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# CREDIT MARKET CONSEQUENCES OF IMPROVED PERSONAL IDENTIFICATION: FIELD EXPERIMENTAL EVIDENCE FROM MALAWI

Xavier Giné
Jessica Goldberg
Dean Yang

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Credit Market Consequences of Improved Personal Identification: Field Experimental Evidence from Malawi
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**ABSTRACT**

We report the results of a randomized field experiment that examines the credit market impacts of improvements in a lender's ability to determine borrowers' identities. Improved personal identification enhances the credibility of a lender's dynamic repayment incentives by allowing it to withhold future loans from past defaulters and expand credit for good borrowers. The experimental context, rural Malawi, is characterized by an imperfect identification system. 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 the borrowers. The change in repayment rates is driven by reductions in adverse selection (smaller loan sizes) and lower moral hazard (for example, less diversion of loan-financed fertilizer from its intended use on the cash crop).

Xavier Giné
The World Bank
xgine@worldbank.org

Jessica Goldberg
University of Maryland
Department of Economics
3115G Tydings Hall
College Park, MD 20742
goldberg@econ.umd.edu

Dean Yang
University of Michigan
Gerald R. Ford School of Public Policy
and Department of Economics
735 S. State Street, Room 3316
Ann Arbor, MI 48109
and NBER
deanyang@umich.edu

## 1. Introduction

Imperfections in credit markets are widely seen as key barriers to growth (King and Levine, 1993). Among such imperfections, asymmetric information problems play a prominent role, as they limit the ability of borrowers to commit to carrying out their obligations under debt contracts. Borrowers cannot credibly reveal their borrower type (adverse selection), promise to exert sufficient effort on their enterprises (ex-ante moral hazard), or promise to repay loans upon realization of enterprise profits, even when such profits are sufficient for repayment (ex-post moral hazard).[1] Lenders seek to mitigate asymmetric information problems by imposing collateral requirements, engaging in costly screening of borrowers prior to approval, and – when a credit reporting system is available – sharing credit information with other lenders. Microcredit institutions have addressed informational problems by relying on non-traditional mechanisms such as group liability. However, microlenders have recently come under attack, especially in India, because of allegations of over-indebtedness of clients driven in part by rapid growth and increased competition. As a result, microlenders are seeking to participate in credit bureaus, much like traditional lenders.[2]

For a credit bureau to function effectively, however, it must be possible to uniquely identify individuals with reasonable certainty. Identification is necessary in order to retrieve a current loan applicant's past credit history from a credit database. Most developed countries have a unique identification system in the form of a social security number or government-issued photo identification. But in many of the world's poorer countries, large segments of the population lack formal identification documents, and even for those who have them, there is often no national system for uniquely identifying individuals in a database. In these countries, lenders accept different forms of identification, such as a passport, a health insurance policy number or even a letter from the local village leader. Because documents can be falsified, and because individuals may simply use different types of identification when dealing with different lenders, it is extremely difficult to track a customer across multiple lenders, and it can even be difficult for lenders to identify defaulters within their own client base. Loan defaulters may avoid sanction for past default by simply applying for new loans under different identities.

[1] For reviews of this literature, see Ghosh, Mookherjee and Ray (2000), and Conning and Udry (2005).
[2] One of the recommendations of the Malegam Committee, set up in October 2010 after the crisis in Andhra Pradesh, India, was the establishment of a credit bureau and the adoption of a customer protection code.

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. In rural areas, the result is that smallholder farmers are severely constrained in their ability to finance crucial inputs such as fertilizer and improved seeds, which limits production of both subsistence and cash crops.

Motivated by the benefits of a unique identification system, a number of efforts in the developing world are underway, many on a massive scale. For example, the Indian government has embarked on a vast effort to fingerprint and assign personal identification numbers that will replace all other forms of identification and enable citizens to access credit markets, public services and subsidies on food, energy and education that now suffer from major pilferage (Planning Commission, 2005; The Economist, 2011; Polgreen, 2011).

Despite its importance, there is essentially no empirical evidence thus far on the impacts of improved personal identification in credit markets. A number of questions are of general interest. First, how do improvements in personal identification affect borrower and lender behavior and ultimately loan repayment rates? Second, how prevalent are adverse selection and moral hazard in the credit market? And finally, how does improved personal identification affect the operation of credit bureaus?

We report the results from a randomized field experiment that sheds light on the above questions. The experiment randomizes fingerprinting of loan applicants to test the impact of improved personal identification. The experiment was carried out in a context—rural Malawi—characterized by an imperfect 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. Few rural Malawian households have access to loans for production purposes: only 11.7% report any production loans in the past 12 months, and among these loans only 40.3% are from formal lenders.[3]

In the experiment, farmers who applied for agricultural input loans to grow paprika were randomly assigned to either 1) a control group, or 2) a treatment group where each member had a

[3] Figures are nationally representative and come from the 2004 Malawi Integrated Household Survey. Formal lenders include commercial banks, NGOs and microfinance institutions; informal lenders include moneylenders, family, and friends.

fingerprint collected as part of the loan application. A key advantage of fingerprints as a form of personal identification is that they are unique to and embodied in each person, so they cannot be forgotten, lost or stolen. Improved borrower identification allows lenders to construct accurate credit histories and condition future lending on past repayment performance. Loan repayment could improve with fingerprinting, by making the lender's threats of future credit denial as well as promises of larger future loans more credible.

To frame the empirics, 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 ensure access to credit in the future.[4] 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 on borrowing, farmer actions during the production phase, and repayment will be largest for the riskiest individuals.

We find that fingerprinting led to substantially higher repayment rates for the subgroup of borrowers with the highest ex-ante default risk.[5] In the context of the model, this result suggests that fingerprinting, by improving personal identification, enhanced the credibility of the lender's dynamic incentive. The impact of fingerprinting on repayment in the highest default risk subgroup (representing 20% of borrowers) is large: the average share of the loan repaid (two months after the due date) was 66.7% in the control group, compared to 92.2% among fingerprinted borrowers.[6] In other words, for these farmers fingerprinting accounts for roughly

[4] 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).

[5] To create the ex-ante default risk measure, we regress loan repayment rates on borrowers' baseline characteristics in the control group, and then predict loan repayment for the entire sample (including the treatment group). This essentially creates a "credit score" for each borrower based on their ex-ante (pre-borrowing) characteristics. We provide further details on this procedure in Section 4.

[6] The treatment effect implied by these figures is not regression-adjusted, but regression-based estimates are (as would be expected) very similar.

three-quarters of the gap between repayment in the control group and full repayment. By contrast, fingerprinting had no impact on repayment for farmers with low ex ante default risk.

While we cannot separate the effects 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 to choose smaller loan sizes. In the context of the theoretical model, this is 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 in the model represents a reduction in moral hazard. When we compare these benefits to estimated costs of implementation, we find that adoption of fingerprinting is cost-effective, with a benefit-cost ratio of 2.34.[7]

The key contribution of the paper, in our view, is that it provides the first empirical evidence of the importance of personal identification for credit market efficiency. Imperfect personal identification is an information problem that has received little attention in the literature. Prior to this study, the extent to which identification of borrowers is a problem for formal lenders – in any sample population – was unknown. Our results indicate that alleviating this specific information asymmetry in rural Malawi has non-negligible benefits for credit markets.

Our analysis is further distinguished by the nature of our outcome data. In addition to using the lender's administrative data to measure impacts on borrowing decisions and repayment, we also use a detailed follow-up survey to estimate impacts on several typically unobserved behaviors related to moral hazard. For example, we provide direct evidence of changes in production decisions and the use of borrowed funds stemming from improved identification.

This paper also has implications for the perceived benefits of a credit reporting system. Despite the absence of a credit bureau in Malawi, study participants were told that their fingerprints and associated credit histories could be shared with other lenders. Since fingerprinting led to positive changes in borrower behavior, the paper underscores the borrowers' belief that improved identification will allow the lender to condition credit decisions on past

[7] As we emphasize below, we have used quite conservative implementation cost estimates, often based on our own field implementation costs. The benefit-cost ratio could be even more attractive in a full-scale implementation that spreads fixed costs over a larger volume of borrowers, particularly in the context of a credit bureau with many participating lenders.

credit performance. This is important, because it suggests how borrowers may respond to the introduction of a credit bureau.

A related paper is Karlan and Zinman (2009), henceforth "KZ", who find experimental evidence of moral hazard and weaker evidence of adverse selection in urban South Africa. KZ introduce a dynamic incentive by making future interest rates conditional on current loan repayment. Our experiment differs from KZ's in several key ways. First, our experiment manipulates the *credibility* of dynamic incentives, while KZ's experiment *informs* borrowers of the *existence* of a dynamic incentive. Second, our follow-up survey provides insight into the specific behaviors that the intervention affects and that result in higher repayment. KZ, by contrast, relies only on the lender's administrative data and so cannot shed light on what borrower behaviors may have changed. Third, the timing of our intervention relative to the borrowing decision differs. In KZ, the dynamic incentive is announced *after* clients have agreed to borrow (and all loan terms have been finalized), so differences in repayment can only be due to moral hazard. In our case, the intervention that improves dynamic incentives is revealed *before* agents decide to borrow. This makes it possible to examine changes in the composition of borrowers and in loan size. In addition, we estimate the more relevant policy parameter because potential borrowers cannot be repeatedly surprised.

