From Loans to Labor: Access to Credit, Entrepreneurship and Child Labor

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Abstract

This paper seeks to understand household business decisions in response to increased credit access in an environment with multiple market failures. A simple model suggests that households at certain wealth thresholds might be able to overcome the fixed costs of entering entrepreneurship when they have increased access to credit. In the presence of labor market imperfections however, these same households may also be more likely to employ child labor. I test these predictions using household and child level panel data from Thailand. To isolate the causal impacts of household borrowing, I exploit the exogenous timing and institutional features of the Million Baht Program, one of the largest government initiatives to increase household access to credit in the world. I find that, consistent with the model, expanded access to credit raises entry into entrepreneurship for households in specific wealth groups while simultaneously increasing the use of child labor in these households. The results suggest that through the avenue of encouraging entrepreneurial activity, expanding credit access may have unintended consequences for the supply of child labor.

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1 Introduction

Understanding the role of financial intermediation in household decisions is important for identifying the underlying determinants of economic growth and for designing effective policy in the developing world. A large body of evidence has shown that the availability of financial tools has considerable impacts on households' ability to smooth consumption, make long-term investments and manage risk (for a survey of this literature, see Conning and Udry (2007)). Another important consequence of financial market imperfections is the limitations that such imperfections place on the structure and organization of entrepreneurial production. Entrepreneurial activity is a key factor in economic development, with the potential to foster innovation and create employment at a macroeconomic level and alleviate poverty at the microeconomic level.

Seminal theoretical work by Eswaran and Kotwal (1986) highlights the role that access to working capital plays in determining which households become entrepreneurs, as well as the composition of labor that entrepreneurs employ. Yet empirical work elucidating the relationship between credit access and entrepreneurial decisions is only now emerging (Paulson and Townsend (2004), McKenzie and Woodruff (2006), Karlan and Zinman (2010)). For example, recent evidence from a randomized evaluation in India has shown that expanding access to credit can have significant effects on entrepreneurship (Banerjee, Duflo, Glennester and Kinan (2010)). However, a key feature of the developing country context is the presence of multiple market imperfections; labor market failures is one well-studied example (Deolalikar and Vijverberg (1983), Newell, Pandya and Symons (1997) and Bharadwaj (2008)). Thus a relevant question in this setting is the extent to which changes in business activity generated by increased credit access affect household labor supply decisions. This paper examines the role of credit constraints in entrepreneurial and labor supply choices in the context of labor market imperfections. In particular, I focus on the use of child labor by households that enter into entrepreneurship.

I develop a simple model in which there are fixed costs associated with entering entrepreneurship and in which there is a positive relationship between credit access and household wealth. These two features generate a wealth threshold that separates business owners from non-business owners. In the presence of underlying labor market imperfections, this initial allocation of entrepreneurship is inefficient; those with positive returns to business ownership are not able to pay the fixed cost and thus remain non-entrepreneurs. An increase

in credit access lowers the wealth threshold, stimulating new entrepreneurship among a subset of households in the middle of the wealth distribution; if the credit expansion is limited, some households will remain shut out of business ownership. The movement from wage work to entrepreneurship has complex effects on the supply of household labor. These impacts depend on the relative increase in labor productivity that results from entering entrepreneurship, the extent of the existing labor market imperfections, and households' marginal rate of substitution between leisure and consumption. Thus theoretically, it is not clear whether entry into entrepreneurship leads to higher or lower levels of household labor.

Empirically identifying the impact of increased credit access is often difficult, as the decision to take out a loan is usually correlated with unobserved characteristics of households that may also influence the outcomes of interest. For example, one might worry that underlying differences between borrowing and non-borrowing households may confound the effect of credit on child labor and entrepreneurial decisions. I tackle the issue of endogeneity in two ways. First, I implement an instrumental variables strategy that exploits the exogenous timing and institutional features of the Million Baht Program, a national credit expansion in Thailand. This program was one of the largest government initiatives aimed to expand household access to credit in the world, totalling about US$1.8 billion in initial funds. The program involved a lump sum transfer from the central government to each village in Thailand - regardless of population - leading to larger per-capita increases in credit availability in villages that were smaller at the time the funds were received. The strategy in this paper builds on previous work by Kaboski and Townsend (forthcoming) by using both the variation in village population as well as the additional variation generated by the random order in which villages received the funds from the central government. Thus, in addition to some households randomly having access to larger per-capita borrowing pools, the expansion led to some households being granted access earlier than others.

Second, the dataset used in this paper is a monthly panel collected at the household and child level over seven years. By studying business and child labor decisions within the same household and even within observations for the same child over time, I can control for time-invariant unobserved heterogeneity across households an children. For example, this accounts for any constant measures of ability or entrepreneurial talent as well as any level differences across villages (such as differences across large and small villages). The combination of a fixed-effects and instrumental variables strategy allows me to cleanly identify the effect of credit on both entrepreneurial activity and child labor in a way that previous

studies have not been able to achieve.

I find that increased access to credit stimulates non-agricultural business ownership only among households in the middle of the wealth distribution. For these households, an additional 1000 baht (approximately US$23) of borrowing leads to a 1.7 percentage point increase in the likelihood of operating a non-agricultural business (10% over the pre-expansion mean).[1] Business investment also increases; a 1000 baht increase in borrowing leads to a 18% increase in the stock of business capital. Child labor in non-agricultural businesses also increases in households at the middle of the wealth distribution. For a 1000 baht increase in borrowing, children are 3 percentage points more likely to work and work an additional 2.4 hours per month. At the average loan size, this translates to nearly 8 additional hours of work per week. The effects on child labor are persistent and sizeable even 12 months after households borrow. These results are consistent with specifications of the model where the returns to labor in entrepreneurial ventures is higher than the prevailing market wage and where the marginal rate of substitution between child leisure and consumption is increasing in child labor. The increases in child labor appear to come predominantly from children's leisure as I find no evidence of decreased school attendance or increased dropout rates in response to the program.

While several papers examine the general relationship between borrowing and child labor (Wydick (1999), Hazarika and Sarangi (2008), Fuwa, Ito, Kubo, Kurosake, Sawada (2009), Islam and Choe (forthcoming)), I show that the effects of credit on child labor vary non-monotonically over the wealth distribution, a relationship that ultimately stems from the nature of credit constraints and barriers to entrepreneurial entry.

An oft-cited goal of microcredit programs is to improve the lives of the poor by financing entrepreneurial ventures among credit constrained households. Indeed, the existing estimates of the return to capital in small enterprises are extremely high, ranging from 50% to 360% depending on the context (Banerjee and Duflo (2008), deMel, McKenzie and Woodruff (2008), McKenzie and Woodruff (2006, 2008), Udry and Anagol (2006)). However, this paper shows that when credit constraints are only partially relaxed, the result is an increase in entrepreneurship for some but not all households. In particular, the limited credit expansions are unlikely to increase business investment and entry among the poorest households, who have few resources with which to supplement loans. Thus while increasing credit access may be an effective tool for poverty reduction in theory, extending limited funds may lead to

[1]The exchange rate used is the 43.8 baht per dollar on January 15, 2002 (Oanda.com).

positive effects for only a subset of households in practice.

Lastly, while the increases in child labor in this paper reflect optimal labor supply decisions on the part of households, policymakers may have other reasons to be concerned about child work. For example, if parents do not fully internalize the return on investing in their children's human capital, the observed increases in child labor could be inefficient. Even though I find that schooling attendance is unaffected by household borrowing, it is still possible that increased child labor negatively affects children in other ways not captured by schooling attendance alone. For example, children who work have lower final educational attainment, perform worse on exams and are more likely marry at younger ages (Beegle, Dehejia and Gatti (2005), Beegle et al. (2008), Heady (2003)). Therefore if policymakers are interested in reducing child labor, the results in this paper suggests that they must take caution when evaluating the full impacts of policies aimed at reducing a particular market friction in settings where other markets are likely to be imperfect.

The remainder of the paper is organized as follows: the second section presents a model of credit constraints, entrepreneurship and decisions over household labor in the presence of labor market imperfections. The third section describes the data used in estimation and the policy intervention under study. The fourth section outlines the empirical strategy for identifying differential effects of increased borrowing by household wealth in the context of endogoneous wealth measures and loan takeup. The fifth section presents and discusses the estimation results and various robustness checks and the final section gives a brief summary of the findings and offers a few concluding remarks.

2 Credit Constraints, Entrepreneurship and Household Labor

The model in this section makes two general points. First, I illustrate that in a setting with fixed costs of entrepreneurial entry and credit constraints that decline with household wealth, there is a threshold of initial wealth that separates entrepreneurs from non-entrepreneurs. In the context of labor market imperfections, these barriers to entry leads to an inefficient allocation of entrepreneurs, as some profitable entrepreneurial ventures will not be taken up. The basic setup and intuition is similar to existing models of the role of credit constraints in entrepreneurial entry; for example, see recent work in the developing country context by

Banerjee, Duflo, Glennerster and Kinnan (2010). However, a key difference in this model from Banerjee et al. (2010) is the source of heterogeneity across households. In the model in this paper, the heterogeneity is in household wealth, which determines the ability to afford the fixed costs of starting up a business.[2] On the other hand, the sources of household heterogeneity in Banerjee et al. (2010) are time preferences and returns to entrepreneurship.[3]

Second, I show that the effects of increased credit access on entrepreneurship vary by household wealth. In particular, a limited credit expansion allows new entrepreneurs to enter only at the middle of the wealth distribution. The movement into entrepreneurship has complex impacts on household labor supply which depend on the extent of the labor market imperfections, the relative return to entrpereneurship over wage work and the marginal rate of substitution between leisure and consumption. Therefore, the net effect of increased entrepreneurship on household labor supply is theoretically ambiguous.

2.1 Entrepreneurship and Household Labor Supply in the Presence of Borrowing Constraints

Consider a simple two period model in which a household maximizes total utility over consumption (C) and leisure (H).

$$\max U(C^1) + U(C^2, H^2)$$

Time is indexed by superscripts (1,2).[4] There is no labor market in period 1, so leisure enters only period 2 utility. There are two types of households: entrepreneurial (indexed by the subscript E) and wage-working households (indexed by the subscript W). Entrepreneurs work for themselves, while wage workers engage in a public sector that pays a fixed rate. Household types face different budget constraints in both periods, so I discuss the maximization problem for each type of household in turn. I begin with entrepreneurial households.

In period 1, entrepreneurial households finance period 1 consumption (C_1^E) and the

[2] In this way, the set up of the model in this section builds on earlier work by Evans and Jovanovic (1989).

[3] Although the Banerjee et al. (2010) predictions are similar to those in this model, their results require that increased credit access lowers the marginal interest rates faced by households. However, there is no evidence that the program I study in this paper has had any effect on interest rates (Kaboski and Townsend, forthcoming) and thus their model may be less appropriate for this setting.

[4] As mentioned previously, heterogeneity in time preferences is not modeled here and so for simplicity, the discount rate is excluded from the analysis.

fixed cost of starting up a business (K_E) using exogenously given initial wealth (W_0) and borrowing from the market (B_E). Households can also save, in which case $B_E < 0$. The first period budget constraints for entrepreneurial households is given by

$$C_E^1 + K_E \leq W_0 + B_E \tag{1}$$

Notice that although households pay the fixed cost of entry (K_E) in period 1, they do not reap the benefits of business ownership in this initial period. Therefore this fixed cost captures the investment that must be made before any revenue is generated. Even small businesses need some amount of initial capital investment, whether in the form of goods to sell, durable assets) or labor hours devoted to planning and initiating a new project.

