Identification Strategy: A Field Experiment on Dynamic Incentives in Rural Credit Markets*

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Abstract

How do borrowers respond to improvements in a lender's ability to punish defaulters? We report the results of a randomized field experiment in rural Malawi that examines the impact of fingerprinting borrowers in a context where a unique identification system is absent. Fingerprinting allows the lender to more effectively use dynamic repayment incentives: withholding future loans from past defaulters while rewarding good borrowers with better loan terms. Consistent with a simple model of borrower heterogeneity and information asymmetries, fingerprinting led to substantially higher repayment rates for borrowers with the highest ex ante default risk, but had no effect for the rest of borrowers. The change in repayment rates is driven by reductions in adverse selection (smaller loan sizes) and lower moral hazard (e.g., less diversion of loan-financed fertilizer from its intended use on the cash crop).

Keywords: credit, microfinance, adverse selection, moral hazard, enforcement

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1. Introduction

Lending in low-income countries is notoriously difficult. Clients typically lack adequate collateral and lenders often have limited information about the profitability of their customers. Information asymmetries coupled with costly enforcement of repayment severely limits the profitability of lenders. The problem is particularly acute in agriculture because the nature of production precludes the use of many of the mechanisms that have made microfinance so successful. For example, lenders cannot schedule frequent repayments because cash flows are only received after harvest, several months after the loan is taken. In addition, all farmers need cash at the same time, so allowing some farmers to borrow only after others have repaid their loans is problematic because some farmers would end up receiving credit when they do not need it. Even if all clients were allowed to borrow at the same time, joint liability may be ineffective if most production shocks are covariate. Finally, and perhaps most importantly, lenders may lack the ability to deny access to future loans to defaulting clients in the absence of a national system that allows individuals to be uniquely identified.

When this happens, loan defaulters can often avoid sanction by simply applying for new loans under different identities. Lenders respond by limiting the supply of credit, due to the inability to sanction unreliable borrowers and, conversely, to reward reliable borrowers with expanded credit. As a result, many smallholder farmers are severely constrained by the inability to finance crucial inputs such as fertilizer and improved seeds, particularly for export crops.[1]

In this paper we implement a randomized field experiment to estimate the impact of biometric identification (fingerprinting) in a context—rural Malawi— characterized by a lack of a unique identification system and limited access to credit.

According to the 2006 Doing Business Report, Malawi ranked 109 out of 129 countries in terms of private credit to GDP, a frequently-used measure of financial development. Malawi also gets the lowest marks in the "depth of credit information index" which proxies for the amount and quality of information about borrowers available to lenders. Using more micro data, 74 percent of cash crop farmers in our baseline survey had not borrowed from a bank or microfinance institution in the last 10 years.

In the experiment, smallholder farmers organized in groups of 15-20 members applied for agricultural input loans to grow paprika and were randomly allocated to either a control group or a treatment group where each member had a fingerprint collected as part of the loan application. Unlike conventional ID cards or passports, a fingerprint is an

[1] The following quote from 1973 by Robert McNamara when he was the World Bank president exemplifies this view: "The miracle of the Green Revolution may have arrived, but for the most part, the poor farmer has not been able to participate in it. He simply cannot afford to pay for the irrigation, the pesticide, the fertilizer… For the small holder operating with virtually no capital, access to capital is crucial."

effective personal identifier because it is unique to and embodied in each person, so it cannot be forgotten, lost or stolen. Thus, fingerprinting customers would allow lenders to construct credit histories and use them to withhold new loans from past defaulters. In essence, fingerprinting can make the threat of future credit denial more credible.

To guide the empirical strategy, we develop a simple two period model in the spirit of Stiglitz and Weiss (1983) that incorporates both adverse selection and moral hazard and show that "dynamic incentives," that is, the ability to deny credit in the second period based on the first period repayment performance, can reduce both types of asymmetric information problems and therefore raise repayment. Adverse selection problems can be mitigated because riskier individuals that would otherwise default may now take out smaller loans (or avoid borrowing altogether) to preserve access to credit in the future.[2] In addition, borrowers may have greater incentives to ensure that agricultural production is successful, either by exerting more effort or by diverting fewer resources away from production (lower moral hazard). Also intuitively, the model predicts that the impact of "dynamic incentives" will be largest for the riskiest individuals.

Consistent with the predictions of the model, fingerprinting led to substantially higher repayment rates for the subgroup of farmers with the highest ex-ante default risk. By contrast, fingerprinting had no impact on repayment for farmers with low ex ante default risk. While we cannot separate the effect of moral hazard and adverse selection on repayment, we collect unique additional evidence that points to the presence of both informational problems. Fingerprinting leads farmers choose smaller loan sizes, consistent with a reduction in adverse selection. In addition, high-default-risk farmers who are fingerprinted also divert fewer inputs away from the contracted crop (paprika), which we interpret as a reduction in moral hazard.

The key contribution of the paper is that, to our knowledge, it is the first randomized field experiment examining the impact of a technology that improves the effectiveness of dynamic incentives in a credit market. Our analysis is further distinguished by the fact that, in addition to measuring impacts on borrowing decisions and repayment (using the lender's administrative data), we also estimate impacts on specific behaviors related to moral hazard using a detailed follow-up survey of borrowers.

Substantively, our intervention most closely resembles the promise of a future lower interest rate conditional on current loan repayment in Karlan and Zinman (2009), henceforth "KZ", who find evidence of moral hazard and weaker evidence of adverse selection in an experiment with a South African provider of consumer loans. Our experiment differs from KZ's in several important respects. First, our experiment is concerned with lending for productive investment in rural areas, while KZ's involves

[2] In this paper we use the term "adverse selection" to mean ex-ante selection effects deriving from borrowers' hidden information. We acknowledge that such selection may occur on the basis of either unobserved risk type (emphasized in the model) or unobserved anticipated effort (as highlighted by Karlan and Zinman, 2009).

consumer loans for urban customers. Second, our experiment estimates the impact of manipulating the ability to impose dynamic incentives via a technological innovation. KZ, on the other hand, measures the impact of informing borrowers of the existence of a dynamic incentive. Third, we implement a follow-up survey of borrowers to provide additional insight into the specific behaviors that are changed by the intervention and that result in higher repayment. KZ, by contrast, relies exclusively on the lender's administrative data for analysis and so cannot shed light on what borrower behaviors may have changed.

The fourth and final key difference is in the timing of the intervention relative to the borrowing decision. In KZ, the dynamic incentive is announced *after* clients have agreed to borrow (and all loan terms have been finalized). As a result, differences in repayment can only be due to moral hazard. In our case, the lender's ability to use dynamic incentives (due to fingerprinting) is revealed *before* agents decide to borrow. Consequently, the composition of borrowers and the choice of loan terms may change as well. Because potential borrowers cannot repeatedly be surprised, an estimate of the impact of dynamic incentives that are revealed prior to the customer's borrowing decision is the more relevant policy parameter.[3]

To be clear, because we informed the lender which clubs had been fingerprinted, loan officers could have changed their behavior towards treated and control clubs in response to this information. For example, they could have devoted more time to monitoring and enforcing repayment from control clubs, since fingerprinted clubs were already subject to dynamic incentives. We provide convincing evidence to the contrary: approval decisions and subsequent monitoring of clubs by loan officers did not differ across treated and control clubs. As a result, we interpret our findings as emerging solely from borrowers' responses.

By documenting impacts on behaviors related to adverse selection and moral hazard, our findings contribute to a burgeoning empirical literature that tests claims made by contract theory and measures the prevalence of asymmetric information (see Chiappori and Salanie, 2003 for a review). A number of recent papers provide empirical evidence of the existence and impacts of asymmetric information in credit markets, in both developed and developing countries. Ausubel (2009) uses a large-scale randomized trial of direct-mail pre-approved solicitations from a major US credit card company and

[3] In principle, one could fingerprint borrowers at different points in time along the loan cycle to identify various asymmetric information problems. For example a subset of borrowers (group 1) could be fingerprinted before loan decisions are made, then another group (group 2) immediately after loans are granted but before funds are invested into production and a yet another group (group 3) could be fingerprinted once production has taken place but before repayment. A final group of borrowers would not be fingerprinted (group 0). With full compliance, that is, when all subjects agree to be fingerprinted, one could then measure adverse selection by comparing group 1 and 2; ex-ante moral hazard by comparing 2 and 3 and strategic default by comparing 3 and 0. Given the number of farmers in our study, it was infeasible to implement this design because power calculations suggested we could have at best two groups. Our study therefore consists of groups 0 and 1.

finds evidence of higher risk individuals selecting less favorable credit cards, consistent with adverse selection. Klonner and Rai (2009) exploit the introduction of a cap in bidding roscas of South India and find higher repayment rates in earlier rounds attributable to changes in the composition of bidders, consistent with lower adverse selection. Visaria (2009) documents the positive impact of expedited legal proceedings on loan repayment among large Indian firms, even among loans that originated before the reform, consistent with a reduction in moral hazard. Giné and Klonner (2005) find that incomplete information about fishermen's ability in coastal India limits their access to credit for technology adoption. Edelberg (1994) also develops a model of adverse selection and moral hazard that is taken to US data from the Survey of Consumer Finance and finds evidence consistent with both informational problems.[4]

The paper is also related to a framed experiment conducted by Giné et al. (forthcoming) in Peru that shows that dynamic incentives can be important. In addition, there is a theoretical and empirical literature on the impact of credit bureaus that are also related to this paper. The exchange of information about borrowers should theoretically reduce adverse selection (Pagano and Jappelli, 1993) and moral hazard (Padilla and Pagano, 2000). Empirically, de Janvry, McIntosh and Sadoulet (forthcoming) study the introduction of a credit bureau in Guatemala and find that it did contribute to efficiency in the credit market. Finally, the paper is related to the literature motivated by the rise in personal bankruptcies in the US in the last decades (Livshits et al. 2010).

The remainder of this paper is organized as follows. Section 2 describes the experimental design and survey data and Section 3 presents a simple model of loan repayment. Section 4 describes the regression specifications, and Section 5 presents the empirical results. Section 6 provides additional discussion and robustness checks. Section 7 presents the benefit-cost analysis of introducing biometric technology, and Section 8 concludes.

2. Experimental design and survey data

The experiment was carried out as part of the Biometric and Financial Innovations in Rural Malawi (BFIRM) project, a cooperative effort among Cheetah Paprika Limited (CP), the Malawi Rural Finance Corporation (MRFC), the University of Michigan, and the World Bank. CP is a privately owned agri-business company established in 1995 that offers extension services and high-quality inputs to smallholder farmers via an out-

[4] Ligon, Thomas and Worrall (1999) write down competing models of risk-sharing that are taken to the data and find evidence of limited commitment. In a paper similar in spirit, Paulson, Townsend, and Karaivanov (2006) estimate structurally competing models of credit markets in Thailand and find moral hazard to be important.

grower paprika scheme.[5] The farmer receives extension services and a package of seeds, pesticides and fungicides at subsidized rates in exchange for the commitment to sell the paprika crop to CP at harvest time. CP is by far the largest paprika purchaser in the country.[6] CP has a staff of six extension officers and 15 field assistants in the locations chosen for the study. The staff maintain a database of all current and past paprika growers and handles the logistics of supplying farmers with the package of inputs as well as the purchase of the crop.

MRFC is a government-owned microfinance institution that provided financing for the in-kind loan package for 1/2 to 1 acre of paprika. The loan did not include any cash to purchase inputs. Instead, borrowers took an authorization form from MRFC to a pre-approved agricultural input supplier who provided the inputs to the farmer and billed MRFC at a later date. The loan amount was roughly 17,000 Malawi Kwacha (approximately $120), varying slightly by location. Sixty percent of the loan went towards fertilizer (one 50 kilogram bag of D-compound fertilizer and two 50 kilogram bags of CAN fertilizer); the rest went toward the CP input package: thirty-three percent covered the cost of nine bags of pesticides and fungicides (2 Funguran, 2 Dithane, 2 Benomyl, 1 Cypermethrin, 1 Acephate and 1 Malathion) and the remaining seven percent for the purchase of 0.4 kilograms of seeds.[7] While all farmers that took the loan were given the CP package, farmers had the option to borrow only one of the two available bags of CAN fertilizer. Expected yield for farmers using the package with two bags of CAN fertilizer on one acre of land was between 400 and 600 kg, compared to 200 kg with no inputs.[8]

In keeping with standard MRFC practices, farmers were expected to raise a 15 percent deposit, and were charged interest of 33 percent per year (or 30 percent for repeat borrowers). Within a group, take-up of the loan was an individual decision, but the subset of farmers who took up the loan was told that they were jointly liable for each others' loans. In practice, however, joint liability schemes in Malawi are seldom enforced.[9]

In the absence of fingerprinting, identification of farmers relies on the personal knowledge of loan officers (who may also rely on local informants such as village and locality leaders). While loan officers could build up reliable knowledge of borrowers over

[5] Extension services consist of preliminary meetings to market paprika seed to farmers and teach them about the growing process, additional group trainings about farming techniques, individual support for growers provided by the field assistants, and information about grading and marketing the crop.
[6] In 2007, CP purchased approximately eighty-five percent of the one thousand tons of paprika produced annually in Malawi.
[7] The loan amount varied across locations because of modest differences in the transport cost for fertilizer. The cost of the CP package was the same in all locations.
[8] Yield is computed under the conservative assumption that farmers will divert one 50 Kg bag of CAN fertilizer towards maize cultivation. While larger quantities of inputs would result in higher output for experienced paprika-growers, the package described here was designed by extension experts to maximize expected profits for novice, small-holder growers.
[9] See Giné and Yang (2009) for another example of limited enforcement of joint liability loans.

time, this identification "technology" is imperfect. Loan officers are sometimes promoted and routinely rotated to other localities. Among the 11 loan officers who handle our study areas, the median number of years at the branch is only two, while the median number of years working for the lender is 13. In the absence of an independent mechanism for identifying borrowers, the institutional memory is lost when the loan officer is transferred to another location. Even when loan officers remain in a given location over time, the sheer number of borrowers can lead them to make mistakes in identification. In this project, loan officers issued an average of 104 loans, and also handled other loan customers not associated with the project.

The timeline of the experiment is presented in Figure 1. In July 2007, CP asked farmers in the study areas to organize themselves into clubs of 15 to 20 members to accommodate MRFC's group lending rules.[10] Most of these clubs were already in existence, primarily to ease delivery of Cheetah extension services and collection of the crop. Our study sample consists of 249 clubs with approximately 3,500 farmers in Dedza, Mchinji, Dowa and Kasungu districts (locations on a map of Malawi are identified in boxes in Figure 2).

Farmer clubs in the study were randomly assigned to be fingerprinted (the treatment group) or not (the control group), with an equal probability of being in either group. During the baseline survey and fingerprinting period (August and September 2007), CP staff provided a list of paprika growing clubs in each locality to be visited in each week, and randomization of treatment status was carried out after stratifying by locality and week of club visit.[11]

Club visits began with private administration of the baseline survey to individual farmers, and were followed by a training session. Both treatment and control groups were given a presentation on the importance of credit history in ensuring future access to credit. The training emphasized that defaulters would face exclusion from future borrowing, while borrowers in good standing could be rewarded with larger loans in the future. Then, in treatment clubs only, individual participants' fingerprints were collected. Our project staff explained how their fingerprint uniquely identified them for credit reporting to all major Malawian rural lenders, and that future credit providers would be able to access the applicant's credit history simply by checking his or her fingerprint.[12] Appendix A provides the script used during the training. See Appendix B for further technical details on the biometric technology used.