We informed the lender which clubs had been fingerprinted, so the lender could have changed its behavior towards treated and control clubs. For example, loan officers could have spent more time to monitoring and enforcing repayment from control clubs, since treatment clubs were already subject to dynamic incentives. We provide evidence to the contrary: approval decisions and monitoring of clubs by loan officers did not differ across treated and control clubs. We therefore 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 (1999) uses a large-scale randomized trial of direct-mail pre-approved solicitations from a major US credit card company and 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 (2004) also develops a model of adverse selection and moral hazard and finds evidence consistent with both informational problems in the U.S.[8]

The paper is also related to a framed experiment conducted by Giné et al. (2010) 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 (2010) study the introduction of a credit bureau in Guatemala and find that it did contribute to efficiency in the credit market. The paper is also related to the literature on the recent rise in personal bankruptcies in the US (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 the intuition of 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-grower paprika scheme. MRFC is a

[8] Ligon (1998) implements empirical tests of the extent to which consumption allocations can be best described by permanent income, full information, or private information models, and finds that the private information model is most consistent with the data in two out of three ICRISAT villages in India. Paulson, Townsend, and Karaivanov (2006) estimate structurally competing models of credit markets in Thailand and find moral hazard to be important.

government-owned microfinance institution and provided financing for the in-kind loan package for 1/2 to 1 acre of paprika. Loaned funds were not disbursed in cash, but rather took the form of a credit at an agricultural input supplier for the financed production inputs. For further details on CP, MRFC, and the loan particulars, please see Online Appendix A.

At the time of the study, the vast majority of farmers in the sample had no access to formal-sector credit. In our baseline survey, only 6.7% of farmers had any formal loans in the previous year. Among these few farmers with formal-sector credit, MRFC was the largest single lender, providing 34% of loans (more than twice the share of the next largest lender).[9] Farmers therefore had a strong interest in maintaining good credit history with MRFC so as to maintain access to what would likely be their primary source of formal credit in the future.

In the absence of fingerprinting, farmer identification relies on the personal knowledge of loan officers. Loan officers do build up knowledge of borrowers over time, which allows MRFC to implement some dynamic incentives: it does attempt to withhold loans from past defaulters, and to reward reliable borrowers with increased loan amounts at lower interest rates. However, the identification "technology" based on personal loan officer knowledge is regarded as imperfect by top management at MRFC, who view the existing dynamic incentives as weak.[10] Loan officers are sometimes promoted and rotated to other localities. Among the 11 loan officers who were responsible for our study participants, the median number of years at the branch is only two, while the median number of years working for the lender is 13.[11] 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 large 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. Loan officers may also rely for identification on local informants,

[9] Across study areas, access to formal credit varies from 4% to 10%. In Dedza, the region with highest access to formal loans, MRFC provides almost half of these formal loans.

[10] While we do not have systematic evidence on past defaulters taking out new loans under false identities, an accumulation of anecdotes had convinced top management at MRFC and other institutions that this was a major obstacle in their effort to expand access to credit.

[11] Because soft information about borrowers is important, one may be surprised by the high loan officer turnover rate. MRFC, like other lenders, rotates credit officers for many reasons. For example, rotation is thought to improve morale and help minimize corruption. Promotion of successful individuals within the organization also leads to replacement of loan officers at the local level and some loss of soft information on borrowers.

local leaders, and other borrowing group members, but such methods are also imperfect because of the possibility of collusion against the lender among fellow villagers.

The timeline of the experiment is presented in Appendix Figure 1. Our study sample consists of 214 clubs with 3,206 farmers in Dedza, Mchinji, Dowa and Kasungu districts. 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. Randomization of treatment status was carried out after stratifying by locality and week of club visit.[12] Each loan officer is assigned to one locality. The stratification by locality and week of club visit thus ensured stratification by loan officer as well (i.e., each loan officer was responsible for roughly the same number of treatment and control clubs).

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.[13] Online Appendix A provides the script used during the training. See Online 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 computer was now able to identify an individual with only a fingerprint. One farmer was chosen at random to have his right thumb re-scanned, and the club was shown that the person's name and demographic information (entered earlier alongside the original fingerprint scan) 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,

[12] In other words, each unique combination of locality and week of initial club visit constituted a stratification cell, within which clubs were evenly divided randomly between treatment and control (or as close as possible to evenly divided, when there was an odd number of clubs in the stratification cell). There are 11 localities in the study, each of which was covered by one loan officer. The full sample of 214 clubs (3,206 farmers) was spread across 31 stratification (location-week) cells.

[13] Our team of enumerators encountered essentially no opposition to fingerprint collection.

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.[14]

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 Online Appendix C.[15]

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 214 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 sample of borrowers consists of 1,147 loan customers from 85 clubs, in 21 stratification (location-week) cells.[16] Loan packages had an average value of MK 16,913 (US$117).[17]

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 at this lender was not enforced. MRFC applies sanctions primarily on individual defaulters and not on other (non-defaulting) members of a borrowing group. In other words, an individual who repaid a previous loan could obtain a new loan even if other borrowers in the same group had failed to repay a past loan, as long as defaulters from the group were removed before the group applied for new loans.

[14] 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.
[15] To ensure that survey answers were not influenced by knowledge of the experiment or the respondent's treatment status, survey data were collected prior to the credit history education and fingerprinting intervention.
[16] 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. Furthermore, treated and untreated borrowers do not differ systematically on the basis of baseline characteristics. These points will be discussed in detail in the results section below.
[17] All conversions of Malawi kwacha to US dollars in this paper assume an exchange rate of MK145/US$, the average exchange rate at the time of the experiment.

During the months of July and August, farmers harvested the paprika crop and sold it to CP at predefined collection points. CP then transferred the proceeds from the sale to MRFC who then deducted the loan repayment and credited the remaining post-repayment proceeds to an individual farmer's savings account. This garnishing of the proceeds for loan repayment essentially allows MRFC to "seize" the paprika crop when farmers sell to CP (and for most farmers it is the only sales outlet).[18] Farmers could also make loan repayments directly to MRFC at their branch locations or during credit officer visits to their villages; this occurred, for example, among the small number of farmers who sold to paprika buyers other than CP. This channel of repayment is less desirable to MRFC because it is riskier.

We also implemented a follow-up survey of farmers in August 2008, once crops had been sold and income received. The sample size of this follow-up survey is 1,226 in total (borrowers plus non-borrowers), among whom 520 were borrowers.[19] 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 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, Online Appendix Table 1 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.

[18] Proceeds from other types of crops of course cannot be seized in this way to secure loan repayment because MRFC does not have analogous garnishing arrangements with other crop buyers.

[19] The 520 borrowers are spread across 17 stratification (location-week) cells. 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 Online Appendix Table 3), there is no evidence of selective attrition related to treatment status. For the full sample as well as the borrower subsample, in no regression is fingerprinting or fingerprinting interacted with predicted repayment statistically significantly associated with attrition from the survey.

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.[20]

## 3. A simple model of borrower behavior

Fingerprinting improves the personal identification of borrowers and thus increases the credibility of dynamic incentives used by the lender. To study how dynamic incentives affect borrower behavior, Online Appendix D develops a simple model that incorporates both adverse selection and moral hazard. We provide here an intuitive discussion of the model.

We assume that prospective borrowers have no liquid assets and decide how much to borrow for cash crop inputs, so the amount invested in production cannot exceed the loan amount. We introduce adverse selection by assuming that borrowers differ in the probability that production is successful, while moral hazard is modeled by allowing borrowers to divert the loan amount instead of investing it in production.[21] Consistent with the credit contract offered in the context of the experiment, we model a lender that offers a loan amount that can take on two values (depending on the number of fertilizer bags borrowed) and a gross interest rate. We also assume that when the smaller amount is borrowed, production can cover loan repayment even if it fails.

When personal identification of clients is not possible, borrowers can obtain a new loan even if they have defaulted in the past simply by using a different identity. As a result, lenders

[20] It turns out, however, that the regression results to come are not substantially affected by the inclusion in the regressions of the "male" indicator and other control variables (results not shown).

[21] 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 be clear, the production of paprika does not reduce moral hazard because paprika faces less *production* risk than other crops, but rather because it is less risky for *the lender*, given the lender's ability to confiscate paprika output for repayment of the loan.

are forced to offer the same one-season contract every period, as they cannot tailor the terms of the contract to individual credit histories.

By contrast, when personal identification is possible, the lender can use dynamic incentives, conditioning future credit on past repayment performance. 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 addition, by choosing the smaller loan amount they obtain lower net income in the first period in return for securing a loan in the future.

With this setup, the model predicts that dynamic incentives will have different effects on the optimal choices of borrowers depending on their probability of success. In particular, borrowers with relatively low probability of success are most affected by the introduction of dynamic incentives. They choose the higher loan amount and divert it all without dynamic incentives, but borrow the lower amount and invest it in cash crop production when dynamic incentives are introduced. Borrowers with the highest probabilities of success are the least affected: even without dynamic incentives, they never divert inputs and always choose the higher loan amount. Finally, borrowers with intermediate values of the probability of success will, upon introduction of dynamic incentives, change either the diversion or the loan size decisions (depending on parameter values and functional forms).

The model provides a reasonable structure for framing the empirical results to come. Its key advantage is a close adherence to the context of the experiment, in which the main simplifying assumptions (e.g., binary loan size and the lender's inability to seize non-cash-crop output) are reasonable. That said, our model may not be the only one that could be used to understand borrower behaviors in this and other contexts; other models may provide a different interpretation of the results. Therefore, our empirical results should be interpreted in the context of this specific model.