In period 2, entrepreneurial households use business profits to pay back their loans at interest rate r, pay for period 2 consumption (C_E^2) and pay a lump sum tax (τ). Household labor supply is denoted L_E^2 and is the only input to entrepreneurial production. It is important to note that there is no market for hired labor for entrepreneurs. This "missing market" assumption is used to reflect the most extreme case of labor market imperfections. However, this case might be especially relevant in the market (or lack thereof) for child labor; even in developing countries, most child work is performed inside the home, suggesting that it is difficult to find wage work for children. Other labor market imperfections may include conventional agency problems associated with hired labor (such as shirking or stealing) or lumpiness in hiring labor (e.g. the existence of a minimum the number of hours that hired workers are willing to work). These intermediate cases in which hiring labor is possible but where there is some wedge between the productivity (or cost) of hired and household labor make the model much more complex but yield the same intuition as the extreme case presented here. Thus the period 2 budget constraint for entrepreneurs can be written as follows:

$$C_E^2 + (1+r)B_E + \tau \leq F(L_E^2) \tag{2}$$

Households also face a total time constraint, i.e. $T = H^2 - L_E^2$. The following first order conditions characterize the optimal choices over consumption, borrowing and labor supply

for entrepreneurial households (at an interior).

$$U_C\left(C_E^{1*}\right) = U_C\left(C_E^{2*}, T - L_E^{2*}\right)(1+r) \tag{3}$$

$$F_L(L_E^{2*}) \cdot U_C\left(C_E^{2*}, T - L_E^{2*}\right) = U_H\left(C_E^{2*}, T - L_E^{2*}\right) \tag{4}$$

$$C_E^{1*} + K_E = W_0 + B_E^* \tag{5}$$

$$C_E^{*2} + (1+r)B_E^* + \tau = F(L_E^{2*}) \tag{6}$$

Households trade off consumption between periods according to the standard intertemporal Euler equation (3). They choose labor supply such that the marginal product of labor is equal to the marginal rate of substitution between leisure and consumption (4). Finally, (5) and (6) are the first and second period budget constraints, respectively. At the optimal choices implied by these conditions, the maximized utility of entrepreneurial households is as follows:

$$U\left(W_0 + B_E^* - K_E\right) + U\left(F(L_E^{2*}) - (1+r)B_E^* - \tau, T - L_E^{2*}\right) \tag{7}$$

The maximization problem for wage-working households is very similar. In the first period households allocate their initial wealth endowment (W_0) between first period consumption (C_W^1) and savings/borrowing (B_W) but do not have to pay any fixed costs for entering wage work in period 2. Therefore, their first period budget constraint is given by

$$C_W^1 \leq W_0 + B_W \tag{8}$$

In period 2, wage-working households use labor income to pay back their loans at interest rate r, pay for period 2 consumption (C_W^2) and pay a lump-sum tax (τ). Household labor supply is denoted L_W^2 and earns the exogenous wage rate w. The second period budget constraint can be written as

$$C_W^2 + (1+r)B_W + \tau \leq wL_W^2 \tag{9}$$

Additionally, wage-working households face a total time constraint, i.e. $T = H^2 - L_W^2$. The following first order conditions characterize the optimal choices over consumption, borrowing

and labor supply (at an interior) for these households.

$$U_C\left(C_W^{1*}\right) = U_C\left(C_W^{2*}, T - L_W^{2*}\right)(1+r) \tag{10}$$

$$wU_C\left(C_W^{2*}, T - L_W^{2*}\right) = U_H\left(C_W^{2*}, T - L_W^{2*}\right) \tag{11}$$

$$C_W^{1*} = W_0 + B_W^* \tag{12}$$

$$C_W^{*2} + (1+r)B_W^* + \tau = wL_W^{2*} \tag{13}$$

Households trade off consumption between periods according to the intertemporal Euler equation (10). They choose labor supply such that the marginal rate of substitution between leisure and consumption is equal to the wage rate, w, following (11). Finally, (12) and (13) are the first and second period budget constraints for wage workers. At the optimal choices implied by these conditions, the maximized utility of wage-working households is as follows:

$$U\left(W_0 + B_W^*\right) + U\left(wL_W^2 - (1+r)B_W^* - \tau, T - L_W^{2*}\right) \tag{14}$$

In order to make the model consistent once scaled up, the public sector functions by using tax revenue to pay for the public sector wage bill. Denoting the number of entrepreneurs as N_E and the number of wage workers as N_W, the balanced budget condition for the public sector is as follows:

$$(N_E + N_W)\tau = w\sum_{n=1}^{N_W} L_{W,n}^{2*} \tag{15}$$

The sum represents the aggregate labor supply of wage workers, and thus $w\sum_{n=1}^{N_W} L_{W,n}^{2*}$ is the total public sector wage bill. $(N_E + N_W)\tau$ represents total tax revenue. (15) reflects that the lump sum taxes collected from all households are devoted to financing the public sector wage bill.[5] Since the tax is lump sum, it does not distort household labor supply decisions.

Now we can use the maximized utility levels of each household type ((7) and (14)) to determine which occupation is more profitable. Entrepreneurship is more profitable if returns to entrepreneurship (net of fixed costs, K_E) are higher than those for wage work,

[5] As specified here, the public sector is not productive. Adding in the production of a public good that is available to all households does not change the predictions of this model.

captured by the following condition:

$$U\left(W_0 + B_E^* - K_E\right) + U\left(F(L_E^{2*}) - (1+r)B_E^* - \tau, T - L_E^{2*}\right) > \\ U\left(W_0 + B_W^*\right) + U\left(wL_W^2 - (1+r)B_W^* - \tau, T - L_W^{2*}\right) \tag{16}$$

For simplicity, consider the case where the entrepreneurial production function exhibits constant returns to scale, i.e. $F_L(L_E^{2*}) = \alpha$ where α is some constant. In this case, when the marginal product of labor in entrepreneurship is higher than the public sector wage rate, i.e. $\alpha > w$, becoming an entrepreneur is profitable as long as the fixed cost of entering entrepreneurship is low enough relative to the implicit wage gain associated with becoming an entrepreneur.[6] This simplifies the profitability condition (16) to

$$U\left(W_0 + B_E^* - K_E\right) + U\left(\alpha L_E^{2*} - (1+r)B_E^* - \tau, T - L_E^{2*}\right) > \\ U\left(W_0 + B_W^*\right) + U\left(wL_W^2 - (1+r)B_W^* - \tau, T - L_W^{2*}\right) \tag{17}$$

Since there is a higher implicit wage rate associated with entrepreneurial activity, it is theoretically ambiguous whether entrepreneurial households work more or less than wage-working households. The labor supply of entrepreneurs relative to wage workers depends on the strength of the substitution effect (higher opportunity cost of leisure from higher wages) relative to the income effect (higher wages yields higher income). The relative sizes of the effects is determined by the shape of the utility function, but there are reasonable functional forms for $U(C^2, H^2)$ such that entrepreneurs work more than wage workers (e.g. Cobb-Douglas preferences). Thus a movement from wage work to entrepreneurship could indicate a rise, fall or no change in household labor supply, depending on the form of the utility function.

In this model, the only dimension of household heterogeneity is the initial endowment of wealth, W_0. As long as becoming an entrepreneur is profitable as in (16), the division between entrepreneurs and wage workers stems only from differences in the ability to afford the fixed cost of entering entrepreneurship. I now introduce credit constraints. Borrowing from the market (B) depends on the amount of collateral posted by the household (χ).

$$\begin{aligned} B &= \theta \cdot \chi, \qquad \theta > 0,\ \chi \leq W_0 \\ &\Rightarrow \overline{B(W_0)} = \theta W_0 \end{aligned} \tag{18}$$

[6]This implies that the returns and fixed costs associated with entrepreneurship are homogeneous across households. This is a simplification of reality but as discussed in the results section, this assumption appears to yield a decent approximation of the wealth threshold associated with entering entrepreneurship.

$\overline{B(W_0)}$ is the maximum amount that a household with initial wealth W_0 can borrow from the market. θ reflects the positive relationship between wealth and borrowing ability and is set exogenously by lenders.[7,8] For any given level of initial wealth, the borrowing constraint is the same for both entrepreneurial and wage-working households, so I do not use subscripts here. Figure 1 shows that in the data (described in the next section), actual borrowing amounts are strongly positively correlated with initial household wealth. Under this borrowing constraint and the assumption on entrepreneurial profitability, we can identify entrepreneurs using the following indicator function, where 1 indicates that a household is entrepreneurial and 0 indicates that a household works for wages:

$$\mathbf{1}_E = \begin{cases} 0 & \text{if } W_0 < \frac{K_E}{1+\theta} \\ 1 & \text{if } W_0 \geq \frac{K_E}{1+\theta} \end{cases} \tag{19}$$

Households below the threshold of initial wealth are unable to start up a business. Denote this wealth threshold as $\underline{W_0}$, such that $\underline{W_0} = \frac{K_E}{1+\theta}$. This is consistent with the observation that wealthier households are more likely to start up a business in the data before any credit expansion (Figure 2) and is supported by evidence from earlier empirical work (Holtz-Eakin et al.(1994), Evans and Jovanovic (1989) and Paulson and Townsend (2004)).

Notice that this allocation of entrepreneurs and wage workers is inefficient. As long as the assumption over relative profitability is satisfied (16), there are positive returns to entrepreneurship for all households. However, households below the wealth threshold are unable to enter business ownership and thus there are profitable ventures that are not being taken up. In this stylized model, the inefficiency exists because credit constraints bind for some households while entrepreneurial returns are positive for all households. With a more general assumption of heterogeneous returns to entrepreneurship, there will still be an inefficiency as long as there exist labor market imperfections. In the perfect labor markets case, entrepreneurial talent can be rewarded on the labor market in the form of higher wages. This allows those who would be profitable entrepreneurs but who are credit-constrained to reap the benefits of their entrepreneurial ability by working for another household who has

[7]The lending rule here is linear in initial wealth, but this is done only for expositional simplicity. The maximum amount households can borrow need only be increasing in W_0 for the results in this section to go through, although more complex functions yield accordingly more complex wealth thresholds. Empirically, I allow this relationship to be nonlinear as outlined in the next section.

[8]A possible alternative is that lenders assess borrowing limits by potential returns to entrepreneurial projects, which are affected by entrepreneurial ability. In this case, entrepreneurship is determined by ability rather than wealth. This possibility is discussed further in the results section.

the funds to afford the fixed cost of entry but lacks the entrepreneurial talent that generates high returns to business ownership. On the other hand, if the labor market is constrained then this trading of entrepreneurial talent does not occur and the resulting distribution of entrepreneurs is inefficient. Finally, although this model focuses on entrepreneurial entry (and does not include variable business investment), in reality credit constraints may also affect investments of existing business owners and result in inefficiently small businesses.

This model also makes clear that the borrowing constraint affects not only the ability to become an entrepreneur but also the consumption smoothing capabilities of households. For example, consider extending the model to include a third period in which households continue to earn income through entrepreneurship and wage work. If households experience a negative income shock in period 2, they may want to borrow against period 3 income to finance consumption in period 2. If households are restricted in their borrowing ability, they make work more in period 2 than they would in the unrestricted case.

2.2 Entrepreneurship after a Limited Credit Expansion

Now consider the effect of a credit expansion where new loans are offered in addition to the existing market for borrowing. Households borrow B^{MC} but there is a strict limit on borrowing from this source ($\overline{B}^{MC}$) that satisfies:

$$B^{MC} \leq \overline{B}^{MC} \tag{20}$$

From this point on, I will refer to this new source of borrowing as microcredit, although this increase in credit access need not impose the restrictions of conventional microcredit programs (for example, I do not assume group liability or targeting of loans towards females). It is important to note that as for market borrowing, there are no restrictions on loan use; households may use this credit to finance consumption or investment. The key assumption in (20) is that the microcredit borrowing limit is the same for all households and is not a function of initial wealth. This relationship is confirmed in the data, where borrowing from the microcredit source is much more equitable across the wealth distribution than borrowing

than from other sources (Figure 1 and Table 1).[9]

This limited credit expansion leads to the following changes in the ability to afford business ownership, where again the following indicator function takes the value of 1 for entrepreneurial households and 0 for wage-working households.

$$\mathbf{1}_E^{'} = \begin{cases} 0 & \text{if } W_0 < \frac{1}{1+\theta}\left(K_E - \overline{B}^{MC}\right) \\ 1 & \text{if } W_0 \geq \frac{1}{1+\theta}\left(K_E - \overline{B}^{MC}\right) \end{cases} \tag{21}$$

(21) implies a new, lower threshold for initial wealth for households to become entrepreneurs. Denoting this threshold as $\underline{\underline{W_0}}$ such that $\underline{\underline{W_0}} = \frac{1}{1+\theta}\left(K_E - \overline{B}^{MC}\right)$, households can now be categorized into three distinct groups by initial wealth. Poor households ($W_0 < \underline{\underline{W_0}}$) never become entrepreneurs, even after the credit expansion because they are still too poor to afford the fixed cost of starting a business. Middle wealth households ($\underline{\underline{W_0}} \leq W_0 < \underline{W_0}$) are "new" entrepreneurs that emerge after the credit expansion. Unconstrained wealthy households are "always entrepreneurs" whose ability to afford the fixed cost of entering business ownership is unaffected by the increase in credit availability.

In sum, if we start in an equilibrium where poorer households are credit constrained and then expand credit access by introducing equal opportunity, low-cap loans, the effect of the expansion on entrepreneurship will be to increase entrepreneurial entry and investment only in the middle of the wealth distribution.[10] The additional borrowing source also increases households' ability to smooth consumption and may increase other types of investment with lower or no fixed costs such as consumption durables and schooling.

[9]Another way in which microcredit could expand credit access is by lowering interest rates. However, the key assumption here is that borrowing from this source is limited and therefore unlikely to fulfill total demand for credit. If this is the case, then the marginal interest rates faced by households are unchanged even if the rate on microcredit loans is lower than on loans from other sources and the lower interest rates do not affect marginal entrepreneurial decisions. As households appear to borrow the maximum amount allowed by the new credit program while continuing to borrow from other sources, it does not appear that the new source of credit was large enough to satisfy total demand for credit. Furthermore as discussed in footnote 3, there is no evidence that the program studied in this paper affected market interest rates.

[10]There is no uncertainty in this model. An alternative mechanism is that in the presence of uncertainty, the increase in the availability of loans may change the risk-coping strategies of households. In this case, we might expect that households may be induced into risky business ventures when more credit is available because it offers a safety net in case the business fails. However, this would suggest that the mere availability of credit rather than actual borrowing increases business ownership. Thus we would expect to find effects on entrepreneurship for non-borrowers as well, but I find no evidence of changes in entrepreneurial activity among this group. Moreover, it is not clear how an improvement in risk-coping ability would yield the same non-monotonic effects of increased borrowing over the wealth distribution as is implied by this model of fixed costs and credit constraints.

