous effecst of the program on borrowing for this subpopulation. Column (1) from Panel A in Table 4 show that baseline productivity does not predict higher borrowing from the program, suggesting that misallocation is also present in this subpopulation. However, Column (2) shows that productivity predicts higher reduced-form effects of the program on total short-term credit (p<0.10). Although this result is noisier when we use fixed-effect productivity estimates, the patterns go in the same directions (See Column (2) in Panel B). One interpretation is that while some high-productivity households were able to borrow from the program, they were not able to fully satisfy their needs for liquidity and ended up borrowing from other sources of credit as well.

Next, we analyze whether there was productivity-based heterogeneity in the effects of the program on profits from preexisting non-agricultural household enterprises. Figure 3 shows that profits increased substantially for high-productivity households, but not so for low-productivity households in this subsample. Column 3 from Table 4 complements the graphical evidence by showing that there is significant productivity-based heterogeneity in the program effects on profits ($p < 0.05$). The results imply that targeting pre-existing businesses is not sufficient to achieve substantial business growth.

We also find that heterogeneity in the reduced-form effects on profits seems to be driven by high-TFP households increasing non-agricultural business assets ($p < 0.10$) and not to changes in inventories (see Columns (4) and (6)). Thus, high-productivity households seem to have used the increased credit supply to scale up their non-agricultural businesses. There is also significant heterogeneity on wage-labor spending, probably triggered by the

[29] We define business owners as households who hold business assets in the period preceding the program.

large business expansions. However, in the case of high-productivity households, the effects on wage-labor spending (0.3, $p < 0.05$) are not substantial and are relatively small with respect to the effects on assets (8.2, $p < 0.01$). These results are robust to estimating productivity following the fixed-effects approach (see Panel B from Table 4).

Figure 4 plots flexible difference-in-difference estimates for high and low productivity owners of pre-existing businesses. While low-productivity households do not increase business assets due to the program, high-productivity households start scaling up their businesses as early as 2001, the year preceding the full rollout of the program. The increase in assets in the case of high-tfp households precedes the increases in business profits due to the program. This pattern is consistent with time-to-build models and suggest that the returns from business expansions are not be immediate.

One possible explanation for observing increases in assets as early as 2001 is that we were able to detect very short-term effects of the program: the 2001 annual resurveys were conducted between May and June over a period of 6 weeks and may include data from villages with early program exposure. Exploring a different dataset which covers other villages in the same region on a monthly basis(the Townsend-Thai Monthly Survey), we find that program resources were disbursed as early as June of 2001 in some villages (See Appendix Figure A3). To analyze the extent to which our results are mostly driven by early reactions in 2001, we drop 2001 from the estimating sample and compute reduced-form effects of the program on non-agricultural business assets. Reassuringly, Appendix Figure A2 shows that this exercise yields estimates of the program effects on assets that are even larger than those from our main specification.[30]

## 5.4 Robustness

**Sensitivity to trimming potential outliers.** Our main results are based on trimmed outcome variables which were top coded with respect to the 99th percentile of the respective outcome distribution. Appendix Tables A6 and A7 present reduced-form effects of the program using raw, untrimmed outcome data. As expected, both tables show that

[30] An alternative explanation is that the announcement of the program triggered rapid investment responses. For instance, providers may sell assets on credit with the idea of recovering part of the value of assets once the program resources are disbursed.

point estimates tend to be larger but noisier than the ones corresponding to our main specification.

**Sensitivity to differences in the effective rollout of the program.** We provide two robustness exercises to test the importance of cross-village differences in the effective rollout of the program. First, as we discussed in the previous sections, some villages obtained program resources as early as 2001. Appendix Tables A8 and A9 show that all the main results are robust to dropping observations corresponding to 2001 from the estimation sample. Second, while the program created village funds in most of the sample villages, there were 7 villages that had other preexisting village funds before the program. Appendix Tables A10 and A11 show that the results are robust to excluding these 7 villages.

**Sensitivity to using productivity rankings.** Our main results highlight differences between households who belong to the top third and the bottom two thirds of the productivity distribution in each village. Appendix Tables A12 and A12 show that the results are robust to using the productivity percentile rank itself and suggest that our results are not dependent on how we group households in terms of productivity.

**Accounting for labor.** One limitation of our empirical analysis is that, due to data constraints, our productivity estimates do not account for the role of labor and only capture variation in output conditional on the stock of capital. As a result, high-productivity households ($HighA$) are the ones that would generate more value added given a certain amount of productive capital, but may not be the ones that would generate more value-added holding constant *both* capital and labor. However, our estimates of productivity would still capture economically meaningful variation in contexts in which the effects of micro-credit programs on household profits are not likely to be driven by adjustments in labor markets. Our results suggest that such a scenario is likely to fit the Thai context.[31]

We report two robustness analyses that try to account for labor using proxies.[32] First, we replicate our analysis estimating the production function in per-capita values in order

[31] Kaboski and Townsend (2012) fail to find average effects on household spending in labor and provide suggestive evidence of impacts on the probability of investment in agricultural assets.

[32] Ideally, we would want to observe information regarding time use. In particular, we would need information on the number of hours allocated to household production by household members and the number of work hours by hired labor. Unfortunately we only observe the number of household members that report mainly working in household enterprises and the number of hired workers for non-agricultural business.

to account for household size which could be correlated with labor. Second, we replicate the control-function approach including the number of household members who reported working in household production as their main occupation plus the number of hired workers for household non-agricultual businesses as a proxy for labor.[33] Appendix Figures A4 and A5 replicate our main results using these approaches. Though noisier, the patterns are still similar to those corresponding to our main empirical approach.

**Accounting for measurement error in capital.** It is possible that capital is measured with error, leading to attenuation bias and over-estimating productivity for capital-intensive households. In order to test the sensitivity of our main results to measurement error in capital, we follow Collard-Wexler and De Loecker (2016) and estimate a value-added production function using expenditures on fixed capital goods in period $t-1$ to instrument for current capital both in the first and second stage of our estimation procedure (see Online Appendix Section(C.4) for details regarding the estimation procedure). This approach leads to higher estimates of $\beta_k$ even after including labor, however the estimates are noisy as investment can be lumpy in the Thai context. Despite these issues, reassuringly panels (e) and (f) from Figures A4 and A5 show that the results are not qualitatively different to our main estimates and confirm our main results: high-productivity households were better able to convert credit into profits.

# 6 IV estimates of the returns to credit

While the reduced-form estimates are important to test the presence of productivity-based heterogeneity, we also provide IV estimates of the local average treatment effect (LATE) of an additional THB of credit on profits corresponding to households who were induced to borrow more due to the program. This approach provides an approximation of the baht-to-baht relationship between total short-term credit and household productive outcomes for program borrowers.

We slightly modify the approach used by Kaboski and Townsend (2012) by using the variation induced by the timing and relative size of the program to instrument for total

[33] We estimate a slightly modified version of our main approach using the two-stage procedure described by Ackerberg et al. (2015). See Online Appendix Section(C.2) a detailed description of the procedure.

short-term credit as opposed to program credit only. We chose that specification because we found evidence suggesting that the program crowded in other sources of credit. [34] We then estimate the effects of short-term credit on household outcomes using the following specification:

$$\begin{aligned} y_{i,n,t} = & \beta_1 STCR_{i,n,t} + \beta_2 \text{High A}_{i,n} \times STCR_{i,n,t} + X_{i,n,t}\Gamma \\ & + \beta_3 \text{High A}_{i,n} + \theta_t \times \text{High A}_{i,n} + \phi_n + \phi_t + \epsilon_{i,n,t} \end{aligned} \tag{10}$$

with first stage:

$$STCR_{i,n,t} = \sum_{\tau=2002}^{\tau=2003} \delta_\tau invHH_n \times \mathbf{I}[t=\tau] + X_{i,n,t}\Sigma + \theta_t + \theta_n + e_{n,t} \tag{11}$$

Here, the parameters of interest are $\beta_1$, which captures the LATE of short-term credit on business profits for low-productivity households, $\beta_2$ which captures the differential effect of credit between high and low productivity households, and $\beta_1+\beta_2$ which captures the LATE for high-productivity households. Note that we also need to instrument for High $\text{A}_{i,n} \times STCR_{i,n,t}$. Because High $\text{A}_{i,n}$ is predetermined, we simply construct the first stage by pre-multiplying all terms in the standard first stage by High $\text{A}_{i,n}$. In the structural equation, this yields two endogenous regressors ($STCR_{i,n,t}$ and High $\text{A}_{i,n} \times STCR_{i,n,t}$) and two sets of instruments.

We focus the analysis on the first two years following the rollout of the program for three reasons. First, most experimental evidence is based on outcomes measured between one and two years from the initial intervention. Second, the reduced-form analysis presented in the previous section suggests that heterogeneity is more precisely estimated during the first couple of years following the introduction of the program. Finally, the exclusion restriction –i.e., the program only affected household outcomes through short-term credit– is less likely

[34] Moreover, potential responses in local credit markets are likely to occur in this setting. For instance, (Kinnan and Townsend, 2012) show that households rely on indirect access to formal credit to smooth consumption and investment decisions.

to hold for a longer time horizon. For instance, households may reinvest resources and general equilibrium effects are more likely to kick in as suggested by Buera et al. (2012); indeed, using data a five-year post-program time span, Kaboski and Townsend (2012) detect increases in wages due to the program. Considering these caveats, we emphasize that results corresponding to IV estimates are rather suggestive but still useful as they constitute a tool to compare the approximated financial returns to an extra unit of credit with other estimates in the literature.

We find that our IV estimates imply sizable returns to credit for high-productivity households. First, we focus on the full sample. Table 5 reports IV estimates on household income and profits. Columns (1) and (2) from Panel A show that in the case of high-productivity households, income increases by THB 1.4 to 2.8 per additional THB of total credit, depending on whether we use trimmed data or raw data respectively. These estimates are significantly different than those corresponding to lower-productivity households. Columns (5) and (6) report large effects on profits in the case of high-productivity households which imply annual returns to credit of 100-250% (See bottom rows form Panel A) and rather small and insignificant negative returns for low-productivity households. In fact, we do find negative significant effects of short-term credit on total income in the case of low-productivity households. One interpretation is that given that selection into the program was not a function of productivity, the program resources ended up financing fruitless projects in the case of lower-productivity households.

Second, we find even higher returns to credit when we focus on high-productivity owners of preexisting non-agricultural businesses. The bottom panel of Table 6 shows that the effects on business profits are on the order of THB 2.9 to 5 per additional THB of total credit. Though noisier, the results are robust to including up to five years following the introduction of the program (see Appendix tables A14 and A15).

The point estimates suggest effects that are similar to the effects found by Crepon et al. (2015) in Morocco (2.4). Relative to the literature estimating the returns to cash grants, the returns to credit for Thai high-productivity business owners are as high as those of entrepreneurs who were identified as being of “high-growth potential” by their peers in India–3.3 increase in annual profits per additional rupee from grants (Hussam et al., 2017),[35] and annual returns to cash grants of 2.3-3.9 for Mexican firms(McKenzie and Woodruff, 2008).[36]

Table 6 also shows that, in the case of high-productivity households, there are neither meaningful effects on inventories nor expenditures on wage work, but that there are substantial increases in business assets in the order of THB 4 to 8 per additional THB of credit. One potential explanation to such magnitudes is that, consistent with the idea of the program crowding in other sources of credit, the results suggest that households may have also used cash holdings to complement credit in financing a lumpy investment. Though noisier, the results are robust to including up to five years following the introduction of the program (see Appendix tables A14 and A15).

**Estimates of returns to fixed capital.** Our empirical exercises documents large returns to additional credit in the case of households with preexisting non-agricultural businesses, and that these returns are mostly driven by increases in non-agricultural assets. Given such findings, it is natural to assess the profitability of such investments.

Under the assumption that credit only affected profits through changes in fixed capital, we use the variation in credit supply induced by the program to quantify the returns to non-agricultural fixed capital. Two pieces of evidence suggest the validity of this assumption. First, our previous results showed that non-wage expenses did not significantly increase due to the program. Second, we also find that wage expenses did increase significantly but not substantially as the effects of credit on wage expenses only represents 3% of the effect of credit on assets.[37] However, we do acknowledge that credit could have also modified the use of unpaid household labor, or could have been spent on improving productivity or on non-tangible inputs which are not measured in our data. With these caveats in mind we conduct some back-of-the-envelope calculations to approximate the returns to fixed capital in the Thai context.

To do so, we simply divide our estimates of the effect of credit on profits by our estimates

[35] Hussam et al. (2017) document returns to capital grants as high as 28% monthly, which multiplied by 12 represent 330%

[36] McKenzie and Woodruff (2008) show that monthly profits increase between 292-487 pesos after receiving 1,500 pesos in cash grants.

[37] Table 6 shows that non-agricultural wage expenses increased by THB 0.18 per additional THB of credit. In contrast, non-agricultural assets increased by THB 4.6 per additional THB of credit.

of the effect of credit on business assets, for the subsample of households with pre-exisitng non-agricultural businesses. Our point estimates suggest that profits increased by THB 2.9 per additional THB of credit and that assets increased by THB 4.6 per additional THB of credit (see the bottom panel in Table 6).We find that annual profits increased by THB 0.63 per additional THB of fixed assets: an annual rate of return of 63% per annum (5.2% per month) . These estimates are substantially higher than the interest rates charged by the existing lenders in the Thai context: 7% per annum in the case of program loans, 12% for loans from BAAC bank and 22% for loans from informal lenders. Moreover, the estimates are consistent with other estimates of the returns to fixed capital in the literature: 39.6% annual for pre-existing firms winning a business plan competition in Nigeria (McKenzie, 2017), or estimates as large as 66-70% annual for the case of SMEs in Sri-Lanka (de Mel et al., 2008).[38]

Overall, the results highlight the existence of high-returns to capital for non-agricultural businesses, though only for high-productivity households with consolidated enterprises. One important implication of these results is that the success of public efforts in expanding access to credit is bounded by the ability of policy makers to effectively deliver resources to high-productivity households. Had there been less misallocation, the population-level effects of the program could have been substantially higher.

# 7 Concluding remarks

We use the context of one of the largest microfinance lending programs to provide two main results. First, we document evidence of missallocation by showing that program borrowing was not a function of productivity. Thus, some high-return entrepreneurs ended up not obtaining credit but other less profitable businesses did. Second, we document a large degree of TFP-based heterogeneity in the effects of the program on business profits. Heterogeneity is stronger for entrepreneurs with pre-existing businesses and implies high returns to credit for high-TFP entrepreneurs. Such returns are similar to returns to cash-grant programs for SMEs in developing countries (de Mel et al., 2008; McKenzie

[38]We obtain these values by multiplying the monthly returns reported in each study by 12.

and Woodruff, 2008; Hussam et al., 2017). Put together, our results show that allocative frictions in credit markets may impede the flow of capital to the most productive firms and that there is substantial heterogeneity in the returns to capital across family enterprises.

Our analysis requires a suitable method to estimate household productivity. Popular methods for estimating production functions assume that firms can freely adjust inputs in response to TFP shocks (Olley and Pakes, 1996; Levinsohn and Petrin, 2003; Ackerberg et al., 2015), which is unlikely in the case of credit-constrained businesses (Shenoy, 2017). We propose a novel implementation of the control-function approach using beliefs about future profits as a proxy for TFP shocks, which makes no assumptions regarding how investment and inputs are adjusted and is suitable to a context with potential credit market frictions.