After fingerprints were collected, a demonstration program was used to show participants that the laptop computer was now able to identify an individual with only his

[10] A typical CP group has between 15 and 30 farmers and is organized around a paprika collection point. MRFC's lending groups have at most 20 farmers, so most of the CP groups participating in the study had to be split to be able to access MRFC's loans.
[11] There were 16 localities or "extension planning areas" (EPAs) in the study. EPAs are administrative boundaries set up for the delivery of agricultural services by Malawi's agriculture ministry.
[12] Our team of enumerators encountered essentially no opposition to fingerprint collection, perhaps due to the novelty of the technology.

or her fingerprint. One farmer was chosen at random to have his right thumb scanned again, and the club was shown that the individual's name and demographic information (entered earlier alongside the original fingerprint scan) subsequently was retrieved by the computer program. During these demonstration sessions all farmers whose fingerprints were re-scanned were correctly identified. The control group was not fingerprinted, but as mentioned previously, also received the same training emphasizing the importance of one's credit history and how it influences one's future credit access.[13]

The baseline survey administered prior to the training and the collection of fingerprints included questions on individual demographics (education, household size, religion), income generating activities and assets including detailed information on crop production and crop choice, livestock and other assets, risk preferences, past and current borrowing activities, and past variability of income. Summary statistics from the baseline survey are presented in Table 1, and variable definitions are provided in Appendix C.[14]

After the completion of the survey, credit history training, and fingerprinting of the treatment group, the names and locations of the members that applied for loans along with their treatment status were handed over to MRFC loan officers so that they could screen and approve the clubs according to their protocols. Among other standard factors, MRFC conditions lending on the club's successful completion of 16 hours of training. MRFC approved loans for 2,063 out of 3,206 customers (in 121 out of 239 clubs). Of the customers approved for loans, some failed to raise the required down payment and others opted not to borrow for other reasons. The final sample consists of 1,147 loan customers from 85 clubs.[15] These loan customers received loan packages with an average value of MK 16,913 (US$117).[16]

We also implemented a follow-up survey of farmers in August 2008, once crops had been sold and income received, with a sample size of 520.[17] The formal loan maturity (payment) date was September 30, 2008. Some additional payments were made after the formal due date; MRFC reports that there is typically no additional loan repayment two

[13] It should be clear that, because we provided education on the importance of credit history to our control group as well, we can estimate neither the impact of fingerprinting without such education, nor the impact of the credit history education alone.
[14] These survey data were collected prior to the farmers' being informed about the role of biometrics in the project and their treatment status, to ensure that farmers' survey answers were not influenced by knowledge of the nature of the experiment.
[15] While a natural question at this point is whether selection into borrowing was affected by treatment status, treatment and control groups did not differ in their rates of MRFC loan approval or the fraction of farmers who ended up with a loan (as will be detailed in the results section below).
[16] All conversions of Malawi kwacha to US dollars in this paper assume an exchange rate of MK145/US$.
[17] The follow-up sample is smaller than the sample of baseline borrowers because for budget reasons we could not visit each borrowing household at their place of residence. Instead, we invited study participants to come to a central location at a certain date and time to be administered the follow-up interview. Not all farmers attended the meeting where the follow-up survey was administered, but as we discuss below in Section 5.C. (see Appendix Table 2), there is no evidence of selective attrition related to treatment status: in no case is fingerprinting or fingerprinting interacted with predicted repayment statistically significantly associated with attrition from the survey.

months past the due date for agricultural loans. In the empirical analysis we obtain our dependent variables from the August 2008 survey data as well as administrative data from MRFC on loan take-up, amount borrowed, and repayment.

Balance of baseline characteristics across treatment vs. control groups

To confirm that the randomization across treatments achieved balance in terms of pre-treatment characteristics, Table 2 presents the means of several baseline variables for the control group as reported prior to treatment, alongside the difference vis-à-vis the treatment group (mean in treatment group minus mean in control group). We also report statistical significance levels of the difference in treatment-control means. These tests are presented for both the full baseline sample and the loan recipient sample.

Overall, we find balance between the two groups in both the full baseline sample and the loan recipient sample. In the full baseline sample, the difference in means for the treatment and control groups is not significant for any of the 11 baseline variables. In the loan recipient sample, for 10 out of these 11 baseline variables, the difference in means between treatment and control groups is not statistically significantly different from zero at conventional levels, and so we cannot reject the hypothesis that the means are identical across treatment groups. For only one variable, the indicator for the study participant being male, is the difference statistically significant (at the 10% level): the fraction male in the treatment group is 6.6 percentage points lower than in the control group.[18]

3. A simple model of borrower behavior

To study how dynamic incentives affect borrower behavior, we develop a simple model of risk-neutral agents that incorporates both moral hazard and adverse selection. By virtue of the experiment, the credit contract is kept fixed, so our goal here is not to solve for the optimal contract in the presence of both information asymmetries (Gesnerie, Picard and Rey, 1988 or Chassagnon and Chiappori, 1997 for risk averse agents), but rather to derive the agents' optimal behavior with and without dynamic incentives.

Agents (or farmers) decide how much to borrow for cash crop inputs and how much to invest. We assume that they do not have collateral or liquid assets, so the maximum they can invest in cash crop production is the loan amount.

We introduce the possibility of adverse selection by allowing farmers to differ in the probability p that cash crop production is successful. Production is given by $f_S(b)$ when successful and by $f_F(b)$ when it fails, which happens with probability $1-p$. The amount b denotes total cash crop inputs invested. We assume that $f_j(b)$, $j \in \{F,S\}$ satisfies the usual properties $f_j(0)=0$, $f_j'(b)>0$ and $f_j''(b)<0$.

[18] It will turn out, however, that the regression results to come are not substantially affected by the inclusion or exclusion in the regressions of a large set of control variables (including the "male" indicator).

We model moral hazard by allowing borrowers to divert inputs instead of investing them in cash crop production. If they decide to divert, they earn q per unit of input diverted, which can be interpreted as the secondary market price for inputs or the expected return if these inputs are invested in another crop. Given the arrangement to buy the cash crop (paprika) in the experiment, we assume that the lender can only seize cash crop production but not the proceeds from diverted inputs. To simplify matters, we assume that the choice of diversion is binary, that is, either all or nothing is diverted.[19]

We consider first the case where identification of clients is not possible, so borrowers can obtain a fresh loan even if they have defaulted in the past by simply using a different identity. Lenders cannot use dynamic incentives and are thus forced to offer the same one season contract every period, as they cannot tailor the terms of the contract to individual credit histories. Though in practice loan officers may recognize clients by sight, loan officers may resign or be transferred and so the new loan officer will not know the clients. Even if loan officers remain on the job, clients could borrow from a different branch or from a different lender altogether.

We then consider the case with biometric technology which provides the lender the ability to use dynamic incentives by denying credit to past defaulters. In this situation, borrowers face a tradeoff between diverting inputs away from cash crop production but jeopardizing chances of a loan in the future versus ensuring repayment of the current loan and therefore securing a loan in the future.

In both cases, the credit contract offered by the lender is given by a loan amount b and gross interest rate R. We assume that the loan size b can take on two values, b_L and b_H where $b_L < b_H$.[20] We also assume that even when cash crop production fails, the borrower has enough funds to cover loan repayment provided that the small amount b_L is borrowed and inputs are not diverted. More formally, $f_F(b_L) = b_L R$. This implies that if the borrower chooses to invest the large amount b_H in paprika production but the crop fails, then the borrower defaults because by concavity of $f_F(\cdot)$, $f_F(b_H) < b_H R$. Finally, we assume that if the crop succeeds, the large loan size yields higher farm profits than the smaller loan size. If we let $y_S(b_k) = f_S(b_k) - b_k R$, for $k \in \{L, H\}$ denote net profits from successful cash crop production, this assumption can be expressed as $y_S(b_H) > y_S(b_L)$.[21]

We assume that there are two periods and no discounting, although the model could easily be extended to an infinite horizon setting with discounting. The timing

[19] One can extend the model to the case where diversion is a continuous variable but the intuition is already captured in the simpler version presented.

[20] This assumption is in accord with the actual details of the loan package, where the most important determinant of loan size is whether the farmer chooses to have the loan fund one vs. two bags of CAN fertilizer. We can think of b_H including two bags, and b_L only one.

[21] Using similar notation, the previous assumption implies that when the crop fails, farm profits are larger under the smaller loan size: $y_F(b_H) < y_F(b_L)$.

within a period follows the set-up of the field experiment: the borrower first learns whether the lender can use dynamic incentives; then the borrower decides how much to borrow and whether to divert inputs; then paprika production takes place; the loan is repaid if sufficient funds are available and finally the borrower consumes any remaining income.

In what follows, we take the credit contract as given and characterize optimal borrower behavior with and without dynamic incentives. Then we briefly discuss the optimality of the credit contract and compare the predictions of the model to those of other models in the literature.

Borrower behavior without dynamic incentives

Since the lender offers the same contract in each period, lifetime optimization coincides with period-by-period optimization. In a given period, the borrower chooses how much to borrow b and whether to divert inputs D by solving the following problem:

$$v(p) = \max_{b\in\{b_L, b_H\}} \left\{ \max_{D\in\{0,1\}} Dqb_H + (1-D)py_S(b_H), \max_{D\in\{0,1\}} Dqb_L + (1-D)py_S(b_L) \right\}$$

The dependency of net income from borrowing v on p is made explicit. If the borrower diverts, consumption is qb because the bank cannot seize it, but if the borrower invests in paprika production, consumption only takes place when production is successful as the bank seizes all output when paprika production fails.

Now let p_D be the success probability that leaves a borrower with the larger loan size b_H indifferent between diverting the inputs or investing them in paprika production. More formally, $qb_H = p_D y_S(b_H)$ as plotted in Figure 3.

If $p < p_D$, the solution to the problem when dynamic incentives are absent is to always borrow the large amount b_H and to divert all inputs ($D = 1$). If $p \geq p_D$, the borrower also borrows the large amount b_H but does not divert and therefore repays with probability p. Expected net income in a period $v(p)$ is

$$v(p) = qb_H \text{ if } p < p_D \text{ and } v(p) = py_S(b_H) \text{ if } p \geq p_D. \tag{1}$$

Borrower behavior with dynamic incentives

In this case, the lender will only provide credit in period two to borrowers that have successfully repaid in period one. Because there are only two periods, in the last period the lender cannot provide additional incentives to elicit repayment, so the optimization problem that borrowers face is the same as the period-by-period optimization when dynamic incentives were absent. Borrowers maximize their lifetime utility by solving the following problem in period one:

$$V(p) = \max_{b\in\{b_L, b_H\}} \left\{ \max_{D\in\{0,1\}} Dqb_H + (1-D)p\left[y_S(b_H) + v(p)\right], \max_{D\in\{0,1\}} Dqb_L + (1-D)py_S(b_L) + v(p) \right\}$$

where again the dependency of V and v on p is made explicit. Net income v in period two is derived in (1). If the lower amount b_L is chosen, the borrower can always repay the loan and so net income from borrowing $v(p)$ in period two is assured. If, on the other hand, the higher amount b_H is chosen, then the borrower will obtain $v(p)$ in period two only if there is no diversion ($D = 0$) and paprika production is successful in period one. Income from not borrowing is normalized to zero.

With dynamic incentives, diversion of inputs in the first period is never optimal. A borrower with a high probability of success $p \geq p_D$ would not divert in the absence of penalties, so he would certainly not do it when penalties are present. More formally, because $py_S(b_H) > qb_H$ if $p \geq p_D$, it follows that $p[y_S(b_H) + v(p)] > qb_H$ since $v(p) > 0$.

When $p < p_D$, borrowers choose to divert in the absence of dynamic incentives. When dynamic incentives are in place, they can increase lifetime utility by choosing the lower amount in the first period. They then secure a loan in the second period which can then be diverted to achieve the same utility as if they had diverted in the first period. In addition, if cash crop production succeeds, then they also consume in the first period.[22]

We now study the choice of loan amount in the first period. Let p_{B0} be the probability of success that leaves a borrower with success probability $p \geq p_D$ indifferent between the two loan amounts. If success probability is such that $p_D < p < p_{B0}$, then the borrower chooses b_L to ensure loan repayment, but if the probability is high enough, so that $p_D < p_{B0} < p$ he then chooses b_H. The subscript 0 denotes the fact that in the absence of dynamic incentives the borrower would not divert because $p \geq p_D$. Probability p_{B0} can be written as

$$p_{B0} = \frac{y_S(b_L)}{y_S(b_H)}. \qquad (2)$$

Now let p_{B1} be analogous to p_{B0} for borrowers with success probability $p < p_D$. Here the subscript 1 indicates that the borrower would divert in the absence of dynamic incentives. If success probability satisfies $p < p_{B1} < p_D$, the borrower will choose the smaller loan amount b_L and if $p_{B1} < p < p_D$ the larger amount b_H. It is easy to show that p_{B1} satisfies

$$qb_H(1 - p_{B1}) = p_{B1}[y_S(b_H) - y_S(b_L)] \qquad (3)$$

or, after some algebra and substitutions,

$$p_{B1} = \frac{p_D}{p_D + 1 - p_{B0}}. \qquad (4)$$

[22] While this result is immediate without discounting, it can be obtained with discounting provided the discount rate is high enough.

As it turns out, depending on the magnitude of $y_S(b_L)$, $y_S(b_H)$ and qb_H only $p_D > p_{B1}$ or $p_D < p_{B0}$ will hold, because $p_D > p_{B1}$ is true if and only if $p_D > p_{B0}$.[23] So either p_{B0} or p_{B1} is relevant. There are three cases, which we label (i), (ii), and (iii), distinguished by the size of the gains from input diversion (qb_H) relative to those from successful cash crop production, $y_S(b_H)$ and $y_S(b_L)$.

The first case is where (i) $qb_H > y_S(b_H)$, in which the gains from diversion are higher than the gains from cash crop production even when the high loan amount is taken and production is successful. In this case, $p_D > 1 > p_{B1} > p_{B0}$ and p_{B0} becomes irrelevant because $p_D < p_{B0}$ is violated. Intuitively, $p_D > 1$ means that there are no borrowers who would repay without dynamic incentives, because the gains from diversion are higher than the gains from cash crop production even for borrowers with the highest success probabilities; p_{B0} is irrelevant because there are no farmers for whom $p > p_D$. In the first period with dynamic incentives, borrowers with $p \geq p_{B1}$ take the larger loan and those for whom $p < p_{B1}$ take the smaller loan size.

The second – and probably most interesting – case is where (ii) $y_S(b_H) > qb_H > y_S(b_L)$, in which the gains from diversion (relative to cash crop production) are intermediate. In this case, in the absence of dynamic incentives, some borrowers (those with highest success probabilities, for whom $p > p_D$) will choose to produce rather than divert, which others with lower success probabilities will divert rather than produce. In this case we have $1 > p_D > p_{B1} > p_{B0}$,[24] and so p_{B0} is irrelevant (those with $p > p_D$ always choose the larger loan in the first period). In the first period with dynamic incentives, borrowers with $p \geq p_{B1}$ take the larger loan and those for whom $p < p_{B1}$ take the smaller loan size.