On the other hand, the investment and consumption smoothing choices of wealthy (unconstrained) households are unaffected by the increase in credit access. This is because they are able to borrow and invest optimally even in the absence of the credit expansion. However, it may be reasonable to assume that the new source of credit is low-cost relative to other sources, i.e. $r^{MC} < r$. This is often a feature of government-financed credit expansions and microcredit programs alike. If this is the case, high wealth households may profit by substituting away from high-cost existing debt to low-cost microcredit.[11] As long as the increase in credit availability introduced by the expansion is not enough to satisfy wealthy households' total demand for borrowing, the marginal interest rate that they face is still the high market rate (r) and total borrowing behavior is unchanged. This arbitrage opportunity increases income for high wealth households if they use the new low cost loans to substitute away from existing high cost debt.

3 Data and Descriptive Statistics

3.1 The Townsend Thai Project

This paper uses a large household panel survey of Thai villages, the Townsend Thai Project. The regression sample is a monthly panel of 426 households (612 children, ages 10-14) in 16 villages in the Northeast and Central regions of Thailand, from 1999-2005.[12] The villages are spread across four districts of Thailand, which vary in terms of environmental factors and main economic activities. However, villages within a district are relatively similar.

Appendix Table 1 displays the summary statistics for the key variables of interest. Nearly 50 percent of children work at some point during the sample period. Working children put in approximately 63 hours of labor per month, or about 14.5 hours per week. Children spend the most time working in household production, where the mean (conditional on working) is 47 hours per month. I observe only the number of days (rather than hours per day) a child spends performing domestic chores such as preparing meals and caring for other household members. The average child spends 14 days per month in domestic chores. School attendance is fairly high with 96 percent of children attending school at an average of 18

[11] This possibility for arbitrage opportunities was first put forth by Banerjee and Duflo (2008).

[12] The sample is unbalanced due to the age restriction, new children moving into the household and a very small amount of attrition (discussed in this section).

days in spent in school per month when school is in session.[13] Dropping out of school is quite rare, occurring in less than one percent of the sample. Attrition is very low in the sample; 6% of the households end up leaving the sample permanently over the 7-year period. Attrition was largely due to migration.

As calculated in Samphantharak and Townsend (2010), I use net household wealth held in the first month a household appears in the survey to capture initial wealth before any credit expansion. This measure includes the stock of assets owned by the household (including land) as well as cash and savings and subtracts all liabilities.[14] Figures 1 and 2 show that both (pre-program) market borrowing and business ownership are highly positively correlated with wealth.

3.2 Characterizing Initial Wealth Thresholds

The model in the previous section implies two relevant wealth thresholds for analyzing the impact of an increase in credit access: $\underline{W_0} = \frac{K_E}{1+\theta}$ and $\underline{\underline{W_0}} = \frac{K_E - \overline{B^{MC}}}{1+\theta}$. However, in practice K_E is not well measured and θ and $\overline{B^{MC}}$ are not directly observed. I use the average reported cost of opening a business by households in the pre-credit expansion period as the measure of K_E. This measure is only reported for the small set of households who opened a business during the period between the first month of the sample and before the credit injection; it is not asked retrospectively for existing businesses. I attempt to back out θ by taking the average amount borrowed from the market by households in each wealth decile individually in the pre-credit expansion period. Unlike in the theoretical framework of the previous section, this allows θ to vary along the wealth distribution, i.e. I allow the relationship between initial household wealth and borrowing ability to be nonlinear across wealth deciles.

Figure 3 plots median "available liquid funds" against the average reported cost of opening a business by initial household wealth decile. Available liquid funds are defined as the sum of current cash on hand, deposits at banks and the average market loan amount by wealth decile using only the pre-credit expansion data.[15] Note that this measure includes my

[13] The high reported attendance rate is likely due to compulsory schooling laws that apply to children ages 6 to 12, which could reflect high rates of actual attendance or simply a higher likelihood of misreporting. This is discussed further in the next section.

[14] See Samphantharak and Townsend (2010) for additional information about the construction of the initial wealth measure, including details concerning depreciation.

[15] Other assets are not included in this measure, as no household reports selling fixed assets (such as land and livestock) to finance business start-ups.

estimate of θ. Given these pre-expansion funds, the median households in deciles 1-6 are not able to finance the average fixed cost of starting up a business (77,000 baht or approximately US$1600) and are shut out of entrepreneurship.

I use the average borrowing from the new credit source (across all wealth deciles) in the post-expansion sample as a proxy for $\overline{B^{MC}}$. I then add this amount to the pre-expansion available funds to get an estimate of post-expansion available funds.[16] I use the mean borrowing amount across the entire (post-program) sample to reduce the endogeneity arising from actual borrowing amounts being correlated with unobserved household characteristics. In practice, the size of new loans generated by the credit expansion are fairly equitable across the wealth distribution (see Figure 1 and Table 1, Panel B). The increase in available funds has the largest impact on deciles 4-6; with the additional funds, the median household in these deciles is almost able to afford the fixed cost of starting a business. This stands in contrast to deciles 1-3, whose post-expansion liquid funds are still far below the fixed cost. Therefore, I categorize households in deciles 1-3 as low wealth (never entrepreneurs), households in deciles 4-6 as middle wealth (new entrepreneurs) and deciles 7-10 as high wealth (old entrepreneurs). In the subsequent analysis, I use these wealth groups for statistical power, although the qualitative results are robust to different definitions of wealth groups and estimation by individual wealth deciles (see Table 3).[17]

Panel A of Table 1 displays the pre-expansion summary statistics for credit for these wealth groups. Low wealth households are significantly more likely to report being credit constrained, defined as ever having been rejected by a lender or having been forced to take a loan for an amount less than requested. Note that this measure of credit constraints excludes both discouraged borrowers and those who are credit constrained through informal channels. While this measure of credit constraints is imperfect, it lends suggestive evidence that credit constraints decrease with household wealth. Both low and middle wealth households are generally less likely to take out a loan than high wealth households. Conditional on borrowing, the size of loans increases with household wealth across all types of borrowing. Yearly interest rates are very high for all groups but generally higher for lower wealth groups. Default (defined as being at least 90 days past due) is high across all sources of credit, ranging from 8-39%. The general takeaway from Table 1 Panel A is that across all definitions, credit access is increasing in household wealth.

[16] This measure does not allow savings, cash or loans for other sources to be affected by the credit expansion.

[17] Table 3 gives some evidence that low and high wealth households exit business ownership in response to the increase in borrowing. These are discussed in more detail in the next section.

3.3 Thailand Village and Urban Revolving Fund

In 2001, the Thai government launched the Thailand Village and Urban Revolving Fund (VF), also referred to as the Million Baht Program. The VF is a large-scale, publicly-funded microfinance initiative that injected one million baht (about US$24,000) into each of 74,000 villages and 4,500 urban communities across Thailand, *regardless of village population*. The total initial outlay of the program was US$1.8 billion (about 1.5% of Thai GDP in 2001) and was funded entirely by the central government. The program was introduced as a "surprise" policy initiative, shortly following the dissolution of the Thai Parliament in November 2000 and the election of Prime Minister Thaksin Shinawatra in January 2001. The disbursement of funds to villages was carried out between mid-2001 and mid-2002.

The primary purpose of the VF initiative was to create permanent, self-sustaining village lending institutions, although the program also included the provision of savings services. Village committees were elected democratically to review applications and allocate funds. There is no evidence that the initial transfer was seen as one-off, as committees lent most of the initial funds in the first year and continued to lend at the same rate or higher in subsequent years. Late payment penalties were imposed and in the event of default, no future loans were to be given. As a consequence, the default rate on Village Fund loans is very low (ranging from 2.3-5.3%), especially relative to other sources of borrowing (Table 1, Panel B). Finally, one aim of the VF was to increase credit access among those with previously limited borrowing capabilities. As a consequence, credit was typically extended to all who applied without collateral requirements (although with guarantors).[18]

Over 70 percent of all sample households borrow from the Village Fund at some point after the initial injection of funds, but low wealth households are still less likely to borrow from the Village Fund (Table 1, Panel B). Conditional on borrowing, the average loan size ranges from 12-15 thousand baht (US$250-315). For low and middle wealth households, these loans are very large relative to other sources of borrowing (50-200% larger on average than other types of loans) but for high wealth households, this represents a much smaller increase in borrowing; for this group, Village Fund loans are only about 40% of the size of loans from other sources. Although there are small differences in borrowing amounts across wealth groups, almost all households borrow at or near the official borrowing limit posed by village councils and most continue to borrow from other sources as well, even after the

[18]See Kaboski and Townsend (forthcoming) for further information about the credit injection.

Village Fund was introduced. This suggests that the small VF loans are not enough to satisfy total borrowing demand for even households in the lowest wealth group. The interest rate on Village Fund loans is very low relative to other types of borrowing; on average interest rates are 45-62 *percentage points* lower for VF loans versus other loans (depending on wealth group). However, given that these loans did not appear to fully satisfy household demand for borrowing, it is unlikely that these low interest rates represent the *marginal* cost of borrowing for any household. Overall, this credit expansion can be seen as a sizeable increase in credit access for all but the highest wealth group.

4 Empirical Strategy

4.1 Baseline Estimating Equation

Translating the implications from Section 2 into estimating equations is fairly straightforward. The baseline regression for effects of credit constraints can be characterized as

$$\begin{aligned} Y_{it} &= \beta_{VF} VF_{it} + \beta_{Low} \left(VF_{it} \text{ x } LowWealth_i \right) \\ &\quad + \beta_{Middle} \left(VF_{it} \text{ x } MiddleWealth_i \right) + \beta_X' X_{it} + v_{it} \end{aligned} \tag{22}$$

where Y_{it} is the outcome of interest for household (or child) i in village j in month t, usually business ownership or child labor; VF_{it} is the amount household i borrows from the Village Fund measured in thousands of baht[19]; $LowWealth_i$ and $MiddleWealth_i$ are dummy variables that take the value of 1 when household i is a low wealth or middle wealth household, respectively; X_{it} is a vector of child- and household-specific time-varying characteristics such as age and education of the household head (all covariates are listed below each table); and v_{it} captures the unobserved household-level determinants of entrepreneurship and child labor supply.

The effect of increased borrowing through the Village Fund on children of the wealthiest households is β_{VF}. The additional impacts of borrowing on children in low and middle wealth households are given by β_{Low} and β_{Middle}. Therefore, the "total" marginal effect of VF loans for a low wealth household is given by $\beta_{VF} + \beta_{Low}$ and similarly by $\beta_{VF} + \beta_{Middle}$ for middle wealth households. When considering the impacts of credit access on entrepreneur-

[19] VF_{it} measure the flow of a new loan taken in period t, not the stock of outstanding VF loans.

ship, $\beta_{VF} + \beta_{Middle}$ is expected to be positive if households use the loans to pay fixed costs of entering or expanding businesses. When considering measures of household labor, the sign of $\beta_{VF} + \beta_{Middle}$ is theoretically ambiguous.

The principal concern in estimating equation (22) is that Village Fund borrowing (VF_{it}) may be endogenous, i.e. $E[v_{it}VF_{it}] \neq 0$, $E[v_{it}(VF_{it} \text{ x } MiddleWealth_i)] \neq 0$ and/or $E[v_{it}(VF_{it} \text{ x } LowWealth_i)] \neq 0$. For example, some households may have more entrepreneurial talent than others, making them more likely to open a business (or make their children work for them) and more likely to take out a loan. If this is the case, then the estimates of β_{VF}, β_{Low}, and β_{Middle} will likely be upwardly biased as they will capture underlying differences between borrowing and non-borrowing rather than the effect of borrowing itself. On the other hand, if there is an unobserved negative labor demand shock for all households, households may be both more likely to take out a loan to finance consumption and less likely to make their children work (as there are fewer opportunities for work). In this case, the coefficients on the loan variables may be downwardly biased. Thus if there are confounding factors captured in v_{it}, the estimates in (22) are likely to be biased and the direction of the bias is unclear.

4.2 Household and Time Fixed Effects

The first strategy I use to account for unobserved heterogeneity is to include fixed effects at both the month and household level (or child level in regressions for child labor). I parse out the time-invariant and household-invariant parts of the error term in (22) as follows

$$v_{it} = \alpha_i + \delta_t + \varepsilon_{it} \tag{23}$$

where δ_t represents the month fixed effects and α_i represents the household (or child) fixed effects. Controlling for these sources of unobserved heterogeneity, the estimating equation with fixed effects becomes

$$\begin{aligned} Y_{it} &= \beta_{VF}VF_{it} + \beta_{Low}(VF_{it} \text{ x } LowWealth_i) \\ &\quad + \beta_{Middle}(VF_{it} \text{ x } MiddleWealth_i) + \beta_X' X_{it} + \alpha_i + \delta_t + \varepsilon_{it} \end{aligned} \tag{24}$$

The inclusion of δ_t captures any unobserved seasonal or period-specific shocks that affect all households equally, such as shocks to labor demand as in the example above as well as other

factors such as aggregate trends in child labor.