## 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) $Y_{ijs} = \alpha + \beta T_{js} + \gamma_s + \varepsilon_{ijs},$

where $Y_{ijs}$ = repayment outcome for individual $i$ in club $j$ in stratification cell $s$ (e.g., equal to 1 if repaying in full and on time, and 0 otherwise), $T_{js}$ is the treatment indicator (1 if fingerprinted and 0 if not), and $\gamma_s$ is a fixed effect for stratification cell $s$. $\varepsilon_{ijs}$ is a mean-zero error term. Treatment assignment at the club level creates spatial and other correlation among farmers within the same club, so standard errors must be clustered at the club level (Moulton 1986). Inclusion of the stratification cell fixed effects can reduce standard errors by absorbing residual variation.[22] The coefficient $\beta$ on the treatment indicator is the average treatment effect (ATE) of fingerprinting on the dependent variable.[23]

The point that $\beta$ in equation 1 is an average treatment effect is important, because we also devote attention to treatment effect heterogeneity. In particular, we are interested in the interaction between the randomized treatment and 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.[24] To test this question, we estimate regression equations of the following form:

(2) $$Y_{ijs} = \alpha + \rho(T_{js} * D_{ijs}) + \beta T_{js} + \chi D_{ijs} + \gamma_s + \varepsilon_{ijs},$$

$D_{ijs}$ is a variable representing the individual's predicted likelihood of repayment. The coefficient $\rho$ on the interaction term $T_{js} * D_{ijs}$ reveals the extent to which the impact of the treatment on repayment varies according to the borrower's predicted repayment. The main effect of predicted repayment, $D_{ijs}$, is included in the regression as well.

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

[22] Recall that stratification cells are defined by unique combinations of locality and week of initial club visit. By definition there are as close as possible to equal numbers of treatment and control clubs in each cell.

[23] Because we had perfect compliance with fingerprinting in the treatment group (and no fingerprinting in the control group), this happens to be a rare situation where $\beta$ is also the average treatment effect on the treated (ATT).

[24] 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 Online Appendix E 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.

characteristics (some of which may not be observable to the lender) and repayment among borrowers in the *control* (non-fingerprinted) group. Limiting the sample to borrowers in the control group (N=563), we run a regression of repayment (fraction of loan repaid by the September 30, 2008 due date) 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.

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 club visit) 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 age and education are specified as categorical variables (instead of being entered linearly).

We then take the coefficient estimates from column 3 of the table and predict the fraction of loan repaid for the *entire* sample (both control and treatment observations). 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.[25]

To investigate heterogeneity in the treatment effect, this index is either interacted linearly with the treatment indicator (as in equation 2), or it is converted into indicators for quintiles of the distribution of predicted repayment in the absence of fingerprinting and then interacted with treatment. For this analysis to be valid, it must be true that randomization leads to balance with respect to predicted repayment across treatment and control groups. This is indeed what we find.[26] In all regression results where the treatment indicator is interacted with predicted

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[25] In the loan-recipient subsample, predicted repayment has a mean of 0.79, with standard deviation 0.26. As expected, predicted repayment is highly skewed, with median predicted repayment of 0.90.

[26] In regressions of the treatment indicator on the continuous predicted repayment variable and indicators for stratification cells, the coefficient on predicted repayment always far from statistical significance at conventional levels in all samples used in this paper. In regressions of the treatment indicator on indicators for each quintile of repayment, the coefficients on the quintile dummies are individually and jointly insignificantly different from zero in all subsamples.

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 3 through 5 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 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 3 through 5 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

[27] For coefficients in regressions in the form of equation (2), we calculate standard errors 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. See Efron and Tibshirani (1993) for details.

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 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 retained or used this information. 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. Loan officers' knowledge about clubs (identity of club officers, number of loans) is not related to treatment status, and in fact loan officers do not appear to know the treatment status of clubs. Borrower reports of contact with loan officers are also uncorrelated with treatment. (For further details on this analysis, see Online Appendix E.) Given that loan officers do not appear to have responded to the treatment, we interpret impacts of the treatment 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 3 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

[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).

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.

It does appear that, conditional on borrowing, fingerprinted borrowers took out smaller loans. In Column 3 of Table 3, the dependent variable is the total amount borrowed in Malawi kwacha. Panel A indicates that loans of fingerprinted borrowers were MK 693 smaller than loans in the control group on average, a difference that is significant at the 10% level.

The patterns of coefficients in Panel C are suggestive that this effect is confined to borrowers in the lowest quintile of expected repayment. Differences between fingerprinted and non fingerprinted borrowers are small and not significant in quintiles two and above, but in quintile one, where fingerprinted borrowers take out loans that are smaller by MK 2,657 (roughly US$18) than those in the corresponding quintile in the control group, the difference is marginally significant (the t-statistic is 1.55).

While the absence of statistical significance in Panel C makes this just a suggestive result, the pattern is in accord with the theoretical model's prediction that the "worst" 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.

These results, while only suggestive, are consistent with fingerprinting reducing adverse selection in the credit market, albeit on a different margin than is usually discussed in the credit context. Existing research tends to emphasize that improved enforcement should lead low-quality borrowers to be excluded from borrowing entirely – i.e., the improvement of the borrower pool operates on the *extensive* margin. Our results are suggestive that low-quality borrowers choose smaller loan sizes, which leads the overall loan pool to be less weighted towards low-quality borrowers. 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 4-6 of Table 3 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 7-9) 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 coefficients in the regressions for outstanding balance and fraction paid on-time are statistically significant at the 10% level).

In Panel B, the fingerprinting-predicted repayment interaction term is statistically significantly different from zero (at the 5% or 1% level) in all regressions. The effect of fingerprinting on repayment is larger the lower is the borrower's ex ante likelihood of repayment. The fingerprint main effect and the "Predicted repayment * fingerprint" interaction term are jointly significantly different from zero at conventional levels (p-values reported in the bottom row of the table).

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 positive coefficient on the fingerprint-quintile 5 interaction for outstanding balance and the negative coefficient corresponding interaction term 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 MK7,249.27 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 32.7 percentage point increase in eventual fraction paid and the 40.8 percentage point increase in the likelihood of being eventually fully paid are also large relative to bottom quintile percentages of 81% and 68% respectively. Put another way, the fingerprinting-induced increase in repayment for the lowest quintile accounts for nearly the entire gap between repayment absent fingerprinting and full repayment.

### 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 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 Online 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.

Online Appendix Table 4 presents regression results for repayment outcomes that are analogous to those in columns 4-9 of main Table 3, 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 4 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), seven 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 (footnote 22), 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) and not other crops. By not producing paprika (or producing less), the borrower is better able to avoid repayment. Therefore, by improving the lender's dynamic incentives, fingerprinting may discourage such diversion of inputs and land to other crops.

While none of the effects of fingerprinting in Table 4 (either overall in Panel A or in interaction with predicted repayment in Panels B and C) are statistically significant at conventional levels, the point estimates provide 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 1.57) and positive, indicating that fingerprinting leads farmers to allocate 7.7 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

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

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.

Columns 1-7 of Table 5 examine 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.[30] In Panel C, the coefficient on the fingerprint-quintile 1 interaction is positive and significantly different from zero at the 1% 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 1.54). 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 5 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 5's results for input use on the paprika crop only (results are available from the authors on request). This suggests that in

[30] Joint tests, at the bottom of the table, indicate that the Panel B coefficients are jointly marginally significant for the fertilizer (col. 2) and all paid inputs (col. 5) regressions, and jointly significant (10% level) for man-days (col. 4).

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 MK6,566 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 8-10 of Table 5 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 11 presents regression results where the dependent variable is the natural log of agricultural profits.[31] The effect of fingerprinting in the bottom quintile of predicted repayment is positive but not statistically significant (t-statistic 1.21). Joint tests (reported at the bottom of the table) indicate that the Panel B coefficients are jointly significant at the 10% level for market crop sales and log agricultural profits.

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.

[31] For seven observations profits are zero or negative, and in these cases ln(profits) is replaced by 0. These observations do not drive the results; results are essentially identical when these observations are excluded.

## 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.

In the context of our theoretical model, we interpret these results as indicating that – for the farmers with the lowest ex ante likelihood of repayment – fingerprinting reduces 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).

In this section we summarize the results of additional robustness checks that are presented in greater detail in the Online Appendix. We then provide additional evidence that our results are not likely to reflect reductions in ex-*post* moral hazard. Finally, we report results of a test of the positive correlation property that reveals the presence of asymmetric information.

***Additional robustness checks***

Online Appendix F provides further detail on all analyses discussed below.

*Impact of fingerprinting in full sample*

Most results presented so far are for the subsample of farmers who took out a loan. We have argued that when restricting ourselves to this subsample, estimated treatment effects are not confounded by selection concerns because treatment has no statistically significant effect on selection into borrowing, either on average or in interaction with predicted repayment (Table 3, column 2). That said, one may raise a concern about statistical power: 95% confidence intervals around the point estimates in Table 3, column 2 admit non-negligible effects of treatment on selection into borrowing. The concern would be that there was in fact selection into borrowing in response to fingerprinting, which would cloud the interpretation of our results. For example, one might worry that that fingerprinting led borrowers in quintile 1 of predicted repayment to be on average different from control group borrowers in quintile 1 (along various observed and unobserved dimensions) in ways that make them more likely to repay, to devote land to paprika, and to use fertilizer on paprika.

Analyses of the full sample of farmers, without restricting the sample only to borrowers, can help address such concerns about selection bias. Estimated effects of treatment (and interactions with predicted repayment) would then represent effects of being fingerprinted on average across treated individuals, whether or not the individual took out a loan. While such an analysis makes little sense for outcomes specific to loans such as repayment (as in the outcomes in columns 4-9 of Table 3), we carry out this analysis for the other examined variables from the August 2008 follow-up survey, namely land use, input use, and profits (the outcomes in Tables 4 and 5).

As it turns out, full-sample regression results are very similar to those from the borrower-only regressions. The general pattern is for coefficients that were significant before to remain statistically significant, but to be only around half the magnitude of the coefficients in the borrowing sample regressions. This reduction in coefficient magnitude is consistent with effect sizes in the full sample representing a weighted average of no effects for non-borrowers and nonzero effects for borrowers (slightly less than half of individuals in the full sample are borrowers). We conclude that selection into borrowing is not driving the treatment effect estimates of Tables 4 and 5.