The third case is where (iii) $y_S(b_L) > qb_H$, in which the gains from diversion are small relative to the gains from successful cash crop production, even when the small loan size is taken. Here, $1 > p_{B0} > p_{B1} > p_D$ so that p_{B1} now becomes irrelevant (because all individuals with $p < p_D$ will take the smaller loan size in the first period with dynamic incentives). Now it is those borrowers for whom $p > p_D$ that show variation in loan size in the first period with dynamic incentives: those with $p \geq p_{B0}$ take the larger loan and those for whom $p < p_{B0}$ take the smaller loan size.

[23] This is easy to see using the expression for p_{B1} derived in (4).

[24] To see this, divide inequalities in (ii) by $y_S(b_H)$ and recall $qb_H = p_D y_S(b_H)$ and expression (4).

Figure 3 is drawn assuming Case (ii) holds. It also plots p_{B0} and p_{B1}, and because $p_D > p_{B0}$, p_{B0} is irrelevant. Probability p_{B1} is shown as the intersection of the left hand side and right hand side of the equality in (3) above.

For each regime (with and without dynamic incentives), Table 3 reports the first period optimal choices of loan size and whether to divert as well as repayment rate as a function of the borrowers' success probability.

Interestingly, dynamic incentives have different effects on the optimal choices of borrowers depending on their probability of success. For example, borrowers with relatively low probability of success are most affected by the introduction of dynamic incentives. They choose the higher loan amount and to divert it all without dynamic incentives but borrow the lower amount and invest it in cash crop production when dynamic incentives are introduced. As a result, their repayment rate changes from zero to one once incentives are introduced.

Borrowers with relatively high probability of success are the least affected, since they never divert inputs and always choose the higher loan amount, except for in Case (i) where they would divert without incentives and not divert with incentives.

Borrowers with an intermediate value of the probability of success will, upon introduction of dynamic incentives, change either the diversion or the loan size decisions depending on the parameter values and functional forms. In Case (ii) they always choose the higher loan amount but move from diversion to no diversion when incentives are introduced. In Case (iii), they never divert but incentives lead them to move from the higher to the lower loan amount.

Discussion

If the lender sets gross interest rate R to break even, and the individual probability of success $p \in [0,1]$ is drawn from the density function $G(p)$, then R satisfies

$$ib_H = [1 - G(p_D)][E(p \mid p \geq p_D) R b_H + (1 - E(p \mid p \geq p_D)) f_F(b_H)], \qquad (5)$$

where i is the deposit rate and $E(p \mid p \geq p_D) = \int_{p_D}^{1} p dG(p)$.

Notice that the bank breaks-even whenever $p_D < 1$, otherwise all borrowers would divert and the bank would be unable to collect repayment. As a result, there is no interest rate R such that case (i) considered before is an equilibrium.

Depending on the parameters, a separating equilibrium may exist where the lender maximizes borrower welfare subject to breaking even by offering a menu of loan sizes and gross interest rates. Borrowers with low probability of success p may either borrow the large amount and default or borrow the lower amount and produce (again depending on the parameters), borrowers with intermediate probability of success will borrow the lower amount and produce and borrowers with high probability of success will borrow the large amount and produce.

When dynamic incentives are introduced, the lender can follow a strategy similar to Stiglitz and Weiss (1983) or Boot and Thakor (1994). In words, the lender could lower the interest rate associated with the lower loan size b_L in the second period below the per period break even interest rate (thereby making a loss) but raise it in the first period so as to satisfy the break even constraint intertemporally. This may be optimal because in the first period the borrower has the added incentive of the promise of a loan in the future, a loan that will be ever more attractive the lower is the interest rate charged.

If collateral was available, then a menu of interest rates and collateral could always be offered in both periods (Bester, 1985). But as Boot and Thakor (1994) point out, dynamic incentives can be more efficient than static incentives like collateral. As in their model, the value of long-term contracting does not arise from the ability to learn the borrower type (in their model all agents are equal) nor from improved risk-sharing (in both models agents are risk neutral). Long term relations are valuable because the lender has the ability to punish defaulters and to reward good borrowers.

Because repayment is higher with dynamic incentives, lenders could lower the interest rate and as a result borrowers might borrow more. The lender should also be willing to extend more credit if dynamic incentives can be used. As a result, overall borrowing could increase, although borrowers with low probability of success may still borrow less to ensure future access to loans. This increase in borrowing is also predicted by the more macro literature that tries to explain the increase in personal bankruptcies over the last few decades as a result of improvements in information technology available to lenders for credit decisions (see for example Livshits, McGee and Tertilt, forthcoming and 2009; Narajabad, 2010 and Sanchez, 2009).

In many multi-period models of limited commitment and asymmetric information, agents are not allowed to save because they could borrow and default and then live in autarky from reinvesting the savings (Bulow and Rogoff, 1989). In Boot and Thakor (1994), the agent has no incentive to save because the long-term contract provides better-than-market interest rates. In this model without dynamic incentives, agents with high probability of success will not find it profitable to default and save for period 2 either, even if a savings technology were available at rate i. But if the probability is low enough, in particular if p is such that

$$p < \frac{(i-1)qb_H}{y_S(b_L)},$$

then agents would borrow the higher amount b_H in period one, divert and hence default and save it into period 2 to earn $i>1$. When dynamic incentives are allowed, then the same argument of Boot and Thakor (1994) applies and so agents would prefer to borrow again in the second period, even if savings technology were available.

4. Regression Specification

Because the treatment is assigned randomly at the club level, its impact on the various outcomes of interest (say, repayment) can be estimated via the following regression equation:

(1) $Y_{ij} = \alpha + \beta B_j + \gamma X_{ij} + \varepsilon_{ij},$

where Y_{ij} = repayment outcome for individual i in club j (e.g., equal to 1 if repaying in full and on time, and 0 otherwise), B_j is biometric identification (1 if fingerprinted and 0 if not), and X_{ij} is a vector of club and individual farmer characteristics collected at baseline. ε_{ij} is a mean-zero error term. Treatment assignment at the club level creates spatial correlation among farmers within the same club, so standard errors must be clustered at the club level (Moulton 1986). Inclusion of the vector X_{ij} of baseline characteristics can reduce standard errors by absorbing residual variation. In our case, we include the baseline characteristics reported in Table 1, as well as indicators for the two stratification variables (locality/EPA fixed effects and week of loan offer fixed effects) and all interactions between the dummy variables for locality and week of loan offer.

The coefficient β on the biometric treatment status indicator is the impact of being fingerprinted on the dependent variable of interest.

We also examine the interactions between the randomized treatment and a particular baseline characteristic: a measure of the ex-ante probability of repayment. Examining this dimension of heterogeneity is a test of the theoretical model's prediction that the impact of dynamic incentives on repayment is negatively related with the ex-ante repayment rate (what the repayment rate would have been in the absence of dynamic incentives): borrowers who, without the dynamic incentive, would have had lower repayment will see their repayment rates rise more when the dynamic incentive is introduced.[25] To test this question, we estimate regression equations of the following form:

(2) $Y_{ij} = \alpha + \rho(B_j * D_{ij}) + \beta B_j + \chi X_{ij} + \varepsilon_{ij},$

D_{ij} is a variable representing the individual's predicted likelihood of repayment (its main effect is included in the vector X_{ij}). The coefficient ρ on the interaction term B_j *

[25] While in the model the single dimension of borrower heterogeneity is the probability of success, p, we have no way to estimate this directly for our full borrowing sample. Note that the repayment rate is monotonic in p, making it a good proxy for p. While in principle one could apply the procedure in Appendix D with crop output as the dependent variable, in practice this would limit us because crop output is only observed in the smaller subsample of borrowers (N=520). The repayment rate, on the other hand, comes from administrative data and so is available for the entire borrowing sample.

D_{ij} reveals the extent to which the impact of biometric identification's on repayment varies according to the borrower's predicted repayment.

To implement equation (2) examining heterogeneity in the effect of fingerprinting, we construct an index of predicted repayment. This involves creating what is essentially a "credit score" for each borrower in the sample on the basis of the relationship between baseline characteristics and repayment in the control (non-fingerprinted) group. (See Appendix D for details on the construction of the predicted repayment variable. Appendix Table 1 presents the auxiliary regression results used in construction of the predicted repayment variable.) This index is either interacted linearly with the treatment indicator, or it is converted into indicators for quintiles of the distribution of predicted repayment in the absence of fingerprinting and then interacted with the treatment indicator.[26] In all regression results where the treatment indicator is interacted with predicted repayment, we report bootstrapped standard errors because the predicted repayment variable is a generated regressor.[27]

5. Empirical Results: Impacts of Fingerprinting

This section presents our experimental evidence on the impacts of fingerprinting on a variety of inter-related outcomes. We examine impacts on loan approval and borrowing decisions, on repayment outcomes, and on intermediate farmer actions and outcomes that may ultimately affect repayment.

Tables 4 through 8 will present regression results from estimation of equations (1) and (2) in a similar format. In each table, each column will present regression results for a given dependent variable. Panel A will present the coefficient on treatment (fingerprint) status from estimation of equation (1).

Then, to examine heterogeneity in the effect of fingerprinting, Panels B and C will present results from estimation of versions of equation (2) where fingerprinting is interacted linearly with predicted repayment (Panel B) or with dummy variables for quintiles of predicted repayment (Panel C). In both Panels B and C the respective main effects of the predicted repayment variables are also included in the regression (but for brevity the coefficients on the predicted repayment main effects will not be presented). In Panel C, the main effect of fingerprinting is not included in the regression, to allow each of the five quintile indicators to be interacted with the indicator for fingerprinting in the regression. Therefore, in Panel C the coefficient on each fingerprint-quintile interaction

[26] In other results that are analogous to the analysis of Table 2 (available from authors on request), we show that there is balance in key baseline characteristics across treatment and control observations within each quintile of predicted repayment.

[27] We calculate standard errors for regressions in the form of equation (2) from 200 bootstrap replications. In each replication, we re-sample borrowing clubs from our original data (which preserves the original club-level clustering), compute predicted repayment based on the new sample, and re-run the regression in question using the new value of predicted repayment for that replication.

should be interpreted as the impact of fingerprinting on borrowers in that quintile, compared to control group borrowers in that same quintile.

Finally, in Tables 4 through 8 the mean of the dependent variable in a given column, for the overall sample as well for each quintile of predicted repayment separately, are reported at the bottom of each table.

A. Loan approval, take-up, and amount borrowed

The first key question to ask is whether fingerprinted farmers were more likely to have their loans approved by the lender, or were more likely to take out loans, compared to the control group. This question is important because the degree of selectivity in the borrower pool induced by fingerprinting status affects interpretation of any effects on repayment and other outcomes.

Although loan officers were told which clubs had been fingerprinted in September 2007 when loan applications were due, they do not appear to have used this information in their loan approval decisions. Since biometric technology can be seen as a substitute for loan officer effort, one would expect loan officers to have better knowledge about non-fingerprinted clubs. However, this is not what we find.

Appendix Table 2 combines the reports from all loan officers collected in August 2008 as well as borrower responses in the August 2008 follow-up survey. Loan officers were first asked about the specific treatment status of five clubs randomly selected from the sample of clubs for which they were responsible. They were then asked whether they knew the secretary or president of the club and finally they were asked to estimate the number of loans given out in each club. The first row of the table shows that loan officers had very little knowledge about the actual treatment status of clubs. Only 54 percent of the fingerprinted clubs are reported correctly as being fingerprinted and an even lower 22 percent of non-fingerprinted clubs are reported correctly as such. Pure guesswork would yield an accuracy rate of 50 percent. This evidence alone suggests that loan officers did not take into account treatment status in their interactions with the clubs.

Loan officers know club officers roughly half of the time, and on average misreport the number of loans disbursed to a club by 1.5 loans. More importantly, there are no statistical differences in the reporting accuracy of fingerprinted clubs compared to non-fingerprinted ones.

Borrower reports in the last three rows of the table paint a similar picture. Loan officers are no more likely to visit non-fingerprinted clubs to collect repayment compared to fingerprinted clubs, and as a result, members of non-fingerprinted clubs report talking the same number of times to loan officers as do members of fingerprinted clubs. Finally, they all report finding it relatively easy to contact the loan officer.

The evidence in the table indicates that loan officers did not respond to the treatment. Therefore, any impacts of the treatment should be interpreted as emerging solely from borrowers' responses to being fingerprinted.

Because loan officers did not take treatment status into account, it is not surprising that fingerprinting had no effect on loan approval. We also find no effect on loan-take-up by borrowers, perhaps because clubs were formed with the expectation of credit availability and fingerprinting did not act as a strong enough deterrent to borrowing to affect farmers' decisions at the extensive margin. Columns 1 and 2 of Table 4 present results from estimation of equations (1) and (2) for the full baseline sample where the dependent variables are, respectively, an indicator for the lender's approving the loan for the given farmer (mean 0.63), and an indicator for the farmer ultimately taking out the loan (mean 0.35).[28]

There is no evidence that the rate of loan approval or take-up differs substantially across the treatment and control groups on average: the coefficient on fingerprinting is not statistically different from zero in either columns 1 or 2, Panel A.

There is also no indication of selectivity in the resulting borrowing pool across subgroups of borrowers with different levels of predicted repayment. The coefficient on the interaction of fingerprinting with predicted repayment is not statistically significantly different from zero in either columns 1 or 2 of Panel B. When looking at interactions with quintiles of predicted repayment (Panel C), while the fingerprint-quintile 2 interaction is positive and significantly different from zero at the 10% level in the loan approval regression, none of the interaction terms with fingerprinting are significantly different from zero in the loan take-up regression.

While there is no indication that the pool of ultimate borrowers was itself substantially affected by fingerprinting, it does appear that – conditional on borrowing – fingerprinted borrowers took out smaller loans. In Column 3 of Table 4, the dependent variable is the total amount borrowed in Malawi kwacha. Panel A indicates that loans of fingerprinted borrowers were MK 697 smaller than loans in the control group on average, a difference that is significant at the 10% level.

Inspecting the coefficients on the interactions of fingerprinting with predicted repayment, it appears that this effect is confined exclusively to borrowers in the lowest quintile of expected repayment. Differences between fingerprinted and non fingerprinted borrowers are small and not significant in quintiles two through four, but in quintile one, where fingerprinted borrowers take out loans that are smaller by MK 2,722 (roughly US$19) than those in the corresponding quintile in the control group, the difference is marginally significant (the t-statistic is 1.63).

This result is in accord with the theoretical model's prediction that the "bad" borrowers (those whose repayment rates would be lowest in the absence of dynamic incentives) will respond to the imposition of a dynamic incentive by voluntarily reducing their loan sizes. We view this result – voluntarily lower borrowing amounts on the part of

[28] Not all farmers who were approved for the loan ended up taking out the loan. Anecdotal evidence indicates that a substantial fraction of non-take-up among approved borrowers resulted when borrowers failed to raise the required deposit (amounting to 15% of the loan amount).

fingerprinted borrowers in the lowest quintile – as evidence that fingerprinting reduces adverse selection in the credit market, albeit on a different margin than is usually discussed in the credit context.