Our results imply that improved screening and targeting could greatly magnify the impacts of credit expansions. While we document high-returns to credit for high productivity households, we also find that program borrowing was orthogonal to baseline productivity. Thus the policy challenge involves effectively targeting high-productivity entrepreneurs. Overcoming this challenge could potentially entail improvements in externally identifying entrepreneurs (see Fafchamps and Woodruff (2017), Hussam et al. (2017), and Mckenzie and Sansone (2017)). The screening mechanism is also likely to matter. For instance, in the case of the Million Baht program, Vera-Cossio (2018) shows that the allocation of program credit was heavily influenced by connections to local leaders. In contrast, Beaman et al. (2014) show that relying only on interest rates as a screening device may allow high-return households to self-select into credit.

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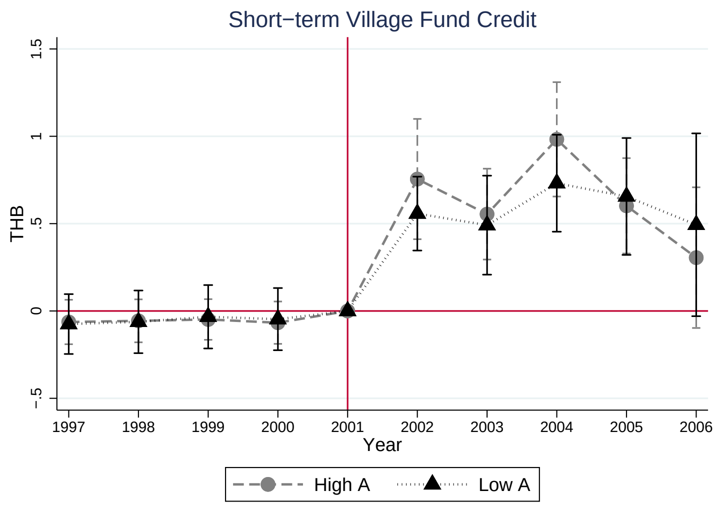
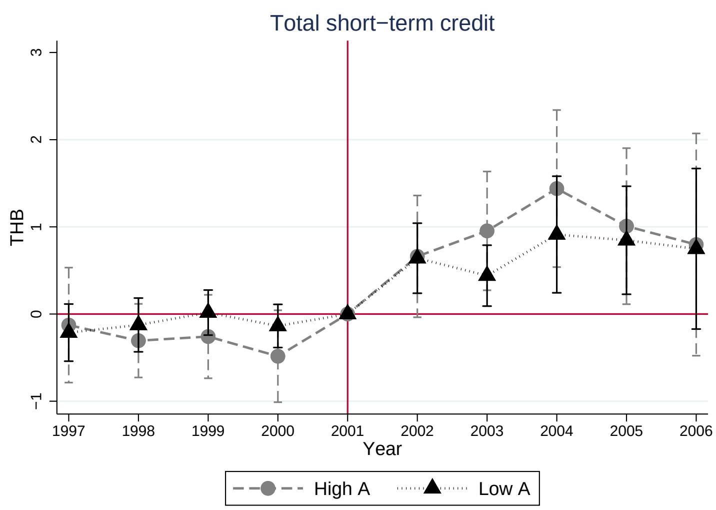
# 8 Figures




(a) Program Credit




(b) Total Short-term Credit

Figure 1: Effects on short-term credit

*Note:* The figure depicts flexible difference-in-differences estimates corresponding to the specification in (5). Each dot represents differences in program borrowing between households from villages with high and low per-capita program funds, for each year, with respect to the year of the announcement of the program (2001). Each coefficient has been scaled down by 1,000,000 in order to capture the effect of an additional per-capita THB in each village on the corresponding outcome (in THB). High A: household belongs to the top-third of the baseline productivity distribution in each village. Low A: household belongs to the bottom two-thirds of the baseline productivity distribution in each village. Productivity estimates correspond to the control-function approach using household beliefs about profits as a proxy variable. 95% confidence intervals are computed based on standard errors, which are clustered at the village level to account for the empirical design.

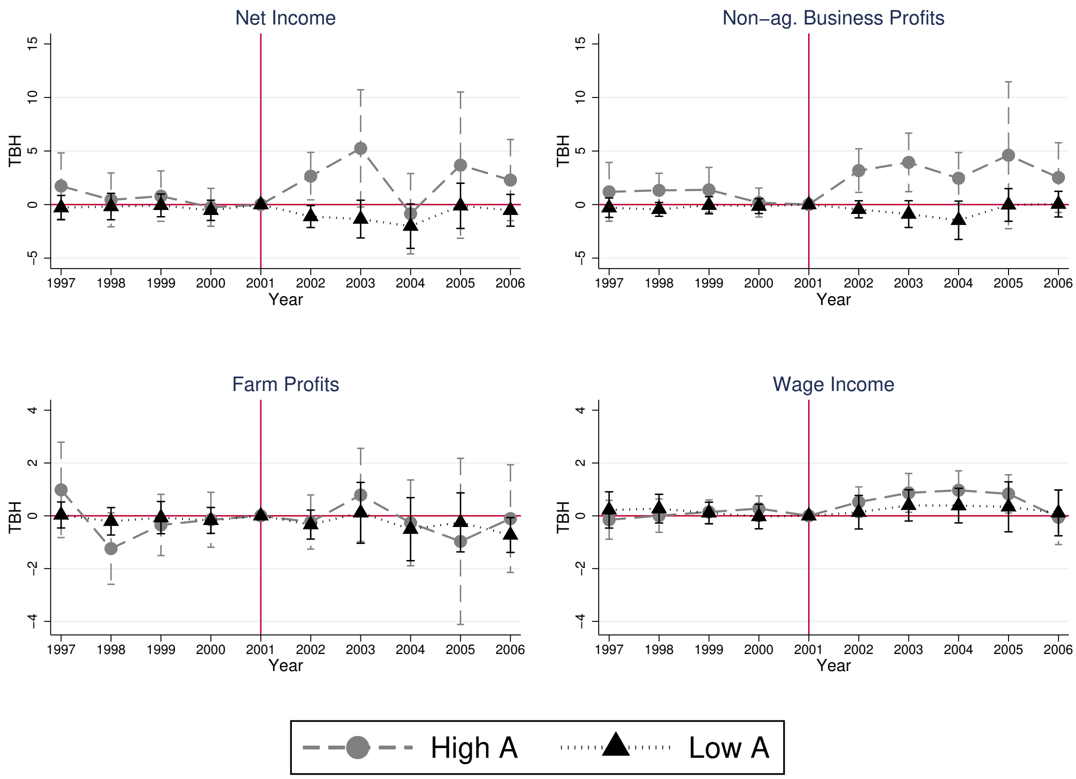



Figure 2: Reduced-form effects on household income - Proxy-variable approach

*Note:* The figure depicts flexible difference-in-differences estimates corresponding to the specification in (5). Each dot represents differences in program borrowing between households from villages with high and low per-capita program funds, for each year, with respect to the year of the announcement of the program (2001). Each coefficient has been scaled down by 1,000,000 in order to capture the effect of an additional per-capita THB in each village on the corresponding outcome (in THB). High A: household belongs to the top-third of the baseline productivity distribution in each village. Low A: household belongs to the bottom two-thirds of the baseline productivity distribution in each village. Productivity estimates correspond to the control-function approach using household beliefs about profits as a proxy variable. 95% confidence intervals are computed based on standard errors, which are clustered at the village level to account for the empirical design.

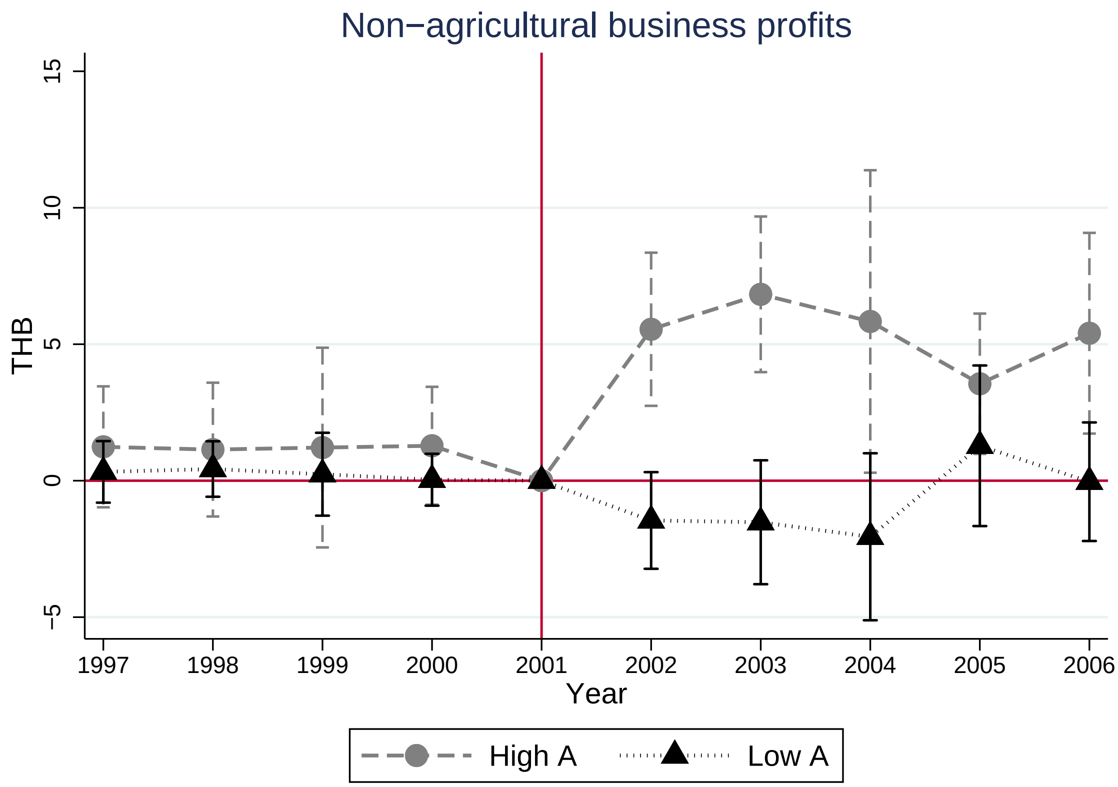



Figure 3: Reduced-form effects on non-agricultural business profits (preexisting non-agricultural businesses)

*Note:* The figure depicts flexible estimates corresponding to the specification in (5).Each dot represents differences in program borrowing between households from villages with high and low per-capita program funds, for each year with respect to the year of the announcement of the program (2001). Each coefficient has been scaled down by 1,000,000 in order to capture the effect of an additional per-capita THB in each village on off-farm business profits (measured in baht). The effects are estimated over a sample of 230 households who reported holding business assets during the year preceding the rollout of the program. High A: household belongs to the top-third of the baseline productivity distribution in each village. Low A: household belongs to the bottom two-thirds of the baseline productivity distribution in each village. Productivity estimates correspond to the control function approach using household beliefs about profits as a proxy variable. 95 % confidence intervals are computed based on standard errors, which are clustered at the village level to account for the empirical design. The dependent variable is winsorized with respect to the top 1% of the distribution.

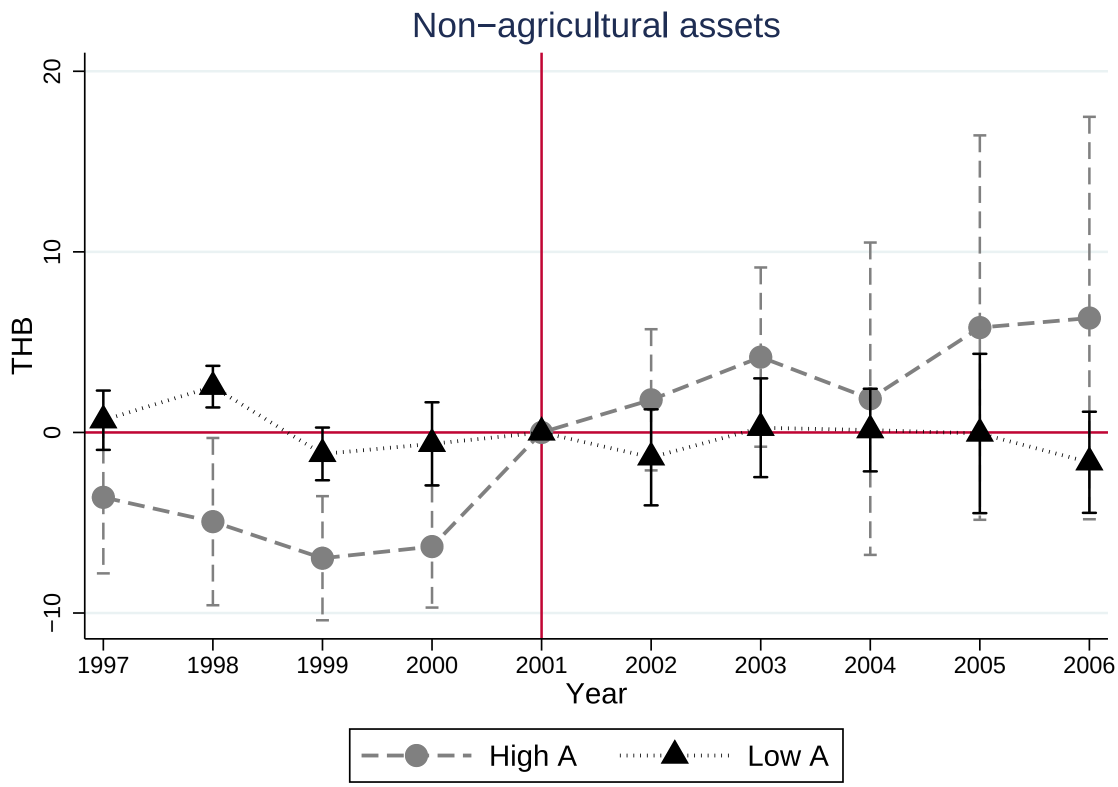



Figure 4: Reduced-form effects on assets (preexisting non-agricultural businesses)

*Note:* The figure depicts flexible estimates corresponding to the specification in (5). Each dot represents differences in program borrowing between households from villages with high and low per-capita program funds, for each year with respect to the year of the announcement of the program (2001). Each coefficient has been scaled down by 1,000,000 in order to capture the effect of an additional per-capita THB in each village on the value of off-farm business assets (measured in baht). The effects are estimated over a sample of 230 households who reported holding business assets during the year preceding the rollout of the program. High A: household belongs to the top-third of the baseline productivity distribution in each village. Low A: household belongs to the bottom two-thirds of the baseline productivity distribution in each village. Productivity estimates correspond to the control function approach using household beliefs about profits as a proxy variable. 95 % confidence intervals are computed based on standard errors, which are clustered at the village level to account for the empirical design. The dependent variable is winsorized with respect to the top 1% of the distribution.