With the inclusion of α_i, the variation in VF_{it} comes from changes in Village Fund borrowing over time within a household rather than across households. This is to ensure that unobserved child and household characteristics that remain fixed over time (such as entrepreneurial talent as in the example above) do not confound the estimated impact of relaxing credit constraints. Note that as the initial wealth of a household is a fixed measure over time, the direct effect of initial wealth is subsumed in α_i.

4.3 IV Implementation

However, there may still be unobserved factors in the household- and time-varying error term (ε_{it}) that are not accounted for in the fixed effects strategy. For example, if households forecast an increase in household production in the future, this forecast may increase both borrowing demand and child labor supply. If this is the case, then the estimates of β_{VF}, β_{Low}, and β_{Middle} will be upwardly biased even when including fixed effects as in (24).

To address this remaining endogeneity issue, I employ an instrumental variables approach that exploits two sources of variation in loan access introduced by the VF program. First, the VF credit expansion was rolled out rapidly as a surprise policy initiative. Furthermore, the order in which villages received funds was random and thus exogenous to individual business investment and labor decisions. The child fixed effect, α_i, subsumes any level differences between early and late receivers, including differences in village structure, distance to urban centers, etc. Thus the exogeneity of the instrument is only threatened by differential trends. Panel A of Table 2 shows that leading up to the VF injection date, villages that received funds early in the year showed no differential trends from those that received the funds later, along the dimensions of child labor and business ownership.

Second, the per-capita amount of funds available varies exogenously from village to village because each village was given the same amount (one million baht) regardless of population. Thus the second layer of variation in the instrument comes from village population at the time of the injection, which ranges from 118 to 646 in the villages under study. In Panel B of Table 3 we see that while there are level differences between the proportion of business owners in large and small villages, the trends in both business ownership and child labor are statistically indistinguishable for large and small villages in the pre-program period. Kaboski and Townsend (forthcoming) show that village size is not spatially or geographically

correlated (e.g. with respect to rivers, mountains, etc); moreover, the relationship between village size and lending becomes strongly significant only after the VF was in place and not in pre-VF years. Finally the measure of population I use comes from the 1997 village census (4 years before Prime Minister Shinawatra was elected) and is unlikely to be manipulated in anticipation of the VF policy.[20]

To construct the instruments, I begin by creating a dummy variable that takes the value of 1 if household i resides in a village j that has received VF funds at time t. I then create another variable that measures the intensity of the injection using $1/[village\ population\ in$ 1997]. I then interact the two to create the base instrument (Z_{jt}) for household i in village j at time t:

$$Z_{jt} = ReceivedFunds_{jt} \text{ x } \frac{1}{Population\ of\ village\ j\ in\ 1997} \tag{25}$$

where

$$ReceivedFunds_{jt} = \begin{cases} 1 & \text{if village } j \text{ has received 1m baht at time } t \\ 0 & \text{otherwise} \end{cases}$$

The remainder of the strategy follows a traditional two-stage approach. In the first stage, I predict the three endogenous loan variables (VF_{it}, VF_{it} x $LowWealth_i$, VF_{it} x $MiddleWealth_i$) using the three instruments (Z_{jt}, Z_{jt} x $LowWealth_i$, Z_{jt} x $MiddleWealth_i$) and all other exogenous covariates.[21] The second stage estimation then repeats (22), but with the exogenous predicted values in place of the three endogenous values.[22]

$$\begin{aligned} Y_{it} &= \beta_{VF}\widehat{VF_{it}} + \beta_{Low}\left(VF_{it} \text{ x } \widehat{LowWealth_i}\right) \\ &\quad + \beta_{Middle}\left(VF_{it} \text{ x } \widehat{MiddleWealth_i}\right) + \beta_X' X_{it} + \alpha_i + \delta_t + \varepsilon_{it} \end{aligned} \tag{26}$$

[20] As a robustness check, I include village-specific yearly linear time trends in the specification below (26) to control for any unobserved differences in trends between villages. The inclusion of village-specific trends does not change the results (Appendix Table 4, discussed in greater detail in the next section).

[21] This approach follows Wooldridge (2003) by treating the loan and each interaction term as endogenous, rather than treating only the loan term as endogenous.

[22] Since some measures of business activity and child work are binary variables, one might prefer using probit or logit analysis rather than a linear probability model. Similarly, since labor supply is bounded below by zero, a Tobit estimator may be more appropriate. However, fixed effects and IV estimation cannot be simultaneously implemented using these nonlinear estimators and since both are critical in ensuring the exogeneity of loan takeup, I continue to use the linear form in (26), although work is progressing in this direction (Chesher et al. (2011)).

where the "hatted" variables are predicted values from the first stage. Again, in all specifications of (26), I also include month fixed effects to capture the short-term, long-term and seasonal trends common to all households. Standard errors are clustered at the village level (the level of the credit expansion) to allow for general correlations of shocks between children and households within the same village and over time.

5 Results

5.1 Results by Wealth Decile

Table 3 displays the results of estimating (26) with the full set of interactions of loan takeup and dummy variables for each wealth decile. The pattern of results are consistent with the method of categorizing wealth groups according to the fixed costs of opening a business and available liquid funds discussed in Section 3. The positive effects on increased credit access are limited to households in deciles 4-6 and some appear quite large; a 1000 baht increase in loans leads to a 0.1 - 5.6 percentage point increase in the likelihood of entering business ownership, though the point estimates are not significant. At the bottom and top of the wealth distribution, the effect of the loans is generally negative, the former of which is not significant. Thus in the remainder of the results, I group together deciles 1-3, 4-6 and 7-10 for greater statistical power and defer a more detailed discussion of the effects of credit access on entrepreneurship for each wealth group to subsequent subsections.

5.2 Reduced Form Results

Table 4 displays the results of regressing measures of business ownership and child labor directly on the instruments and can therefore be thought of as estimating the intent-to-treat effect of the credit expansion. While the coefficients may seem large, once we take into account the scaling of the instrument and its cumulative nature, the effects are quite reasonable. In particular, these effects reflect the overall impact of the credit expansion (rather than a specific amount of borrowing on the margin). Additionally, the value of the instrument is the inverse of village population and thus very small on average (0.003). Therefore we can interpret the average total effect of the credit expansion as increasing the likelihood of business ownership by (12.9)(0.003)= 3.87 percentage points for households at

the middle of the wealth distribution (significant at the 1% level). Similarly, the expansion increases the probability that a child works by 3.15 percentage points and work hours spent in household businesses by 2.5 hours per month on average for children from middle wealth households (significant at the 1% level). There is some evidence that the program reduces entrepreneurship at the bottom end of the wealth distribution by nearly 4 percentage points, although this is only significant at the 10% level.

5.3 First Stage Results

Before turning to the main IV results, Table 5 displays the results of the first stage estimation of (26), which exploits the exogenous variation in the timing and intensity of the Village Fund financing. The Shea partial R-squared values satisfy the Murray (2006) "rule of thumb" and the Angrist-Pischke F-statistics for joint tests of significance exceed the Stock and Yogo (2005) critical values. The Kleibergen-Paap LM p-values and Kleibergen-Paap Wald F statistics also do not indicate any concerns related to under- or weak identification. This holds for both the household and child level regressions. As the system is exactly identified, I cannot perform any tests of validity. However, robustness checks are discussed later in this section.

5.4 Effects of Village Fund Loans on Entrepreneurship

Panel A of Table 6 displays the second stage results of IV estimating equation (26) when the outcomes of interest are measures of business activity. For households in the middle of the wealth distribution, a 1000 baht (approximately US$23) increase in Village Fund borrowing leads to a 1.7 percentage point increase in the likelihood of starting a business (10% over the pre-program mean). Village Fund loans also increase the stock of business capital (this could include, for example, a vehicle for transporting goods the local market or kitchen equipment for a restaurant); a 1000 baht increases the value of business capital by about 1,500 baht (18% over the pre-program mean). These results are large and statistically significant at conventional levels. However, the loans do not translate into significant increases in any other type of business inputs; the flows of non-labor inputs (such as goods purchased for resale) and hired labor actually fall in response to loans (although not significantly). Village Fund borrowing also seems to increase business revenue and profits but these effects are not

statistically significant.

There is some evidence low wealth and high households decrease business activity in response to increased borrowing. However, these effects are significant only for business ownership for the poor and purchases of non-labor inputs for the rich. Among poor households, this might be due to movement out of low-return self-employment used for supplemental income during adverse income shocks or lulls between harvest seasons when agricultural income is received. Consistent with this notion, while business ownership and investments drop, profits actually increase (the effect sizes are large relative to the pre-program mean but are not significant). This may suggest that the remaining businesses are the more profitable ones. Columns 1 and 2 of Appendix Table 3 report the results of the IV estimation (26) when the outcome of interest is the standard deviation of future consumption (6 and 12 months ahead). While the total marginal effect of the loans is not significant, the pattern and magnitude of the effects are compatible with the hypothesis that the negative effect on self employment for low wealth households is due to improved ability to smooth consumption. However lacking exogenous variation in negative income shocks, I am unable to further investigate that hypothesis here.

Table 7 displays the effects of Village Fund loans on agricultural household production (crop cultivation, livestock activities, fish and shrimp farming). There are no systematic effects of the loans on agricultural production for middle wealth households, suggesting that the loans are being used for non-agricultural businesses instead. The lack of significant effects on agricultural activities may be explained in part by the high incidence of agricultural production (over 85%) even before the credit expansion took place. The only significant effects on investments in agricultural production are for low wealth households, who decrease purchases of non-labor inputs (such as seeds, fertilizer and pesticides). However, agricultural revenue and profits in these households rises (Panel A, columns 7 and 8); a 1000 baht increase in loans increases agricultural profits by 1,434 baht for low-wealth households.

Finally in Table 8, I investigate the effects of increased credit access on the composition of household activities. Middle wealth households are significantly more likely to engage in non-agricultural activities and significantly less likely to only operate farms. Taken together, it appears that Village Fund borrowing encourages households at the middle of the wealth distribution to diversify their income streams by investing in non-agricultural businesses. The opposite holds for households in the extremes of the wealth distribution, who are less likely to own a business and more likely to engage only in agricultural production. Over-

all, the results presented in Tables 6-8 suggest that the net impact of increased credit access is to reallocate business ownership and agricultural activity among the various wealth groups rather than to encourage overall increases in entrepreneurship. However, this reallocation seems to result in more profitable businesses, although the positive impact on profits is not significant for non-agricultural businesses and only significant for agricultural operations of low-wealth households.

5.5 Effects of Village Fund Loans on Household Labor

Surprisingly, there are few effects of the Village Fund loans on adult labor supply (Table 9). For middle wealth households, the patterns are consistent with a shift out of wage work and into work inside the home (both agricultural and non-agricultural) but these effects are not significant. The only significant effect is for low wealth households, who decrease hours worked in agriculture in response to borrowing from the Village Fund. This could be because the market for adult labor is well-functioning, yet there is no increase in hired labor in middle-wealth households. Alternatively, adults could be already pushing up against their time constraints, although the sample means do not indicate that this is the case (total average adult work hours across all activities observed in the data is 37.4 hours per week).

Table 10 shows that this is not the case for the labor supply of children. Children in middle wealth households experience significant increases in the the likelihood and intensity of work in non-agricultural businesses when their families borrow from the Village Fund. A 1000 baht loan increase in borrowing leads to a 3 percentage point increase in the likelihood of child work (column 1). While the 2.4 hour per month increase on the intensive margin of child labor may seem small at first (column 2), the effect is large relative to the pre-program mean; a 1000 baht increases work hours by nearly 150%. Moreover, the average loan size for middle wealth households is 14,200 baht. Thus the effect at the average loan size is over 34 hours per month, or about 8 hours per week. However, it is important to note that the increase in hours worked is almost all due to the increase in child labor on the extensive margin in that it comes from entry into child labor rather than increases in hours worked by children who were working before the credit expansion. Appendix Table 2 demonstrates that the impact of borrowing on child labor in businesses is persistent; children work more even a full year after their households borrow from the Village Fund. Children in middle wealth households are also more likely to spend time performing domestic chores when their families

borrow from the Village Fund. A 1000 baht increase in borrowing raises the propensity to perform household duties by nearly 5 percentage points, although the effect on the number of days spent on chores is insignificant. This may be due to the increased need for children to take on the household responsibilities (such as cooking, cleaning and caring for other family members) when adults spend more time working in their businesses, although as discussed before, the increase in adult labor supply is not significant.