The existing literature tends to emphasize that improved enforcement should lead low-quality borrowers to be excluded from borrowing entirely – in other words, the improvement of the borrower pool operates on the *extensive* margin of borrowing. Our result here that low-quality borrowers (those in the lowest quintile of predicted repayment) voluntarily take out smaller loans leads the overall loan pool in money terms to be less weighted towards the low-quality borrowers, but in this case the improvement in the borrowing pool operates on the *intensive* margin of borrowing, rather than the extensive margin.

Interpretation of subsequent differences in the repayment rates (discussed below) should keep this result in mind. Improvements in repayment among fingerprinted borrowers (particularly among those in the lowest quintile) may in part result from their decisions to take out smaller loans at the very outset of the lending process and improve their eventual likelihood of repayment.

B. Loan repayment

How did fingerprinting affect ultimate loan repayment? Columns 1-3 of Table 5 present estimated effects of fingerprinting for the loan recipient sample on three outcomes: outstanding balance (in Malawi kwacha), fraction of loan paid , and an indicator for whether the loan is fully paid, all by September 30, 2008 (the official due date of the loan, after which the loan is officially past due). The next three columns (columns 4-6) are similar, but the three variables refer to “eventual” repayment as of the end of November 2008. The lender makes no attempt to collect past-due loans after November of each agricultural loan cycle, so the eventual repayment variables represent the final repayment status on these loans.

Results for all loan repayment outcomes are similar: fingerprinting improves loan repayment, in particular for borrowers expected ex ante to have poorer repayment performance. Coefficients in Panel A indicate that fingerprinted borrowers have lower outstanding balances, higher fractions paid, and are more likely to be fully paid on-time as well as eventually (and the coefficient in the regression for fraction paid on-time is statistically significant at the 10% level).

In Panel B, the fingerprinting-predicted repayment interaction term is statistically significantly different from zero (at least at the 5% level) in all regressions. The effect of fingerprinting on repayment is larger the lower is the borrower’s ex ante likelihood of repayment. In Panel C, it is evident that the effect of fingerprinting is isolated in the lowest quintile of expected repayment, with coefficients on the fingerprint-quintile 1 interaction all being statistically significantly different from zero at the 5% or 1% level and indicating beneficial effects of fingerprinting on repayment (lower outstanding balances, higher fraction paid, and higher likelihood of full repayment). Coefficients on other fingerprint-quintile interactions are all smaller in magnitude and not statistically significantly different from zero (with the exception of the negative coefficient on the fingerprint-quintile 5 interaction for fraction paid, which is odd and may simply be due to sampling variation).

The magnitudes of the repayment effect found for the lowest predicted-repayment quintile are large. The MK7,202.65 effect on eventual outstanding balance amounts to 40% of the average loan size for borrowers in the lowest predicted-repayment quintile. While outstanding balance should mechanically be lower due to the lower loan size in the lowest predicted-repayment quintile, the effect is almost three times the size of the reduction in loan size, so by itself lower loan size cannot explain the treatment effect on repayment. The 31.7 percentage point increase in eventual fraction paid and the 39.6 percentage point increase in the likelihood of being eventually fully paid are also large relative to bottom quintile percentages of 81% and 68% respectively.

C. Intermediate outcomes that may affect repayment

In this section we examine decisions that farmers make throughout the planting and harvest season that may contribute to higher repayment among fingerprinted farmers. The dependent variables in the remaining results tables (Tables 6-8) are available from a smaller subset of loan recipients (N=520) who were successfully interviewed in the August 2008 follow-up survey round. To help rule out the possibility that selection into the 520-observation August 2008 follow-up survey sample might bias the regression results for that sample, Column 2 of Appendix Table 3 examines selection of loan recipients into the follow-up survey sample. The regressions are analogous in structure to those in the main results tables (Panels A, B, and C), and the dependent variable is a dummy variable for attrition from the baseline (September 2007) to the August 2008 survey. There is no evidence of selective attrition related to treatment status: in no case is fingerprinting or fingerprinting interacted with predicted repayment statistically significantly associated with attrition from the survey.

Appendix Table 4 presents regression results for repayment outcomes that are analogous to those in Table 5, but where the sample is restricted to this 520-observation sample. The results confirm that the repayment results in the 520-observation sample are very similar to those in the overall loan recipient sample, in terms of both magnitudes of effects and statistical significance levels.

Land area allocated to various crops

One of the first decisions that farmers make in any planting season (which typically starts in November and December) is the proportion of land allocated to different crops. Table 6 examines the average and heterogeneous impact of fingerprinting on land allocation; the dependent variables across columns are fraction of land used in maize (column 1), 7 cash crops (columns 2-8), and all cash crops combined (column 9).[29]

Why might land allocation to different crops respond to fingerprinting? As discussed in the context of the theoretical model, non-production of paprika is a form of moral hazard, since the lender can only feasibly seize paprika output (in collaboration with the paprika buyer, Cheetah Paprika) and not other types of crop output. By not producing paprika (or producing less), the borrower is better able to avoid repayment on the loan. Therefore, by improving the lender's dynamic incentives, fingerprinting may discourage such diversion of inputs and land to other crops, as farmers face increased incentives to generate cash profits that are sufficient for loan repayment.

While none of the effects of fingerprinting in Table 6 (either overall in Panel A or in interaction with predicted repayment in Panels B and C) are statistically significant at conventional levels, there is suggestive evidence that there is an impact of fingerprinting on land allocation for borrowers in the first predicted-repayment quintile. In this group, the effect of fingerprinting on land allocated to paprika (column 5, first row of Panel C) is marginally significant (with a t-statistic of 1.63) and positive, indicating that fingerprinting leads farmers to allocate 8.3 percentage points more land to paprika. This effect is roughly half the size of the paprika land allocation in the lowest quintile of predicted repayment.

It is worth considering that the effect on land allocated to paprika may be smaller than it might be otherwise because farmers began preparing and allocating land earlier in the agricultural season than our treatment. If land is less easily reallocated than other inputs from one crop to another, then we would anticipate smaller short run effects on land allocation than on the use of inputs such as fertilizer and chemicals (to which we now turn). In the long run, when farmers incorporate the additional cost of default due to fingerprinting into their agricultural planning earlier in the season, we might find larger impacts on land allocation.

Inputs used on paprika

After allocating land to different crops, the other major farming decision made by farmers is input application. Non-application of inputs on the paprika crop facilitates default on the loan and is therefore another form of moral hazard, again since only paprika output can feasibly be seized by the lender.

It is worth keeping in mind that input application takes place later in the agricultural cycle than land allocation, and agricultural inputs are more fungible than land. Also, inputs are added multiple times throughout the season, so farmers can incorporate new information about the cost of default into their use of inputs but cannot change land allocation after planting. Thus, we may expect use of inputs to respond more quickly to the introduction of fingerprinting than would allocation of land.

[29] For each farmer, the value of the variables across columns 1-8 add up to 1.

Table 7 examines the effect of fingerprinting on the use of inputs on the paprika crop. The dependent variables in the first 5 columns (all denominated in Malawi kwacha) are applications of seeds, fertilizer, chemicals, man-days (hired labor), and all inputs together. Columns 6 and 7 look at, respectively, manure application (denominated in kilograms because this input is typically produced at home and not purchased) and the number of times farmers weeded the paprika plot. We view the manure and weeding dependent variables as more purely capturing labor effort exerted on the paprika crop, while the other dependent variables capture both labor effort and financial resources expended.

The results for paid inputs (columns 1-5) indicate that – particularly for farmers with lower likelihood of repayment – fingerprinting leads to higher application of inputs on the paprika crop. In Panel B, the coefficients on the fingerprint-predicted repayment interaction are all negative in sign, and the effects on the use of fertilizer and paid inputs in aggregate are statistically significantly different from zero. In Panel C, the coefficient on the fingerprint-quintile 1 interaction is positive and significantly different from zero at the 5% confidence level for spending on seeds and is marginally significant for spending on fertilizer (t-statistic 1.44) and for all paid inputs (t-statistic 1.55). The negative and significant impact on use of paid labor in the fourth quintile is puzzling and may be attributable to sampling variation.

Results for inputs not purchased in the market are either nonexistent or ambiguous. No coefficient is statistically significantly different from zero in the regressions for manure (column 6) or times weeding (column 7).

It is worth asking whether the impact of fingerprinting seen in Table 7 means that farmers are less likely to divert input to use on other crops, or, alternatively, less likely to sell or barter the inputs for their market value. To address this, we examined the impact of fingerprinting on use of inputs on all crops combined. Results were very similar to Table 7's results for input use on the paprika crop only (results are available from the authors on request). This suggests that in the absence of fingerprinting, inputs were not used on other non-paprika crops. (If fingerprinting simply led inputs to be substituted away from non-paprika crops to paprika, the estimated impact of fingerprinting on input use on all crops would be zero.) It therefore seems most likely that fingerprinting made farmers less likely to dispose of the inputs via sale or barter.

In sum: for borrowers with a lower likelihood of repayment, fingerprinting leads to increased use of marketable inputs in growing paprika. While this effect is at best only marginally significant for borrowers in the lowest predicted repayment quintile, the magnitudes in that quintile are substantial. For the lowest predicted-repayment subgroup, fingerprinted farmers used MK6,540 more paid inputs in total, which is substantial compared to the mean in the lowest predicted-repayment subgroup of MK7,440.

Farm profits

Given these effects of fingerprinting on intermediate farming decisions such as land allocation and input use, what is the effect on agricultural revenue and profits? Columns 1-3 of Table 8 present regression results where the dependent variables are market crop sales, the value of unsold crops, and profits (market sales plus value of unsold crops minus value of inputs used), all denominated in Malawi kwacha. The magnitudes of the overall impacts of fingerprinting on value of sales, unsold harvest, and total profits (Panel A), and in the bottom two quintiles (Panel C) are large and positive, but the effects are imprecisely estimated and none are statistically significantly different from zero.

To help deal with the problem of outliers in the profit figures, column 4 presents regression results where the dependent variable is the natural log of agricultural profits.[30] The effect of fingerprinting in the bottom quintile of predicted repayment is positive but not statistically significant (t-statistic 1.11).

In sum, then, it remains possible that increased use of paid inputs led ultimately to higher revenue and profits among fingerprinted farmers in our sample, but the imprecision of the estimates prevents us from making strong statements about the impact of fingerprinting on farm profits.

6. Discussion and additional analyses

In sum, the results indicate that for the lowest predicted-repayment quintile, fingerprinting leads to substantially higher loan repayment. In seeking explanations for this result, we have provided evidence that for this subgroup fingerprinting leads farmers to take out smaller loans, devote more land to paprika, and apply more inputs on paprika.

We view these results so far as indicating that – for the farmers with the lowest ex ante likelihood of repaying their loans – fingerprinting leads to reductions in adverse selection and ex-ante moral hazard. The reduction in adverse selection (a reduction in the riskiness of the loan pool) comes about not via the extensive margin of loan approval and take-up, but through farmers' decisions to take out smaller loans if they are fingerprinted (the intensive margin of loan take-up).

Ex-ante moral hazard is the problem that borrower behavior that is unobserved to the lender may be detrimental for repayment. We interpret changes in intermediate outcomes and behaviors – such as increased land use and input application for paprika – as reductions in ex-ante moral hazard. We believe that the most likely scenario is that in the absence of fingerprinting, borrowers in the lowest predicted-repayment subgroup were not using the paprika inputs received as part of the loan for paprika production. Rather, they are most likely to have sold them in the market or bartered them away. Then

[30] For seven (7) observations profits are zero or negative, and in these cases ln(profits) is replaced by 0. These observations are not driving the results, as results are essentially identical when simply excluding these 7 observations from the regression.

when such borrowers were fingerprinted, they became more likely to use the inputs as intended, expanding land allocated to paprika and using the inputs on that crop as the loan required. Below we provide a test of whether increased repayment as a result of fingerprinting in part reflects reductions in ex-post moral hazard.

Also, at the end of this section, we report results of a test of the positive correlation property that reveals the presence of asymmetric information (Chiappori and Salanié, 2003 and Chiappori, Jullien, Salanié and Salanié, 2006).

Evidence for a reduction in ex-post moral hazard

Reductions in ex-ante moral hazard may help encourage higher loan repayment by improving farm output so that farmers have higher incomes with which to make loan repayments. Reductions in adverse selection – reduced loan sizes for the "bad" borrowers – also help increase repayment performance. But a question that remains is whether any of the increase in repayment is due to reductions in *ex-post* moral hazard. In other words, are there reductions in strategic or opportunistic default by borrowers, holding constant loan size and farm profits?

We investigate this by running regressions where repayment outcomes are the dependent variables, but where we include as independent variables in the regression controls for agricultural profits and the total originally borrowed. Results are reported in Table 9.[31] The profits and total borrowed variables are flexibly specified as indicators for the borrower being in the 1st through 10th decile of the distribution of the variable (one indicator is excluded in each resulting group of 10 indicators, so there so there are 18 additional variables in each regression.)

We cannot reject the hypothesis that fingerprinting has no effect on eventual repayment (columns 4-6) once we control for agricultural profits and original loan size. Coefficient estimates that were previously statistically significant (in Appendix Table 4) are now uniformly smaller in magnitude and not statistically significantly different from zero. Indeed, the previously significant coefficients on the fingerprint * quintile 1 interaction across the columns are roughly cut in half. Results are similar for repayment by the due date (columns 1-3), with the exception of the regression for "Balance, Sept. 30" where the linear interaction term and the interaction term with quartile 1 of predicted repayment remain statistically significant at the 5% and 10% levels, respectively. Even in this latter cases, however, the coefficient magnitudes are reduced substantially vis-à-vis the corresponding estimates in Appendix Table 4.

All told, we view these results as providing no strong support for the idea that a reduction in ex-post moral hazard – increases in repayment even conditional on amount borrowed and agricultural profits – is also an important contributor to the increased

[31] We limit ourselves to the 520-observation sample because of the need to control for profits, which was only observed among those in the August 2008 survey. These results should therefore be compared with Appendix Table 4, which is also for the 520-observation sample.

repayment we observe among fingerprinted farmers in the lowest predicted-repayment quintile.

Test of the positive correlation property

Following several recent articles that use data from insurance markets to test for the presence of asymmetric information (Chiappori and Salanié, 2003; Chiappori, Jullien, Salanié and Salanié, 2006), the predictions of the theoretical model of Section 3 can be used to perform a similar test. In the insurance market context, many models of adverse selection and possibly moral hazard that assume competitive insurance markets predict a positive correlation between coverage and the probability of the event insured, conditioning on the information available to the insurer. In our context, the test involves a positive correlation between loan size and default.

In order to test this prediction, multiple loan contracts must coexist in equilibrium, but according to the model (see Table 3), all agents should borrow the high amount b_H when dynamic incentives cannot be used, and so there should be no correlation. With dynamic incentives however, both high and low loan sizes (b_L and b_H) will be taken and so the correlation can be tested. Using data on the loan size and default at maturity date, we find, as expected, no correlation for borrowers in the control group (t-stat = 1.13), but find a strong positive correlation in the treatment group (t-stat=3.30). In the treatment group, a MK1,000 increase in the loan amount is associated with a decrease in the probability of default (not being fully paid at the loan due date) of roughly 3 percentage points.