# 9 Tables

Table 1: Baseline Summary Statistics

| Variable | N | Mean | SD |
|---|---|---|---|
| Household head is a male | 4423 | 0.74 | 0.44 |
| Age (household head) | 4423 | 52.85 | 13.43 |
| Years of schooling (household head) | 4343 | 6.04 | 3.18 |
| Number of household members | 4603 | 4.44 | 2.06 |
| Farm (share of net operating income) | 4291 | 0.5 | 2.29 |
| Fish/shrimp (share of net operating income) | 4291 | -0.03 | 2.4 |
| Off-farm business (share of net operating income) | 4291 | 0.1 | 0.82 |
| Wage income (share of net operating income) | 4291 | 0.43 | 1.88 |
| Number of household off-farm businesses | 4423 | 0.35 | 0.55 |
| Household opened a new business (past 12 months) | 4603 | 0.04 | 0.2 |
| Net per-capita income (THB) | 4423 | 21299 | 34592 |
| Per-capita consumption spending (THB) | 4423 | 12046 | 35602 |
| Household borrows (institution or informal) | 4603 | 0.78 | 0.41 |
| Household borrows from formal/quasi-formal sources of credit | 4603 | 0.56 | 0.5 |
| Household borrows from informal sources of credit | 4603 | 0.49 | 0.5 |

*Note:* The table presents summary statistics corresponding to the study sample and survey waves preceding the program (1997-2001). Farm activities include cultivation of several crops as well as produce from livestock. Institutional credit includes credit from commercial banks, BAAC (the state-owned bank) and other quasi-formal sources of credit such as cooperatives, and village-credit groups. Exchange rate THB/USD (2001) : 44.51

Table 2: Estimates of value-added production functions

| Panel A: Capital elasticities | | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| | OLS | FE | Control function |
| $\beta_k$ | 0.360*** | 0.015 | 0.380*** |
| | (0.025) | (0.036) | (0.050) |
| Observations | 2,622 | 2,622 | 2,622 |
| R-squared | 0.137 | 0.004 | |
| # of Households | 835 | 835 | 835 |
| **Panel B: Productivity characteristics** | | | |
| | OLS | FE | Control function |
| Persistence $\omega$ | | | 0.238*** |
| | | | (0.032) |
| Mean $\omega$ | 5.15 | 9.67 | 4.98 |
| SD $\omega$ | 1.02 | 1.17 | 0.06 |

$***p < 0.01, **p < 0.05, *p < 0.1$
Standard errors in parentheses

*Note:* The table presents estimates of the elasticity of value-added with respect to capital $\beta_k$ obtained using the 5 survey waves preceding the program (1997-2001). Column (1) presents OLS estimates for reference, columns (2)-(3) present estimates computed through the fixed-effects approach and the control-function approach, respectively. The bottom panel presents summary statistics for the estimates of log-productivity ($\omega = log(A)$). Standard errors corresponding to the control-function approach are computed using block bootstrap with 1000 iterations.

Table 3: Reduced-form effects of the program on household income and profits

| Panel A: Proxy-variable approach | (1) VF short-term Credit | (2) Short-term Credit | (3) Household Income | (4) Wage Income | (5) Profits | (6) Farm Profits | (7) Shrimp/Fish Profits | (8) Business Profits |
|---|---|---|---|---|---|---|---|---|
| Post X Inv HH X High Productivity | -0.0313 | 0.288 | 2.103** | 0.411 | 1.346* | 0.315 | 0.0219 | 1.01* |
| | (0.0731) | (0.265) | (0.993) | (0.375) | (0.505) | (0.357) | (0.0241) | (0.404) |
| | [0.056] | [0.316] | [0.871] | [0.443] | [0.705] | [0.333] | [0.049] | [0.575] |
| Post X Inv HH | 0.591*** | 0.793*** | -0.901** | 0.0871 | -0.655** | -0.346 | -0.0196 | -0.289* |
| | (0.122) | (0.215) | (0.380) | (0.203) | (0.262) | (0.208) | (0.0339) | (0.280) |
| | [0.106] | [0.304] | [0.381] | [0.223] | [0.236] | [0.208] | [0.044] | [0.15] |
| Effect for High Productivity | 0.560*** | 1.081*** | 1.202 | 0.498 | 0.691 | -0.0312 | 0.002 | 0.720 |
| SE | 0.102 | 0.307 | (0.823) | (0.294) | (0.491) | (0.319) | (0.016) | (0.364) |
| SE (bootstrap) | [0.114] | [0.346] | [0.752] | [0.339] | [0.635] | [0.307] | [0.028] | [0.552] |
| Observations | 8659 | 8659 | 8659 | 8659 | 8659 | 8659 | 8659 | 8659 |
| Number of households | 922 | 922 | 922 | 922 | 922 | 922 | 922 | 922 |
| R-Squared | 0.601 | 0.537 | 0.575 | 0.714 | 0.445 | 0.421 | 0.161 | 0.408 |
| **Panel B: Fixed-effects approach** | (1) VF short-term Credit | (2) Short-term Credit | (3) Household Income | (4) Wage Income | (5) Profits | (6) Farm Profits | (7) Shrimp/Fish Profits | (8) Business Profits |
| Post X Inv HH X High Productivity | -0.294*** | 0.0145 | 1.443 | 0.623* | 0.582 | -0.0545 | -0.0435 | 0.680 |
| | (0.0886) | (0.272) | (0.976) | (0.352) | (0.565) | (0.396) | (0.0629) | (0.411) |
| | [0.119] | [0.471] | [1.266] | [0.387] | [0.607] | [0.435] | [0.069] | [0.529] |
| Post X Inv HH | 0.668*** | 0.877*** | -0.528 | 0.0542 | -0.317 | -0.183 | 0.00167 | -0.136 |
| | (0.0851) | (0.181) | (0.357) | (0.181) | (0.288) | (0.206) | (0.0320) | (0.279) |
| | [0.096] | [0.245] | [0.396] | [0.191] | [0.245] | [0.209] | [0.015] | [0.177] |
| Effect for High Productivity | 0.374*** | 0.892* | 0.915 | 0.677 | 0.265 | -0.237 | -0.0419 | 0.544 |
| SE | 0.142 | 0.348 | (0.868) | (0.301) | (0.542) | (0.369) | (0.051) | (0.381) |
| SE (bootstrap) | [0.141] | [0.517] | [1.116] | [0.343] | [0.614] | [0.409] | [0.075] | [0.533] |
| Observations | 8659 | 8659 | 8659 | 8659 | 8659 | 8659 | 8659 | 8659 |
| Number of households | 922 | 922 | 922 | 922 | 922 | 922 | 922 | 922 |
| R-Squared | 0.604 | 0.539 | 0.575 | 0.715 | 0.444 | 0.421 | 0.161 | 0.408 |

$***p < 0.01, **p < 0.05, *p < 0.1$

*Note:* The table reports the reduced-form estimates of the effects of the program as a function of productivity estimated through the proxy-variable approach (Panel A) and the fixed-effects approach (Panel B). All dependent variables are winsorized with respect to the top 1% of the full sample distribution. The coefficients have been scaled down by 1,000,000 in order to capture the effect of an additional per-capita THB in each village on the corresponding outcome. Each column reports coefficients from regressions following the specification on equation 4. Post X Inv HH captures the reduced-form effect of the program for households belonging to the bottom-two terciles of the baseline productivity distribution. Post X Inv HH X High Productivity report the difference in the reduced form effects of the program between households in the top-third and bottom-two-thirds of the baseline productivity distribution. Reduced-form effects for high-productivity households are computed by adding up the coefficients on Post X Inv HH and Post X Inv HH X High Productivity. Standard errors, in parentheses, are clustered at the village level (64 clusters) to account for the quasi-experimental design. Bootstrapped standard errors are also presented in brackets and are computed using 500 bootstrap samples and clustered at the village level. Short-term credit involves loans with a term shorter than a year. Household profits include farm, fishing and shrimping and non-agricultural business profits. Farm profits include profits from agriculture and livestock. All dependent variables are measured in THB. Exchange rate THB/USD (2001) : 44.51.

Table 4: Reduced-form effects of the program on preexising non-agricultural businesses

**Panel A: Proxy-variable approach**

| | (1) VF short-term credit | (2) Total short-term credit | (3) Profits | (4) Non-wage Expenses | (5) Wage Expenses | (6) Assets |
|---|---|---|---|---|---|---|
| Post X Inv HH X High Productivity | -0.0476 | 1.102* | 5.680*** | 1.406 | 0.294** | 9.241*** |
| | (0.150) | (0.570) | (1.559) | (4.416) | (0.125) | (2.751) |
| | [0.144] | [0.656] | [1.79] | [3.998] | [0.139] | [2.711] |
| Post X Inv HH | 0.566*** | 0.624 | -1.009 | -1.570 | -0.0204 | -0.964 |
| | (0.187) | (0.426) | (0.766) | (1.245) | (0.0443) | (0.794) |
| | [0.203] | [0.571] | [0.652] | [1.78] | [0.076] | [1.772] |
| Effect-High Productivity | 0.518*** | 1.726** | 4.671** | -0.165 | 0.273** | 8.277*** |
| SE | (0.156) | (0.592) | (1.446) | (4.303) | (0.123) | (3.137) |
| SE (bootstrap) | [0.183] | [0.58] | [1.691] | [3.915] | [0.121] | [2.677] |
| Baseline mean (DV) | 11.75 | 19149.5 | 31903.0 | 92164.4 | 3362.5 | 92236.6 |
| Observations | 2190 | 2190 | 2190 | 2190 | 2190 | 2190 |
| Number of households | 229 | 229 | 229 | 229 | 229 | 229 |
| R-Squared | 0.595 | 0.522 | 0.391 | 0.548 | 0.525 | 0.645 |

**Panel B: Fixed-effects approach**

| | (1) VF short-term credit | (2) Total short-term credit | (3) Profits | (4) Non-wage Expenses | (5) Wage Expenses | (6) Assets |
|---|---|---|---|---|---|---|
| Post X Inv HH X High Productivity | -0.407*** | 0.328 | 4.042** | 0.396 | 0.350** | 6.046** |
| | (0.141) | (0.632) | (1.602) | (4.983) | (0.135) | (2.561) |
| | [0.17] | [0.689] | [1.496] | [3.973] | [0.152] | [3.405] |
| Post X Inv HH | 0.685*** | 0.930*** | -0.294 | -1.224 | -0.0328 | 0.454 |
| | (0.152) | (0.287) | (0.760) | (0.904) | (0.0442) | (1.698) |
| | [0.232] | [0.403] | [0.692] | [1.577] | [0.071] | [1.544] |
| Effect High Productivity | 0.278 | 1.257 | 3.748 | -0.829 | 0.318 | 6.500 |
| SE | (0.229) | (0.801) | (1.567) | (4.847) | (0.132) | (2.491) |
| SE (bootstrap) | [0.199] | [0.785] | [1.296] | [3.835] | [0.138] | [3.377] |
| Observations | 2190 | 2190 | 2190 | 2190 | 2190 | 2190 |
| Number of households | 229 | 229 | 229 | 229 | 229 | 229 |
| R-Squared | 0.597 | 0.523 | 0.386 | 0.547 | 0.523 | 0.640 |

$***p < 0.01, **p < 0.05, *p < 0.1$

*Note:* The table reports the reduced-form estimates of the effects of the program as a function of productivity estimated through the proxy-variable approach (Panel A) and the fixed-effects approach (Panel B). All dependent variables are winsorized with respect to the top 1% of the full sample distribution. The coefficients have been scaled down by 1,000,000 in order to capture the effect of an additional per-capita THB in each village on the corresponding outcome. Each column reports coefficients from regressions following the specification on equation 4. Post X Inv HH captures the reduced-form effect of the program for households belonging to the bottom-two terciles of the baseline productivity distribution. Post X Inv HH X High Productivity report the difference in the reduced form effects of the program between households in the top-third and bottom-two-thirds of the baseline productivity distribution. Reduced-form effects for high-productivity households are computed by adding up the coefficients on Post X Inv HH and Post X Inv HH X High Productivity. Standard errors, in parentheses, are clustered at the village level (64 clusters) to account for the quasi-experimental design. PBootstrapped standard errors are also presented in brackets and are computed using 500 bootstrap samples and clustered at the village level. The estimating sample only includes households who reported owning non-agricultural business assets the year preceding the rollout of the program. All dependent variables are measured in THB. Exchange rate THB/USD (2001) : 44.51.

Table 5: IV effects of total credit on income and profits (all households)

**Panel A: IV estimates of the effect of credit on household income (in THB)**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Household Income | | Wage Income | | Total Profits | |
| | Raw | Winsorized | Raw | Winsorized | Raw | Winsorized |
| Total Short Term Credit * High Productivity | 4.317*** | 2.708*** | 0.336 | 0.248 | 3.346*** | 1.784*** |
| | (1.768) | (1.076) | (0.432) | (0.403) | (1.301) | (0.595) |
| | [1.12] | [0.69] | [0.431] | [0.43] | [0.956] | [0.508] |
| Total Short Term Credit | -1.449* | -1.238* | 0.202 | 0.193 | -0.802 | -0.748 |
| | (0.833) | (0.683) | (0.264) | (0.254) | (0.688) | (0.464) |
| | [0.79] | [0.562] | [0.352] | [0.349] | [0.626] | [0.372] |
| Effect- High Productivity | 2.868*** | 1.47*** | 0.54 | 0.44 | 2.544*** | 1.04 |
| SE | (1.26) | (0.33) | (1.02) | (0.53) | (0.16) | (0.86) |
| SE bootstrap | [0.79] | [0.24] | [0.81] | [0.34] | [0.14] | [0.69] |

**Panel B: IV estimates of the effect of credit on Profits by source (in THB)**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Farm | | Fishing/Shrimping | | Off-farm Business | |
| | Raw | Winsorized | Raw | Winsorized | Raw | Winsorized |
| Total Short Term Credit * High Productivity | 0.519 | 0.215 | -0.119 | 0.0210 | 2.946*** | 1.548*** |
| | (0.330) | (0.250) | (0.117) | (0.0322) | (1.229) | (0.575) |
| | [0.45] | [0.34] | [0.094] | [0.057] | [0.999] | [0.44] |
| Total Short Term Credit | -0.0445 | -0.208 | 0.00790 | -0.0179 | -0.766 | -0.522 |
| | (0.414) | (0.239) | (0.108) | (0.0682) | (0.568) | (0.404) |
| | [0.437] | [0.339] | [0.085] | [0.055] | [0.597] | [0.247] |
| Effect- High Productivity | 0.47 | 0.01 | -0.11 | 0.00 | 2.18*** | 1.025*** |
| SE | (0.53) | (0.16) | (0.86) | (0.7) | (0.27) | (0.36) |
| SE bootstrap | [0.34] | [0.14] | [0.69] | [0.45] | [0.23] | [0.36] |
| First-stage F-stat: Short Term Credit | 10.68 | | | | | |
| First-Stage F-stat: Interaction | 11.92 | | | | | |
| Observations | 6117 | 6117 | 6117 | 6117 | 6117 | 6117 |
| Number of households | 911 | 911 | 911 | 911 | 911 | 911 |

$***p < 0.01, **p < 0.05, *p < 0.1$

*Note:* The table reports instrumental-variables estimates of the effects of total short-term credit as a function of productivity estimated through the proxy-variable approach following the specification in equation 10. Panel A presents the effects of total short-term credit on income by source and Panel B presents effects of total short-term credit on profits, by type of activity. Odd-numbered columns report IV coefficients after truncating the dependent variable at the top 1%. “Total Short Term Credit” denotes the effects of short-term credit on the outcome of interest for households from bottom two thirds of the baseline productivity distribution. “Total Short Term Credit X High Productivity” denotes differences in the effects of short-term credit between high and low productivity households. “Effect- High Productiviy” is computed by adding the coeffcients of “Total Short Term Credit”’ and “Total Short Term Credit X High Productivity ”, and represents the effects of short-term credit for households from top-third of the baseline productivity distribution. Standard errors, in parentheses, are clustered at the village level (64 clusters) to account for the quasi-experimental design. Bootstrapped standard errors are presented in brackets and are computed using 500 bootstrap samples and clustered at the village level. Short-term credit involves program loans with a term shorter than a year and has been top coded with respect to the 99th percentile for precision. Household profits include farm, fishing and shrimping and off-farm business profits. All dependent variables are measured in THB. Exchange rate THB/USD (2001) : 44.51.