*Results with "simple" predicted repayment regression*

Results discussed so far on treatment effect heterogeneity construct the predicted repayment variable from the regression in column 3 of Table 2. The right-hand-side of this regression has farmer-level characteristics, as well as stratification cell (locality * week of initial club visit) fixed effects.

Because the baseline farmer-level characteristics listed in Table 2 are the most readily interpretable, we check the robustness of the results to constructing predicted repayment using only baseline farmer-level characteristics. The alternative predicted repayment regression is that of column 3 of Table 2, except that stratification cell fixed effects are dropped. This regression is then used to predict repayment for the full sample, and the predicted repayment variable is interacted with treatment to examine heterogeneity in the treatment effect.

Regression results are very similar when using this simpler index of predicted repayment. Overall, the general conclusion stands: fingerprinting has more substantial effects on repayment and activities on the farm for individuals with lower predicted repayment, even when repayment is predicted using only a restricted set of baseline farmer-level variables.

*Results where predicted repayment coefficients obtained from partition of control group*

In heterogeneous treatment effect results presented so far, there may be a concern that – for idiosyncratic reasons – control farmers in some geographic areas could have unusually low repayment rates compared to treatment farmers in the same areas. If this were the case, then the main analyses we have conducted so far might mechanically find a positive effect of treatment in cohorts where control group farmers had idiosyncratically low repayment rates.

We address this type of concern in two ways. First, we point to the robustness check just described above, where we find that results are very similar when the predicted repayment index is estimated without stratification cell fixed effects. These results reveal that the patterns of treatment effect heterogeneity we emphasize are not simply an artifact of inclusion of these (locality * week of initial club visit) fixed effects in the predicted repayment regression.

Second, we gauge the extent to which our main results diverge from those of an alternative approach that involves partitioning the control group into two parts: one part used to generate coefficients in the predicted repayment regression, and the other part used as a counterfactual for the treatment group in the main regressions. Because observations used to generate coefficients in the auxiliary predicted repayment regression are not then used as counterfactuals for the treatment observations, this approach avoids the possibility that our results arise mechanically from overfitting the repayment model.

Due to sampling variation, different randomly-determined partitions of the control group will yield different results, so we conduct this exercise 1,000 times and then examine the distribution of the regression coefficients generated. We focus our attention on coefficients on the interaction between the treatment indicator and the indicator for quintile 1 of predicted repayment (in Panel C) for the dependent variables of Tables 3 to 5.

We find that in all cases the quintile 1 interaction term coefficient falls within the 95 percent confidence interval of the coefficients generated in the partitioning exercise. Furthermore, whenever the interaction term coefficient is statistically significantly different from zero in Tables 3 to 5, the 95 percent confidence interval of the coefficients generated in the partitioning exercise does not include zero or coefficients of the opposite sign.

We therefore conclude that our main results are not mechanically driven by idiosyncratically low repayment among some control farmers in certain localities.

***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 "worst" 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 for repayment outcomes, but including controls for profits and loan size. Results are reported in Online Appendix Table 12.[32] 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.)

When controlling for loan size and profits, the effect of fingerprinting on on-time repayment for the worst borrowers declines in magnitude. For all key coefficients in columns 1-3 (those on the Panel B interaction term and the Panel C interaction with quintile 1), magnitudes fall substantially vis-à-vis corresponding estimates in Appendix Table 4. The tests of differences in these coefficients vis-à-vis those in Appendix Table 4, reported in the bottom of the table, indicate that the key coefficients are statistically significantly different when the controls for loan size and profits are included in the regression. That said, in the regression for "Balance, Sept. 30", the linear interaction term and the interaction term with quintile 1 of predicted repayment remain statistically significant at the 5% and 10% levels, respectively. The interaction term with quintile 1 is also significant at the 10% level in the "Fraction Paid by Sept. 30" regression.

Results for eventual repayment are less conclusive. 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 Online Appendix Table 4) are now uniformly smaller in magnitude and not statistically significantly different from zero. But significance tests at the bottom of the table indicate that for 5 out of 6 key coefficients in columns 4-6 (those on the Panel B interaction term

[32] We restrict to the N=520 sample because of the need to control for profits, which was only observed in the August 2008 survey. These results should be compared with Online Appendix Table 4, which is for the same sample.

and the Panel C interaction with quintile 1), we cannot reject the null that corresponding coefficients in Appendix Table 4 are the same.

These results suggest that when the dependent variable is on-time repayment, reductions in both ex-ante and ex-post moral hazard may be driving the increase in repayment: effects of fingerprinting for the worst borrowers remain statistically significant (or nearly so) in columns 1-3 even when controls for loan size and profits are included in the regression (suggesting a role for ex-post moral hazard), but are statistically significantly smaller in magnitude than when such controls are not included (consistent with the presence of ex-ante moral hazard). For eventual repayment (columns 4-6), this test is inconclusive because coefficients decline but are not statistically significantly different when loan size and profits controls are added to the regression.

***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 Appendix Figure 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 an increase 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. We conduct 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.

Under reasonable assumptions, total benefit per individual fingerprinted is MK490.50 (US$3.38). We consider three types of costs: equipment costs (which need to be amortized across all farmers fingerprinted), loan officer time costs, and transaction costs per fingerprint checked. Summing these 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 2.34. (Details of this calculation are in Online Appendix G.)

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.[33]

## 8. Conclusion

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

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.

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. In the context of a simple model of asymmetric information in a credit market, we interpret the treatment-induced reduction in loan size as a reduction in adverse selection, and the increase in agricultural input use as a decline in moral hazard.[34]

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' 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

[34] In practice, adverse selection and moral hazard may be more intertwined than is typically formulated in theoretical models (see Karlan and Zinman, 2009). In the context of this experiment, one could in principle, isolate the various asymmetric information problems by fingerprinting borrowers at different points in time along the loan cycle. 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.

lender, the credit contract terms could be made more attractive to borrowers, which may further improve repayment.[35]

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), ex-ante moral hazard (actions during the production season that may affect farm profits), and ex-post moral hazard (strategic or opportunistic default).[36]

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.[37] 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 empirical analysis, it is important to replicate this study in other contexts to gauge the external validity of the results. Our experiment was conducted in a context

[35] After learning about the benefits of biometric technology, MRFC applied for a grant from a donor agency to finance the purchase of handheld devices and software to mainstream the collection of biometric information from all its clients. OIBM, a competitor that operates in mostly urban areas, collects an electronic fingerprint from every borrower.
[36] But keep in mind that our results on ex-post moral hazard must be taken as merely suggestive.
[37] To be sure, this research sheds no light on the impact of microcredit availability on household well-being.

where there is currently no unique identification system and the credit market is still undeveloped. So while our findings might approximate impacts in other parts of rural sub-Saharan Africa with similar levels of economic and financial development, effects in other environments could be quite different. It would be important to gauge the extent to which impacts are different in populations that are – for example – more urban, more accessible to microcredit, and for which personal identification technologies (e.g., government-issued photo ID) have been implemented more widely. 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.

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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 |
| Balance, eventual | 2080.86 | 5663.98 | 0 | 0 | 9282 | 1147 |
| Fraction Paid, eventual | 0.89 | 0.29 | 0 | 1 | 1 | 1147 |
| Fully paid, eventual | 0.79 | 0.41 | 0 | 1 | 1 | 1147 |

**Table 2: Auxiliary regression predicting loan repayment**

| Dependent variable: | (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)*** |
| Locality * week of initial club visit 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.