Child labor generally declines for households at the top and bottom of the wealth distribution; low and high wealth children work fewer hours in agriculture and children of wealthy households also reduce labor outside of the home. The only exception is that the likelihood that a child spends time performing domestic chores increases with borrowing from the Village Fund. For low-wealth households, the reduction in work hours is consistent with the consumption-smoothing benefits of the loans; earlier work has shown that the supply of child labor can be used to buffer households against negative income shocks (Jacoby and Skoufias (1997), Beegle, Dehejia and Gatti (2006)). Thus the increased availability of loans may reduce the need for low-wealth households to draw from their children's labor supply. Again the evidence presented in Appendix Table 3 (columns 1 and 2) lends suggestive but not statistically signifcant evidence in support of this hypothesis. Yet lacking any exogenous shock to income streams I am unable to confirm that this is the causal mechanism behind this decrease.[23]

At the top of the wealth distribution, the decreases in labor supply could be due to the income effect generated by substituting away from high cost market debt to low cost Village Fund debt. Appendix Table 3 (columns 3 and 4) shows that high wealth households are significantly more likely to make voluntary payments (i.e. over and beyond the scheduled payments) on existing debt from other sources. Recall from Table 1 that interest rates on Village Fund loans are over 50 percentage points lower than the average for other sources of loans for high wealth households. The average high wealth household borrows 14,600 from the Village Fund; if all of this were used to substitute for high cost debt the interest payment savings would be 7,300 baht on average, or 80% of average monthly net income for high wealth households. However the caveat in interpreting these results is that the sample includes only households who have existing market loans. As argued in the theoretical model, households who are able to borrow from the market are markedly different from those who

[23]The decrease in child labor for low-wealth households in agricultural activities is also consistent with the observed decrease in agricultural inputs purchased by the household, as labor and productive inputs to production appear to be complementary.

cannot. Thus the estimates in Appendix Table 3 are only valid for the sample of market borrowers and must therefore be interpreted with caution.

Taken all together, the results are consistent with imperfections in the market for child labor but not for adult labor. This could be because child labor is not easily hired out (only 7.5% of children ever work outside the home for wages). It may also reflect the notion that hired labor is lumpy; it may not be possible to find outside labor willing to work for 8 hours of work per week (the increase in child labor at the middle of the wealth distribution). The bottom 5th percentile of the distribution of hired labor for businesses is less than 12.5 hours of hired labor per week, whereas the bottom 5th percentile of the distribution of household labor for businesses is 5.5 hours per week or less. Finally, traditional issues with hired labor such as shirking and stealing may create a preference for child labor over hired labor.[24]

5.6 Effects of Village Fund Loans on Schooling

Are there corresponding movements in schooling? Panel A of Table 11 display the results of estimating (26) when the outcome considered is whether a child attends school, how many days a child attends school and whether a child drops out of school. VF loans have no systematic impact on schooling outcomes for any wealth group. This could be due in part to the fact that there is very little margin for schooling to increase; most children are already attending school (over 96 percent of the sample in any given school month) and attend frequently (16 days per month). Furthermore, it is possible that schooling data are overreported because of the laws surrounding compulsory schooling.[25] Overall, the results indicate that human capital accumulation is neither benefitted nor sacrificed in response to loans. In combination with the results in Table 10, these findings suggest that labor changes may be instead adding to or taking from leisure. Even if schooling attendance is truthfully reported, it is still possible that increased child labor negatively affects children in other ways not captured by schooling attendance alone. For example, children who work have

[24]The previous literature has established that shocks and credit constraints often affect boys and girls very differently (Edmonds (2006), Hazarika and Sarangi (2006)). When I allow for heterogeneity in the effects of loans by gender, I find that girls' labor supply is much more elastic with respect to household borrowing than boys' labor. These results are discussed in further detail in a companion paper.

[25]Therefore, these schooling results may not extend to other countries where it is easier to pull children out of school in order to work; in these countries, both the labor and schooling impacts may be magnified. Indeed, Fuwa, Ito, Kubo, Kurosake, Sawada (2009) and Islam and Choe (forthcoming) find that schooling attendance and enrollment falls when a household borrows from a microcredit program in India and Bangladesh, respectively.

lower final educational attainment, perform worse on exams and are more likely marry at younger ages (Beegle, Dehejia and Gatti (2005), Beegle et al. (2008), Heady (2003)).

5.7 Robustness Checks

5.7.1 Village-specific time trends

As discussed in the previous section, one threat to the validity of the instrument is the potential for pre-expansion trends that are systematically different in villages that receive funds relatively early or where the per-capita injection is relatively large (i.e. small villages). Table 2 illustrated that small and large villages and villages that received their funds early and late displayed similar trends in the main outcomes of interest in the years leading up to the introduction of the Village Fund. However, these groupings into large versus small and early versus late receivers may not fully capture unobserved differences in trends across villages. Thus as a robustness check, I re-run the specifications in (26) including village-specific linear time trends (monthly and yearly). Appendix Table 4 shows that the main results are robust to the inclusion of village-specific time trends at both levels; significance levels and magnitudes of effects change very little from one specification to another. The one exception is when I examine the effects of VF loans on business ownership and include village-specific year trends; the effect is no longer significant at conventional levels although it is marginally significant (p-value = 0.166) and the magnitude of the effect is similar to the main specification. Therefore it does not appear that the results are being driven solely by differential trends in the pre-Village Fund period.

5.7.2 General Equilibrium Effects of VF Injections

If the introduction of the Village Fund Program was large enough to change relative prices within villages, such general equilibrium or spillover effects of the credit expansion would complicate the interpretation of the results in this section. This is because the effect of individual borrowing would be confounded with responses to changes in these relative prices. To test for general equilibrium effects, I regress adult wages for work outside the household on the village injection instrument (using both the population-weighted instrument and the one that relies solely on differences in timing), conditioning on individual characteristics such as age, experience and education as well as household-level variables and an individual

fixed effect. Columns 1 and 2 of Appendix Table 5 displays the results. Neither the village injection status nor the interaction between the injection status and the inverse initial village population has a significant impact on wages.[26] Similarly, to test for spillovers, I first restrict the sample to households that never borrow from the VF during the entire sample period. I then regress the business ownership and child labor on the instrument along with the same covariates as in the main regression tables and household/child fixed effects. Appendix Table 5 displays regression results. As can be seen in columns 3-6, there is no evidence that credit expansion had an impact on the business ownership and child labor of non-borrowing households. Note that if there were any general equilibrium effects of the expansion (including but not limited to wage effects), we would expect to see changes in the behavior of non-borrowers. These results indicate that at least with respect to these outcomes, there is no evidence of general equilibrium or spillover effects of the Village Fund program. However one caveat to interpreting these results is that this sample of non-borrowers is subject to selection on unobserved characteristics. For example, if non-borrowers are inherently non-entrepreneurial, these results may not detect general equilibrium effects even if they do exist. Thus, these results should be viewed with caution but seen as consistent with the larger body of evidence presented in this section.

5.7.3 Heterogeneity by Education

One worry is that wealth is correlated with other unobserved characteristics that vary across households and that the true heterogeneity in the effects of loans is actually along this unobserved dimension rather than by wealth. For example, if entrepreneurial ability is correlated with wealth, then the results may be picking up differences in the effects of the credit expansion by unobserved ability. Since I lack an instrument for household wealth, I am unable to claim that the relationship between wealth and the effects of loans is causal. However, I can introduce heterogeneity along other observable dimensions to help rule out alternative channels of influence. Appendix Table 6 repeats the estimation in (26) but allows

[26]Kaboski and Townsend (forthcoming) find some evidence that individual wages rise in response to his or her households's stock of short-term credit from the VF lagged 12 months. I believe that the difference in results can be explained by differences in methodology and sample. First, I estimate the impact of the village-level intervention rather than of individual borrowing. There are reasons to believe wage income may increase as a result of individual borrowing (e.g. using the loan to make individual human capital investments) but that the village-level injection did not change aggregate wages. Second, the results reported in Appendix Table 5 are based on selectivity-corrected wages so the sample includes all adults (including non-wage workers) whereas the Kaboski and Townsend (forthcoming) results are based on smaller samples of actual wage workers in each occupation.

for heterogeneity by the education of the household head as well. Here, I use education to proxy for unobserved ability. Columns 1 and 4 of Panel A report the main regression results from Tables 6 and 10. Columns 2,3,5 and 6 allow for heterogeneity in loan effects by education in addition to heterogeneity by wealth groups. For both business ownership and child labor, there is no evidence of heterogeneity by education; the effects for different wealth groups are the same regardless of the education of the household head and very similar in magnitude and significance to the main sample results. Finally, columns 7 and 8 in Panel B give the results when I model heterogeneity only in education. The effects for business ownership are statistically indistinguishable between education levels. For child, the effects of loans are stronger for more educated households, but notice that this pattern of effects is increasing education. This stands in contrast to the non-monotonic pattern of effects in both the theoretical framework and the empirical results in presented in Table 3. Thus overall the evidence in Appendix Table 6 does not support the theory that the heterogeneous effects of loans by wealth are simply picking up differences in education levels.

5.7.4 Correcting for Bias in Clustered Standard Errors

As established in previous work (Donald and Lang (2007), Imbens and Wooldridge (2008)), estimates of standard errors using standard clustering methods can be biased downwards when the number of observations per cluster is high but the number of clusters is low. The Townsend Thai monthly panel includes only 16 villages. The potential bias in the estimated standard errors may affect the inference of the previous section. To address this issue, I implement a wild cluster bootstrap estimator, following Cameron, Gelbach and Miller (2008) and Finlay and Magnusson (2009). Within each bootstrap iteration, I resample the residuals from a restricted model that imposes the null hypothesis (zero effect of loans) at the cluster level to preserve any correlation between individuals within a village and over time. I use these residuals and the covariates to create a predicted Y that does not contain the effect of the loans. I then regress the predicted Y on the full set of covariates (including the loan variables) and store the corresponding Wald statistics. Finally, I use the distribution of these Wald statistics collected over 999 iterations to compute critical values for the test statistics calculated in Tables 6 and 10 (note that the distribution is under the null hypothesis). The original Wald statistics from running (26) and the bootstrapped critical values corresponding to the 5% and 10% significance levels are reported in Appendix Table 7. I find that as in the main results (reported in Tables 6 and 10), the coefficients on the interaction between

loans and the middle wealth group dummy are significant for both child labor and business ownership (at the 10% level). This suggests that the findings are significant despite any intra-village or intertemporal correlation and are not the result of the bias due to a low number of clusters.

5.7.5 Other Checks and Remaining Issues

Lastly, I run a number of other robustness checks to ensure that the coefficient estimates are not an artefact of a particular specification. The results are insensitive to the inclusion of other covariates which are omitted in the main regressions because they are available for only a subsample of observations. The results also hold if I differentiate households by wealth quintiles and deciles rather than wealth groups. They do not change if I exclude households with extremely low or high wealth. I also run a simple falsification test to check for reverse causality and find that child labor never significantly predicts Village Fund loan take-up, regardless of the lag used. When viewed in addition to the other evidence presented in this section, these checks indicate a large and positive causal impact of increased credit access on entrepreneurship and child labor for only middle wealth households.[27]

6 Conclusion

This paper adds to the existing literature on the role of credit constraints in household decisions by examining the effect of increasing credit access on entrepreneurship and child labor in the presence of labor market imperfections. I show that the impact of the loans on non-agricultural business ownership and investment are heterogeneous by household wealth, a consequence of credit constraints that decline with wealth and fixed costs of entering entrepreneurship. Households at the middle of the wealth distribution are 10% more likely to become business owners and invest 18% more in business capital for a 1000 baht increase

[27]The Village Fund Program was introduced in the same general period as other community improvement programs (although none involving loans). For example, the central government implemented a schooling initiative around the same time as the VF credit expansion. If these programs were implemented at exactly the same time as the funds were received by villages, the effects of both these programs and the VF loans could be confounded. In the data, I am unable to tell whether such programs arrived in the same month as the VF loans were disbursed. However, the high baseline (pre-VF) attendance rates and intensity lead me to believe that schooling interventions are not driving the results in this paper. Moreover since other programs were not bundled with the lending and saving services of the Village Fund, there is no obvious reason why they would have the same non-linear pattern of effects over the wealth distribution as VF loans.

in borrowing. Additionally, child labor in these same households rises when they borrow; children are 3.1 percentage points more likely to work and they work an additional 2.4 hours per month in response to a 1000 baht increase in credit. The effects on child labor are persistent and are sizeable even 12 months after households borrow.

The entrepreneurship results are consistent with the work of Banerjee et al. (2010), who find the effects of microcredit on business outcomes in India are strongest for "likely entrepreneurs." However, an important difference between their approach and the one taken in this paper is the source of heterogeneity in households' propensity to become an entrepreneur. Banerjee et al. (2010) model heterogeneity in the returns to entrepreneurship and in time preferences as captured by the number, education and work status of women in the household. In this paper, I model heterogeneity in household wealth to proxy for the ability to afford the start up costs associated with business ownership. This aspect of household heterogeneity is critical when the expansion of credit does not change the marginal interest rate faced by households, which is likely to be the case when the credit expansion is characterized by low limits on borrowing. Nonetheless, the intuition and empirical findings in the two papers are complementary and both lend evidence to the overall theme that the impacts of easing credit constraints are likely to differ across household types. As policies are often targeted to improving the living conditions of the poorest households, these results suggest that limited credit expansions in particular may not be the most effective tool for encouraging high-return entrepreneurial ventures as a method for poverty alleviation.

If policymakers see child labor as an inefficient outcome, the results in this paper show that a policy aimed solely at reducing credit market imperfections can have unintended consequences for the supply of child labor. Although I find no systematic effects of increased borrowing availability on schooling attendance or dropout rates, it is still possible that increased child labor negatively affects children in other ways not captured by schooling attendance alone. For example, increased child labor may still decrease human capital formation if labor hours cause children to perform worse in school, an effect which I cannot address with these data.