Table 6: IV effect of total credit on preexisting non-agricultural businesses

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Profits | | Non-wage Expenses | | Wage Expenses | | Assets | |
| | Raw | Winsorized | Raw | Winsorized | Raw | Winsorized | Raw | Winsorized |
| Total Short Term Credit * High Productivity | 8.525** | 6.596** | 0.819 | 2.317 | 0.219 | 0.287** | 6.692 | 5.760* |
| | (3.625) | (2.596) | (2.918) | (2.417) | (0.301) | (0.134) | (5.051) | (2.968) |
| | [3.844] | [2.061] | [3.136] | [2.543] | [0.315] | [0.133] | [5.478] | [2.862] |
| Total Short Term Credit | -2.696 | -3.621* | -2.916 | -2.962 | -0.0742 | -0.106 | 1.501 | -1.079 |
| | (2.810) | (2.175) | (3.729) | (2.994) | (0.252) | (0.113) | (2.409) | (1.347) |
| | [2.744] | [1.483] | [3.202] | [2.495] | [0.295] | [0.114] | [3.02] | [2.027] |
| Effect- High Productivity | 5.829** | 2.975** | -2.10 | -0.65 | 0.14 | 0.181** | 8.19 | 4.681** |
| SE | (1.45) | (0.88) | (2.23) | (1.9) | (0.18) | (0.06) | (5.25) | (2.44) |
| SE bootstrap | [2.07] | [1.16] | [2.51] | [1.999] | [0.21] | [0.07] | [5.09] | [1.95] |
| First-stage F-stat: Short-term credit | 4.74 | | | | | | 0 | |
| First-Stage F-stat: Interaction | 9.05 | | | | | | | |
| Observations | 1544 | 1544 | 1544 | 1544 | 1544 | 1544 | 1544 | 1544 |
| Number of households | 227 | 227 | 227 | 227 | 227 | 227 | 227 | 227 |

$***p < 0.01, **p < 0.05, *p < 0.1$

*Note:* The table reports instrumental-variables estimates of the effects of total short-term credit as a function of productivity estimated through the proxy-variable approach following the specification in equation 10. The estimating sample includes households with preexisting non-agricultural businesses. Odd-numbered columns report IV coefficients after truncating the dependent variable at the top 1%. 'Total Short Term Credit" denotes the effects of short-term credit on the outcome of interest for households from bottom two thirds of the baseline productivity distribution. "Total Short Term Credit X High Productivity" denotes differences in the effects of short-term credit between high and low productivity households. "Effect- High Productiviy" is computed by adding the coeffcients of "Total Short Term Credit"' and "Total Short Term Credit X High Productivity ", and represents the effects of short-term credit for households from top-third of the baseline productivity distribution. Standard errors, in parentheses, are clustered at the village level (64 clusters) to account for the quasi-experimental design. Bootstrap standard errors are also reported in brackets and are computed using 500 bootstrap samples and clustered at the village level. Short-term credit involves program loans with a term shorter than a year. Business ownership is defined as owning non-agricultural business assets the year preceding the rollout of the program. All dependent variables are measured in THB. Exchange rate THB/USD (2001) : 44.51.

APPENDIX

# A Supportive evidence and robustness checks

Table A1: Correlation between beliefs and value-added

| VARIABLES | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | log Value Added | | | |
| Log beliefs | 0.0530*** | 0.0296** | 0.0512*** | 0.0334** |
| | (0.0170) | (0.0143) | (0.0167) | (0.0150) |
| Observations | 1,915 | 1,911 | 1,915 | 1,911 |
| R-squared | 0.010 | 0.137 | 0.155 | 0.240 |
| Control for capital | No | Yes | No | Yes |
| Village F.E. | No | No | Yes | Yes |

$***p < 0.01, **p < 0.05, *p < 0.1$

*Note:* The table presents estimates of a regression of value-added in logs on household beliefs regarding current profits for several specifications. Standard errors are clustered at the household level to account for serial correlation. Beliefs are measured as the self-reported household projected income for period $t$ predicted in $t-1$.

Table A2: Correlation between within-village productivity rankings

| | (1) | (2) |
|---|---|---|
| | **Percentile ranking - Proxy-variable Method** | |
| Percentile ranking - Fixed Effects Method | 0.470*** | 0.499*** |
| | (0.0315) | (0.0341) |
| Constant | 0.331*** | 0.316*** |
| | (0.0187) | (0.0165) |
| Observations | 821 | 821 |
| R-squared | 0.208 | 0.266 |
| Village FE | NO | YES |

$***p < 0.01, **p < 0.05, *p < 0.1$

*Note:* The table presents correlations between the within-village productivity rankings obtained by the proxy-variable method and the fixed effects method. Standard errors are clustered at the village level.

Table A3: Correlates of baseline productivity and demographic characteristics

| | Proxy-variable | | | Fixed-effects | | |
|---|---|---|---|---|---|---|
| | (1) Productivity | (2) Productivity Rank | (3) High-productivity | (4) Productivity | (5) Productivity Rank | (6) High-productivity |
| Number of Adult Males in Household | 0.0626** | 0.0199* | 0.0159 | 0.158*** | 0.0335*** | 0.0516*** |
| | (0.0305) | (0.0104) | (0.0185) | (0.0417) | (0.0111) | (0.0179) |
| Number of Adult Females in Household | 0.259*** | 0.0923*** | 0.126*** | 0.271*** | 0.0432*** | 0.0673*** |
| | (0.0333) | (0.0118) | (0.0235) | (0.0505) | (0.0129) | (0.0219) |
| Number of Children in Household | -0.0638*** | -0.00754 | 0.00103 | 0.0295 | 0.00666 | -0.00554 |
| | (0.0239) | (0.00795) | (0.0136) | (0.0285) | (0.00743) | (0.0129) |
| Dummy: Male Head of Household | 0.146* | 0.0438 | 0.0515 | 0.188* | 0.0448 | 0.0394 |
| | (0.0742) | (0.0270) | (0.0426) | (0.106) | (0.0280) | (0.0425) |
| Head's main occupation: Farm (agriculture/livestock) | 0.254*** | 0.0447* | 0.0965** | 0.371*** | 0.0572** | 0.136*** |
| | (0.0732) | (0.0243) | (0.0408) | (0.123) | (0.0260) | (0.0471) |
| Number of Businessowners in Household | 0.340*** | 0.0591*** | 0.0588* | 0.370*** | 0.0618*** | 0.0718** |
| | (0.0637) | (0.0215) | (0.0334) | (0.100) | (0.0197) | (0.0322) |
| Age of Head of Household | -0.00481* | 0.0000670 | 0.00229* | -0.0166*** | -0.00253*** | -0.00334** |
| | (0.00245) | (0.000820) | (0.00136) | (0.00320) | (0.000716) | (0.00136) |
| Years of schooling - HH head | 0.0253** | 0.0122*** | 0.0163** | 0.0357** | 0.00649* | 0.00961* |
| | (0.00975) | (0.00338) | (0.00622) | (0.0149) | (0.00327) | (0.00516) |
| Observations | 886 | 886 | 886 | 811 | 811 | 811 |
| R-Squared | 0.257 | 0.134 | 0.0845 | 0.218 | 0.127 | 0.0893 |

$***p < 0.01, **p < 0.05, *p < 0.1$

*Note:* The table reports within village correlations between estimates of productivity and demographic characteristics. All regressions include village fixed-effects. Standard errors are clustered at the village level.

Table A4: Reduced-form effects of the program on consumption.

**Panel A: Proxy-variable approach**

| | (1) Food | (2) Durables | (3) Total |
|---|---|---|---|
| Post X Inv HH X High Productivity ($\delta_2$) | 0.0528 | 0.137 | 0.345 |
| | (0.0654) | (0.152) | (0.722) |
| | [0.071] | [0.147] | [0.659] |
| Post X Inv HH ($\delta_1$) | 0.0324 | 0.100 | 0.601* |
| | (0.0441) | (0.105) | (0.340) |
| | [0.049] | [0.121] | [0.429] |
| RF effect - High Productivity ($\delta_1 + \delta_2$) | 0.0852 | 0.237 | 0.946 |
| SE | (0.0543) | (0.181) | (0.845) |
| SE (bootstrap) | [0.059] | [0.158] | [0.729] |
| Observations | 8659 | 8659 | 8619 |
| Number of households | 922 | 922 | 922 |
| R-Squared | 0.528 | 0.221 | 0.569 |

**Panel B: Fixed-effects approach**

| | (1) Food | (2) Durables | (3) Total |
|---|---|---|---|
| Post X Inv HH X High Productivity ($\delta_2$) | 0.0743 | 0.259 | 1.105 |
| | (0.0613) | (0.216) | (0.765) |
| | [0.062] | [0.228] | [0.869] |
| Post X Inv HH ($\delta_1$) | 0.0310 | 0.0712 | 0.347 |
| | (0.0448) | (0.144) | (0.402) |
| | [0.046] | [0.075] | [0.41] |
| RF effect - High Productivity ($\delta_1 + \delta_2$) | 0.105 | 0.331 | 1.453* |
| SE | (0.0490) | (0.178) | (0.866) |
| SE (bootstrap) | [0.063] | [0.244] | [0.88] |
| Observations | 8659 | 8659 | 8619 |
| Number of households | 922 | 922 | 922 |
| R-Squared | 0.528 | 0.222 | 0.570 |

$***p < 0.01, **p < 0.05, *p < 0.1$

*Note:* The table reports the reduced-form estimates of the effects of the program on food consumption as a function of productivity estimated through the proxy-variable approach (Panel A) and the fixed-effects approach (Panel B). Column (1) reports effets on food spending, Column (2) presents effects on spending on durables (vehicle and house repairs). Column (3) presents effects on total consumption. Each column reports coefficients from regressions following the specification on equation 4. Post X Inv HH captures the reduced-form effect of the program for households belonging to the bottom-two terciles of the baseline productivity distribution. Post X Inv HH X High Productivity report the difference in the reduced form effects of the program between households in the top-third and bottom-two-thirds of the baseline productivity distribution. Reduced-form effects for high-productivity households are computed by adding up the coefficients on Post X Inv HH and Post X Inv HH X High Productivity. Standard errors, in parentheses, are clustered at the village level (64 clusters) to account for the quasi-experimental design. Bootstrapped standard errors are also presented in brackets and are computed using 500 bootstrap samples and clustered at the village level. Short-term credit involves loans with a term shorter than a year. Household profits include farm, fishing and shrimping and non-agricultural business profits. Farm profits include profits from agriculture and livestock. All dependent variables are measured in THB. Exchange rate THB/USD (2001) : 44.51.

Table A5: Reduced-form effects of the program on business creation.

**Panel A: Proxy-variable approach**

| | (1) # of Non-ag Biz. | (2) New Non-ag Biz. | (3) # of Farm Biz. | (4) New Farm Biz. |
|---|---|---|---|---|
| Post X Inv HH X High Productivity ($\delta_2$) | 2.908 | 1.440 | 0.652 | -0.0252 |
| | (3.587) | (1.779) | (1.173) | (0.354) |
| | [3.461] | [1.554] | [1.423] | [0.458] |
| Post X Inv HH ($\delta_1$) | 0.727 | 0.0641 | -0.522 | -0.105 |
| | (3.533) | (1.395) | (0.946) | (0.217) |
| | [4.105] | [1.209] | [1.007] | [0.314] |
| RF effect - High Productivity ($\delta_1 + \delta_2$) | 3.636 | 1.504 | 0.129 | -0.130 |
| SE | (5.472) | (1.325) | (1.236) | (0.294) |
| SE (bootstrap) | [4.587] | [1.507] | [1.399] | [0.284] |
| Observations | 8658 | 8658 | 8658 | 8658 |
| Number of households | 922 | 922 | 922 | 922 |
| R-Squared | 0.953 | 0.132 | 0.557 | 0.137 |

**Panel B: Fixed-effects approach**

| | (1) # of Non-ag Biz. | (2) New Non-ag Biz. | (3) # of Farm Biz. | (4) New Farm Biz. |
|---|---|---|---|---|
| Post X Inv HH X High Productivity ($\delta_2$) | 6.655 | 1.912 | 2.181 | 0.804** |
| | (6.997) | (1.618) | (1.329) | (0.361) |
| | [6.639] | [1.911] | [2.262] | [0.49] |
| Post X Inv HH ($\delta_1$) | 1.209 | 0.173 | -0.960 | -0.353 |
| | (3.123) | (1.220) | (0.986) | (0.248) |
| | [2.842] | [1.131] | [0.841] | [0.243] |
| RF effect - High Productivity ($\delta_1 + \delta_2$) | 7.864 | 2.085 | 1.221 | 0.451 |
| SE | (8.214) | (1.472) | (1.326) | (0.259) |
| SE (bootstrap) | [6.841] | [1.732] | [2.248] | [0.424] |
| Observations | 8658 | 8658 | 8658 | 8658 |
| Number of households | 922 | 922 | 922 | 922 |
| R-Squared | 0.953 | 0.132 | 0.137 | 0.557 |

$***p < 0.01, **p < 0.05, *p < 0.1$

*Note:* The table reports the reduced-form estimates of the effects of the program on business creation as a function of productivity estimated through the proxy-variable approach (Panel A) and the fixed-effects approach (Panel B). Each column reports coefficients from regressions following the specification on equation 4. Post X Inv HH captures the reduced-form effect of the program for households belonging to the bottom-two terciles of the baseline productivity distribution. Post X Inv HH X High Productivity report the difference in the reduced form effects of the program between households in the top-third and bottom-two-thirds of the baseline productivity distribution. Reduced-form effects for high-productivity households are computed by adding up the coefficients on Post X Inv HH and Post X Inv HH X High Productivity. Standard errors, in parentheses, are clustered at the village level (64 clusters) to account for the quasi-experimental design. Bootstrapped standard errors are also presented in brackets and are computed using 500 bootstrap samples and clustered at the village level. Short-term credit involves loans with a term shorter than a year. Household profits include farm, fishing and shrimping and non-agricultural business profits. Farm profits include profits from agriculture and livestock. All dependent variables are measured in THB. Exchange rate THB/USD (2001) : 44.51.