**Table 3: Impact of fingerprinting on borrowing and repayment**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| Sample: | All Respondents | All Respondents | Loan Recipients | Loan recipients | Loan recipients | Loan recipients | Loan recipients | Loan recipients | Loan recipients |
| Dependent variable: | Approved | Any Loan | Total Borrowed (MK) | Balance, Sept. 30 | Fraction Paid by Sept. 30 | Fully Paid by Sept. 30 | Balance, Eventual | Fraction Paid, Eventual | Fully Paid, Eventual |
| **Panel A** | | | | | | | | | |
| Fingerprint | 0.045 | 0.056 | -692.743* | -1489.945* | 0.069* | 0.088 | -975.181 | 0.044 | 0.080 |
| | (0.054) | (0.045) | (381.745) | (836.931) | (0.041) | (0.066) | (762.090) | (0.037) | (0.061) |
| **Panel B** | | | | | | | | | |
| Fingerprint | 0.215 | 0.118 | -2872.348 | -15173.560*** | 0.719*** | 0.847*** | -9800.693** | 0.447** | 0.614*** |
| | (.161) | (.146) | (2438.851) | (2712.601) | (.108) | (.180) | (4150.1) | (.183) | (.225) |
| Predicted repayment * fingerprint | -0.220 | -0.081 | 2693.752 | 16987.019*** | -0.807*** | -0.942*** | 10958.377*** | -0.500** | -0.663** |
| | (.196) | (.169) | (2630.912) | (3010.827) | (.120) | (.199) | (4452.911) | (.196) | (.245) |
| **Panel C** | | | | | | | | | |
| Fingerprint * Quintile 1 | 0.099 | 0.081 | -2657.315 | -10844.701*** | 0.506*** | 0.549*** | -7249.271** | 0.327** | 0.408*** |
| | (.116) | (.113) | (1716.684) | (2622.283) | (.125) | (.144) | (2918.825) | (.135) | (.156) |
| Fingerprint * Quintile 2 | 0.191** | 0.113 | -357.168 | -1007.857 | 0.056 | 0.154 | -1006.419 | 0.057 | 0.165 |
| | (.096) | (.087) | (856.156) | (2033.207) | (.105) | (.165) | (1870.044) | (.098) | (.152) |
| Fingerprint * Quintile 3 | -0.022 | 0.057 | -585.469 | -275.604 | -0.001 | -0.007 | -261.204 | -0.003 | 0.002 |
| | (.083) | (.073) | (562.841) | (950.669) | (.048) | (.092) | (878.856) | (.044) | (.087) |
| Fingerprint * Quintile 4 | 0.004 | -0.032 | -198.714 | 812.241 | -0.040 | -0.064 | 701.779 | -0.032 | -0.041 |
| | (.088) | (.083) | (569.119) | (915.645) | (.044) | (.078) | (863.69) | (.042) | (.075) |
| Fingerprint * Quintile 5 | -0.009 | 0.044 | -234.098 | 1702.297* | -0.075* | -0.085 | 1429.524 | -0.060 | -0.051 |
| | (.088) | (.089) | (765.383) | (968.333) | (.044) | (.074) | (906.418) | (.041) | (.071) |
| **Observations** | 3206 | 3206 | 1147 | 1147 | 1147 | 1147 | 1147 | 1147 | 1147 |
| **Mean of dependent variable** | 0.63 | 0.35 | 16912.60 | 2912.91 | 0.84 | 0.74 | 2080.86 | 0.89 | 0.79 |
| Quintile 1 | 0.58 | 0.29 | 17992.53 | 6955.67 | 0.62 | 0.52 | 4087.04 | 0.81 | 0.68 |
| Quintile 2 | 0.64 | 0.36 | 17870.61 | 4024.05 | 0.77 | 0.63 | 3331.17 | 0.81 | 0.67 |
| Quintile 3 | 0.71 | 0.44 | 16035.10 | 1571.44 | 0.92 | 0.83 | 1301.79 | 0.93 | 0.84 |
| Quintile 4 | 0.70 | 0.47 | 15805.54 | 877.80 | 0.95 | 0.85 | 781.59 | 0.95 | 0.87 |
| Quintile 5 | 0.59 | 0.30 | 16886.56 | 1214.19 | 0.94 | 0.85 | 950.29 | 0.95 | 0.88 |
| **Joint significance of Panel B coefficients (p-value)** | 0.48 | 0.59 | 0.88 | 0.00 | 0.00 | 0.00 | 0.02 | 0.01 | 0.00 |

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 stratification cell (location * week of initial club visit) fixed effects. Panel B regressions include the main effect of the level of predicted repayment, and Panel C regressions include dummies for quintile of predicted repayment main effects. 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. The p-values in the bottom row are from tests that in Panel B, the fingerprinting main effect and the "Predicted repayment * fingerprint" interaction term are jointly statistically significantly different from zero.

**Table 4: Impact of fingerprinting on land us**

| 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 | -0.003 | 0.015 | -0.011 | -0.007 | 0.010 | -0.001 | -0.002 | -0.000 | 0.003 |
| | (0.020) | (0.019) | (0.016) | (0.016) | (0.014) | (0.003) | (0.003) | (0.001) | (0.020) |
| **Panel B** | | | | | | | | | |
| Fingerprint | -0.047 | -0.005 | -0.003 | -0.029 | 0.073 | 0.013 | 0.003 | -0.005 | 0.047 |
| | (.101) | (.087) | (.058) | (.064) | (.058) | (.009) | (.014) | (.004) | (.101) |
| Predicted repayment * fingerprint | 0.057 | 0.024 | -0.012 | 0.027 | -0.079 | -0.017 | -0.006 | 0.006 | -0.057 |
| | (.111) | (.098) | (.068) | (.068) | (.068) | (.012) | (.015) | (.005) | (.111) |
| **Panel C** | | | | | | | | | |
| Fingerprint * Quintile 1 | -0.087 | 0.002 | 0.005 | -0.007 | 0.077 | 0.008 | 0.004 | -0.003 | 0.087 |
| | (.074) | (.058) | (.050) | (.050) | (.049) | (.007) | (.013) | (.003) | (.074) |
| Fingerprint * Quintile 2 | 0.055 | 0.019 | -0.015 | -0.024 | -0.023 | -0.003 | -0.008 | -0.001 | -0.055 |
| | (.055) | (.041) | (.039) | (.0290) | (.036) | (.007) | (.008) | (.002) | (.055) |
| Fingerprint * Quintile 3 | -0.006 | -0.000 | -0.010 | -0.003 | 0.014 | 0.007 | -0.002 | -0.000 | 0.006 |
| | (.041) | (.043) | (.032) | (.021) | (.036) | (.007) | (.006) | (.002) | (.041) |
| Fingerprint * Quintile 4 | 0.005 | 0.013 | -0.022 | 0.003 | 0.003 | -0.003 | -0.001 | 0.003 | -0.005 |
| | (.041) | (.041) | (.035) | (.019) | (.037) | (.009) | (.007) | (.003) | (.041) |
| Fingerprint * Quintile 5 | 0.007 | 0.036 | -0.017 | -0.002 | -0.009 | -0.011 | -0.003 | -0.001 | -0.007 |
| | (.041) | (.037) | (.036) | (.022) | (.033) | (.008) | (.006) | (.002) | (.041) |
| **Observations** | 520 | 520 | 520 | 520 | 520 | 520 | 520 | 520 | 520 |
| **Mean of dependent variable** | 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 |
| **Joint significance of Panel B coefficients (p-value)** | 0.69 | 0.62 | 0.45 | 0.54 | 0.21 | 0.18 | 0.58 | 0.82 | 0.39 |

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 stratification cell (location * week of initial club visit) fixed effects. Panel B regressions include the main effect of the level of predicted repayment, and Panel C regressions include dummies for quintile of predicted repayment main effects. 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. The p-values in the bottom row are from tests that in Panel B, the fingerprinting main effect and the "Predicted repayment * fingerprint" interaction term are jointly statistically significantly different from zero. Sample limited to individuals who took out loans in 2008 and who were included in follow-up survey in 2009.

**Table 5**: **Impact of fingerprinting on agricultural inputs and profits**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Dependent variable: | Seeds (MK) | Fertilizer (MK) | Chemicals (MK) | Man-days (MK) | All Paid Inputs (MK) | KG Manure | Times Weeding | 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) |
| **Panel A** | | | | | | | | | | | |
| Fingerprint | 84.536 | 1037.378 | 357.103 | -408.599** | 1070.419 | 44.863 | 0.048 | 5808.270 | 3571.446 | 11457.127 | 0.043 |
| | (54.312) | (1297.753) | (219.533) | (188.581) | (1523.582) | (37.258) | (0.141) | (9376.512) | (10525.289) | (14071.809) | (0.094) |
| **Panel B** | | | | | | | | | | | |
| Fingerprint | 279.401** | 10953.768* | 424.276 | 425.804 | 12083.249* | 34.902 | 0.176 | 70072.212 | -33288.163 | 23141.194 | 0.682 |
| | (138.828) | (5856.494) | (510.020) | (484.087) | (6270.836) | (162.308) | (.450) | (55485.33) | (59698.16) | (78424.26) | (.426) |
| Predicted repayment * fingerprint | -243.229 | -12236.414* | -77.737 | -1043.976 | -13601.356* | 12.306 | -0.162 | -79154.870 | 42761.646 | -16899.862 | -0.793 |
| | (185.108) | (6503.934) | (673.749) | (663.119) | (7108.123) | (183.395) | (.570) | (57954.66) | (72680.72) | (91455.98) | (.495) |
| **Panel C** | | | | | | | | | | | |
| Fingerprint * Quintile 1 | 214.555*** | 5852.606 | 384.382 | 114.901 | 6566.444 | 56.139 | 0.406 | 32123.244 | 168.559 | 25730.854 | 0.434 |
| | (82.610) | (4058.444) | (339.435) | (207.522) | (4262.700) | (124.425) | (.329) | (39966.77) | (33675.88) | (53903.61) | (.359) |
| Fingerprint * Quintile 2 | 91.985 | 4241.768 | 260.137 | -206.938 | 4386.952 | 53.378 | -0.379 | 44570.113 | 11320.950 | 53763.203 | 0.249 |
| | (96.385) | (3043.436) | (400.035) | (443.448) | (3383.939) | (75.779) | (.314) | (36480.13) | (62585.49) | (71637.71) | (.260) |
| Fingerprint * Quintile 3 | 121.291 | -316.432 | 484.330 | -427.907 | -138.718 | 97.375 | -0.118 | -22828.231 | -21841.768 | -35149.036 | -0.242 |
| | (107.279) | (2332.776) | (449.485) | (485.596) | (2741.751) | (95.154) | (.320) | (17754.30) | (59742.76) | (63280.91) | (.218) |
| Fingerprint * Quintile 4 | -18.632 | -1315.729 | 201.476 | -973.256* | -2106.140 | -8.974 | -0.196 | -14121.491 | 19577.449 | 12923.138 | -0.036 |
| | (121.874) | (2501.755) | (457.487) | (532.087) | (3066.267) | (73.012) | (.328) | (14551.19) | (47941.73) | (50773.18) | (.218) |
| Fingerprint * Quintile 5 | 47.757 | -1874.942 | 431.041 | -417.203 | -1813.347 | 37.594 | 0.548 | -2035.855 | 6086.165 | 4342.320 | -0.078 |
| | (121.474) | (2343.55) | (438.313) | (561.762) | (2853.287) | (91.110) | (.362) | (14838.68) | (58414.17) | (61584.75) | (.230) |
| **Observations** | 520 | 520 | 520 | 520 | 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 | 65004.30 | 80296.97 | 117779.16 | 11.44 |
| Quintile 1 | 174.13 | 6721.24 | 401.30 | 143.48 | 7440.15 | 97.39 | 1.47 | 60662.57 | 82739.24 | 121222.50 | 11.36 |
| Quintile 2 | 140.00 | 6080.46 | 620.67 | 238.94 | 7080.08 | 39.25 | 1.55 | 89028.25 | 29995.27 | 91652.71 | 11.55 |
| Quintile 3 | 269.90 | 8927.65 | 674.48 | 836.98 | 10709.00 | 105.73 | 2.05 | 57683.74 | 96247.91 | 123242.30 | 11.44 |
| Quintile 4 | 292.07 | 7649.51 | 715.08 | 936.29 | 9592.95 | 93.23 | 2.24 | 61088.27 | 104927.50 | 136467.50 | 11.45 |
| Quintile 5 | 340.18 | 8078.58 | 892.05 | 1065.18 | 10375.99 | 118.13 | 2.28 | 56593.43 | 85817.08 | 115172.50 | 11.39 |
| ***Mean of dependent variable (US $)*** | 1.70 | 51.72 | 4.63 | 4.59 | 62.65 | n.a. | n.a. | 448.31 | 553.77 | 812.27 | n.a. |
| **Joint significance of Panel B coefficients (p-value)** | 0.19 | 0.14 | 0.44 | 0.07 | 0.13 | 0.47 | 0.35 | 0.08 | 0.56 | 0.20 | 0.08 |