7. Benefit-cost analysis

The analysis so far has estimated the gains to the financial institution (MRFC) from using fingerprinting to identify new borrowers as part of the process of loan screening. These gains need to be weighed against the costs of fingerprinting. In this section, we present a benefit-cost analysis of biometric fingerprinting of borrowers. The analysis is most valid for institutions similar in characteristics to those of our partner institution, MRFC, but we have made the elements of the calculation very transparent so that they can be easily modified for other institutions with different characteristics.

The benefit-cost calculation is presented in Table 10. The uppermost section of the table is the calculation of benefits per individual fingerprinted. At the suggestion of MRFC, we assume that all new loan applicants are fingerprinted, and that 50% of applicants are approved for loans. Based on our experimental results we assume that the increase in repayment due to fingerprinting is confined to the first quintile (20% of borrowers), and that for this subgroup fingerprinting causes an increase in repayment amounting to 31.7% of the loan balance (from column 5 of Table 5). We assume that the total amount to be repaid is MK15,000 on average. Total benefit per individual fingerprinted is therefore MK475.50 (US$3.28).

The next section of the table calculates cost per individual fingerprinted. There are three general types of costs. First, equipment costs need to be amortized across farmers fingerprinted. We assume each equipment unit (a laptop computer and external fingerprint scanner) costs MK101,500,[32] and is amortized over three years, for annual cost of each equipment package of MK33,833. Twelve (12) of these equipment packages (two for each of six branches) will be required to fingerprint MRFC's borrowers throughout the country. With an estimated 5,000 new loan applicants per year, each of these equipment units will be used to fingerprint 417 farmers on average. The equipment cost per farmer fingerprinted is therefore MK81.20.

The second type of cost is loan officer time. We estimate that it takes 5 minutes to fingerprint a customer and enter his or her personal information into the database. At a salary of MK40,000 per month and 173.2 work hours per month, this comes out to a cost of MK19.25 per customer fingerprinted.

The third type of cost is the transaction cost per fingerprint checked, MK108.75 (US$0.75). We assume here that MRFC hires a private firm to provide the fingerprint identification services, in which case the fingerprint database is stored on the firm's server overseas and batches of fingerprints to be checked are sent electronically by MRFC to the firm during loan processing season. Lists of identified defaulters are sent back to MRFC with fast turnaround. In consultation with a U.S. private firm that provides such services, we were given a range of $0.03-$0.75 per fingerprint identification transaction. Per-fingerprint transaction costs are higher when the client has a relatively low number of transactions per year, and MRFC's 5,000 transactions per year is considered low, so we conservatively assume the transaction cost per fingerprint at the higher end of this range, $0.75 (MK108.75).

Summing up these three types of costs, total cost per individual fingerprinted is MK209.20. The net benefit per individual fingerprinted is therefore MK266.30 (US$1.84), and the benefit-cost ratio is an attractive 2.27. [33]

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[32] This is the actual cost of each equipment unit we purchased for the project, which included a laptop computer ($480), an extra laptop battery ($120), a laptop carrying case ($20), and an external fingerprint scanner ($80).

[33] An alternative is for a lending institution to purchase its own fingerprint matching software and do fingerprint identification in-house instead of subcontracting this function to an outside firm. This would eliminate the $0.75 (MK108.75) transaction cost per fingerprint checked. According to a U.S. fingerprint identification services firm we consulted, the initial fixed cost of installing an off-the-shelf fingerprint matching software system is in the range of $15,000 to $50,000 (depending on specifications), with an annual maintenance cost of 10-20% of the initial fixed cost. In addition, there would be personnel costs for staff to operate the system. Assuming an initial fixed cost of $15,000, maintenance cost of 10% of the original fixed cost, and an additional full-time staff member to run the system costing the same as a current MRFC loan officer, NPV is lower when fingerprint identification is done in-house than when this function is contracted out (which is why Table 9's calculation assumes contracting out). But with a high enough annual volume of transactions (perhaps in the context of a credit bureau in which many or all of Malawi's lenders participate), in-house fingerprint identification could make economic sense.

For several reasons, this benefit-cost calculation is likely to be quite conservative. First of all, under reasonable circumstances some of the individual costs could be brought down considerably. The cost for equipment units could fall substantially if a fingerprinting function were integrated into equipment packages that had multiple functionalities, such as the hand-held computers that MRFC is considering providing for all of its loan officers. Transaction costs for fingerprint checking could fall due to volume discounts if the lending institution banded together with other lenders to channel all their fingerprint identification through a single service provider (in the context of a credit bureau, for example).

In addition, there are other benefits to the lending institution that this benefit-cost calculation is not capturing. The impact of fingerprinting on loan repayment may become larger in magnitude over time as the lender's threat of enforcement becomes more credible. We have also assumed that all the benefits come from fingerprinting new loan customers (the subject of this experiment), but there may also be increases in repayment among existing customers who are fingerprinted (on which this experiment does not shed light). Finally, there may be broader benefits that are not captured by the lending institution, such as increased income due to more intensive input application by fingerprinted farmers.[34]

8. Conclusion

We conducted a field experiment where we randomly selected a subset of potential loan applicants to be fingerprinted, which improved the effectiveness of dynamic repayment incentives for these individuals. For all the recent empirical work on microcredit markets in developing countries, to our knowledge this is the first randomized field experiment of its kind, and the first to shed light (thanks to a detailed follow-up survey of borrowers) on the specific behaviors germane to the presence of asymmetric information problems.

Consistent with a simple model of asymmetric information in credit markets, we find heterogeneous effects of being fingerprinted, with the strongest effects among borrowers expected (ex ante) to have the worst repayment performance. Fingerprinting leads these "worst" borrowers to raise their repayment rates dramatically, partly as a result of voluntarily choosing lower loan sizes as well as devoting more agricultural inputs to the cash crop that the loan was intended to finance. The treatment-induced reduction in loan size represents a reduction in adverse selection, while the increased use of agricultural inputs on the cash crop represents a reduction in ex-ante moral hazard.

The short-term improvements in repayment estimated in this paper may indeed be smaller than the effects that would be found over a longer horizon. First of all, borrowers'

[34] Unfortunately, our estimates of the impact of fingerprinting on profits are too imprecise to say whether profits definitely increased due to this intervention.

assessments of the effectiveness of the technology and the credibility of the threat to withhold credit would likely rise over time as they gained further exposure to the system, observed that their past credit performance was being correctly retrieved by the lender, and saw that credit history information was indeed being shared with other lenders. In addition, the lender should be able to selectively allocate credit to the pool of good-performing borrowers over time, further improving overall repayment performance of the borrowing pool. Finally, because there is less risk involved for the lender, the credit contract terms could be made more attractive to borrowers, which may further improve repayment.

By revealing the presence of specific asymmetric information problems and the behaviors that result from them, this paper's findings can help guide future theoretical work on rural credit markets. To be specific, models of credit markets in contexts similar to rural Malawi should allow for adverse selection on the intensive margin of loan take-up (i.e., the choice of loan size), as well as ex-ante moral hazard (actions during the production season that may affect farm profits). On the other hand, our results suggest that it may be less important for models to incorporate ex-post moral hazard (strategic or opportunistic default), since we find no evidence of it in this context.

Our results also have implications for microlending practitioners, by quantifying the benefits from exploiting a commercially-available technology to raise repayment rates. Beyond improving the profitability and financial sustainability of microlenders, increased adoption of fingerprinting (or other identification technologies) can bring additional benefits if lenders are thereby encouraged to expand the supply of credit, and if this expansion of credit supply has positive effects on household well-being.[35] Credit expansions enabled by improved identification technology may be particularly large in previously underserved areas, such as the rural sub-Saharan context of our experiment, where problems with personal identification are particularly severe.

Another potential implication of this research is that in the absence of an alternative national identification system, fingerprints could serve as the unique identifier that allows individual credit histories to be stored and accessed in a cross-lender credit bureau. It has been noted that a key obstacle to establishment of credit bureaus is the lack of a unique identification system (Conning and Udry 2005, Fafchamps 2004, Mylenko 2007). Our results indicate that borrowers (particularly the worst borrowers) do perceive fingerprinting as an improvement in the lender's dynamic enforcement technology, and so support the use of fingerprints as an identifier in a national credit bureau.

As is the case with all field experiments, it is important to replicate this study in other contexts to gauge the external validity of the results. In addition to conducting similar studies in other rural sub-Saharan African contexts, it is also crucial to gauge the extent to which impacts of fingerprinting-enabled dynamic incentives are different in

[35] To be sure, however, this research does not shed any light on the impact of microcredit availability on household well-being.

urban areas or areas with greater access to microcredit, for example. As mentioned above, the effects of fingerprinting on repayment could very well rise over time, and so future studies should monitor effects beyond a single loan cycle. Future work should also make sure to examine responses by the lender, such as changes in the credit contract, approval rates or in loan officer monitoring. While in our case loan officers did not behave differently towards treated borrowers, in other contexts, perhaps under different loan officer incentives, this may not be the case. We view these and related questions as promising areas for future research.

Appendix A: Biometric training script

Benefits of Good Credit
Having a record of paying back your loans can help you get bigger loans or better interest rates.

Credit history works like trust. When you know someone for a long time, and that person is honest and fair when you deal with him, then you trust him. You are more likely to help him, and he is more likely to help you. You might let him use your hoe (or something else that is important to you), because you feel sure that he will give it back to you. Banks feel the same way about customers who have been honest and careful about paying back their loans. They trust those customers, and are more willing to let them borrow money.

MRFC already gives customers who have been good borrowers a reward. It charges them a lower interest rate, 30 percent instead of 33 percent. That means that for the loan we have described today, someone who has a good credit history would only have to pay back 8855, instead of 8971.[36]

Another way that banks might reward customers they trust is by letting them borrow bigger amounts of money. Instead of 7700 MK to grow one acre of paprika, MRFC might lend a trusted customer 15400, to grow two acres.

To earn trust with the bank, and get those rewards, you have to be able to prove to the bank that you have taken loans before and paid them back on time. You can do that by making sure that you give the bank accurate information when you fill out loan applications. But if you call yourself John Jacob Phiri one year, and Jacob John Phiri the next year, then the bank might not figure out that you are the same person, so they won't give you the rewards you have earned.

Costs of Bad Credit
But trust can be broken. If your neighbor borrows your radio and does not give it back or it gets ruined, then you probably wouldn't lend him anything else until the radio had been replaced.

Banks work the same way. If you take a loan and break the trust between yourself and the bank by not paying back the loan, then the bank won't lend to you again. This is especially true if you have a good harvest but still choose not to pay back the loan.

When you apply for a loan, one of the things that a bank does to decide whether or not to accept your application is to look in its records to see if you have borrowed money before. If you have borrowed but not paid back, then you will be turned down for the new loan. This is like you asking your neighbors if someone new shows up in the village and asks you to work for him. You might first ask around to see if the person is fair to his employees and pays them on time. If you learn that the person does not pay his workers, then you won't work for him. Banks do the same thing by checking their records.

MRFC does not ever give new loans to people who still owe them money. And MRFC shares information about who owes money with other banks, so if you fail to pay back a loan from MRFC, it can stop you from getting a new loan from OIBM or another lender, also.

Remainder of script is administered to fingerprinted clubs only

Biometric Technology
Fingerprints are unique, which means that no two people can ever have the same fingerprints. Even if they look similar on a piece of paper, people with special training, or special computer equipment, can always tell them apart.

[36] Loan amounts mentioned in the script are lower than actual loan amounts observed in the data because fertilizer prices rose somewhat in the time between the initial intervention (in Aug-Sep 2007) and loan disbursement (Nov 2007).

Your fingerprint can never change. It will be the same next year as it is this year. Just like the spots on a goat are the same as long as the goat lives, but different goats have different spots.

Fingerprints can be collected with ink and paper, or they can be collected with special machines. This machine stores fingerprints in a computer. Once your fingerprint is stored in the computer, then the machine can recognize you, and know your name and which village you come from, just by your fingerprint! The machine will recognize you even if the person who is using it is someone you have never met before. The information from the machines is saved in many different ways, so if one machine breaks, the information is still there. Just like when Celtel's building burned, people's phone numbers did not change.

Administer the following after all fingerprints have been collected:

Demo
Now, I can figure out your name even if you don't tell me. Will someone volunteer to test me? *(Have a volunteer swipe his finger, and then tell everyone who it was).*

The bank will store information about your loans with your fingerprint. That means that bank officers will know not just your name, but also what loans you have taken and whether or not you have paid them back. They will be able to tell all of this just by having you put your finger on the machine.

Before, banks used your name and other information to find out about your credit history. But now they will use fingerprints to find out. This means that even if you tell the bank a different name, they will still be able to find all of your loan records. Names can change, but fingerprints cannot.

Having your fingerprint on file can make it easier to earn the rewards for good credit history that we talked about earlier. It will be easy for the bank to look up your records and see that you have paid back your loans before. It will also be easier to apply for loans, because there will be no new forms to fill out in the future!

But, having your fingerprint on file also makes the punishment for not paying back your loan much more certain. Even if you tell the bank a different name than you used before, or meet a different loan officer, or go to a different branch, the bank will just have to check your fingerprint to find out whether or not you paid your loans before. Having records of fingerprints also makes it easy for banks to share information. Banks will share information about your fingerprints and loans. If you don't pay back a loan to MRFC, OIBM will know about it!

Appendix B: Details on biometric fingerprinting technology

In consultation with MRFC's management, fingerprint recognition was chosen over face, iris or retina recognition because it is the cheapest, best known and most widely used biometric identification technology. Fingerprinting technology extracts features from impressions made by the distinct ridges on the fingertips and has been commercially available since the early 1970s.

Loan applicants from fingerprinted clubs had the image of their right thumb fingerprint captured by an optical fingerprint scanner attached to a laptop. To maximize accuracy, farmers washed their thumbprints prior to scanning, and the scanner was also cleaned after each impression. During collection, about 2 per cent of farmers had the left thumbprint recorded (instead of the right) because the right thumbprint was worn out.

(Many farmers grow tobacco, which involves thumb usage during seedling transplantation that can wear out a thumbprint over many years.)

Upon scanning, the fingerprint image was enhanced and added to the borrower database. We purchased the VeriFinger 5.0 Software Development Kit from Fulcrum Biometrics and had a programmer develop a data capture program that would allow the user to (i) enter basic demographic information such as the name, address, village, loan size and the unique BFIRM identifier, (ii) capture the fingerprint with the scanner and (iii) review the fingerprint alongside the demographic information.

Appendix C: Variable definitions

Data used in this paper come from two surveys: a baseline conducted in August-September 2007 and a follow-up survey about farm outputs and other outcomes conducted in August 2008. We also used administrative data about loan take-up and repayment, obtained from MRFC's internal records.

Baseline characteristics (from baseline survey)

Male equals 1 for men and 0 for women.
Married equals 1 for married respondents and 0 for respondents who are single, widowed, or divorced.
Age is respondent's age in years. In regressions, we use dummies for 5-year age categories rather than a continuous measure of age.
Years of education is years of completed schooling, and is top-coded at 13. In regressions, we use dummies for years of completed schooling, rather than a continuous measure of education.
Risk taker equals 1 for respondents who report that they frequently take risks, and 0 for respondents who do not.
Days of hunger last year is the number of days in the 2006-2007 season that individuals reduced the number of meals they ate per day.
Late paying previous loan equals 1 for respondents who report paying back a previous loan late, and 0 for respondents who do not.
Income SD is the standard deviation of income between the self-reported best and worst incomes of the 5 most recent years.
Years of experience growing paprika is the self reported number of seasons in which the respondent has grown paprika before the season studied in this project.
Previous default equals 1 for respondents who report that they have defaulted on a previous loan and 0 otherwise.
No previous loans equals 1 for respondents who report that they have not had any other loans from formal financial institutions (including micro lenders, savings and credit cooperatives, and NGO schemes) and 0 otherwise.