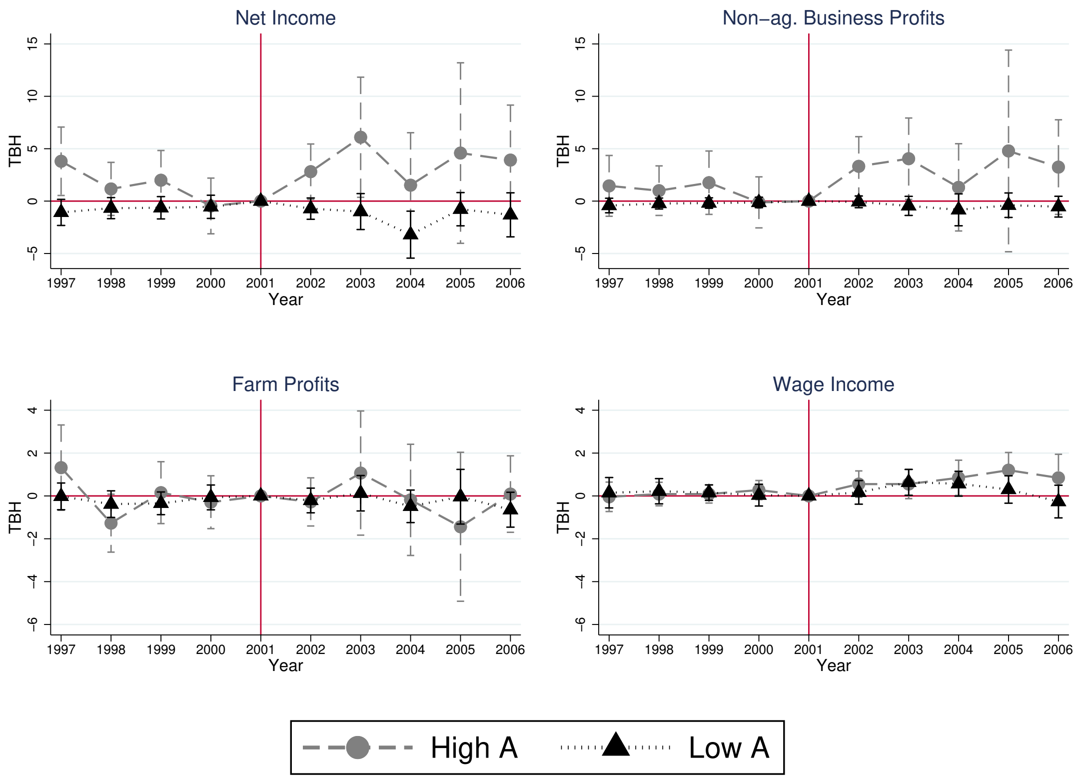



Figure A1: Effects of program rollout on household income - Fixed-effects approach

*Note:* The figure depicts flexible estimates corresponding to the specification in (5). Each dot represents differences in income between households from villages with high and low per-capita program funds, for each year with respect to the year preceding the announcement of the program (2001). Each coefficient has been scaled down by 1,000,000 in order to capture the effect of an additional per-capita THB in each village on the corresponding outcome. High A: household belongs to the top-third tercile of the baseline productivity distribution in each village. Low A: household belongs to the bottom two-thirds of the baseline productivity distribution in each village. Productivity estimates correspond to the control function approach using household beliefs about profits as a proxy variable. 95 % confidence intervals are computed based on standard errors, which are clustered at the village level to account for the empirical design.

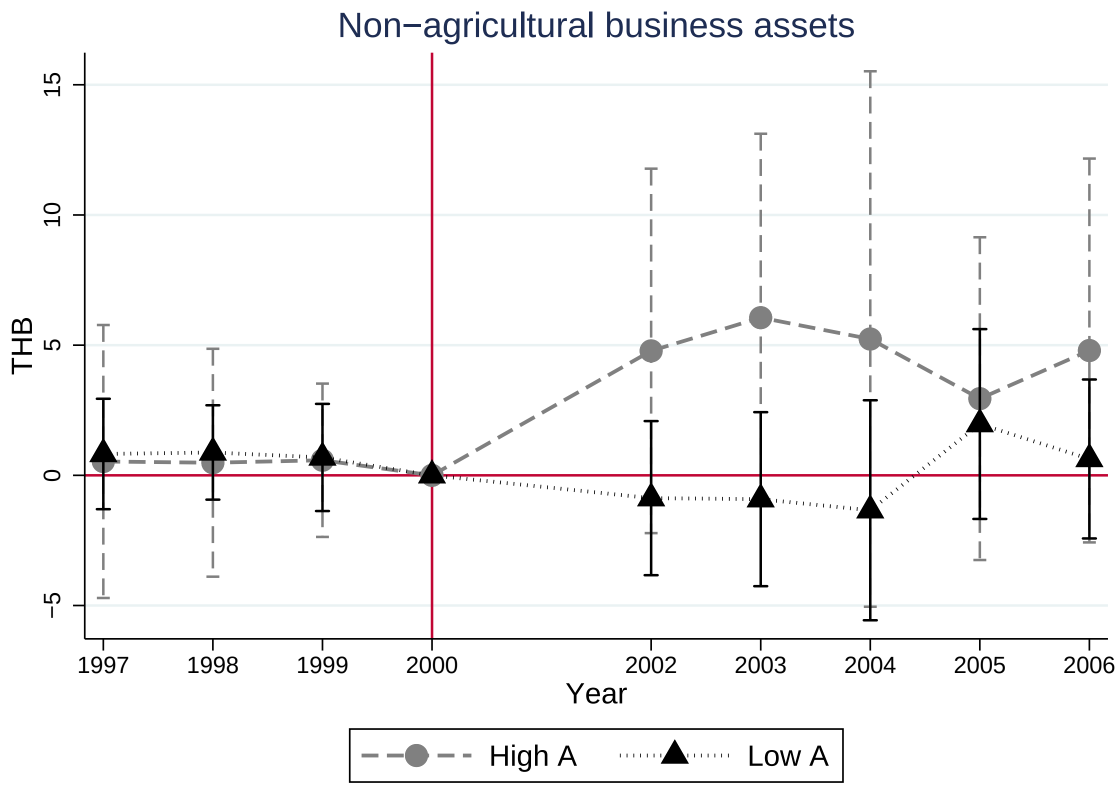



Figure A2: Reduced-form effects on assets (preexisting non-agricultural businesses)
*Note:* The figure depicts flexible estimates corresponding to the specification in (5). Each dot represents differences in program borrowing between households from villages with high and low per-capita program funds, for each year with respect to 2000. Observations corresponding to the 2001 wave were dropped from the estimating sample. Each coefficient has been scaled down by 1,000,000 in order to capture the effect of an additional per-capita THB in each village on the value of off-farm business assets (measured in baht). The effects are estimated over a sample of 230 households who reported holding business assets during the year preceding the rollout of the program. High A: household belongs to the top-third of the baseline productivity distribution in each village. Low A: household belongs to the bottom two-thirds of the baseline productivity distribution in each village. Productivity estimates correspond to the control function approach using household beliefs about profits as a proxy variable. 95 % confidence intervals are computed based on standard errors, which are clustered at the village level to account for the empirical design. The dependent variable is winsorized with respect to the top 1% of the distribution.

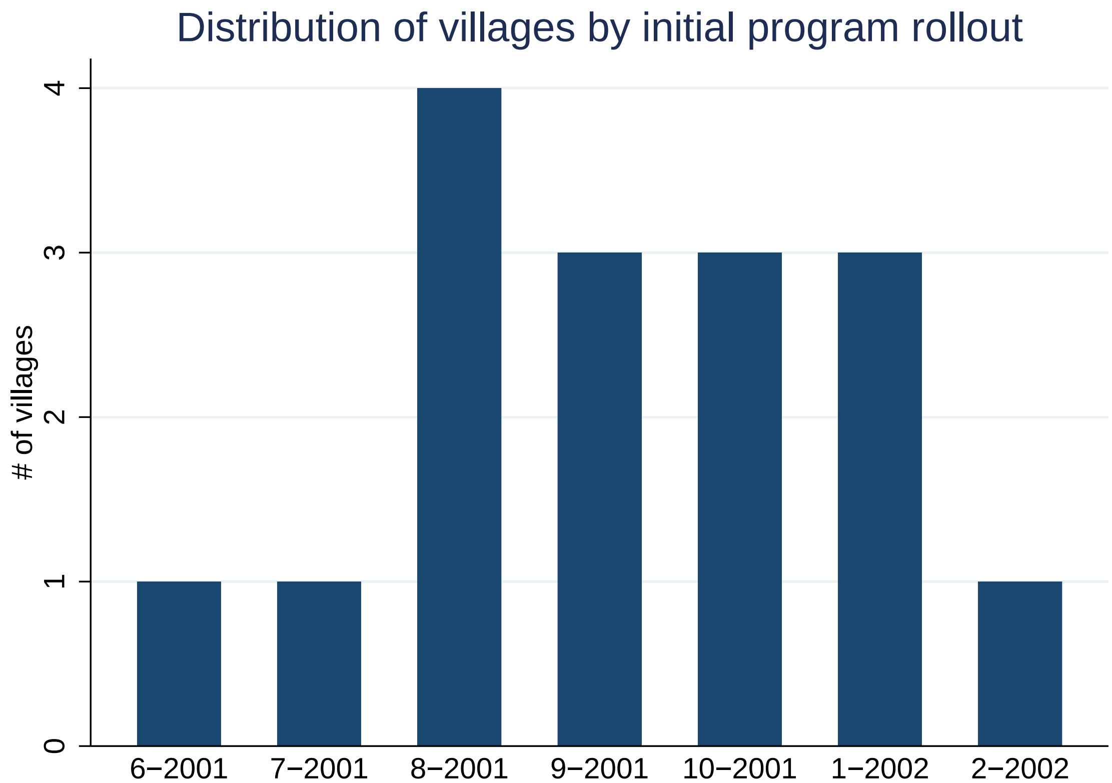



Figure A3: Distribution of villages by month of initial program rollout (Townsend-Thai Monthly Survey)

*Note:* The figure depicts the distribution of villages by program rollout month for the 16 villages in the Townsend-Thai Monthly survey. The villages belong to the same provinces as the ones included in the original annual survey.

## Table A6: Reduced-form effects of the program on income and profits- Without winsorizing

**Panel A: Proxy-variable approach**

| | (1) VF short-term Credit | (2) Short-term Credit | (3) Household Income | (4) Wage Income | (5) Profits | (6) Farm Profits | (7) Shrimp/Fish Profits | (8) Business Profits |
|---|---|---|---|---|---|---|---|---|
| Post X Inv HH X High Productivity | 0.0314 | 0.643 | 3.309** | 0.471 | 3.286** | 0.579 | -0.109 | 2.816** |
| | (0.106) | (0.383) | (1.642) | (0.393) | (1.274) | (0.437) | (0.133) | (1.225) |
| | [0.082] | [0.445] | [1.499] | [0.464] | [1.387] | [0.435] | [0.162] | [1.283] |
| Post X Inv HH | 0.612*** | 0.781** | -0.830 | 0.106 | -0.601 | -0.227 | -0.0391 | -0.335 |
| | (0.124) | (0.282) | (0.541) | (0.209) | (0.465) | (0.287) | (0.0845) | (0.431) |
| | [0.108] | [0.365] | [0.526] | [0.229] | [0.483] | [0.281] | [0.081] | [0.412] |
| Effect for High Productivity | 0.644*** | 1.424** | 2.478* | 0.577 | 2.685* | 0.352 | -0.148 | 2.481* |
| SE | (0.067) | (0.485) | (1.421) | (0.329) | (1.301) | (0.459) | (0.189) | (1.122) |
| SE (bootstrap) | [0.08] | [0.549] | [1.349] | [0.378] | [1.387] | [0.475] | [0.23] | [1.183] |
| Observations | 8659 | 8659 | 8659 | 8659 | 8659 | 8659 | 8659 | 8659 |
| Number of households | 922 | 922 | 922 | 922 | 922 | 922 | 922 | 922 |
| R-Squared | 0.586 | 0.383 | 0.516 | 0.729 | 0.479 | 0.252 | 0.301 | 0.425 |

**Panel B: Fixed-effects approach**

| | (1) VF short-term Credit | (2) Short-term Credit | (3) Household Income | (4) Wage Income | (5) Profits | (6) Farm Profits | (7) Shrimp/Fish Profits | (8) Business Profits |
|---|---|---|---|---|---|---|---|---|
| Post X Inv HH X High Productivity | -0.239*** | 0.0243 | 3.282 | 0.568 | 2.678 | 0.328 | -0.172 | 2.521 |
| | (0.0697) | (0.509) | (1.782) | (0.381) | (1.528) | (0.607) | (0.205) | (1.528) |
| | [0.11] | [0.837] | [2.195] | [0.385] | [1.882] | [0.767] | [0.412] | [1.788] |
| Post X Inv HH | 0.696*** | 0.983*** | -0.555 | 0.114 | -0.149 | -0.0676 | -0.0147 | -0.0672 |
| | (0.0827) | (0.228) | (0.426) | (0.227) | (0.382) | (0.256) | (0.0691) | (0.341) |
| | [0.096] | [0.282] | [0.462] | [0.224] | [0.333] | [0.272] | [0.028] | [0.239] |
| Effect for High Productivity | 0.457*** | 1.007 | 2.727 | 0.682** | 2.529 | 0.261 | -0.186 | 2.454 |
| SE | (0.101) | (0.624) | (1.683) | (0.3) | (1.594) | (0.641) | (0.238) | (1.462) |
| SE (bootstrap) | [0.107] | [0.921] | [2.035] | [0.342] | [1.932] | [0.763] | [0.421] | [1.763] |
| Observations | 8659 | 8659 | 8659 | 8659 | 8659 | 8659 | 8659 | 8659 |
| Number of households | 922 | 922 | 922 | 922 | 922 | 922 | 922 | 922 |
| R-Squared | 0.588 | 0.387 | 0.516 | 0.729 | 0.479 | 0.252 | 0.302 | 0.426 |

$***p < 0.01, **p < 0.05, *p < 0.1$

*Note:* The table reports the reduced-form estimates of the effects of the program as a function of productivity estimated through the proxy-variable approach (Panel A) and the fixed-effects approach (Panel B). Reduced-form effects for high-productivity households are computed by adding up the coefficients on Post X Inv HH and Post X Inv HH X High Productivity. The coefficients have been scaled down by 1,000,000 in order to capture the effect of an additional per-capita THB in each village on the corresponding outcome. Standard errors are clustered at the village level (64 clusters) to account for the quasi-experimental design. Panel A also presents bootstrap standard errors in brackets, which are computed using 500 bootstrap samples and clustered at the village level.

Table A7: Reduced-form effects of the program on non-agricultural preexisting businesses-Without winsorizing

**Panel A: Proxy-variable approach**

| | (1) VF short-term credit | (2) Total short-term credit | (3) Profits | (4) Non-wage Expenses | (5) Wage Expenses | (6) Assets |
|---|---|---|---|---|---|---|
| Post X Inv HH X High Productivity | -0.00998 | 2.148* | 11.21** | 1.138 | 0.480 | 16.35* |
| | (0.167) | (0.990) | (4.401) | (5.938) | (0.352) | (8.814) |
| | [0.16] | [1.055] | [4.559] | [5.079] | [0.332] | [9.032] |
| Post X Inv HH | 0.577*** | 0.606 | -0.487 | -3.751 | -0.0840 | -0.304 |
| | (0.194) | (0.516) | (1.403) | (2.424) | (0.120) | (1.144) |
| | [0.21] | [0.712] | [1.984] | [2.631] | [0.202] | [2.572] |
| Effect-High Productivity | 0.567*** | 2.754** | 10.72** | -2.613 | 0.396 | 16.04* |
| SE | (0.135) | (1.154) | (3.937) | (5.335) | (0.368) | (9.403) |
| SE (bootstrap) | [0.17] | [1.172] | [3.898] | [4.816] | [0.35] | [9.183] |
| Baseline mean (DV) | 12 | 21237 | 50974 | 109802 | 7292 | 119204 |
| Observations | 2190 | 2190 | 2190 | 2190 | 2190 | 2190 |
| Number of households | 229 | 229 | 229 | 229 | 229 | 229 |
| R-Squared | 0.590 | 0.509 | 0.439 | 0.468 | 0.470 | 0.589 |

**Panel B: Fixed-effects approach**

| | (1) VF short-term credit | (2) Total short-term credit | (3) Profits | (4) Non-wage Expenses | (5) Wage Expenses | (6) Assets |
|---|---|---|---|---|---|---|
| Post X Inv HH X High Productivity | -0.434*** | 1.211 | 11.61*** | -3.524 | 0.409 | 13.52 |
| | (0.145) | (1.152) | (3.689) | (6.485) | (0.490) | (9.362) |
| | [0.177] | [1.106] | [3.615] | [5.712] | [0.607] | [9.075] |
| Post X Inv HH | 0.719*** | 1.012*** | -0.159 | -2.055 | -0.031 | 1.161 |
| | (0.144) | (0.364) | (1.103) | (1.343) | (0.108) | (2.308) |
| | [0.227] | [0.514] | [1.497] | [2.392] | [0.23] | [2.293] |
| Effect High Productivity | 0.285 | 2.223 | 11.45 | -5.578 | 0.379 | 14.68 |
| SE | (0.231) | (1.394) | (3.812) | (6.237) | (0.462) | (9.6) |
| SE (bootstrap) | [0.202] | [1.34] | [3.804] | [5.146] | [0.515] | [9.331] |
| Observations | 2190 | 2190 | 2190 | 2190 | 2190 | 2190 |
| Number of households | 229 | 229 | 229 | 229 | 229 | 229 |
| R-Squared | 0.592 | 0.511 | 0.439 | 0.469 | 0.471 | 0.588 |

$***p < 0.01, **p < 0.05, *p < 0.1$

*Note:* The table reports the reduced-form estimates of the effects of the program as a function of productivity estimated through the proxy-variable approach (Panel A) and the fixed-effects approach (Panel B). Reduced-form effects for high-productivity households are computed by adding up the coefficients on Post X Inv HH and Post X Inv HH X High Productivity. The coefficients have been scaled down by 1,000,000 in order to capture the effect of an additional per-capita THB in each village on the corresponding outcome. Standard errors are clustered at the village level (64 clusters) to account for the quasi-experimental design. Panel A also presents bootstrap standard errors in brackets, which are computed using 500 bootstrap samples and clustered at the village level. The estimating sample only includes households who reported owning business assets the year preceding the rollout of the program. The estimating sample only includes households who reported owning non-agricultural business assets the year preceding the rollout of the program.