Nonetheless, it is also important to keep in mind that it is not clear that increasing child labor reduces overall household welfare. In circumstances where child labor is the only means of generating enough income for subsistence, it may be optimal for households to choose to work their children more, even given the costs of such work. Moreover, in the framework in this paper household wealth is fixed over time, but in reality there is scope

for upward mobility if households can permanently increase their productivity of household enterprises. In other words, for some households expanded credit access may lead to higher levels of child labor in the medium run, but it also may enable households to permanently raise wealth levels and result in higher household welfare in the long run. This possibility is not capture in the model and analysis here but these results in this paper suggest that this is a promising area for future research.

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Figure 1: Borrowing by Initial Household Wealth Decile

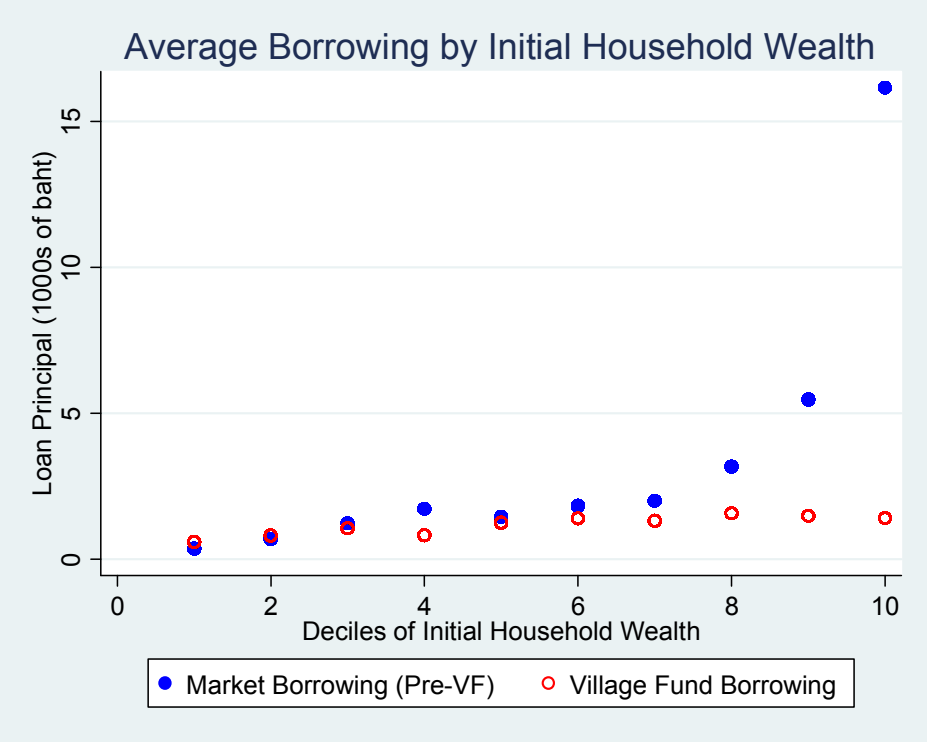


Figure 2: Business Ownership by Initial Household Wealth Decile

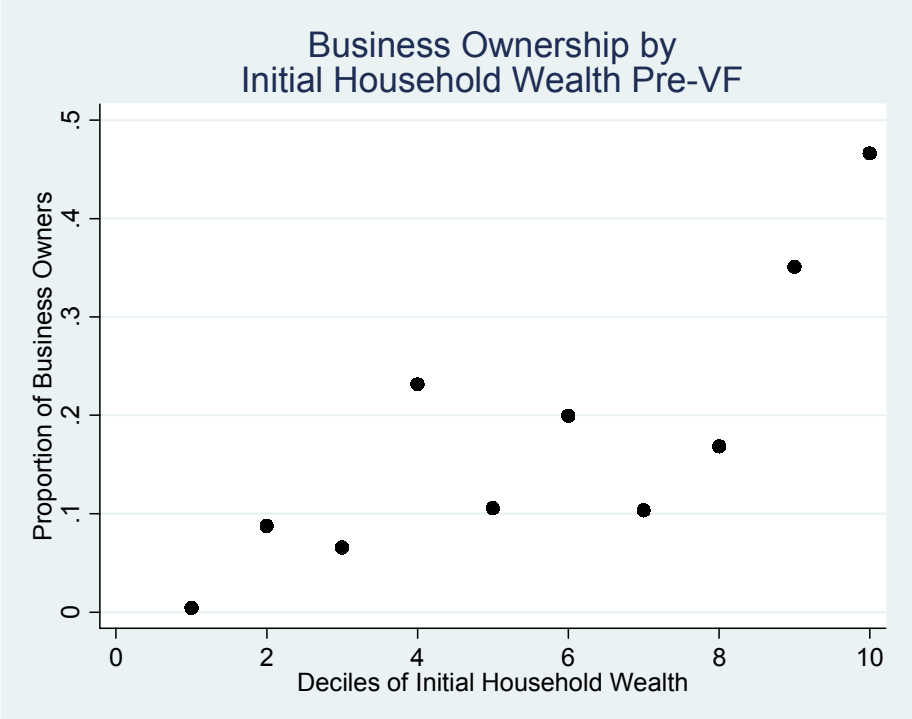
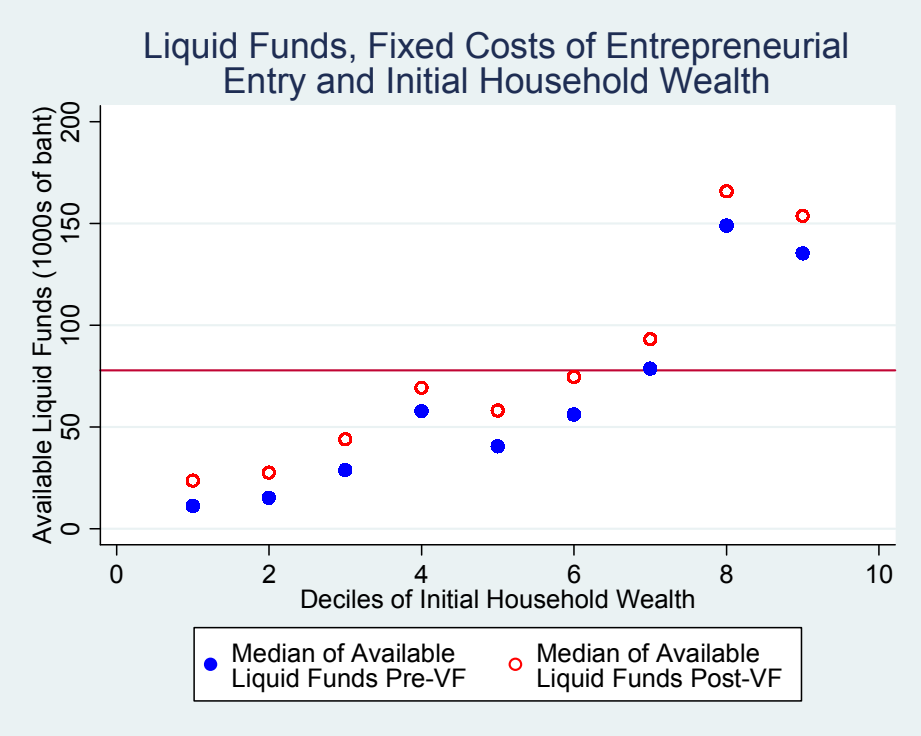


Figure 3: Liquid Funds and Fixed Costs of Entry by Initial Household Wealth Decile



Notes: Pre-VF Available Liquid Funds defined as the sum of current cash on hand, deposits at banks and average market loan amount by wealth decile (pre-program period only). Post-VF Available Liquid Funds are the sum of Pre-VF Available Liquid Funds and average VF Fund loan amount across the sample. Average fixed costs of entrepreneurial entry are 77,700 baht (horizontal line). Decile 10 is omitted for scaling purposes; median of Pre-VF Funds = 310.6, median of Post-VF Funds = 326.5.

Table 1. Credit Statistics by Wealth Group

Panel A: Pre-program credit statistics

	Low Wealth (1)	Middle Wealth (2)	High Wealth (3)	p-value for difference [(2) - (1)] (4)	p-value for difference [(3) - (1)] (5)	p-value for difference [(3) - (2)] (6)
Proportion of households that … (%)						
Self-report being credit constrained	7.06	2.02	1.71	0.096	0.055	0.867
Borrow from institutional sources	47.1	55.6	67.5	0.253	0.003	0.072
Borrow from family members	29.4	22.2	14.5	0.267	0.010	0.144
Borrow from non-family individuals (including moneylenders)	52.9	46.5	34.2	0.384	0.008	0.067
Average amount borrowed from … (thousands of baht)						
Institutional sources	11.5	15.7	34.6	0.112	0.000	0.000
Family members	4.55	6.85	18.7	0.073	0.000	0.001
Other Individuals (including moneylenders)	3.78	6.72	37.8	0.000	0.000	0.049
All	7.03	11.5	36.9	0.001	0.000	0.000
Average yearly interest rate faced for loans from … (%)						
Institutional sources	67.9	51.7	49.2	0.194	0.126	0.794
Family members	128.9	93.1	89.6	0.254	0.294	0.901
Other Individuals (including moneylenders)	111.2	97.9	118.5	0.397	0.704	0.195
All	87.6	71.1	65.2	0.094	0.030	0.484
Average default rate for loans from … (%)						
Institutional sources	14.8	11.9	7.83	0.000	0.000	0.012
Family members	8.33	38.9	8.33	0.000	1.000	0.008
Other Individuals (including moneylenders)	18.3	30.3	18.7	0.000	0.917	0.116
All	15.4	14.6	8.65	0.692	0.000	0.000

Panel B: Village Fund credit statistics

	Low Wealth (1)	Middle Wealth (2)	High Wealth (3)	p-value for difference [(2) - (1)] (4)	p-value for difference [(3) - (1)] (5)	p-value for difference [(3) - (2)] (6)
Proportion of borrowers	57.4	70.9	76.4	0.032	0.000	0.304
Average loan principal	11.9	14.2	14.6	0.001	0.000	0.000
Average yearly interest rate	25.5	19.8	19.6	0.503	0.728	0.408
Average default rate	3.45	5.30	2.26	0.000	0.248	0.004

Notes: With the exception of the Village Fund statistics, all statistics are for the pre-program period only. Village Fund statistics are for the post program period only.

Table 2. Pre-program summary statistics and testing for differential trends by village size and timing

Panel A: Early vs. Late Receivers	Early Receiver		Late Receiver		Difference	p-value for
	Obs	Mean	Obs	Mean	[(4) - (2)]	difference
	(1)	(2)	(3)	(4)	(5)	(6)
Child Performs Any Work (1=Yes, 0=No)	3094	0.059	2849	0.051	-0.008	0.656
Owns Non-agricultural Business (1=Yes, 0=No)	2636	0.141	2534	0.241	0.100	0.056

	p-value for differential time trend between early and late receivers	
	Monthly Trend	Yearly Trend
Child Performs Any Work (1=Yes, 0=No)	0.773	0.683
Owns Non-agricultural Business (1=Yes, 0=No)	0.103	0.165

Panel B: Small vs. Large Villages	Small Village		Large Village		Difference	p-value for
	Obs	Mean	Obs	Mean	[(4) - (2)]	difference
	(1)	(2)	(3)	(4)	(5)	(6)
Child Performs Any Work (1=Yes, 0=No)	2635	0.044	3308	0.064	0.020	0.250
Owns Non-agricultural Business (1=Yes, 0=No)	2199	0.240	2872	0.150	-0.090	0.073

	p-value for differential time trend between large and small villages	
	Monthly Trend	Yearly Trend
Child Performs Any Work (1=Yes, 0=No)	0.190	0.173
Owns Non-agricultural Business (1=Yes, 0=No)	0.314	0.244

Notes: Early receivers are defined as all villages receiving funds from the central government on or before August 2001. Late receivers are defined for villages receiving funds after August 2001. Small villages are defined as all villages with total population less than or equal to 300 in August 1997. Large villages are those with population more than 300 in August 1997.

Table 3. Effects of Village Fund Borrowing on Business Ownership, by Wealth Decile (IV)

	Dependent Variable: Own a Non-Agricultural Business (1=Yes, 0=No)
Initial Wealth Decile = 1	-0.0732*
	(0.0442)
Initial Wealth Decile = 2	-0.0327
	(0.0224)
Initial Wealth Decile = 3	-0.0197
	(0.0126)
Initial Wealth Decile = 4	0.0011
	(0.0562)
Initial Wealth Decile = 5	0.0117
	(0.0169)
Initial Wealth Decile = 6	0.0362
	(0.0309)
Initial Wealth Decile = 7	-0.0284*
	(0.0158)
Initial Wealth Decile = 8	-0.0248**
	(0.0106)
Initial Wealth Decile = 9	-0.0207*
	(0.0118)
Initial Wealth Decile = 10	0.0070
	(0.0276)
Pre-program mean of dependent variable	0.179
Household Fixed Effects	Yes
Month Fixed Effects	Yes
Observations	18,021
Number of hhid	425

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

Notes: Standard errors clustered at the village level. Other controls include age and education of the household head, average age and education in the household, number of household members, proportion of female household members, number of children, number of sons.