Take-up and repayment (from administrative data)

Approved equals 1 if the respondent was approved by MRFC for a loan and 0 otherwise.

Any loan equals 1 if the respondent borrowed money from MRFC and 0 otherwise (this could differ from *Approved* if the respondent chose not to take out the loan after it was approved by MRFC).
Total borrowed is the amount owed to MRFC, in Malawi kwacha (MK 145 = $US 1). This includes the loan principal and 33 percent interest charged by MRFC.

Balance is the unpaid loan amount remaining to be paid to MRFC. The balance includes principal and accumulated interest, and is reported in MK.
Fraction paid is the amount paid on the loan, divided by the *total borrowed* defined above.
Fully paid equals 1 if the respondent has completely repaid the loan and 0 if there is an outstanding balance.

We examine different versions of the variables *Balance*, *Fraction paid*, and *Fully paid* that vary by the date at which loan repayment status is measured. One set of variables refers to loan repayment status as of September 30, 2008, which is the formal due date of the loan. Another set of variables refers to "eventual" repayment as of the end of November 2008. MRFC considers loan repayment status at the end of November 2008 as the final repayment status of the loan, and makes no subsequent attempts to collect loan repayments after that point.

Land use and inputs (from follow-up survey)

Fraction of land used for various crops is the land used for the given crop, divided by total land cultivated.
Seeds is the value of paprika seeds used by the respondent, in MK.
Fertilizer is the value of all chemical fertilizer used by the respondent on the paprika crop, in MK.
Chemicals is the value of all pesticides and herbicides used by the respondent on the paprika crop, in MK.
Man-days is the amount of money spent on hired, non-family labor for the paprika crop, in MK.
All paid inputs is the total amount of money spent on inputs for the paprika crop, in MK. Mathematically, it is the sum of *Seeds, Fertilizer, Chemicals,* and *Man-days* defined above.
KG manure is the kilograms of manure applied to the paprika crop.
Times weeding is the number of times the paprika crop was weeded, by the respondent or hired labor.

Output, revenue and profits (from follow-up survey)

KG of various crops is the self-reported kilograms harvested of each crop.
Market sales is the amount of MK received from any sales of maize, soya, groundnuts, tobacco, paprika, tomatoes, leafy vegetables, and cabbage between April and August, which encompasses the entire main harvest and selling season for these crops.

Profits is the value of *Market sales*, plus the value of unsold crop estimated based on the farmer's reported quantity, valued at district average price reported by the EPA office (*Value of unsold harvest,* defined below), minus *All paid inputs* as defined above.
Value of unsold harvest is the value, in MK, of the difference between the kg harvested and the kg sold of each crop. We use district average prices, as reported by the EPA office.

Appendix D: Construction of predicted repayment variable

To construct the predicted repayment variable, we first limit the sample to individuals in the *control* group (N=563), and run a regression of a repayment outcome (fraction of loan repaid by September 30, 2008) on various farmer- and club-level baseline characteristics. Conceptually, the resulting index will be purged of any bias introduced by effects of fingerprinting on repayment because it is constructed using coefficients from a regression predicting repayment for only the control (non-fingerprinted) farmers.

Appendix Table 2 presents results from this exercise. Statistically significant results in column 1, which only includes farmer-level (individual) variables on the right-hand-side, indicates that older farmers and those who do not self-identify as risk-takers have better repayment performance on the loan. Inclusion of a complete set of fixed effects for locality * week of initial loan offer interactions raises the R-squared substantially (from 0.05 in column 1 to 0.46 in column 2). The explanatory power of the regression is marginally improved further in column 3 (to an R-squared of 0.48) when the age and education are broken up into categorical variables (instead of being entered linearly).

We then take the coefficient estimates from column 3 of Appendix Table 2 and predict the fraction of loan repaid for the *entire* sample (both control and treatment groups). This variable, which we call "predicted repayment", is useful for analytical purposes because it is a single index that incorporates a wide array of baseline information (at the individual and locality level) correlated with repayment outcomes. Average predicted repayment for those receiving loans is 0.79, with standard deviation 0.26. As expected, predicted repayment is highly skewed, with median predicted repayment of 0.90. Predicted repayment reaches 100 percent at the 84^{th} percentile.

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Table 1: Summary statistics

	Mean	Standard Deviation	10th Percentile	Median	90th Percentile	Observations
Baseline Characteristics						
Male	0.80	0.40	0	1	1	1147
Married	0.94	0.24	1	1	1	1147
Age	39.96	13.25	24	38	59	1147
Years of Education	5.35	3.50	0	5	10	1147
Risk Taker	0.56	0.50	0	1	1	1147
Days of Hunger Last Year	6.05	11.05	0	0	30	1147
Late Paying Previous Loan	0.13	0.33	0	0	1	1147
Income SD	27568.34	46296.41	3111.27	15556.35	57841.34	1147
Years of Experience Growing Paprika	2.22	2.36	0	2	5	1147
Previous Default	0.02	0.14	0	0	0	1147
No Previous Loans	0.74	0.44	0	1	1	1147
Predicted repayment	0.79	0.26	0.33	0.90	1.02	1147
Take-up						
Approved	0.99	0.08	1	1	1	1147
Any Loan	1.00	0.00	1	1	1	1147
Total Borrowed (MK)	16912.60	3908.03	13782	16100	20136.07	1147
Land Use						
Fraction of Land used for Maize	0.43	0.16	0.28	0.40	0.63	520
Fraction of land used for Soya/Beans	0.15	0.16	0.00	0.11	0.38	520
Fraction of land used for Groundnuts	0.13	0.12	0.00	0.11	0.29	520
Fraction of land used for Tobacco	0.08	0.12	0.00	0.00	0.27	520
Fraction of land used for Paprika	0.19	0.13	0.00	0.18	0.36	520
Fraction of land used for Tomatoes	0.01	0.03	0.00	0.00	0.00	520
Fraction of land used for Leafy Vegetables	0.00	0.02	0.00	0.00	0.00	520
Fraction of land used for Cabbage	0.00	0.01	0.00	0.00	0.00	520
Fraction of Land used for all cash crops	0.57	0.16	0.38	0.60	0.72	520
Inputs						
Seeds (MK, Paprika)	247.06	348.47	0	0	560	520
Fertilizer (MK, Paprika)	7499.85	7730.05	0	5683	18200	520
Chemicals (MK, Paprika)	671.31	1613.13	0	0	2500	520
Man-days (MK, Paprika)	665.98	1732.99	0	0	2400	520
All Paid Inputs (MK, Paprika)	9084.19	8940.13	0	8000	19990	520
KG Manure, Paprika	90.84	313.71	0	0	250	520
Times Weeding, Paprika	1.94	1.18	0	2	3	520
Outputs						
KG Maize	1251.30	1024.36	360	1080	2160	520
KG Soya/Beans	83.14	136.86	0	40	200	520
KG Groundnuts	313.89	659.34	0	143	750	520
KG Tobacco	165.47	615.33	0	0	400	520
KG Paprika	188.14	396.82	0	100	364	520
KG Tomatoes	30.56	126.29	0	0	0	520
KG Leafy Vegetables	29.94	133.24	0	0	0	520
KG Cabbage	12.02	103.79	0	0	0	520
Revenue and Profits						
Market sales (MK)	65004.30	76718.29	9800	44000	137100	520
Profits (market sales + value of unsold crop - cost of inputs, MK)	117779.20	303100.80	33359	95135	261145	520
Value of Unsold Harvest (Regional Prices, MK)	80296.97	288102.70	24645	70300	180060	520
Repayment						
Balance, Sept. 30	2912.91	6405.77	0	0	13981	1147
Fraction Paid by Sept. 30	0.84	0.33	0	1	1	1147
Fully Paid by Sept. 30	0.74	0.44	0	1	1	1147

Table 2: **Tests of balance in baseline characteristics between treatment and control group**

Variable:	Full baseline sample: Mean in control group	Full baseline sample: Difference in treatment (fingerprinted) group	Loan recipient sample: Mean in control group	Loan recipient sample: Difference in treatment (fingerprinted) group
Male	0.81	-0.036 (0.022)	0.80	-0.066* (0.037)
Married	0.92	-0.004 (0.011)	0.94	0.003 (0.016)
Age	39.50	0.019 (0.674)	39.96	-0.088 (1.171)
Years of education	5.27	-0.046 (0.175)	5.35	-0.124 (0.272)
Risk taker	0.57	-0.033 (0.032)	0.56	0.013 (0.051)
Days of hunger in previous season	6.41	-0.647 (0.832)	6.05	-0.292 (1.329)
Late paying previous loan	0.14	0.005 (0.023)	0.13	0.030 (0.032)
Standard deviation of past income	25110.62	1289.190 (1756.184)	27568.34	-1158.511 (2730.939)
Years of experience growing paprika	2.10	0.096 (0.142)	2.22	0.299 (0.223)
Previous default	0.03	-0.002 (0.010)	0.02	0.008 (0.010)
No previous loan	0.74	-0.006 (0.027)	0.74	-0.020 (0.041)
P-value for test of joint significance	0.91		0.66	
Observations	3206		1147	

Stars indicate significance at 10% (*), 5% (**), and 1% (***) levels.
Notes: Each row presents mean of a variable in the baseline (September 2008) survey in the control group, and the difference between the treatment group mean and the control group mean of that variable (standard error in parentheses). Differences and standard errors calculated via a regression of the baseline variable on the treatment group indicator; standard errors are clustered at the club level.

Table 3: Borrower behavior under various theoretical cases, with and without dynamic incentives

	Without Dynamic Incentives			With Dynamic Incentives		
Case (i): $qb_H > y_S(b_H)$						
	$p < P_{B1}$	$p \geq P_{B1}$		$p < P_{B1}$	$p \geq P_{B1}$	
Loan size b	b_H	b_H		b_L	b_H	
Diversion D	1	1		0	0	
Repayment Rate	0	0		1	p	
Case (ii): $y_S(b_H) > qb_H > y_S(b_L)$						
	$p < P_{B1}$	$P_{B1} \leq p < P_D$	$p \geq P_D$	$p < P_{B1}$	$P_{B1} \leq p < P_D$	$p \geq P_D$
Loan size b	b_H	b_H	b_H	b_L	b_H	b_H
Diversion D	1	1	0	0	0	0
Repayment Rate	0	0	p	1	p	p
Case (iii): $y_S(b_L) > qb_H$						
	$p < P_D$	$P_D \leq p < P_{B0}$	$p \geq P_{B0}$	$p < P_D$	$P_D \leq p < P_{B0}$	$p \geq P_{B0}$
Loan size b	b_H	b_H	b_H	b_L	b_L	b_H
Diversion D	1	0	0	0	0	0
Repayment Rate	0	p	p	1	1	p

Table 4: **Impact of fingerprinting on loan approval, loan take-up, and amount borrowed**

	(1)	(2)	(3)
Sample:	All Respondents	All Respondents	Loan Recipients
Dependent variable:	Approved	Any Loan	Total Borrowed (MK)
Panel A			
Fingerprint	0.038	0.051	-696.799*
	(0.053)	(0.044)	(381.963)
Panel B			
Fingerprint	0.207	0.108	-2812.766
	(.161)	(.145)	(2371.685)
Predicted repayment * fingerprint	-0.219	-0.074	2630.653
	(.197)	(.168)	(2555.167)
Panel C			
Fingerprint * Quintile 1	0.093	0.075	-2721.780
	(.115)	(.111)	(1666.068)
Fingerprint * Quintile 2	0.180*	0.102	-258.179
	(.096)	(.086)	(828.500)
Fingerprint * Quintile 3	-0.030	0.061	-458.924
	(.082)	(.073)	(596.109)
Fingerprint * Quintile 4	-0.001	-0.037	-101.028
	(.086)	(.082)	(575.968)
Fingerprint * Quintile 5	-0.017	0.039	-400.620
	(.100)	(.089)	(784.509)
Observations	3206	3206	1147
Mean of dependent variable	0.63	0.35	16912.60
Quintile 1	0.58	0.29	17992.53
Quintile 2	0.64	0.36	17870.61
Quintile 3	0.71	0.44	16035.10
Quintile 4	0.70	0.47	15805.54
Quintile 5	0.59	0.30	16886.56

Stars indicate significance at 10% (*), 5% (**), and 1% (***) levels.
Notes: Each column presents estimates from three separate regressions: main effect of fingerprinting in Panel A, linear interaction with predicted repayment in Panel B, and interactions with quintiles of predicted repayment in Panel C. All regressions include credit officer * week of initial loan offer fixed effects, baseline characteristics (male, five-year age categories, one-year education categories, and marriage), and baseline risk indicators (dummy for self-reported risk-taking, days of hunger in the previous season, late payments on previous loans, standard deviation of income, years of experience growing paprika, dummy for default on previous loan, and dummy for no previous loans). Standard errors on Panel A coefficients are clustered at the club level, while those in Panels B and C are bootstrapped with 200 replications and club-level resampling.

Table 5: Impact of fingerprinting on loan repayment

	(1)	(2)	(3)	(4)	(5)	(6)
Sample:	Loan recipients	Loan recipients	Loan recipients	Loan recipients	Loan recipients	Loan recipients
Dependent variable:	Balance, Sept. 30	Fraction Paid by Sept. 30	Fully Paid by Sept. 30	Balance, Eventual	Fraction Paid, Eventual	Fully Paid, Eventual
Panel A						
Fingerprint	-1556.383*	0.073*	0.096	-996.430	0.045	0.085
	(824.174)	(0.040)	(0.062)	(754.301)	(0.036)	(0.058)
Panel B						
Fingerprint	-15174.149***	0.716***	0.842***	-9727.739**	0.438**	0.602***
	(2743.271)	(.110)	(.178)	(4199.085)	(.184)	(.224)
Predicted repayment * fingerprint	16930.139***	-0.799***	-0.928***	10855.103**	-0.489**	-0.643***
	(3047.515)	(.121)	(.196)	(4499.549)	(.196)	(.243)
Panel C						
Fingerprint * Quintile 1	-10844.169***	0.499***	0.543***	-7202.647**	0.317**	0.396**
	(2681.861)	(.127)	(.147)	(2969.045)	(.136)	(.156)
Fingerprint * Quintile 2	-1104.582	0.066	0.163	-1028.696	0.060	0.170
	(2025.425)	(.105)	(.160)	(1871.298)	(.097)	(.148)
Fingerprint * Quintile 3	-307.761	0.005	-0.004	-297.918	0.002	0.007
	(966.586)	(.048)	(.091)	(901.013)	(.045)	(.087)
Fingerprint * Quintile 4	818.275	-0.037	-0.045	775.231	-0.035	-0.028
	(942.466)	(.046)	(.078)	(883.076)	(.044)	(.075)
Fingerprint * Quintile 5	1674.419	-0.078*	-0.084	1404.812	-0.061	-0.050
	(1022.895)	(.046)	(.074)	(951.535)	(.043)	(.071)
Observations	1147	1147	1147	1147	1147	1147
Mean of dependent variable	2912.91	0.84	0.74	2080.86	0.89	0.79
Quintile 1	6955.67	0.62	0.52	4087.04	0.81	0.68
Quintile 2	4024.05	0.77	0.63	3331.17	0.81	0.67
Quintile 3	1571.44	0.92	0.83	1301.79	0.93	0.84
Quintile 4	877.80	0.95	0.85	781.59	0.95	0.87
Quintile 5	1214.19	0.94	0.85	950.29	0.95	0.88

Stars indicate significance at 10% (*), 5% (**), and 1% (***) levels.
Notes: Each column presents estimates from three separate regressions: main effect of fingerprinting in Panel A, linear interaction with predicted repayment in Panel B, and interactions with quintiles of predicted repayment in Panel C. All regressions include credit officer * week of initial loan offer fixed effects, baseline characteristics (male, five-year age categories, one-year education categories, and marriage), and baseline risk indicators (dummy for self-reported risk-taking, days of hunger in the previous season, late payments on previous loans, standard deviation of income, years of experience growing paprika, dummy for default on previous loan, and dummy for no previous loans). Standard errors on Panel A coefficients are clustered at the club level, while those in Panels B and C are bootstrapped with 200 replications and club-level resampling. Sample limited to individuals who took out loans in 2008 (columns 1-3), or who took out loans and were included in follow-up survey in 2009 (columns 4-6).