Table A8: Reduced-form effects on selected outcomes- Excluding 2001

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | | | Raw data | | | | Truncated top 1% | |
| | VF short-term Credit | Short-term Credit | Household Income | Profits | Off-farm Business profits | Household Income | Profits | Off-farm Business profits |
| Post X Inv HH X High Productivity | 0.0240 | 0.547 | 3.589** | 3.358** | 2.516* | 2.389*** | 1.441* | 0.827 |
| | (0.102) | (0.423) | (1.653) | (1.466) | (1.280) | (0.921) | (0.587) | (0.452) |
| | [0.075] | [0.516] | [1.586] | [1.601] | [1.382] | [0.894] | [0.818] | [0.68] |
| Post X Inv HH | 0.600*** | 0.813*** | -0.849 | -0.474 | -0.212 | -0.958** | -0.588** | -0.235 |
| | (0.122) | (0.277) | (0.569) | (0.499) | (0.447) | (0.386) | (0.287) | (0.282) |
| | [0.106] | [0.361] | [0.559] | [0.556] | [0.456] | [0.393] | [0.287] | [0.173] |
| Effect for High Productivity | 0.624*** | 1.360** | 2.740* | 2.884* | 2.305* | 1.430** | 0.853 | 0.592 |
| SE | (0.072) | (0.514) | (1.439) | (1.501) | (1.161) | (0.781) | (0.543) | (0.417) |
| SE (bootstrap) | [0.084] | [0.603] | [1.414] | [1.589] | [1.237] | [0.774] | [0.696] | [0.619] |
| Observations | 7764 | 7764 | 7764 | 7764 | 7764 | 7764 | 7764 | 7764 |
| Number of households | 922 | 922 | 922 | 922 | 922 | 922 | 922 | 922 |
| R-Squared | 0.593 | 0.385 | 0.530 | 0.495 | 0.438 | 0.572 | 0.438 | 0.404 |

$***p < 0.01, **p < 0.05, *p < 0.1$

*Note:* The table presents reduced-form estimates of the program dropping 2001 from the estimating sample. Productivity is estimated through the proxy-variable approach. The coefficients have been scaled down by 1,000,000 in order to capture the effect of an additional per capita THB in each village on the corresponding outcome. Standard errors are clustered at the village level (64 clusters) to account for the quasi-experimental design. Bootstrapped standard errors are reported in brackets and are computed using 500 bootstrap samples and clustered at the village level.

Table A9: Reduced-form effects on selected outcomes for households with preexiting non-agricultural businesses - Excluding 2001

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | | Raw data | | | Truncated top 1 % | |
| | VF short-term credit | Total short-term credit | Profits | Assets | Profits | Assets |
| Post X Inv HH X High Productivity | -0.0321 | 2.184** | 10.41** | 19.36* | 5.349** | 10.98*** |
| | (0.155) | (0.953) | (4.646) | (10.00) | (2.015) | (3.087) |
| | [0.152] | [1.038] | [4.922] | [10.095] | [2.122] | [3.078] |
| Post X Inv HH | 0.580*** | 0.648 | -0.168 | -0.789 | -0.928 | -1.428 |
| | (0.182) | (0.462) | (1.508) | (1.582) | (0.786) | (1.107) |
| | [0.206] | [0.63] | [2.175] | [3.169] | [0.734] | [2.285] |
| Effect-High Productivity | 0.548*** | 2.832*** | 10.25*** | 18.57** | 4.421** | 9.553*** |
| SE | (0.14) | (1.08) | (4.14) | (10.83) | (1.87) | (3.72) |
| SE (bootstrap) | [0.17] | [1.16] | [4.12] | [10.43] | [1.94] | [3.02] |
| Baseline mean (DV) | 0 | 17644.6 | 65887.3 | 125723.4 | 31519.2 | 86039.0 |
| Observations | 1963 | 1963 | 1963 | 1963 | 1963 | 1963 |
| Number of households | 229 | 229 | 229 | 229 | 229 | 229 |
| R-Squared | 0.596 | 0.513 | 0.452 | 0.568 | 0.389 | 0.631 |

$***p < 0.01, **p < 0.05, *p < 0.1$

*Note:* The table presents reduced-form estimates of the program dropping 2001 from the estimating sample. Productivity is estimated through the proxy-variable approach. The coefficients have been scaled down by 1,000,000 in order to capture the effect of an additional per-capita THB in each village on the corresponding outcome. Standard errors are clustered at the village level (64 clusters) to account for the quasi-experimental design. Bootstrapped standard errors are reported in brackets and are computed using 500 bootstrap samples and clustered at the village level. The estimating sample only includes households who reported owning non-agricultural business assets the year preceding the rollout of the program.

Table A10: Reduced-form effects on selected outcomes- Excluding villages with pre-program village funds

| Panel A: Proxy-variable approach | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | Raw data | | | | | Truncated top 1% | | |
| | VF short-term Credit | Short-term Credit | Household Income | Profits | Off-farm Business profits | Household Income | Profits | Off-farm Business profits |
| Post X Inv HH X High Productivity | 0.0524 | 0.920 | 1.641 | 3.047* | 2.592 | 1.052 | 1.550*** | 1.142** |
| | (0.137) | (0.582) | (2.082) | (1.757) | (1.726) | (1.172) | (0.551) | (0.541) |
| | [0.122] | [0.704] | [2.027] | [1.869] | [1.861] | [1.01] | [0.707] | [0.677] |
| Post X Inv HH | 0.651*** | 1.018*** | -0.0598 | -0.299 | -0.156 | -0.445 | -0.725** | -0.507* |
| | (0.169) | (0.369) | (0.602) | (0.632) | (0.542) | (0.426) | (0.336) | (0.302) |
| | [0.163] | [0.5] | [0.701] | [0.716] | [0.591] | [0.486] | [0.272] | [0.212] |
| Effect for High Productivity | 0.704 | 1.938 | 1.581 | 2.748 | 2.436 | 0.606 | 0.826 | 0.635 |
| SE | (0.082) | (0.728) | (2) | (1.961) | (1.675) | (1.084) | (0.486) | (0.447) |
| SE (bootstrap) | [0.113] | [0.885] | [1.991] | [2.159] | [1.843] | [0.989] | [0.725] | [0.659] |
| Observations | 6838 | 6838 | 6838 | 6838 | 6838 | 6838 | 6838 | 6838 |
| Number of households | 729 | 729 | 729 | 729 | 729 | 729 | 729 | 729 |
| R-Squared | 0.576 | 0.457 | 0.445 | 0.406 | 0.367 | 0.564 | 0.466 | 0.415 |

$***p < 0.01, **p < 0.05, *p < 0.1$

*Note:* The table reports the reduced-form estimates of the effects of the program as a function of productivity estimated through the proxy-variable approach. The sample excludes the 10 villages with pre-program village funds. The coefficients have been scaled down by 1,000,000 in order to capture the effect of an additional per-capita THB in each village on the corresponding outcome. Standard errors are clustered at the village level (64 clusters) to account for the quasi-experimental design. Bootstrapped standard errors are reported in brackets and are computed using 500 bootstrap samples and clustered at the village level.

Table A11: Reduced-form effects on selected outcomes for households with preexiting non-agricultural businesses - Excluding villages with pre-program village funds

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | | Raw data | | | Truncated top 1 % | |
| | VF short-term credit | Total short-term credit | Profits | Assets | Profits | Assets |
| Post X Inv HH X High Productivity | 0.0120 | 1.786 | 9.768* | 21.53* | 4.796*** | 11.09*** |
| | (0.215) | (1.151) | (5.688) | (11.13) | (1.622) | (3.314) |
| | [0.242] | [1.495] | [6.596] | [12.875] | [2.355] | [3.628] |
| Post X Inv HH | 0.540** | 0.940 | 0.690 | 1.595 | -1.402 | 0.209 |
| | (0.235) | (0.779) | (2.114) | (1.359) | (0.989) | (0.688) |
| | [0.294] | [0.974] | [3.226] | [3.225] | [0.929] | [2.112] |
| Effect-High Productivity | 0.55 | 2.73 | 10.46 | 23.12 | 3.39 | 11.30 |
| SE | (0.17) | (1.54) | (5.54) | (11.85) | (1.26) | (3.57) |
| SE (bootstrap) | [0.27] | [1.8] | [6.12] | [12.98] | [2.15] | [3.45] |
| Baseline mean (DV) | 0 | 23127 | 54689 | 123911 | 32260 | 91420 |
| Observations | 1679 | 1679 | 1679 | 1679 | 1679 | 1679 |
| Number of households | 175 | 175 | 175 | 175 | 175 | 175 |
| R-Squared | 0.570 | 0.509 | 0.372 | 0.583 | 0.388 | 0.632 |

$***p < 0.01, **p < 0.05, *p < 0.1$

*Note:* The table reports the reduced-form estimates of the effects of the program as a function of productivity estimated through the proxy-variable approach. The sample excludes the 10 villages with pre-program village funds. The coefficients have been scaled down by 1,000,000 in order to capture the effect of an additional per-capita THB in each village on the corresponding outcome. Standard errors are clustered at the village level (64 clusters) to account for the quasi-experimental design. Bootstrapped standard errors are reported in brackets and are computed using 500 bootstrap samples and clustered at the village level. The estimating sample only includes households who reported owning business assets the year preceding the rollout of the program.

Table A12: Reduced-form effects on selected outcomes- Productivity ranking

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | | | Raw data | | | | Truncated top 1% | |
| | VF short-term Credit | Short-term Credit | Household Income | Profits | Off-farm Business profits | Household Income | Profits | Off-farm profits |
| Post X Inv HH X Productivity rank | -0.00354 | 1.017 | 4.422 | 5.058*** | 4.083* | 3.134 | 2.745* | 2.063 |
| | (0.145) | (0.679) | (2.796) | (1.946) | (1.708) | (1.869) | (1.235) | (0.920) |
| | [0.148] | [0.801] | [3.02] | [2.633] | [2.433] | [1.967] | [1.596] | [1.325] |
| Post X Inv HH | 0.625*** | 0.464 | -1.795 | -1.908 | -1.313 | -1.661 | -1.526 | -0.926 |
| | (0.139) | (0.308) | (1.356) | (0.904) | (0.737) | (0.939) | (0.562) | (0.407) |
| | [0.142] | [0.354] | [1.297] | [1.031] | [0.971] | [0.932] | [0.689] | [0.528] |
| Observations | 8578 | 8578 | 8578 | 8578 | 8578 | 8578 | 8578 | 8578 |
| Number of households | 910 | 910 | 910 | 910 | 910 | 910 | 910 | 910 |
| R-Squared | 0.587 | 0.383 | 0.517 | 0.479 | 0.425 | 0.577 | 0.444 | 0.405 |

$***p < 0.01, **p < 0.05, *p < 0.1$

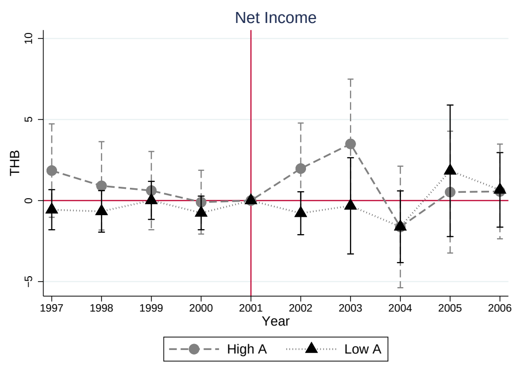
*Note:* The table reports the reduced-form estimates of the effects of the program as a function of productivity estimated through the proxy-variable approach. The coefficients have been scaled down by 1,000,000 in order to capture the effect of an additional per-capita THB in each village on the corresponding outcome. Standard errors are clustered at the village level (64 clusters) to account for the quasi-experimental design. Bootstrapped standard errors are reported in brackets and are computed using 500 bootstrap samples and clustered at the village level.

Table A13: Reduced-form effects on selected outcomes for households with preexiting non-agricultural businesses- Productivity ranking

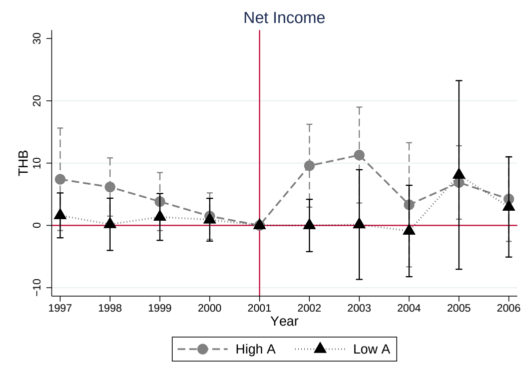
| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | | Raw data | | | Truncated top 1 % | |
| | VF short-term credit | Total short-term credit | Profits | Assets | Profits | Assets |
| Post X Inv HH X Productivity rank | -0.239 | 2.745 | 18.58*** | 27.83* | 10.53*** | 15.55*** |
| | (0.252) | (1.921) | (6.815) | (15.97) | (2.847) | (5.650) |
| | [0.278] | [1.908] | [8.376] | [15.381] | [3.768] | [5.007] |
| Post X Inv HH | 0.693*** | -0.288 | -6.674* | -10.59* | -4.812*** | -6.685** |
| | (0.161) | (0.955) | (3.382) | (6.224) | (1.299) | (3.002) |
| | [0.272] | [0.779] | [3.788] | [5.485] | [1.528] | [2.604] |
| Baseline mean (DV) | 11.75 | 21237.0 | 50973.5 | 119203.8 | 31903.0 | 92236.6 |
| Observations | 2161 | 2161 | 2161 | 2161 | 2161 | 2161 |
| Number of households | 225 | 225 | 225 | 225 | 225 | 225 |
| R-Squared | 0.589 | 0.509 | 0.440 | 0.588 | 0.390 | 0.631 |

$***p < 0.01, **p < 0.05, *p < 0.1$

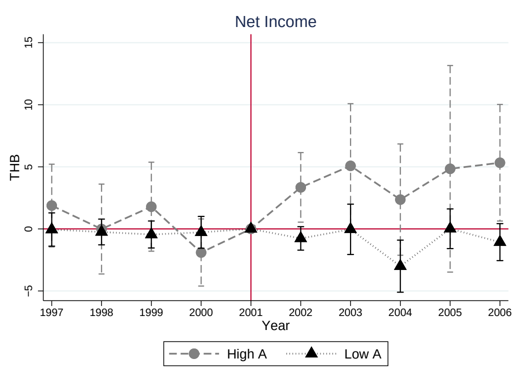
*Note:* The table reports the reduced-form estimates of the effects of the program as a function of productivity estimated through the proxy-variable approach. The coefficients have been scaled down by 1,000,000 in order to capture the effect of an additional per-capita THB in each village on the corresponding outcome. Standard errors are clustered at the village level (64 clusters) to account for the quasi-experimental design. Bootstrapped standard errors are reported in brackets and are computed using 500 bootstrap samples and clustered at the village level. The estimating sample only includes households who reported owning business assets the year preceding the rollout of the program.