Table 4: Reduced Form Effect of Village Fund Program

Panel A: Total Effects

	Business Ownership	Child Performs Any Work in Business	Child Work Hours Business (per month)
	(1)	(2)	(3)
Low Wealth	-13.1*	0.864	-95.0
(F-Statistic)	(3.09)	(0.47)	(0.13)
Middle Wealth	12.9***	10.5***	830.7**
(F-Statistic)	(14.22)	(18.22)	(6.63)
High Wealth	-7.39	1.50	166.8
(F-Statistic)	(1.56)	(1.17)	(1.71)

Panel B: Regression Output

	Business Ownership	Child Performs Any Work in Business	Child Work Hours Business (per month)
	(1)	(2)	(3)
Low Wealth * Instrument	-5.77	-0.638	-261.8
	(4.24)	(0.717)	(258.3)
Middle Wealth * Instrument	20.3**	9.043**	663.9*
	(7.21)	(3.48)	(373.4)
Instrument	-7.39	1.502	166.8
	(5.73)	(1.40)	(133.7)
Pre-program mean of dependent variable	0.179	0.017	1.17
Mean of Instrument	0.003	0.003	0.003
Child/HH Fixed Effects	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes
Observations	18,020	20,711	20,711
Number of HHs/Children	426	612	612

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

Notes: Standard errors clustered at the village level. F-statistics for the significance of total marginal effects are presented in parenthesis below the effects. Other controls include age and age squared (child regressions only), age and education of the household head, average age and education in the household, number of household members, proportion of female household members, number of children, number of sons. Mean of instrument is calculated for post-program period only.

Table 5: First Stage Statistics

	Household-level Regressions			Child-level Regressions		
	Low Wealth * VF Loan Principal	Middle Wealth * VF Loan Principal	Village Fund Loan Principal	Low Wealth * VF Loan Principal	Middle Wealth * VF Loan Principal	Village Fund Loan Principal
	(1)	(2)	(3)	(4)	(5)	(6)
Angrist-Pischke F-statistic	107.2	24.3	15.9	73.6	53.8	30.4
P-value of excluded instruments	0.000	0.000	0.001	0.000	0.000	0.000
Shea Partial R-Squared	0.015	0.016	0.008	0.009	0.009	0.007
Kleibergen-Paap rk LM p-value		0.062			0.082	
Kleibergen-Paap rk Wald F-statistic		21.354			24.363	
Household/Child Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	18,020	18,020	18,020	20,711	20,711	20,711
Number of Households	426	426	426	612	612	612

Notes: Loan Principal measured in 1000s of baht. Standard errors clustered at the village level. Other controls include age and age squared (child regressions only), age and education of the household head, average age and education in the household, number of household members, proportion of female household members, number of children, number of sons.

Table 6: Effects of Village Fund Program on Non-Agricultural Businesses Activity

Panel A: Total Marginal Effects

	Business Ownership	Capital Stock (1000s of baht)	Non-Labor Inputs (1000s of baht)	Hired Labor (hours per month)	Revenue (1000s of baht)	Profits (1000s of baht)
	(1)	(2)	(3)	(4)	(5)	(6)
Low Wealth	-0.026**	0.975	-0.046	-5.22	-0.927	0.213
(F-Statistic)	(4.22)	(1.13)	(0.04)	(0.12)	(1.43)	(0.57)
Middle Wealth	0.017***	1.491**	-0.222	-3.38	0.063	0.344
(F-Statistic)	(7.11)	(3.88)	(2.36)	(0.86)	(0.01)	(1.75)
High Wealth	-0.014	-0.175	-0.147*	4.76	-0.398	0.136
(F-Statistic)	(1.85)	(0.16)	(3.72)	(0.51)	(0.80)	(0.41)

Panel B: Regression Output

	Business Ownership	Capital Stock (1000s of baht)	Non-Labor Inputs (1000s of baht)	Hired Labor (hours per month)	Revenue (1000s of baht)	Profits (1000s of baht)
	(1)	(2)	(3)	(4)	(5)	(6)
Low Wealth * Loan Principal	-0.012	1.150	0.101	-9.98	-0.529	0.202
	(0.012)	(0.778)	(0.251)	(8.46)	(0.703)	(0.411)
Middle Wealth * Loan Principal	0.031**	1.666*	-0.075	-8.14	0.461	0.208
	(0.013)	(0.865)	(0.110)	(7.59)	(0.477)	(0.194)
Loan Principal	-0.014	-0.175	-0.147*	4.76	-0.398	0.136
	(0.010)	(0.431)	(0.076)	(6.65)	(0.446)	(0.213)
Pre-program mean of dependent variable	0.179	8.23	1.93	7.57	4.57	0.484
Household Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	18,020	18,020	18,020	18,020	18,020	18,020
Number of HHs	426	426	426	426	426	426

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

Notes: Loan Principal measured in 1000s of baht. F-statistics for the significance of total marginal effects are presented in parenthesis below the effects. Standard errors clustered at the village level. Capital stock is the stock of existing business fixed capital (taking into account depreciation). Non-labor and labor inputs represent flows. Other controls include age and education of the household head, average age and education in the household, number of household members, proportion of female household members, number of children, number of sons.

Table 7: Effects of Village Fund Program on Agricultural Activity

Panel A: Total Marginal Effects

	Any Farm Activity	Capital Stock (1000s of baht)	Non-Labor Inputs (1000s of baht)	Hired Labor (hours per month)	Revenue (1000s of baht)	Profits (1000s of baht)
	(1)	(2)	(3)	(4)	(5)	(6)
Low Wealth	0.005	-2.48	-27.3**	9.79	2.14**	1.434*
(F-Statistic)	(0.35)	(2.28)	(5.97)	(1.70)	(4.83)	(5.84)
Middle Wealth	-0.006	-0.380	-7.55	6.05	0.460	-0.308
(F-Statistic)	(0.32)	(0.08)	(2.24)	(0.80)	(0.40)	(0.33)
High Wealth	0.022	1.84	8.61	4.71	-1.11	-0.230
(F-Statistic)	(2.70)	(1.65)	(1.77)	(1.16)	(0.76)	(0.05)

Panel B: Regression Output

	Any Farm Activity	Capital Stock (1000s of baht)	Non-Labor Inputs (1000s of baht)	Hired Labor (hours per month)	Revenue (1000s of baht)	Profits (1000s of baht)
	(1)	(2)	(3)	(4)	(5)	(6)
Low Wealth * Loan Principal	-0.017*	-4.32*	-35.90**	5.08	3.25*	1.664
	(0.009)	(2.38)	(14.85)	(5.30)	(1.81)	(1.321)
Middle Wealth * Loan Principal	-0.028*	-2.22	-16.16*	1.34	1.57	-0.078
	(0.015)	(2.24)	(9.47)	(4.71)	(1.63)	(1.100)
Loan Principal	0.022	1.84	8.61	4.71	-1.11	-0.230
	(0.014)	(1.43)	(6.48)	(4.37)	(1.27)	(1.033)
Pre-program mean of dependent variable	0.854	41.3	26.7	36.2	10.2	2.23
Household Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	18,020	17,817	17,815	18,020	18,020	18,020
Number of HHs	426	426	426	426	426	426

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

Notes: Loan Principal measured in 1000s of baht. F-statistics for the significance of total marginal effects are presented in parenthesis below the effects. Standard errors clustered at the village level. Capital stock is the stock of existing business fixed capital (taking into account depreciation). Non-labor and labor inputs represent flows. Other controls include age and education of the household head, average age and education in the household, number of household members, proportion of female household members, number of children, number of sons.

Table 8: Effects of Village Fund Program on the Composition of Household Activities

Panel A: Total Marginal Effects

	Business Ownership	Any Farm Activity	Business Only	Farm Only
	(1)	(2)	(3)	(4)
Low Wealth	-0.026**	0.005	-0.012	0.019*
(F-Statistic)	(4.22)	(0.35)	(2.04)	(2.80)
Middle Wealth	0.017***	-0.006	0.001	-0.022*
(F-Statistic)	(7.11)	(0.32)	(0.12)	(2.91)
High Wealth	-0.014	0.022	-0.012**	0.024*
(F-Statistic)	(1.85)	(2.70)	(4.19)	(3.05)

Panel B: Regression Output

	Business Ownership	Any Farm Activity	Business Only	Farm Only
	(1)	(2)	(3)	(4)
Low Wealth * Loan Principal	-0.012	-0.017*	-0.000	-0.005
	(0.012)	(0.009)	(0.005)	(0.015)
Middle Wealth * Loan Principal	0.031**	-0.028*	0.013*	-0.046**
	(0.013)	(0.015)	(0.007)	(0.020)
Loan Principal	-0.014	0.022	-0.012**	0.024*
	(0.010)	(0.014)	(0.006)	(0.014)
Pre-program mean of dependent variable	0.179	0.854	0.038	0.713
Household Fixed Effects	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes
Observations	18,020	18,020	18,020	18,020
Number of HHs	426	426	426	426

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

Notes: Loan Principal measured in 1000s of baht. F-statistics for the significance of total marginal effects are presented in parenthesis below the effects. Standard errors clustered at the village level. Other controls include age and education of the household head, average age and education in the household, number of household members, proportion of female household members, number of children, number of sons.

Table 9: Effects of Village Fund Program on Adult Labor Supply

Panel A: Total Marginal Effects

	Any Work in Business	Hours of Work in Business (per month)	Any Work in Agriculture (in home)	Hours of Work in Agric. (per month)	Any Wage Work (outside home)	Hours of Wage Work (per month)
	(1)	(2)	(3)	(4)	(5)	(6)
Low Wealth	-0.010	0.650	-0.033	-14.8**	0.002	-0.24
(F-Statistic)	(0.47)	(0.08)	(1.16)	(5.79)	(0.01)	(0.00)
Middle Wealth	0.009	0.360	0.018	7.21	-0.025	-6.14
(F-Statistic)	(1.30)	(0.06)	(0.11)	(0.63)	(1.33)	(0.93)
High Wealth	-0.010	-0.880	-0.022	-4.96	0.006	0.20
(F-Statistic)	(1.50)	(0.69)	(1.00)	(0.78)	(0.08)	(0.00)

Panel B: Regression Output

	Any Work in Business	Hours of Work in Business (per month)	Any Work in Agriculture (in home)	Hours of Work in Agric. (per month)	Any Wage Work (outside home)	Hours of Wage Work (per month)
	(1)	(2)	(3)	(4)	(5)	(6)
Low Wealth * Loan Principal	-0.000	1.53	-0.011	-9.83**	-0.004	-0.44
	(0.015)	(2.69)	(0.032)	(4.06)	(0.025)	(7.95)
Middle Wealth * Loan Principal	0.019	1.24	0.040	12.17	-0.031	-6.34
	(0.014)	(2.12)	(0.041)	(8.68)	(0.026)	(7.42)
Loan Principal	-0.010	-0.88	-0.022	-4.96	0.006	0.20
	(0.008)	(1.06)	(0.023)	(5.63)	(0.024)	(6.49)
Pre-program mean of dependent variable	0.096	8.99	0.500	59.0	0.484	94.3
Household Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	18,020	16,947	18,020	16,992	18,020	16,993
Number of HHs	426	421	426	421	426	421

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

Notes: Loan Principal measured in 1000s of baht. F-statistics for the significance of total marginal effects are presented in parenthesis below the effects. Standard errors clustered at the village level. Other controls include age and education of the household head, average age and education in the household, number of household members, proportion of female household members, number of children, number of sons.

Table 10: Effects of Village Fund Program on Child Labor Supply

Panel A: Total Marginal Effects

	Any Work in Business	Hours of Work in Business (per month)	Any Work in Agriculture (in home)	Hours of Work in Agric. (per month)	Any Wage Work (outside home)	Hours of Wage Work (per month)	Any Domestic Chores	Days Performing Domestic Chores
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Low Wealth	0.002	-0.319	-0.012	-1.207**	-0.003	-1.449	0.025	0.699
(F-Statistic)	(0.32)	(0.19)	(1.80)	(4.32)	(0.31)	(0.89)	(1.01)	(0.42)
Middle Wealth	0.030***	2.42**	-0.008	0.497	-0.011	-1.732	0.049***	0.383
(F-Statistic)	(11.65)	(6.31)	(0.59)	(0.23)	(2.68)	(1.78)	(5.31)	(0.16)
High Wealth	0.003	0.401	-0.018*	-0.523*	-0.008**	-1.06*	0.054**	0.536
(F-Statistic)	(1.30)	(2.09)	(3.24)	(2.72)	(5.96)	(3.30)	(8.77)	(0.68)

Panel B: Regression Output

	Any Work in Business	Hours of Work in Business (per month)	Any Work in Agriculture (in home)	Hours of Work in Agric. (per month)	Any Wage Work (outside home)	Hours of Wage Work (per month)	Any Domestic Chores	Days Performing Domestic Chores
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Low Wealth * Loan Principal	-0.001	-0.720	0.006	-0.684	0.005	-0.388	-0.029	0.163
	(0.002)	(0.724)	(0.015)	(0.692)	(0.005)	(1.159)	(0.030)	(1.306)
Middle Wealth * Loan Principal	0.027**	2.022*	0.010	1.020	-0.003	-0.671	-0.005	-0.153
	(0.011)	(1.087)	(0.008)	(1.017)	(0.004)	(0.846)	(0.020)	(1.184)
Loan Principal	0.003	0.401	-0.018*	-0.523*	-0.008**	-1.061*	0.054***	0.536
	(0.003)	(0.277)	(0.010)	(0.317)	(0.003)	(0.584)	(0.018)	(0.652)
Pre-program mean of dependent variable	0.017	1.68	0.034	1.28	0.008	1.34	0.614	12.0
Child Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	20,711	20,711	20,711	20,711	20,711	20,711	17,158	17,158
Number of Children	612	612	612	612	612	612	556	556

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

Notes: Loan Principal measured in 1000s of baht. F-statistics for the significance of total marginal effects are presented in parenthesis below the effects. Standard errors clustered at the village level. Other controls include age, age squared, age and education of the household head, average age and education in the household, number of household members, proportion of female household members, number of children, number of sons.