Table 6: Impact of fingerprinting on land use

Dependent variable: Fraction of land used for...	(1) Maize	(2) Soya/Beans	(3) Groundnuts	(4) Tobacco	(5) Paprika	(6) Tomatoes	(7) Leafy Vegetables	(8) Cabbage	(9) All cash crops
Panel A									
Fingerprint	0.001	0.015	-0.012	-0.004	0.005	-0.001	-0.003	-0.001	-0.001
	(0.019)	(0.019)	(0.016)	(0.016)	(0.014)	(0.003)	(0.003)	(0.001)	(0.019)
Panel B									
Fingerprint	-0.009	-0.025	-0.025	-0.033	0.079	0.009	0.006	-0.003	0.009
	(.092)	(.094)	(.060)	(.062)	(.064)	(.010)	(.015)	(.004)	(.092)
Predicted repayment * fingerp	0.013	0.049	0.016	0.036	-0.092	-0.013	-0.011	0.003	-0.013
	(.101)	(.105)	(.068)	(.066)	(.073)	(.013)	(.016)	(.005)	(.101)
Panel C									
Fingerprint * Quintile 1	-0.061	-0.013	-0.008	-0.012	0.083	0.005	0.007	-0.002	0.061
	(.066)	(.063)	(.052)	(.050)	(.051)	(.008)	(.012)	(.003)	(.066)
Fingerprint * Quintile 2	0.065	0.019	-0.014	-0.019	-0.035	-0.005	-0.010	-0.002	-0.065
	(.052)	(.042)	(.041)	(.030)	(.037)	(.008)	(.008)	(.002)	(.052)
Fingerprint * Quintile 3	-0.012	0.002	-0.009	0.004	0.009	0.008	-0.002	-0.001	0.012
	(.044)	(.045)	(.033)	(.022)	(.038)	(.008)	(.007)	(.002)	(.044)
Fingerprint * Quintile 4	0.008	0.015	-0.026	0.009	-0.003	-0.002	-0.003	0.002	-0.008
	(.041)	(.040)	(.034)	(.021)	(.037)	(.009)	(.007)	(.003)	(.041)
Fingerprint * Quintile 5	-0.005	0.043	-0.001	-0.001	-0.018	-0.012	-0.005	-0.002	0.005
	(.044)	(.040)	(.036)	(.023)	(.034)	(.009)	(.006)	(.003)	(.044)
Observations	520	520	520	520	520	520	520	520	520
Mean of dependent variabl	0.43	0.15	0.13	0.08	0.19	0.01	0.00	0.00	0.57
Quintile 1	0.44	0.07	0.13	0.18	0.17	0.01	0.01	0.00	0.56
Quintile 2	0.49	0.10	0.13	0.13	0.15	0.00	0.00	0.00	0.51
Quintile 3	0.42	0.21	0.12	0.03	0.20	0.01	0.00	0.00	0.58
Quintile 4	0.42	0.19	0.12	0.04	0.21	0.01	0.01	0.00	0.58
Quintile 5	0.40	0.17	0.14	0.04	0.23	0.01	0.01	0.00	0.60

Stars indicate significance at 10% (*), 5% (**), and 1% (***) levels.
Notes: Each column presents estimates from three separate regressions: main effect of fingerprinting in Panel A, linear interaction with predicted repayment in Panel B, and interactions with quintiles of predicted repayment in Panel C. All regressions include credit officer * week of initial loan offer fixed effects, baseline characteristics (male, five-year age categories, one-year education categories, and marriage), and baseline risk indicators (dummy for self-reported risk-taking, days of hunger in the previous season, late payments on previous loans, standard deviation of income, years of experience growing paprika, dummy for default on previous loan, and dummy for no previous loans). Standard errors on Panel A coefficients are clustered at the club level, while those in Panels B and C are bootstrapped with 200 replications and club-level resampling. Sample limited to individuals who took out loans in

Table 7: Impact of fingerprinting on agricultural inputs used on paprika crop

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable:	Seeds (MK)	Fertilizer (MK)	Chemicals (MK)	Man-days (MK)	All Paid Inputs (MK)	KG Manure	Times Weeding
Panel A							
Fingerprint	74.107	733.419	345.328*	-395.501**	757.354	29.649	0.019
	(47.892)	(1211.905)	(190.262)	(181.958)	(1389.230)	(32.593)	(0.147)
Panel B							
Fingerprint	262.116*	11115.814**	466.677	411.043	12255.650**	52.882	0.182
	(146.417)	(5660.459)	(594.037)	(579.097)	(5987.210)	(144.033)	(.466)
Predicted repayment * fingerprin	-234.438	-12946.332**	-151.316	-1005.720	-14337.806**	-28.970	-0.203
	(183.931)	(6245.378)	(701.923)	(732.887)	(6700.416)	(161.334)	(.591)
Panel C							
Fingerprint * Quintile 1	188.703**	5871.126	374.260	106.406	6540.496	78.234	0.445
	(95.018)	(4062.716)	(406.741)	(347.367)	(4210.469)	(111.980)	(.367)
Fingerprint * Quintile 2	78.717	3597.540	244.449	-236.338	3684.368	27.058	-0.443
	(95.343)	(3026.725)	(414.863)	(454.498)	(3362.245)	(81.930)	(.338)
Fingerprint * Quintile 3	124.548	-585.618	500.669	-348.598	-309.000	58.670	-0.191
	(97.766)	(2250.453)	(427.366)	(458.033)	(2602.025)	(94.443)	(.333)
Fingerprint * Quintile 4	-10.190	-1790.213	283.962	-1065.690**	-2582.132	-25.080	-0.254
	(110.489)	(2503.022)	(430.040)	(537.142)	(2952.953)	(73.404)	(.348)
Fingerprint * Quintile 5	18.589	-2444.617	264.620	-315.018	-2476.427	21.879	0.564
	(110.367)	(2201.579)	(445.234)	(572.589)	(2635.638)	(93.481)	(.379)
Observations	520	520	520	520	520	520	520
Mean of dependent variable	247.06	7499.85	671.31	665.98	9084.19	90.84	1.94
Quintile 1	174.13	6721.24	401.30	143.48	7440.15	97.39	1.47
Quintile 2	140.00	6080.46	620.67	238.94	7080.08	39.25	1.55
Quintile 3	269.90	8927.65	674.48	836.98	10709.00	105.73	2.05
Quintile 4	292.07	7649.51	715.08	936.29	9592.95	93.23	2.24
Quintile 5	340.18	8078.58	892.05	1065.18	10375.99	118.13	2.28

Stars indicate significance at 10% (*), 5% (**), and 1% (***) levels.
Notes: Each column presents estimates from three separate regressions: main effect of fingerprinting in Panel A, linear interaction with predicted repayment in Panel B, and interactions with quintiles of predicted repayment in Panel C. All regressions include credit officer * week of initial loan offer fixed effects, baseline characteristics (male, five-year age categories, one-year education categories, and marriage), and baseline risk indicators (dummy for self-reported risk-taking, days of hunger in the previous season, late payments on previous loans, standard deviation of income, years of experience growing paprika, dummy for default on previous loan, and dummy for no previous loans). Standard errors on Panel A coefficients are clustered at the club level, while those in Panels B and C are bootstrapped with 200 replications and club-level resampling. Sample limited to individuals who took out loans in 2008 and who were included in follow-up survey in 2009.

<u>Table 8</u>: Impact of fingerprinting on revenue and profits

	(1)	(2)	(3)	(4)
<u>Dependent variable</u>:	Market sales (Self Report, MK)	Value of Unsold Harvest (Regional Prices, MK)	Profits (market sales + value of unsold harvest - cost of inputs, MK)	Ln(profits)
<u>Panel A</u>				
Fingerprint	7246.174	5270.320	14509.457	0.060
	(8792.055)	(14879.349)	(16679.311)	(0.095)
<u>Panel B</u>				
Fingerprint	69102.211	-29468.424	24207.068	0.651
	(49177.370)	(85252.270)	(90535.890)	(.423)
Predicted repayment * fingerprint	-77131.415	43317.493	-12092.441	-0.737
	(51232.390)	(103316)	(108112.600)	(.501)
<u>Panel C</u>				
Fingerprint * Quintile 1	30766.147	7940.835	31915.287	0.401
	(36850.940)	(50587.570)	(63206.880)	(.363)
Fingerprint * Quintile 2	41981.091	6364.782	45650.027	0.283
	(33084.250)	(75026.680)	(81848.520)	(.264)
Fingerprint * Quintile 3	-20925.441	-14911.454	-26932.651	-0.202
	(17938.730)	(59934.020)	(63400.760)	(.227)
Fingerprint * Quintile 4	-12785.841	7481.854	3609.228	-0.038
	(14733.930)	(57096.050)	(60385.110)	(.231)
Fingerprint * Quintile 5	1053.151	33336.147	34125.843	-0.054
	(15282.460)	(71891.840)	(74254.990)	(.240)
Observations	520	520	520	520
Mean of dependent variable	65004.30	80296.97	117779.16	11.44
Quintile 1	60662.57	82739.24	121222.50	11.36
Quintile 2	89028.25	29995.27	91652.71	11.55
Quintile 3	57683.74	96247.91	123242.30	11.44
Quintile 4	61088.27	104927.50	136467.50	11.45
Quintile 5	56593.43	85817.08	115172.50	11.39
Mean of dependent variable (US	464.32	573.55	841.28	n.a.

Stars indicate significance at 10% (*), 5% (**), and 1% (***) levels.
Notes: Each column presents estimates from three separate regressions: main effect of fingerprinting in Panel A, linear interaction with predicted repayment in Panel B, and interactions with quintiles of predicted repayment in Panel C. All regressions include credit officer * week of initial loan offer fixed effects, baseline characteristics (male, five-year age categories, one-year education categories, and marriage), and baseline risk indicators (dummy for self-reported risk-taking, days of hunger in the previous season, late payments on previous loans, standard deviation of income, years of experience growing paprika, dummy for default on previous loan, and dummy for no previous loans). Standard errors on Panel A coefficients are clustered at the club level, while those in Panels B and C are bootstrapped with 200 replications and club-level resampling. Sample limited to individuals who took out loans in 2008 and who were included in follow-up survey in 2009. Value of unsold harvest computed using regional prices.

Table 9: Ex post moral hazard

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Balance, Sept. 30	Fraction Paid by Sept. 30	Fully Paid by Sept. 30	Balance, Eventual	Fraction Paid, Eventual	Fully Paid, Eventual
Panel A						
Fingerprint	-102.571	-0.005	0.000	424.455	-0.031	-0.003
	(775.942)	(0.039)	(0.072)	(565.064)	(0.028)	(0.058)
Panel B						
Fingerprint	-8282.762*	0.320	0.399	-3537.222	0.082	0.173
	(4698.647)	(.237)	(.298)	(5140.761)	(.237)	(.280)
Predicted repayment * fingerprint	9794.172**	-0.389	-0.478	4743.330	-0.134	-0.211
	(4903.271)	(.246)	(.326)	(5378.012)	(.247)	(.308)
Panel C						
Fingerprint * Quintile 1	-7589.870*	0.304	0.314	-4443.517	0.149	0.221
	(4479.864)	(.211)	(.248)	(4252.255)	(.187)	(.216)
Fingerprint * Quintile 2	2964.264	-0.164	-0.142	2679.579	-0.151	-0.132
	(2231.033)	(.118)	(.166)	(1950.827)	(.104)	(.149)
Fingerprint * Quintile 3	-597.239	0.035	0.049	-358.930	0.024	0.050
	(1105.313)	(.062)	(.108)	(978.83)	(.055)	(.100)
Fingerprint * Quintile 4	419.174	-0.026	-0.058	763.678	-0.042	-0.052
	(1000.512)	(.057)	(.113)	(909.161)	(.052)	(.102)
Fingerprint * Quintile 5	460.280	-0.019	-0.023	732.027	-0.029	-0.005
	(1134.012)	(.062)	(.119)	(955.498)	(.052)	(.099)
Observations	520	520	520	520	520	520
Mean of dependent variable	2071.21	0.89	0.79	1439.16	0.92	0.83
Quintile 1	6955.67	0.62	0.52	3472.29	0.83	0.71
Quintile 2	4024.05	0.77	0.63	2610.41	0.85	0.75
Quintile 3	1571.44	0.92	0.83	476.63	0.97	0.91
Quintile 4	877.80	0.95	0.85	661.79	0.96	0.86
Quintile 5	1214.19	0.94	0.85	311.66	0.98	0.93

Stars indicate significance at 10% (*), 5% (**), and 1% (***) levels.
Notes: Each column presents estimates from three separate regressions: main effect of fingerprinting in Panel A, linear interaction with predicted repayment in Panel B, and interactions with quintiles of predicted repayment in Panel C. All regressions include credit officer * week of initial loan offer fixed effects, baseline characteristics (male, five-year age categories, one-year education categories, and marriage), and baseline risk indicators (dummy for self-reported risk-taking, days of hunger in the previous season, late payments on previous loans, standard deviation of income, years of experience growing paprika, dummy for default on previous loan, and dummy for no previous loans). Standard errors on Panel A coefficients are clustered at the club level, while those in Panels B and C are bootstrapped with 200 replications and club-level resampling. Sample limited to individuals who took out loans in 2008 and who were included in follow-up survey in 2009.