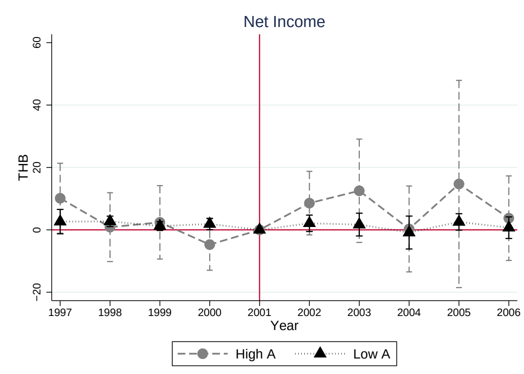

(a) Full sample - Percapita model



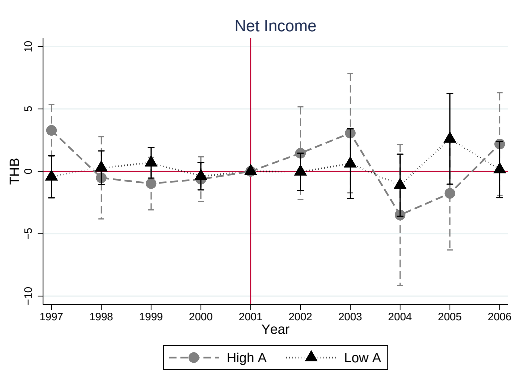

(b) Preexisting non-ag businesses - Per capita model



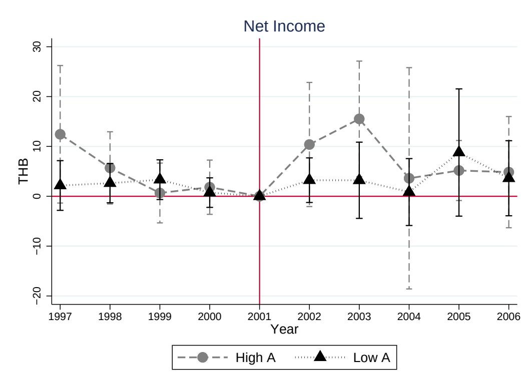

(c) Full sample - Including number of workers




(d) Preexisting non-ag businesses - Including number of workers



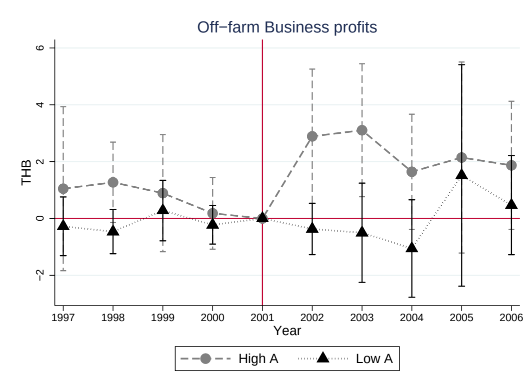

(e) Full sample - Including number of workers & M.E. in $k$



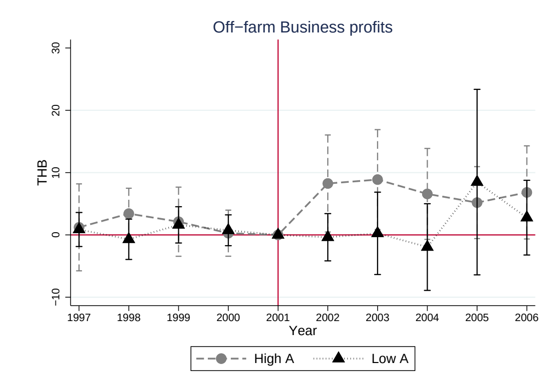

(f) Preexisting non-ag businesses - Including number of workers & M.E. in $k$

Figure A4: Effects of program rollout on household income by alternative measures of productivity

*Note:* The figure depicts flexible estimates corresponding to the specification in (5). Each dot represents differences in income between households from villages with high and low per capita program funds, for each year with respect to the year of the of the announcement of the program (2001). Each coefficient has been scaled down by 1,000,000 in order to capture the effect of an additional per capita THB in each village on the corresponding outcome. All dependent variables are winsorized with respect to the top 1% of the full sample distribution. High A: household belongs to the top-third 33% of the baseline productivity distribution in each village. Low A: household belongs to the bottom two-thirds of the baseline productivity distribution in each village. Productivity estimates correspond to the control function approach using household beliefs about profits as a proxy variable. 95% confidence intervals are computed based on standard errors, which are clustered at the village level to account for the empirical design.

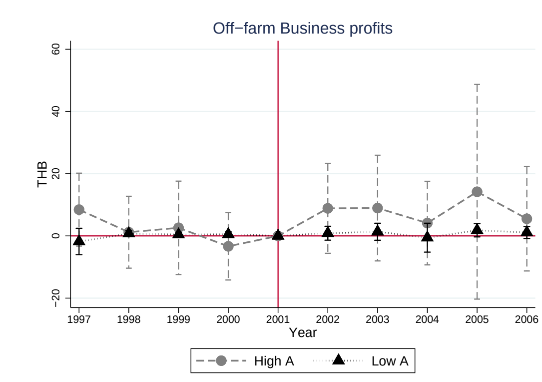



(a) Full sample - Per capita Model

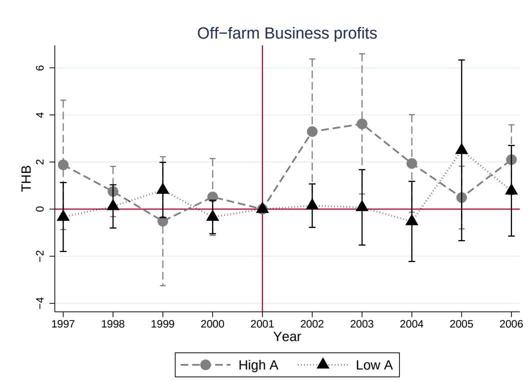



(b) Preexisting non-ag businesses - Per-capita Model

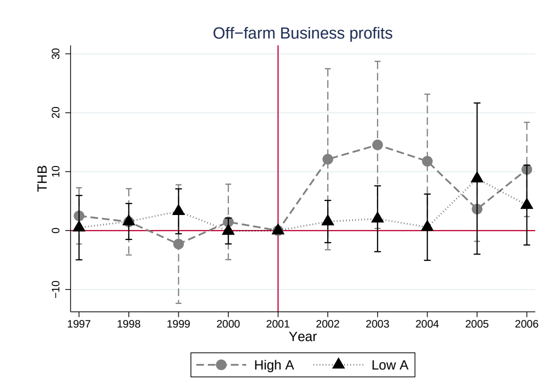

(c) Full sample - Including number of workers




(d) Preexisting non-ag businesses - Including number of workers




(e) Full sample - Including number of workers & M.E. in $k$




(f) Preexisting non-ag businesses - Including number of workers & M.E. in $k$

Figure A5: Effects of program rollout on business profits by alternative measures of productivity

*Note:* The figure depicts flexible estimates corresponding to the specification in (5). Each dot represents differences in income between households from villages with high and low per capita program funds, for each year with respect to the year in which the program was announced (2001). Each coefficient has been scaled down by 1,000,000 in order to capture the effect of an additional per capita THB in each village on the corresponding outcome. All dependent variables are winsorized with respect to the top 1% of the full sample distribution. High A: household belongs to the top-third of the baseline productivity distribution in each village. Low A: households belong to the bottom two-thirds of the baseline productivity distribution in each village. Productivity estimates correspond to the control function approach using household beliefs about profits as a proxy variable. 95 % confidence intervals are computed based on standard errors, which are clustered at the village level to account for the empirical design.

Table A14: IV effects of total credit on income and profits - 5 post-program years

**Panel A: IV estimates of the effect of credit on household income (full sample)**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Household Income | | Wage Income | | Total Profits | |
| | Raw | Winsorized | Raw | Winsorized | Raw | Winsorized |
| Total Short Term Credit * High Productivity | 2.292* | 1.658** | 0.341 | 0.314 | 2.535** | 1.211*** |
| | (1.345) | (0.839) | (0.399) | (0.380) | (1.022) | (0.399) |
| | [1.239] | [0.727] | [0.404] | [0.406] | [1.062] | [0.580] |
| Total Short Term Credit | -0.932 | -0.978** | 0.147 | 0.123 | -0.778 | -0.825*** |
| | (0.625) | (0.476) | (0.279) | (0.273) | (0.503) | (0.308) |
| | [0.879] | [0.564] | [0.357] | [0.352] | [0.738] | [0.416] |
| Effect- High Productivity | 1.361* | 0.681 | 0.488** | 0.436 | 1.757** | 0.386 |
| SE | 1.060 | 0.616 | 0.243 | 0.211 | 0.936 | 0.352 |
| SE bootstrap | [0.778] | [0.455] | [0.245] | [0.226] | [0.777] | [0.423] |

**Panel B: IV estimates of the effect of credit on Profits by source (full sample)**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Farm | | Fishing/Shrimping | | Off-farm Business | |
| | Raw | Winsorized | Raw | Winsorized | Raw | Winsorized |
| Total Short Term Credit * High Productivity | 0.471 | 0.418 | -0.0849 | 0.0239 | 2.149* | 0.769** |
| | (0.359) | (0.301) | (0.0955) | (0.0291) | (1.076) | (0.387) |
| | [0.499] | [0.394] | [0.097] | [0.060] | [1.125] | [0.519] |
| Total Short Term Credit | -0.332 | -0.495** | -0.0291 | -0.0153 | -0.417 | -0.315 |
| | (0.322) | (0.245) | (0.0836) | (0.0285) | (0.501) | (0.323) |
| | [0.494] | [0.408] | [0.090] | [0.059] | [0.759] | [0.285] |
| Effect- High Productivity | 0.139 | -0.0766 | -0.114 | 0.00860 | 1.732** | 0.454 |
| SE | 0.309 | 0.244 | 0.147 | 0.00694 | 0.869 | 0.283 |
| SE bootstrap | [0.334] | [0.208] | [0.151] | [0.013] | [0.679] | [0.373] |
| First-stage F-stat: Short Term Credit | 5.403 | | | | | |
| First-Stage F-stat: Interaction | 5.126 | | | | | |
| Observations | 8650 | | | | | |
| Number of households | 914 | | | | | |

$***p < 0.01, **p < 0.05, *p < 0.1$

*Note:* The table reports the instrumental-variables estimates of the effects of total short-term credit as a function of productivity estimated through the proxy-variable approach. The estimating sample includes 5 pre and post program years. Panel A presents the effects on income by source and Panel B presents effects on profits by type of activity. Odd-numbered columns report IV coefficients after truncating the dependent variable at the top 1%. Standard errors, in parentheses, are clustered at the village level (64 clusters) to account for the quasi-experimental design. Bootstrapped standard errors are presented in brackets and are computed using 500 bootstrap samples and clustered at the village level. Short-term credit involves program loans with a term shorter than a year and has been top coded with respect to the 99th percentile for precision. Household profits include farm, fishing and shrimping and off-farm business profits.

Table A15: IV effects of total credit on preexiting non-agricultural businesses - 5 post-program years

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Profits | | Non-wage Expenses | | Wage Expenses | | Assets | |
| | Raw | Winsorized | Raw | Winsorized | Raw | Winsorized | Raw | Winsorized |
| Total Short Term Credit * High Productivity | 6.331** | 4.029*** | 3.648 | 1.239 | 0.38 | 0.152 | 7.009* | 4.688*** |
| | (2.263) | (1.187) | (3.044) | (2.209) | (0.196) | (0.0712) | (4.186) | (1.833) |
| | [2.847] | [1.579] | [3.377] | [2.364] | [0.279] | [0.103] | [3.858] | [1.744] |
| Total Short Term Credit | -2.174 | -2.200** | -4.509 | -1.012 | -0.144 | -0.0293 | 0.180 | -0.941 |
| | (1.857) | (1.062) | (3.199) | (1.817) | (0.203) | (0.0692) | (1.077) | (1.165) |
| | [2.534] | [1.391] | [3.054] | [1.699] | [0.292] | [0.094] | [2.106] | [1.566] |
| Effect- High Productivity | 4.157*** | 1.830** | -0.861 | 0.227 | 0.236 | 0.123** | 7.188** | 3.747*** |
| SE | 1.502 | 0.559 | 2.250 | 1.777 | 0.146 | 0.0433 | 4.026 | 1.500 |
| SE bootstrap | [1.326] | [0.802] | [2.537] | [1.764] | [0.175] | [0.057] | [3.117] | [0.991] |
| First-stage F-stat: Short-term credit | 4.902 | | | | | | | |
| First-Stage F-stat: Interaction | 6.306 | | | | | | | |
| Observations | 2188 | | | | | | | |
| Number of households | 228 | | | | | | | |

$***p < 0.01, **p < 0.05, *p < 0.1$

*Note:* The table reports instrumental-variables estimates of the effects of total short-term credit as a function of productivity estimated through the proxy-variable approach. The estimating sample includes 5 pre and post program years. Odd-numbered columns report IV coefficients after truncating the dependent variable at the top 1%. Standard errors, in parentheses, are clustered at the village level (64 clusters) to account for the quasi-experimental design. Bootstrap standard errors are also reported in brackets and are computed using 500 bootstrap samples and clustered at the village level. Short-term credit involves program loans with a term shorter than a year. The estimating sample includes household with pre-existing businesses only. All variables are measured in THB.

ONLINE APPENDIX

# B Variable definition

## B.1 Productivity estimates

1. **Value added:** We compute value added as the difference between gross total revenues (across all household businesses) and total spending in wages (across all household businesses). We do not consider revenues derived from wage work provision of household members to other households, nor transportation spending on these activities.

2. **Capital:** Capital is measured as the self-reported total value of the stock of fixed assets for all household businesses. This excludes household assets such as appliances or other durable goods.

3. **Beliefs:** We construct our measure of beliefs as: $b = \frac{\text{Profits regular}-\text{Profits pessimistic}}{\text{Profits optimistic}-\text{Profits pessimistic}}$ We use households self-reported projections of total income for the next year (in THB). Households report forecasts in three scenarios: a pessimistic scenario, an optimistic scenario, and a regular scenario.

4. **Labor:** We approximate labor as the sum of the following components:

    - Total number of out-of-household workers hired for a wage. This only includes off-farm businesses.

    - Total number of out-of-household workers that provide unpaid labor. This only includes off-farm businesses.

    - Number of household members that report working in a family business as their main occupation. This includes all family enterprises.

## B.2 Outcomes

1. **Short-term credit:** Total amount of credit, which is obtained by each household from any lender. This measure only includes loans with repayment periods under 12

months.

2. **Income:** We measure total household income as the sum of profits from household enterprises, net earnings (after taxes), donations/transfers from other households, and government transfers.