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 stratification cell (location * week of initial club visit) fixed effects. Panel B regressions include the main effect of the level of predicted repayment, and Panel C regressions include dummies for quintile of predicted repayment main effects. 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. The p-values in the bottom row are from tests that in Panel B, the fingerprinting main effect and the "Predicted repayment * fingerprint" interaction term are jointly statistically significantly different from zero. Sample limited to individuals who took out loans in 2008 and who were included in follow-up survey in 2009.

**NOT FOR PUBLICATION**

**Online Appendix (to be posted on the web upon publication)**

for

**Credit Market Consequences of Improved Personal Identification: Field Experimental Evidence from Malawi**

## Appendix A: Background on project partners, loan details, and full text of training script

*Background on Cheetah Paprika (CP)*

Extension services provided by CP 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. The farmer receives extension services and a package of seeds, pesticides and fungicides at wholesale rates in exchange for the commitment to sell the paprika crop to CP at harvest time. Although CP is by far the largest paprika purchaser in the country, it does not provide credit to farmers because of the risks involved in contract enforcement.[1] 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.

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.[2] Most of these clubs were already in existence, primarily to ease delivery of Cheetah extension services and collection of the crop. 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.

*Background on MRFC and loan details*

MRFC is a government-owned microfinance institution and is the largest provider of rural finance, with a nationwide outreach of 210,000 borrowers in 2007. To obtain the loan-financed production inputs, 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. 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

[1] In 2007, CP purchased approximately eighty-five percent of the one thousand tons of paprika produced annually in Malawi.

[2] 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 some of the CP groups participating in the study had to be split to be able to access MRFC's loans.

the purchase of 0.4 kilograms of seeds.[3] 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.[4] 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).

### *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.[5]

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

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[3] 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.
[4] 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.
[5] 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).

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.

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: The Model

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) are risk-neutral and 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 $p$ (unobserved by the lender) 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 $f_j''(b) < 0$.

We model moral hazard by allowing borrowers to divert inputs instead of investing them in cash crop production. The decision to divert inputs is not observable by the lender. 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.[6]

Following the experiment, the credit contract offered by the lender is given by a loan amount $b$ and gross interest rate $R$, regardless of whether the lender can use dynamic incentives. We assume that the loan size $b$ can take on two values, $b_L$ and $b_H$ where $b_L < b_H$.[7] 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 assumption and the fact that $f_F(0) = 0$ 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

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[6] 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.

[7] 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.

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 $y_S(b_H) > y_S(b_L)$.[8]

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 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 is forced to offer 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 income, but if the borrower invests in paprika production, consumption only takes place when production is successful as the bank seizes all output if 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 Appendix Figure 2.

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

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

***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

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

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.

It is easy to see that 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 the lender can impose penalties. 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.[9]

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

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

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

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

or, after some algebra and substitutions,

$$p_{B1} = \frac{p_D}{p_D + 1 - p_{B0}}. \tag{4}$$

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 $p_D > p_{B0}$.[10] 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

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

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

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, while others with lower success probabilities will divert rather than produce. In this case we have $1 > p_D > p_{B1} > p_{B0}$,[11] 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.

Appendix Figure 2 is drawn assuming Case (ii) holds. It 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), Appendix Figure 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, and as mentioned in the text, 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.

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[11] 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).

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

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

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.[12]

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

[12] The observation that only a unique (pooling) contract exists may be used to rule out parameter combinations where the separating equilibrium is optimal. Of course, other considerations outside the model may be responsible for only observing one contract, even if it is sub-optimal. For example, before the study MRFC gave only a few loans for paprika and so it may still be learning about the optimal contract.

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).

The source of heterogeneity in the model is the probability of success $p$. If there was heterogeneity in the discount rate, then dynamic incentives would only be relevant for agents that are patient (ie with low enough discount rate). In this alternative model, if borrowers prefer to divert in the absence of dynamic incentives, repayment would be low without fingerprinting and would only increase for agents with low discount rate when fingerprinting is introduced.

In many multi-period models of limited commitment and asymmetric information, agents are not allowed to save because they could borrow and default and subsequently live in autarky by 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.

## Appendix E: Checking for loan officer responses to fingerprinting

In this appendix section we describe in further detail the findings that loan officers do not appear to have responded to whether or not a club was fingerprinted (summarized in Section 5.A. of the main text). Online Appendix Table 2 examines 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, we interpret impacts of the treatment as emerging solely from borrowers' responses to being fingerprinted.

## Appendix F: Additional robustness checks

### *Impact of fingerprinting in full sample*

Analyses of the full sample of farmers, without restricting the sample only to borrowers, can help address concerns about selection bias. Appendix Tables 5 and 6 present results from regressions analogous to the main Tables 4 and 5, respectively, with the difference that the regressions include *all* 1,226 individuals interviewed in the follow-up survey (borrowers plus nonborrowers).

Full-sample regression results in Appendix Tables 5 and 6 are very similar to those from the borrower-only regressions. As discussed in the main text, the general pattern is for coefficients that were significant before to remain statistically significant, but to be only around half the magnitude of the coefficients in the borrowing sample regressions. This reduction in coefficient magnitude is consistent with effect sizes in the full sample representing a weighted average of no effects for nonborrowers and nonzero effects for borrowers.

To be specific, in the land-use full-sample regressions (Appendix Table 5), fingerprinting leads farmers in quintile 1 of predicted repayment to devote 5.8 percentage points more of their land to paprika (significant at the 5% level). In the inputs regressions (Appendix Table 6, Columns 1-7), the interaction of fingerprinting with predicted repayment in Panel B is negative and significant at the 5% level in the regressions for fertilizer and all paid inputs, compared to significance at the 10% level in main Table 5. The fingerprinting * (quintile 1) interaction term is also positive and statistically significant at the 10% level or better for all input types in the table except for man-days. Results in the sales and profits regressions of Appendix Table 6, Columns 8-11 are similar to corresponding ones in main Table 5, but as before they are not statistically significantly different from zero.

### *Results with "simple" predicted repayment regression*

We discuss here robustness of treatment effect heterogeneity results to constructing the predicted repayment variable when excluding the locality*(week of initial club visit) fixed effects. Compared with the predicted repayment regression used in the main results (column 3, Table 2), when (locality)*(week of initial club visit) fixed effects are dropped the R-squared of the regression falls from 0.48 to 0.08. These alternative specifications are reported in Appendix Table 10.

This simpler regression is then used to predict repayment for the full sample, and the predicted repayment variable is interacted with treatment to examine heterogeneity in the treatment effect. Results from this exercise are presented in Appendix Tables 7 through 9, which should be compared (respectively) to the main Tables 3 through 5.

Results are very similar when using this simpler index of predicted repayment. For example, the coefficients on the interaction between linear predicted repayment and fingerprinting in Panel B remain large in magnitude and retain statistical significance in the repayment and inputs regressions (Appendix Table 7, Columns 4-9, and Appendix Table 9, Columns 1-7, respectively). In Panel C, where fingerprinting is interacted with quintiles of predicted repayment, a slight difference vis-à-vis previous results is that typically the significant interaction term is (fingerprinting)*(quintile 2) rather than the interaction with quintile 1. In sum, the general pattern that fingerprinting has more substantial effects on repayment and activities on the farm for individuals with lower predicted repayment is robust to using this simpler predicted repayment regression.

***Results where predicted repayment coefficients obtained from partition of control group***

This section describes our approach to estimating predicted repayment using a partition of the control group separate from a partition used as a counterfactual for the treatment group in the main regressions. We conduct this exercise 1,000 times, where in each replication we first randomly select 50% of the control group for inclusion in the auxiliary regression to predict repayment. We then predict repayment for the other half of the control group and the full treatment group. Finally, we estimate the heterogeneous effects of treatment on repayment, land use, input use, and farm profits using equation (2) on a sample that includes the full treatment group and the half of the control group not randomly chosen for the auxiliary regression.

We report the 95 percent confidence interval for coefficients obtained from this procedure in Appendix Table 11. We focus on results for the interaction between the treatment indicator and the indicator for quintile 1 of predicted repayment.[13] Panel A of Appendix Table 11 corresponds to Table 3, Columns 4-9; Panel B corresponds to Table 4; Panel C corresponds to Table 5, Columns 1-7, and Panel D corresponds to Table 5, Columns 8-11. The coefficient and standard error reported are the original estimates and bootstrap replications using the full sample, as described previously.