Table 11: Effects of Village Fund Program on Child Schooling Outcomes

Panel A: Total Marginal Effects

	Attends School	Days Attended School	Dropout of School
	(1)	(2)	(3)
Low Wealth	0.003	-0.014	0.000
(F-Statistic)	(0.02)	(0.96)	(0.05)
Middle Wealth	0.017	0.144	0.002
(F-Statistic)	(1.27)	(0.30)	(1.54)
High Wealth	-0.002	0.072	0.002
(F-Statistic)	(0.04)	(0.10)	(1.05)

Panel B: Regression Output

	Attends School	Days Attended School	Dropout of School
	(1)	(2)	(3)
Low Wealth * Loan Principal	0.005	-0.086	-0.002
	(0.021)	(0.362)	(0.002)
Middle Wealth * Loan Principal	0.019	0.072	-0.000
	(0.018)	(0.349)	(0.003)
Loan Principal	-0.002	0.072	0.002
	(0.010)	(0.228)	(0.002)
Pre-program mean of dependent variable	0.861	15.7	0.001
Child Fixed Effects	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes
Observations	17,158	17,158	17,158
Number of Children	556	556	556

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

Notes: Loan Principal measured in 1000s of baht. F-statistics for the significance of total marginal effects are presented in parenthesis below the effects. Standard errors clustered at the village level. Other controls include age, age squared, age and education of the household head, average age and education in the household, number of household members, proportion of female household members, number of children, number of sons.

Appendix Table 1. Full Sample Summary Statistics

Child Outcomes	Obs.	Mean	Std. Dev.	Min	Max
Child Performs Any Work during the Month (Dummy)	20711	0.058		0	1
Child Ever Works during Sample Period (Dummy)	20711	0.489		0	1
(Conditional on Working)					
Total Child Work Hours	1206	62.4	79.1	.025	579.5
Total Hours in Any Type of Home Production	1101	51.2	67.5	.025	579.5
Child Attends School (Dummy)	17158	0.962		0	1
Child Drops out of School (Dummy)	17158	0.001		0	1
Total Days Spent in School	17158	15.7	7.7	0	31
Total Days Spent in Domestic Activities	17158	14.6	13.6	0	31

Selected Explanatory Variables *(All Children/Households)*	Obs.	Mean	Std. Dev.	Min	Max
Age	20711	12.0	1.15	10	14
Gender (=1 if Male)	20711	0.494		0	1
No. of children	18020	1.40	0.558	1	5
Age of household head	18020	53.2	15.0	0	95
Education of household head	18020	2.00	0.727	0	5
Total no. of household members	18020	5.18	2.08	1	16
Initial Wealth (thousands of baht)	18020	1286	3108	0	37568

Education of household head: 0 = none, 1 = at least some pre-primary, 2 = at least some primary, 3 = at least some secondary, 4 = at least some university, 5 = beyond university. Avg. Education of Household in years of schooling. School outcomes are for during school year only.

Appendix Table 2: Dynamic Effects of Village Fund Program on Child Labor

	Any Work in Business	Hours of Work in Business (per month)
	(1)	(2)
Contemporaneous		
Low Wealth	0.004	-0.287
(F-Statistic)	(0.29)	(0.06)
Middle Wealth	0.021**	1.304**
(F-Statistic)	(8.41)	(5.92)
High Wealth	0.006	0.596**
(F-Statistic)	(1.45)	(4.06)
6-Month Lag		
Low Wealth	-0.002	0.383
(F-Statistic)	(0.05)	(0.26)
Middle Wealth	0.011***	1.492***
(F-Statistic)	(10.30)	(17.86)
High Wealth	-0.005	-0.019
(F-Statistic)	(0.75)	(0.00)
12-Month Lag		
Low Wealth	0.001	-0.149
(F-Statistic)	(0.08)	(0.05)
Middle Wealth	0.008	0.917*
(F-Statistic)	(2.49)	(2.79)
High Wealth	0.006	0.072
(F-Statistic)	(0.64)	(0.02)
Child Fixed Effects	Yes	Yes
Month Fixed Effects	Yes	Yes
Observations	13,799	13,799
Number of Children	595	595

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

Notes: Loan Principal measured in 1000s of baht. Standard errors clustered at the village level. Total marginal effects and relevant F-statics are reported. Other controls include age, age squared, age and education of the household head, average age and education in the household, number of household members, proportion of female household members, number of children, number of sons.

Appendix Table 3: Effects of Village Fund Program on Consumption Smoothing & Debt Substitution

Panel A: Total Marginal Effects

	Standard Deviation of Future Consumption (6-months ahead)	Standard Deviation of Future Consumption (12-months ahead)	Made Voluntary Payment on Existing Debt (Institutional Loans Only)	Made Voluntary Payment on Existing Debt (Institutional Loans & Money Lenders)
	(1)	(2)	(3)	(4)
Low Wealth	-566.9	-514.1	-0.002	-0.004
(F-Statistic)	(1.21)	(0.83)	(0.14)	(0.39)
Middle Wealth	-268.5	44.8	-0.003	-0.007*
(F-Statistic)	(1.18)	(0.02)	(0.34)	.2.91
High Wealth	235.3	523.9	0.018***	0.012*
(F-Statistic)	(0.31)	(0.42)	(8.71)	(2.74)

Panel B: Regression Output

	Standard Deviation of Future Consumption (6-months ahead)	Standard Deviation of Future Consumption (12-months ahead)	Made Voluntary Payment on Existing Debt (Institutional Loans Only)	Made Voluntary Payment on Existing Debt (Institutional Loans & Money Lenders)
	(1)	(2)	(3)	(4)
Low Wealth * Loan Principal	-802.2	-1038.0	-0.000	1.53
	(681.2)	-958.8	(0.015)	(2.69)
Middle Wealth * Loan Principal	-503.8	-479.1	0.019	1.24
	(599.4)	(873.2)	(0.014)	(2.12)
Loan Principal	235.3	523.9	-0.010	-0.88
	(421.9)	(654.0)	(0.008)	(1.06)
Pre-program mean of dependent variable	2056.2	2477.1	0.012	0.017
Household Fixed Effects	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes
Observations	16,994	16,994	14,805	14,805
Number of HHs	426	426	395	395

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

Notes: Loan Principal measured in 1000s of baht. F-statistics for the significance of total marginal effects are presented in parenthesis below the effects. Standard errors clustered at the village level. Other controls include age and education of the household head, average age and education in the household, number of household members, proportion of female household members, number of children, number of sons.

Appendix Table 4: Including Village-specific Time Trends

Panel A: Total Marginal Effects

	Dependent Variable: Business Ownership			Dependent Variable: Child Performs Any Work in Business		
	Common Trends	Village-specific Monthly Trends	Village-specific Yearly Trends	Common Trends	Village-specific Monthly Trends	Village-specific Yearly Trends
	(1)	(2)	(3)	(4)	(5)	(6)
Low Wealth	-0.026**	-0.028***	-0.032***	0.002	-0.008	-0.008
(F-Statistic)	(4.22)	(8.41)	(7.73)	(0.32)	(2.17)	(0.10)
Middle Wealth	0.017***	0.016**	0.012	0.030***	0.020***	0.027**
(F-Statistic)	(7.11)	(4.72)	(2.37)	(11.65)	(7.29)	(5.28)
High Wealth	-0.014	-0.017*	-0.021*	0.003	0.00	0.004
(F-Statistic)	(1.85)	(2.87)	(2.88)	(1.30)	(0.01)	(0.52)

Panel B: Regression Output

	Dependent Variable: Business Ownership			Dependent Variable: Child Performs Any Work in Business		
	Common Trends	Village-specific Monthly Trends	Village-specific Yearly Trends	Common Trends	Village-specific Monthly Trends	Village-specific Yearly Trends
	(1)	(2)	(3)	(4)	(5)	(6)
Low Wealth * Loan Principal	-0.012	-0.019	-0.019	-0.001	-0.006	-0.006
	(0.012)	(0.016)	(0.016)	(0.002)	(0.004)	(0.005)
Middle Wealth * Loan Principal	0.031**	0.042*	0.042*	0.027**	0.021*	0.023*
	(0.013)	(0.021)	(0.021)	(0.011)	(0.012)	(0.013)
Loan Principal	-0.014	-0.014	-0.014	0.003	-0.001	0.004
	(0.010)	(0.013)	(0.013)	(0.003)	(0.006)	(0.005)
Household Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	18,020	18,020	18,020	20,711	20,711	20,711
Number of HHs	426	426	426	612	612	612

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

Notes: Loan Principal measured in 1000s of baht. Standard errors clustered at the village level. Other controls include age and education of the household head, average age and education in the household, number of household members, proportion of female household members, number of children, number of sons.

Appendix Table 5: General Equilibrium and Spillover Effects

	All Individuals		Non Borrowers Only			
	Dependent Variable: Wages		Business Ownership		Child Performs Any Work in Business	
	(1)	(2)	(3)	(4)	(5)	(6)
Instrument (Timing only)	0.236		-0.000		0.010	
	(1.181)		(0.000)		(0.013)	
Instrument (Timing + Population)		-352.881		-0.040		0.293
		(502.111)		(0.062)		(7.736)
Household Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	22,641	22,641	5,095	5,095	4,500	4,500
Number of Individuals/HHs	818	818	117	117	161	161

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

Notes: Mean of Instrument (Timing + Population) = 0.003 in post-program period. Loan Principal measured in 1000s of baht. Standard errors clustered at the village level. Other controls include age and education of the household head, average age and education in the household, number of household members, proportion of female household members, number of children, number of sons.

Appendix Table 6: Adding Heterogeneity by Education

Panel A: Heterogeneity in Total Marginal Effects by Eduation and Wealth

	Dependent Variable: Business Ownership			Dependent Variable: Child Performs Any Work in Business		
	No Heterogeneity by Education	Below Secondary Education	At least Some Secondary Education	No Heterogeneity by Education	Below Secondary Education	At least Some Secondary Education
	(1)	(2)	(3)	(4)	(5)	(6)
Low Wealth	-0.026**	-0.024**	-0.028*	0.002	0.003	0.001
(F-Statistic)	(4.22)	(4.62)	(3.65)	(0.32)	(0.36)	(0.00)
Middle Wealth	0.017***	0.022**	0.018*	0.030***	0.031***	0.029***
(F-Statistic)	(7.11)	(6.64)	(3.53)	(11.65)	(9.03)	(10.57)
High Wealth	-0.014	-0.012	-0.016*	0.003	0.004	0.002
(F-Statistic)	(1.85)	(1.12)	(3.15)	(1.30)	(1.30)	(0.03)

Panel B: Heterogeneity in Total Marginal Effects by Education Only

	Dependent Variable: Business Ownership	Dependent Variable: Child Performs Any Work in Business
	(7)	(8)
No Education	-0.015	0.011
(F-Statistic)	(0.08)	(2.02)
At least Some Primary Education	0.001	0.014***
(F-Statistic)	(0.07)	(8.12)
At least Some Secondary Education	-0.006	0.013*
(F-Statistic)	(0.30)	(3.65)

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

Notes: Loan Principal measured in 1000s of baht. F-statistics for the significance of total marginal effects are presented in parenthesis below the effects. Standard errors clustered at the village level. Other controls include age and education of the household head, average age and education in the household, number of household members, proportion of female household members, number of children, number of sons, household (child) fixed effects, month fixed effects.

Appendix Table 7. Critical Values for Wald Statistics from Wild Cluster Bootstrap

	Dependent Variable: Business Ownership			Dependent Variable: Child Performs Any Work in Business		
	Actual Value, |ω|	α=0.05	α=0.10	Actual Value, |ω|	α=0.05	α=0.10
Loan Size X Low Wealth	1.04	1.83	1.64	0.743	1.88	1.62
Loan Size X Middle Wealth	2.43	2.75	2.25	2.48	2.87	2.39
Loan Size	1.24	2.21	1.72	1.14	2.38	1.86
Number of bootstraps, *B*		999			999	

NOTE: The actual values reported are in absolute value. The critical values are drawn from the distribution of the absolute value of the Wald statistics from 999 bootstrap iterations. The rejection rule used is to reject H_0 if and only if $|\omega| > \omega^*_{[\alpha]}$ where $\omega^*_{[q]}$ denotes the qth quantile of $\omega^*_{[1]} \ldots \omega^*_B$.