3. **Profits:** We measure profits as gross revenues (sales) net of operations costs such as wages (when available) and non-labor spending. We do not consider the shadow wage for household workers nor the costs associated to labor-sharing schemes. It excludes income derived from providing labor to other households.

4. **Labor costs (non-agricultural business only):** Total spending in wage workers.

5. **Consumption:** Total consumption expenditure. It is computed as the weighted sum of consumption expenditure across several categories of goods. The weights represent the relative weight of such categories in the Thai Socio Economic Surveys.

# C Identification and Estimation details.

In this section, we provide a more technical description of the identification assumptions of our method. We also discuss the steps to implement our method. We focus on a value-added function with only one predetermined input for the sake of simplicity. We also describe extensions to accommodate potential issues that are likely to arise in empirical work such as issues with the measurement of the proxy variable, the introduction of non-predetermined inputs and measurement error in capital.

## C.1 Baseline model: using beliefs as proxy variable with predetermined regressors.

Consider the following value added function in which we assume that log value-added ($va_{i,t}$) is a function of log productivity $\omega_{i,t}$, log capital $k_{i,t}$, and shocks to production $\epsilon_{i,t}$.

$$va_{i,t} = \omega_{i,t} + \beta_k k_{i,t} + \epsilon_{i,t}$$

We assume that capital is pre-determined with respect to $\epsilon_{i,t}$ –i.e., households pick capital without observing shocks to production. However, capital is chosen based on $\omega_{i,t}$. Thus estimating $\beta_k$ through OLS will yield biased estimates of the contribution of capital to value-added.

**Assumption 1:** We assume that $\omega$ follows a first-order Markov process:

$$\omega_{i,t} = \mathbb{E}[\omega_{i,t}|\omega_{i,t-1}] + \zeta_{i,t}$$

The previous structure suggests that households make choices based on foreseen variation in productivity ($\mathbb{E}[\omega_{i,t}|\omega_{i,t-1}]$) and in response to unforeseen productivity ( $\zeta_{i,t}$). This structure is assumed in traditional control-function models such as Levinsohn and Petrin (2003); Olley and Pakes (1996) and Ackerberg et al. (2015). Similarly, our approach proxies for productivity using a variable that, conditional on input use, is a strict monotonic function of $\omega_{i,t}$. However, we do not rely on first-order conditions in the choice of intermediate inputs to derive such relation, since a one-to-one relation between productivity and intermediate inputs may not hold in the presence of credit constraints (Shenoy, 2017). Instead, we use beliefs about future profits to proxy for productivity. The rationale for this choice is simple: household may use information that is not availble to the researcher–i.e., productivity–to construct forecasts about future profits, which are observed by the researcher.

More formally, we assume that beliefs about profits in period $b_{i,t}$ are a function of input use and productivity;

$$b_{i,t} = b(\omega_{i,t}, k_{i,t})$$

**Assumption 2:** There is a strict monotonic relationship between productivity and household beliefs, conditional on input use.

One consequence of the previous assumption is that it is possible to invert $b$ and hence write down productivity as a function of beliefs and capital: $\omega_{i,t} = b^{-1}(b_{i,t}, k_{i,t})$.

Combining assumptions 1 and 2, it is possible to achieve identification of $\beta_k$. First, using Assumption 2, we plug in $\omega_{i,t} = b^{-1}(b_{i,t}, k_{i,t})$ into the value-added production function to obtain:

$$va_{i,t} = b^{-1}(b_{i,t}, k_{i,t}) + \beta_k k_{i,t} + \epsilon_{i,t} \tag{A1}$$

Or

$$va_{i,t} = \phi(b_{i,t}, k_{i,t}) + \epsilon_{i,t}$$

Where $\phi_{i,t} = b^{-1}(b_{i,t}, k_{i,t}) + \beta_k k_{i,t}$ is an unknown function of beliefs and capital. Note that because household beliefs and capital are pre-determined with respect to $\epsilon_{i,t}$, $\phi_{i,t}$ is identified. Thus, it is possible to use non-parametric or semi-parametric methods to estimate $\hat{\phi}_{i,t}$. In this case, $\hat{\phi}_{i,t}$ captures variation in value added explained by productivity and input use. Note that while Assumption 2 allows us to identify $\phi_{i,t}$, Assumption 2 is not sufficient to identify $\beta_k$. The intuition is simple; $\hat{\phi}$ captures the combined contribution of variation in capital to variation in value added. The former is composed of the contribution of capital to production ($\beta_k$) but also endogenous responses of capital to productivity, which in turn explains variation in value-added.

We now invoke Assumption 1 to exploit panel variation in $\omega_{i,t}$ to identify $\beta_k$. First, note that for a guess value $\tilde{\beta}_k$, we can use $\hat{\phi}_{i,t}$ to recover estimates of productivity $\hat{\omega}(\tilde{\beta}_k) = \hat{\phi}(b_{i,t}, k_{i,t}) - \tilde{\beta}_k k_{i,t}$. Using Assumption 1, it is possible to write down current productivity as an unknown function $g$ of lagged productivity plus an unforeseen productivity shock $\hat{\zeta}$:

$$\hat{\omega}(\tilde{\beta}_k)_{i,t} = g(\hat{\omega}(\tilde{\beta}_k)_{i,t-1}) + \hat{\zeta_{i,t}} \tag{A2}$$

Note that because $\hat{\omega}(\beta_k)_{i,t-1}$ is only a function of $b_{i,t-1}$ and $k_{i,t-1}$, which are predetermined with respect to the unforeseen shocks to productivity, the following moment condition is satisfied under Assumptions 1 and 2:

$$\mathbb{E}[\hat{\zeta}_{it}(\tilde{\beta}_k)|b_{i,t-1}, k_{i,t}] = 0$$

Thus it is possible to use GMM to recover:

$$\hat{\beta}_k = \text{argmin} \frac{1}{N} \sum_{i=1} \hat{\zeta}_{it}(\tilde{\beta}_k) \tag{A3}$$

### C.1.1 Estimation

We estimate $\beta_k$ following 5 steps:

1. We use third-order polynomials to approximate $\phi_{i,t}$:

$$va_{i,t} = \sum_{h=0}^{h=3} \sum_{j=0}^{j=3} \delta_{i,j} b_{i,t}^{h} k_{i,t}^{j} + \epsilon_{it}$$

2. We use $\hat{\phi}_{i,t} = \sum_{h=0}^{h=3} \sum_{j=0}^{j=3} \hat{\delta}_{i,j} b_{i,t}^{h} k_{i,t}^{j}$ and a candidate for $\beta_k$ to compute $\hat{\omega}_{i,t} = \hat{\phi}_{i,t} - \tilde{\beta}_k k_{i,t}$. For this, we use an OLS regression of value-added on capital to obtain our first guess ( $\tilde{\beta}_k$ ).

3. We estimate equation (A2) using a third-order polynomial to approximate $g$. We then compute the residuals $\hat{\zeta}_{i,t}(\tilde{\beta}_k)$ from the following regression:

$$\hat{\omega}_{i,t}(\tilde{\beta}_k) = \sum_{n=0}^{n=3} \theta_n \hat{\omega}_{i,t-1}^{n}(\tilde{\beta}_k) + \zeta_{i,t}$$

4. We iterate across different values for $\beta_k$ in order to minimize:

$$\frac{1}{N} \sum_{i=1} \hat{\zeta}_{it}(\tilde{\beta}_k)$$

5. Finally, we use our estimates of $\hat{\beta}_k$ to recover estimates of productivity $\omega_{i,t} = \hat{\phi}_{i,t} - \hat{\beta}_k k_{i,t}$.

## C.2 Extension: using beliefs as proxies with non-predetermined regressors

Our method can accommodate models in which other inputs such as labor $l_{i,t}$ which are likely to respond to unforeseen productivity shocks $\zeta_{i,t}$. If we assume that contemporary choices of inputs $l_{i,t}$ are correlated with $\zeta_{i,t}$, but previous input choices are not, then $\beta_k$ and the factor elasticities corresponding to non-predetermined inputs ($\beta_l$) are identified based on the following moment condition:

$$\mathbb{E}[\hat{\zeta}_{it}(\tilde{\beta}_k\tilde{\beta}_l)|b_{i,t-1}, k_{i,t}, l_{i,t-1}] = 0$$

That means, that we can use lagged versions of labor to instrument for current labor. This result is analogous to the identification results of Ackerberg et al. (2015).

### C.2.1 Estimation

The process is similar to the one without non-predetermined regressors with only two differences.

- First, $\hat{\phi}_{i,t} = \sum_{n=0}^{n=3}\sum_{i=0}^{i=3}\sum_{j=0}^{j=3}\hat{\delta}_{i,j} b_{i,t}^{i} k_{i,t}^{j} l_{i,t}^{n}$ in the first stage.
- Second, using two guess values$\tilde{\beta}_k$ and $\tilde{\beta}_l$, we proceed to compute $\hat{\omega}_{i,t} = \hat{\phi}_{i,t} - \tilde{\beta}_k k_{it} - \tilde{\beta}_l l_{i,t}$. We then estimate equation (A2) and computed the associated residuals to construct the sample analog of:

$$\mathbb{E}\left[\hat{\zeta}_{i,t}(\tilde{\beta})\begin{pmatrix} b_{i,t-1} \\ k_{i,t} \\ l_{i,t-1} \end{pmatrix}\right] = 0 \tag{A4}$$

Finally we use the previous set of moment conditions to recover GMM estimates of $\beta = \{\beta_k, \beta_l\}$ and productivity.

### C.3 Extension: using beliefs measured in the previous period

One possible scenario is that the researcher does not obtain contemporaneous measures of beliefs but only beliefs about profits in period $t$ which are constructed with information in period $t-1$. This is the case of our empirical application and we discuss the implications of this caveat for identification. For simplicity, we focus on the one-input case.

In this case, we only observe household beliefs about future profits based on the available information from the previous periods. Let $b_{i,t|t-1}$denote the beliefs about profits in $t$ which are based on previous information. Because households may only observe the foreseen part of productivity ($\mathbb{E}[\omega_{i,t}|\omega_{i,t-1}]$) and not the unforeseen part of productivity ($\zeta_{i,t}$), we can not write beliefs as a function of $\omega_{i,t}$ as in our benchmark especificiation. However, we can write beliefs about profits in period $t$ measured in $t-1$ as a function of the foreseen part of productivity and the available stock of capital :

$$b_{i,t|t-1} = \tilde{b}(\mathbb{E}(\omega_{i,t}|\omega_{i,t-1}), k_{i,t}) \tag{A5}$$

If, conditional on capital, there is a strict monotonic relation between expected productivity and $b_{i,t|t-1}$, then it is possible to write down the value added function as:

$$va_{i,t} = \tilde{b}^{-1}(b_{i,t|t-1}, k_{i,t}) + \beta_k k_{i,t} + \zeta_{i,t} + \epsilon_{i,t} \tag{A6}$$

The previous expression is very similar to equation (A1). The key difference is that the part of productivity that is unforseen to the farmer ($\zeta_{i,t}$) is not present in equation (A1)but shows up in (A6). Thus, our measure of beliefs captures productivity with measurement error, arising from the fact that we measure beliefs at the end of period $t-1$ which precedes the realization of the unforeseen shocks to productivity.

Note that in this case we can still write down value added as an unknown function of beliefs and capital and shocks:

$$va_{i,t} = \tilde{\phi}(b_{i,t|t-1}, k_{i,t}) + \zeta_{i,t} + \epsilon_{i,t} \tag{A7}$$

In this case, identification of $\tilde{\phi}_{i,t}$ requires that $b_{i,t|t-1}$ and $k_{i,t}$ are not correlated to $\zeta_{i,t} + \epsilon_{i,t}$. This could be a problem in terms of identification of $\phi$, depending on the assumptions made about how households adjust inputs.

**Predetermined regressors.** In this setting, the former condition is satisfied as both beliefs and capital are measured at the end of period $t-1$ and are not correlated to $\zeta_{i,t}+\epsilon_{i,t}$. Thus, we can apply our framework with our measures of beliefs and still consistently estimate $\phi$.

**Regressors correlated with $\zeta_{i,t}$.** It could be the case that household are able to partially modify inputs in the aftermath of unforeseen productivity shocks. If that is the case, then current measures of input would be correlated with $\zeta_{i,t}$ and $\phi$ in equation (A7) will not be identified. However, it is possible to use lagged versions of input to instrument for current input usage and consistently recover $\phi$.

Once$\hat{\phi}$ is recovered, the rest of the procedure remains unchanged.

### C.4 Extension: measurement error in capital

Collard-Wexler and De Loecker (2016) highlight the consequences of measurement error in capital in the context of the estimation of production functions. In this paper we are particularly interested in understanding the extent to which our results are robust to productivity estimates that account for potential measurement error in capital. Note that in the context of control-function methods, measurement error can affect identification $\beta_k$ directly and indirectly through biased estimates of $\phi$.

While $k_{i,t}$ may be measured with error arising from failure to recall the initial level of capital and assumptions regarding depreciation, we argue that investment spending in period $t-1$ should be highly predictive of the stock capital at $t$ and is unlikely to suffer from measurement error related to imperfect recall and depreciation. Using this insight we apply Collard-Wexler and De Loecker (2016)'s procedure to correct for measurement error.

1. We approximate $\phi$ using a linear regression in which we use investment in $t-1$ ($i_{i,t-1}$)

as an instrument for $k_{i,t}$ in the following regression:

$$va_{i,t} = \delta_0 + \delta_1 b_{i,t} + \delta_2 k_{i,t} + \delta_3 l_{i,t} + \epsilon_{it}$$

2. We use $\hat{\phi}_{i,t} = \hat{\delta}_0 + \hat{\delta}_1 b_{i,t} + \hat{\delta}_2 k_{i,t} + \hat{\delta}_3 l_{i,t}$ and candidates for $\beta_l, \beta_k$ to compute $\hat{\omega}_{i,t} = \hat{\phi}_{i,t} - \tilde{\beta}_l l_{i,t} - \tilde{\beta}_k k_{i,t}$. Note that $\hat{\delta}_2$is estimated using lagged investment as an instrument for $k$, and that we assume a linear process as opposed to a semi-parametric approach as the latter would imply further assumptions regarding the use of investment as an instrument (Collard-Wexler and De Loecker, 2016).

3. We estimate equation (A2) using a an AR(1) process. Again, note that we assume a linear process as opposed to a semi-parametric approach as the latter would imply further assumptions regarding the use of investment as an instrument. We then compute the residuals $\hat{\zeta}_{i,t}(\tilde{\beta}_k)$ from the following regression:

$$\hat{\omega}_{i,t}(\tilde{\beta}_k) = \rho \hat{\omega}_{i,t-1}(\tilde{\beta}_k) + \zeta_{i,t}$$

4. We obtain the GMM estimates of $\beta_l$ and $\beta_k$ based on the following moment conditions:

$$\mathbb{E}\left[\hat{\zeta}_{i,t}(\tilde{\beta})\begin{pmatrix} b_{i,t-1} \\ i_{i,t-1} \\ l_{i,t-1} \end{pmatrix}\right] = 0 \tag{A8}$$

Where $i_{i,t-1}$ denotes investment expenses of household $i$ during the period $t-1$. In this case identification is based on a different moment condition which implies that lagged investment is uncorrelated with foreseen shocks to productivity.

5. Finally, we use our GMM estimates ( $\hat{\beta}_l, \hat{\beta}_k$) to recover estimates of productivity $\omega_{i,t} = \hat{\phi}_{i,t} - \hat{\beta}_l l_{i,t} - \hat{\beta}_k k_{i,t}$.