In every case, the coefficient from the estimate using the full sample falls within the 95 percent confidence interval from the procedure using the partitioned sample. Furthermore, in every case where the original coefficient is significant, all coefficients in the 95 percent confidence interval of the partitioning exercise have the same sign as the coefficient in the main regressions of the paper, and the confidence interval never includes zero.

## Appendix G: Details of benefit-cost calculation

The benefit-cost calculation is presented in Appendix Table 13. 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 32.7% of the loan balance (from column 8 of Table 3). We assume that the total amount to be repaid is MK15,000 on average. Total benefit per individual fingerprinted is therefore MK490.50 (US$3.38).

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

[13] The confidence interval is the 2.5$^{th}$ to 97.5$^{th}$ percentile of coefficients from the 1,000 replications.

MK101,500,[14] 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 MK281.30 (US$1.94), and the benefit-cost ratio is an attractive 2.34. [15]

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[14] 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).

[15] 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 Appendix Table 13'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.

**Appendix Table 1: Tests of balance in baseline characteristics between treatment and control group**
(For online appendix; not for publication)

| 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<br>(0.022) | 0.80 | -0.066*<br>(0.037) |
| Married | 0.92 | -0.004<br>(0.011) | 0.94 | 0.003<br>(0.016) |
| Age | 39.50 | 0.019<br>(0.674) | 39.96 | -0.088<br>(1.171) |
| Years of education | 5.27 | -0.046<br>(0.175) | 5.35 | -0.124<br>(0.272) |
| Risk taker | 0.57 | -0.033<br>(0.032) | 0.56 | 0.013<br>(0.051) |
| Days of hunger in previous season | 6.41 | -0.647<br>(0.832) | 6.05 | -0.292<br>(1.329) |
| Late paying previous loan | 0.14 | 0.005<br>(0.023) | 0.13 | 0.030<br>(0.032) |
| Standard deviation of past income | 25110.62 | 1289.190<br>(1756.184) | 27568.34 | -1158.511<br>(2730.939) |
| Years of experience growing paprika | 2.10 | 0.096<br>(0.142) | 2.22 | 0.299<br>(0.223) |
| Previous default | 0.03 | -0.002<br>(0.010) | 0.02 | 0.008<br>(0.010) |
| No previous loan | 0.74 | -0.006<br>(0.027) | 0.74 | -0.020<br>(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.

**Appendix Table 2: Impact of fingerprinting on loan officer knowledge and behavior**
(For online appendix; not for publication)

| | 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**
(For online appendix; not for publication)

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

| | (1) | (2) |
|---|---|---|
| Sample: | All respondents | Loan recipients |
| **Panel A** | | |
| Fingerprint | -0.062* | -0.092 |
| | (0.036) | (0.069) |
| **Panel B** | | |
| Fingerprint | -0.046 | -0.085 |
| | (.096) | (.167) |
| Predicted repayment * fingerprint | -0.021 | -0.008 |
| | (.118) | (.192) |
| **Panel C** | | |
| Fingerprint * Quintile 1 | -0.032 | -0.172 |
| | (.075) | (.129) |
| Fingerprint * Quintile 2 | -0.074 | 0.015 |
| | (.073) | (.107) |
| Fingerprint * Quintile 3 | -0.068 | -0.094 |
| | (.070) | (.107) |
| Fingerprint * Quintile 4 | -0.089 | -0.089 |
| | (.078) | (.124) |
| Fingerprint * Quintile 5 | -0.090 | -0.137 |
| | (.072) | (.125) |
| **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 stratification cell (location * week of initial club visit) fixed effects. Panel B regressions include the main effect of the level of predicted repayment, and Panel C regressions include dummies for quintile of predicted repayment main effects. 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**
**(Only borrowers responding to follow-up survey)**
(For online appendix; not for publication)

| | (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 | -1529.644* | 0.063 | 0.079 | -875.314 | 0.031 | 0.060 |
| | (884.322) | (0.043) | (0.069) | (670.297) | (0.032) | (0.057) |
| **Panel B** | | | | | | |
| Fingerprint | -15727.893*** | 0.713*** | 0.794*** | -8931.946* | 0.362 | 0.390 |
| | (3782.488) | (.196) | (.213) | (5162.708) | (.237) | (.257) |
| Predicted repayment * fingerprint | 17587.934*** | -0.805*** | -0.887*** | 10046.221* | -0.413* | -0.411 |
| | (4018.014) | (.206) | (.240) | (5446.717) | (.250) | (.284) |
| **Panel C** | | | | | | |
| Fingerprint * Quintile 1 | -12602.785*** | 0.573*** | 0.616*** | -8016.543* | 0.334* | 0.373* |
| | (3969.935) | (.190) | (.197) | (4382.064) | (.201) | (.205) |
| Fingerprint * Quintile 2 | 1538.937 | -0.094 | -0.069 | 1799.143 | -0.104 | -0.090 |
| | (2111.189) | (.110) | (.166) | (1857.158) | (.099) | (.151) |
| Fingerprint * Quintile 3 | -364.091 | 0.021 | 0.046 | -586.977 | 0.032 | 0.062 |
| | (891.085) | (.051) | (.101) | (792.850) | (.046) | (.095) |
| Fingerprint * Quintile 4 | 560.375 | -0.038 | -0.085 | 549.532 | -0.033 | -0.034 |
| | (762.879) | (.044) | (.103) | (707.901) | (.041) | (.096) |
| Fingerprint * Quintile 5 | 454.471 | -0.022 | 0.002 | 289.061 | -0.008 | 0.044 |
| | (814.791) | (.046) | (.104) | (674.962) | (.038) | (.090) |
| **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 stratification cell (location * week of initial club visit) fixed effects. Panel B regressions include the main effect of the level of predicted repayment, and Panel C regressions include dummies for quintile of predicted repayment main effects. 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 were included in follow-up survey in 2009.

**<u>Appendix Table 5</u>: Impact of fingerprinting on land use**
**(Full follow-up survey sample, including non-borrowers)**
(For online appendix; not for publication)

| <u>Dependent variable</u>: 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 |
|---|---|---|---|---|---|---|---|---|---|
| **<u>Panel A</u>** | | | | | | | | | |
| Fingerprint | -0.014 | -0.004 | -0.003 | -0.004 | 0.024** | 0.001 | -0.001 | 0.000 | 0.013 |
| | (0.013) | (0.012) | (0.009) | (0.009) | (0.011) | (0.002) | (0.002) | (0.001) | (0.013) |
| **<u>Panel B</u>** | | | | | | | | | |
| Fingerprint | -0.018 | -0.041 | 0.008 | -0.008 | 0.052 | 0.005 | 0.004 | -0.001 | 0.020 |
| | (.035) | (.034) | (.026) | (.031) | (.036) | (.004) | (.007) | (.001) | (.034) |
| Predicted repayment * fingerprint | 0.007 | 0.052 | -0.017 | 0.006 | -0.039 | -0.006 | -0.008 | 0.001 | -0.010 |
| | (.042) | (.040) | (.032) | (.035) | (.044) | (.005) | (.009) | (.002) | (.041) |
| **<u>Panel C</u>** | | | | | | | | | |
| Fingerprint * Quintile 1 | -0.009 | -0.039 | -0.003 | -0.010 | 0.058** | 0.003 | -0.000 | -0.000 | 0.009 |
| | (.029) | (.027) | (.020) | (.023) | (.024) | (.003) | (.006) | (.001) | (.029) |
| Fingerprint * Quintile 2 | -0.033 | 0.019 | 0.030 | 0.001 | -0.017 | -0.000 | -0.000 | -0.000 | 0.033 |
| | (.033) | (.031) | (.023) | (.023) | (.026) | (.005) | (.005) | (.001) | (.033) |
| Fingerprint * Quintile 3 | -0.007 | -0.019 | -0.016 | -0.010 | 0.051* | 0.003 | -0.004 | 0.001 | 0.007 |
| | (.029) | (.028) | (.021) | (.016) | (.027) | (.005) | (.005) | (.002) | (.029) |
| Fingerprint * Quintile 4 | -0.011 | 0.017 | -0.017 | 0.002 | 0.011 | -0.002 | -0.003 | 0.002 | 0.011 |
| | (.030) | (.030) | (.024) | (.017) | (.029) | (.006) | (.005) | (.003) | (.030) |
| Fingerprint * Quintile 5 | -0.000 | 0.027 | -0.024 | -0.009 | 0.006 | -0.003 | -0.002 | -0.001 | -0.005 |
| | (.033) | (.028) | (.023) | (.020) | (.028) | (.005) | (.004) | (.002) | (.033) |
| **Observations** | 1226 | 1226 | 1226 | 1226 | 1226 | 1226 | 1226 | 1226 | 1226 |
| **Mean of dependent variable** | 0.46 | 0.16 | 0.12 | 0.09 | 0.15 | 0.01 | 0.01 | 0.00 | 0.54 |
| Quintile 1 | 0.46 | 0.11 | 0.13 | 0.16 | 0.12 | 0.00 | 0.01 | 0.00 | 0.54 |
| Quintile 2 | 0.49 | 0.12 | 0.13 | 0.13 | 0.12 | 0.00 | 0.00 | 0.00 | 0.51 |
| Quintile 3 | 0.45 | 0.22 | 0.12 | 0.03 | 0.17 | 0.01 | 0.01 | 0.00 | 0.55 |
| Quintile 4 | 0.44 | 0.21 | 0.12 | 0.04 | 0.19 | 0.01 | 0.01 | 0.00 | 0.56 |
| Quintile 5 | 0.47 | 0.17 | 0.11 | 0.05 | 0.17 | 0.01 | 0.01 | 0.00 | 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 stratification cell (location * week of initial club visit) fixed effects. Panel B regressions include the main effect of the level of predicted repayment, and Panel C regressions include dummies for quintile of predicted repayment main effects. 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 were included in follow-up survey in 2009.