Table 10: Benefit-cost analysis

Benefit		
(a) Increase in repayment due to fingerprinting in Quintile 1	4,755.00	Malawi kwacha
(b) Quintile 1 as share of all borrowers	20.0%	
(c) Borrowers as share of all fingerprinted	50%	
(d) Total benefit per individual fingerprinted [= (a)*(b)*(c)]	475.50	Malawi kwacha
Cost		
(e) Cost per equipment unit	101,500	Malawi kwacha
(f) Equipment amortization period	3	years
(g) Annual equipment amortization [= (e) / (f)]	33,833	
(h) Fingerprinted individuals per equipment unit	417	individuals
(i) Equipment cost per farmer [= (g) / (h)]	81.20	Malawi kwacha
(j) Loan officer time cost per farmer	19.25	Malawi kwacha
(k) Transaction cost per fingerprint checked	108.75	Malawi kwacha
(l) Total cost per individual fingerprinted [= (i) + (j) + (k)]	209.20	Malawi kwacha
(m) Net benefit per fingerprinted farmer [= (d) - (l)]	266.30	Malawi kwacha
(n) Benefit-cost ratio [= (d) / (l)]	2.27	

Assumptions:

Exchange rate:	145	MK/US$
Loan size	15,000	Malawi kwacha
Increase in share of loan repaid due to fingerprinting in Quintile 1	31.7%	
Cost per equipment unit (laptop computer + fingerprint scanner)	700	USD
Number of equipment units	12	
New loan applicants fingerprinted per year	5,000	
Fingerprinting time per individual	5	minutes
Monthly salary of MRFC loan officer	40,000	Malawi kwacha
Hours worked per month by MFRC loan officer	173.2	hours

<u>Appendix Table 1</u>: Auxilary regression for predicting loan repayment

<u>Dependent variable</u>:	(1) Fraction Paid by Sept. 30	(2) Fraction Paid by Sept. 30	(3) Fraction Paid by Sept. 30
Male	0.080	0.061	0.058
	(0.073)	(0.048)	(0.048)
Married	-0.071	-0.091	-0.101
	(0.060)	(0.044)**	(0.046)**
Age	0.004	0.001	
	(0.001)***	(0.001)	
Years of education	-0.005	-0.003	
	(0.005)	(0.004)	
Risk taker	-0.078	0.008	0.013
	(0.041)*	(0.031)	(0.031)
Days of Hunger in previous season	0.001	-0.000	-0.001
	(0.002)	(0.001)	(0.001)
Late paying previous loan	-0.058	-0.084	-0.084
	(0.071)	(0.046)*	(0.047)*
Standard deviation of past income	-0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
Years of experience growing paprika	0.005	0.007	0.007
	(0.013)	(0.011)	(0.011)
Previous default	0.088	0.128	0.097
	(0.163)	(0.079)	(0.078)
No previous loan	-0.012	0.015	0.013
	(0.062)	(0.032)	(0.034)
Constant	0.729	0.949	0.982
	(0.114)***	(0.072)***	(0.090)***
Loan officer * week of initial loan offer fixed effects	--	Y	Y
Dummy variables for 5-year age groups	--	--	Y
Dummy variables for each year of education	--	--	Y
Observations	563	563	563
R-squared	0.05	0.46	0.48

Robust standard errors in parentheses

Stars indicate significance at 10% (*), 5% (**), and 1% (***) levels.
Notes: Sample is non-fingerprinted loan recipients from the September 2008 baseline survey. All standard errors are clustered at the club level.

Appendix Table 2: Impact of fingerprinting on loan officer knowledge and behavior

	Means				
	All	Treatment	Control	P-value of T-test of (2)=(3)	Num. of obs.
	(1)	(2)	(3)	(4)	(5)
Loan officer reports					
Knows treatment status of club (1=yes)	0.37	0.54	0.22	0.16	51
Knows identity of club officers (1=Yes)	0.47	0.46	0.48	0.88	51
Abs. diff. between actual and officer report of number of loans	1.6	1.3	1.9	0.47	50
Borrower reports					
Number of times loan officer visited club to request loan repayment	0.35	0.41	0.27	0.41	396
Number of times borrower spoke to loan officer since April 2008	2.62	2.57	2.68	0.74	450
Difficulty in locating loan officer (1=easy 2=moderate 3=difficult)	1.2	1.17	1.24	0.32	453

Notes: The first three rows present loan officer reports about knowledge of clubs and treatment status collected in August 2008. The last three rows present borrower reports about interactions with the loan officer collected in the follow-up survey of August 2008.

Appendix Table 3: Impact of fingerprinting on attrition from sample

Dependent variable: Indicator for attrition from September 2008 baseline survey to August 2009 survey

	(1)	(2)
Sample:	All respondents	Loan recipients
Panel A		
Fingerprint	-0.057	-0.086
	(0.036)	(0.070)
Panel B		
Fingerprint	-0.042	-0.134
	(.107)	(.197)
Predicted repayment * fingerprint	-0.020	0.059
	(.128)	(.225)
Panel C		
Fingerprint * Quintile 1	-0.023	-0.148
	(.075)	(.136)
Fingerprint * Quintile 2	-0.074	0.035
	(.071)	(.109)
Fingerprint * Quintile 3	-0.069	-0.106
	(.068)	(.105)
Fingerprint * Quintile 4	-0.086	-0.109
	(.076)	(.124)
Fingerprint * Quintile 5	-0.080	-0.115
	(.071)	(.128)
Observations	3206	1147
Mean of dependent variable	0.63	0.55
Quintile 1	0.58	0.59
Quintile 2	0.57	0.54
Quintile 3	0.63	0.58
Quintile 4	0.60	0.50
Quintile 5	0.70	0.52

Stars indicate significance at 10% (*), 5% (**), and 1% (***) levels.
Notes: Each column presents estimates from three separate regressions: main effect of fingerprinting in Panel A, linear interaction with predicted repayment in Panel B, and interactions with quintiles of predicted repayment in Panel C. All regressions include credit officer * week of initial loan offer fixed effects, baseline characteristics (male, five-year age categories, one-year education categories, and marriage), and baseline risk indicators (dummy for self-reported risk-taking, days of hunger in the previous season, late payments on previous loans, standard deviation of income, years of experience growing paprika, dummy for default on previous loan, and dummy for no previous loans). Standard errors on Panel A coefficients are clustered at the club level, while those in Panels B and C are bootstrapped with 200 replications and club-level resampling.

Appendix Table 4: Impact of fingerprinting on loan repayment

	(1)	(2)	(3)	(4)	(5)	(6)
Sample:	Loan recipients included in August 2009 survey	Loan recipients included in August 2009 survey	Loan recipients included in August 2009 survey	Loan recipients included in August 2009 survey	Loan recipients included in August 2009 survey	Loan recipients included in August 2009 survey
Dependent variable:	Balance, Sept. 30	Fraction Paid by Sept. 30	Fully Paid by Sept. 30	Balance, Eventual	Fraction Paid, Eventual	Fully Paid, Eventual
Panel A						
Fingerprint	-1743.102*	0.073*	0.085	-875.314	0.031	0.060
	(885.950)	(0.044)	(0.069)	(670.297)	(0.032)	(0.057)
Panel B						
Fingerprint	-15386.752***	0.684***	0.759***	-8931.946*	0.362	0.390
	(3782.488)	(.196)	(.213)	(5162.708)	(.237)	(.257)
Predicted repayment * fingerprint	17012.956***	-0.761***	-0.841***	10046.221*	-0.413*	-0.411
	(4018.014)	(.206)	(.240)	(5446.717)	(.250)	(.284)
Panel C						
Fingerprint * Quintile 1	-12684.695***	0.566***	0.599***	-8016.543*	0.334*	0.373*
	(4085.065)	(.195)	(.198)	(4347.488)	(.195)	(.201)
Fingerprint * Quintile 2	1699.375	-0.098	-0.071	1799.143	-0.104	-0.090
	(2125.301)	(.111)	(.168)	(1914.282)	(.101)	(.152)
Fingerprint * Quintile 3	-690.017	0.038	0.052	-586.977	0.032	0.062
	(973.012)	(.055)	(.105)	(871.625)	(.050)	(.097)
Fingerprint * Quintile 4	443.620	-0.029	-0.065	549.532	-0.033	-0.034
	(924.169)	(.053)	(.113)	(821.086)	(.047)	(.103)
Fingerprint * Quintile 5	212.990	-0.006	0.007	289.061	-0.008	0.044
	(978.124)	(.054)	(.110)	(804.733)	(.045)	(.092)
Observations	520	520	520	520	520	520
Mean of dependent variable	2071.21	0.89	0.79	1439.16	0.92	0.83
Quintile 1	6955.67	0.62	0.52	3472.29	0.83	0.71
Quintile 2	4024.05	0.77	0.63	2610.41	0.85	0.75
Quintile 3	1571.44	0.92	0.83	476.63	0.97	0.91
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Figure 1: Experimental Timeline

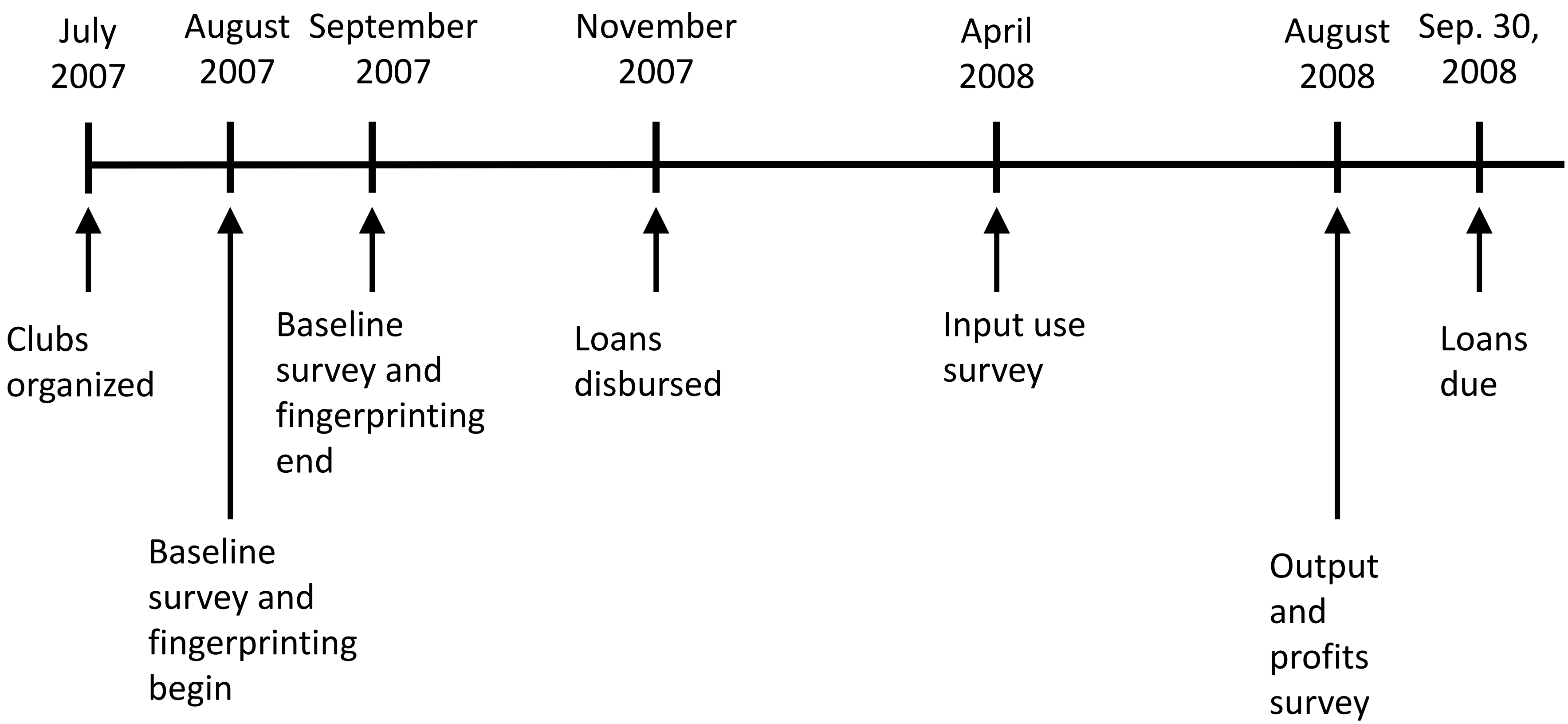

Figure 2: Malawi Study Areas

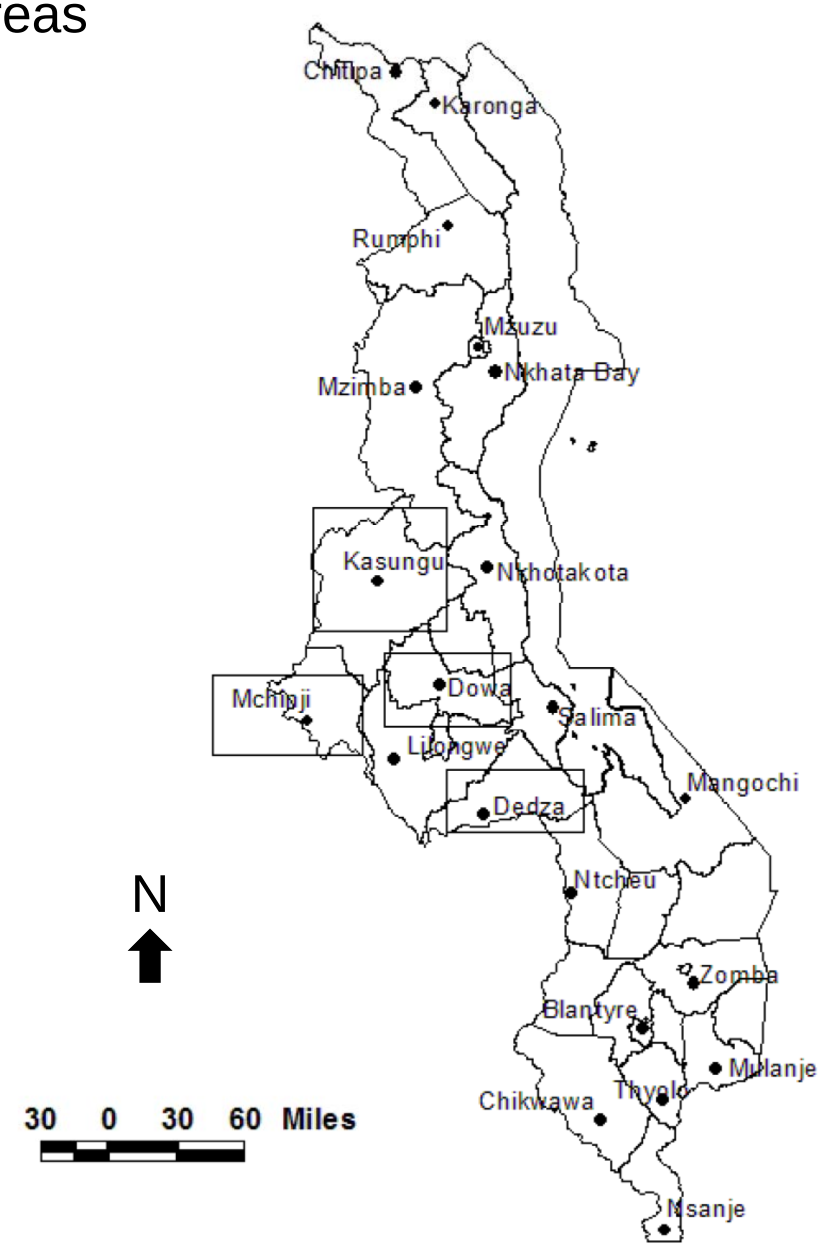

Figure 3: Optimal behavior as a function of p