**Appendix Table 6: Impact of fingerprinting on agricultural inputs and profits**
**(Full follow-up survey sample, including non-borrowers)**
(For online appendix; not for publication)

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Dependent variable: | Seeds (MK) | Fertilizer (MK) | Chemicals (MK) | Man-days (MK) | All Paid Inputs (MK) | KG Manure | Times Weeding | 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) |
| **Panel A** | | | | | | | | | | | |
| Fingerprint | 60.585** | 920.128 | 282.100** | -136.780 | 1126.032 | 11.992 | 0.080 | 4228.220 | 4734.442 | 7570.113 | 0.039 |
| | (29.350) | (667.006) | (106.284) | (115.473) | (792.932) | (27.904) | (0.135) | (4962.283) | (23344.030) | (24615.887) | (0.079) |
| **Panel B** | | | | | | | | | | | |
| Fingerprint | 49.011 | 3977.114** | 296.011 | 272.076 | 4594.213** | 122.326 | 0.365 | 23198.561 | 28031.981 | 43860.629 | 0.255 |
| | (72.217) | (1687.829) | (220.025) | (210.036) | (1874.643) | (87.106) | (.328) | (16993.96) | (71967.95) | (76156.27) | (.215) |
| Predicted repayment * fingerprint | 15.221 | -4271.866** | -19.358 | -577.541* | -4853.543** | -154.387 | -0.400 | -26665.392 | -33873.351 | -52029.370 | -0.305 |
| | (94.333) | (2099.599) | (295.322) | (305.778) | (2400.958) | (105.886) | (.439) | (20686.96) | (80794.67) | (87493.76) | (.245) |
| **Panel C** | | | | | | | | | | | |
| Fingerprint * Quintile 1 | 113.462*** | 2636.880** | 264.742* | 147.140 | 3162.224** | 118.604* | 0.468* | 9768.595 | 52077.286 | 57907.978 | 0.129 |
| | (44.024) | (1304.867) | (142.305) | (121.034) | (1415.625) | (70.055) | (.251) | (13316.04) | (74406.57) | (77360.26) | (.171) |
| Fingerprint * Quintile 2 | 50.930 | 1956.152 | 285.580 | -115.827 | 2176.834 | -49.929 | -0.368 | 24668.058 | -39885.974 | -21083.297 | 0.197 |
| | (59.562) | (1453.069) | (212.523) | (280.486) | (1635.342) | (69.820) | (.317) | (18019.75) | (76408.28) | (79611.89) | (.216) |
| Fingerprint * Quintile 3 | 86.537 | -593.742 | 353.570 | -237.639 | -391.274 | -56.392 | -0.123 | -20898.037 | -9621.667 | -25345.653 | -0.193 |
| | (67.608) | (1452.306) | (251.477) | (311.853) | (1701.534) | (73.851) | (.317) | (13647.96) | (51298.79) | (54260.54) | (.186) |
| Fingerprint * Quintile 4 | 6.885 | -1049.852 | 250.558 | -573.530 | -1365.938 | -32.321 | -0.056 | -4020.628 | 5859.113 | 5128.222 | 0.019 |
| | (78.286) | (1612.908) | (273.541) | (370.818) | (2028.824) | (68.537) | (.346) | (12271.19) | (41342.84) | (44092.51) | (.190) |
| Fingerprint * Quintile 5 | 76.413 | 85.736 | 305.314 | -157.983 | 309.481 | 16.648 | 0.352 | 1890.188 | -5784.984 | -7574.496 | -0.050 |
| | (79.747) | (1474.584) | (240.184) | (327.777) | (1796.701) | (80.069) | (.336) | (12186.01) | (56867.20) | (61201.66) | (.177) |
| **Observations** | 1226 | 1226 | 1226 | 1226 | 1226 | 1226 | 1226 | 1226 | 1226 | 1226 | 1226 |
| **Mean of dependent variable** | 185.56 | 3948.93 | 362.92 | 396.56 | 4893.98 | 83.63 | 1.54 | 53965.29 | 86793.08 | 119870.13 | 11.28 |
| Quintile 1 | 129.13 | 2335.99 | 182.64 | 152.25 | 2800.01 | 85.47 | 1.20 | 48912.14 | 103543.10 | 138101.00 | 11.23 |
| Quintile 2 | 132.03 | 2924.01 | 277.92 | 178.55 | 3512.50 | 59.29 | 1.28 | 70582.23 | 60989.97 | 109699.20 | 11.33 |
| Quintile 3 | 198.08 | 5481.54 | 426.67 | 593.28 | 6699.57 | 125.01 | 1.78 | 44931.14 | 86190.55 | 108497.00 | 11.27 |
| Quintile 4 | 237.16 | 5837.92 | 543.52 | 726.95 | 7345.56 | 83.53 | 1.91 | 54127.28 | 98467.02 | 125928.20 | 11.34 |
| Quintile 5 | 239.52 | 4786.95 | 516.97 | 579.91 | 6123.35 | 80.35 | 1.83 | 47991.75 | 84126.01 | 109740.80 | 11.26 |
| ***Mean of dependent variable (US $)*** | 1.28 | 27.23 | 2.50 | 2.73 | 33.75 | n.a. | n.a. | 372.17 | 598.57 | 826.69 | 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 stratification cell (location * week of initial club visit) fixed effects. Panel B regressions include the main effect of the level of predicted repayment, and Panel C regressions include dummies for quintile of predicted repayment main effects. 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 were included in follow-up survey in 2009.

**Appendix Table 7: Impact of fingerprinting on borrowing and repayment**
**(simple predicted repayment regression)** (For online appendix; not for publication)

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| Sample: | All Respondents | All Respondents | Loan Recipients | Loan recipients | Loan recipients | Loan recipients | Loan recipients | Loan recipient | Loan recipients |
| Dependent variable: | Approved | Any Loan | Total Borrowed (MK) | Balance, Sept. 30 | Fraction Paid by Sept. 30 | Fully Paid by Sept. 30 | Balance, Eventual | Fraction Paid, Eventual | Fully Paid, Eventual |
| **Panel A** | | | | | | | | | |
| Fingerprint | 0.045 | 0.056 | -692.743* | -1489.945* | 0.069* | 0.088 | -975.181 | 0.044 | 0.080 |
| | (0.054) | (0.045) | (381.745) | (836.931) | (0.041) | (0.066) | (762.090) | (0.037) | (0.061) |
| **Panel B** | | | | | | | | | |
| Fingerprint | -0.064 | 0.012 | -717.084 | -11562.473*** | 0.570*** | 0.654*** | -7303.437** | 0.342** | 0.423* |
| | (.151) | (.146) | (2351.208) | (3481.01) | (.168) | (.243) | (3428.208) | (.169) | (.230) |
| Predicted repayment * fingerprint | 0.135 | 0.054 | 30.107 | 12415.234*** | -0.618*** | -0.698*** | 7800.066** | -0.367** | -0.423* |
| | (.171) | (.176) | (2656.956) | (3947.51) | (.185) | (.261) | (3817.025) | (.185) | (.246) |
| **Panel C** | | | | | | | | | |
| Fingerprint * Quintile 1 | 0.023 | 0.069 | 125.465 | -2550.686* | 0.138* | 0.147 | -1258.495 | 0.065 | 0.086 |
| | (.073) | (.062) | (837.873) | (1494.379) | (.076) | (.101) | (1454.586) | (.074) | (.097) |
| Fingerprint * Quintile 2 | 0.036 | 0.041 | -1193.165* | -3306.017** | 0.149** | 0.204** | -2516.761* | 0.120* | 0.178* |
| | (.070) | (.063) | (699.703) | (1538.999) | (.075) | (.102) | (1456.644) | (.071) | (.100) |
| Fingerprint * Quintile 3 | 0.076 | 0.032 | -1790.115*** | -1819.843 | 0.060 | 0.110 | -1190.697 | 0.026 | 0.112 |
| | (.070) | (.068) | (673.809) | (1259.105) | (.065) | (.100) | (1198.203) | (.061) | (.096) |
| Fingerprint * Quintile 4 | 0.031 | 0.053 | -311.359 | -391.905 | 0.026 | 0.003 | -423.401 | 0.028 | 0.039 |
| | (.070) | (.063) | (663.88) | (1089.182) | (.054) | (.085) | (969.012) | (.048) | (.075) |
| Fingerprint * Quintile 5 | 0.054 | 0.085 | -263.503 | 337.027 | -0.013 | -0.010 | 304.142 | -0.006 | -0.002 |
| | (.070) | (.068) | (590.087) | (979.543) | (.044) | (.072) | (893.653) | (.040) | (.068) |
| **Observations** | 3277 | 3277 | 1147 | 1147 | 1147 | 1147 | 1147 | 1147 | 1147 |
| **Mean of dependent variable** | 0.63 | 0.35 | 16912.60 | 2912.91 | 0.84 | 0.74 | 2080.86 | 0.89 | 0.79 |
| Quintile 1 | 0.58 | 0.29 | 17992.53 | 6955.67 | 0.62 | 0.52 | 4087.04 | 0.81 | 0.68 |
| Quintile 2 | 0.64 | 0.36 | 17870.61 | 4024.05 | 0.77 | 0.63 | 3331.17 | 0.81 | 0.67 |
| Quintile 3 | 0.71 | 0.44 | 16035.10 | 1571.44 | 0.92 | 0.83 | 1301.79 | 0.93 | 0.84 |
| Quintile 4 | 0.70 | 0.47 | 15805.54 | 877.80 | 0.95 | 0.85 | 781.59 | 0.95 | 0.87 |
| Quintile 5 | 0.59 | 0.30 | 16886.56 | 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 stratification cell (location * week of initial club visit) fixed effects. The auxiliary regression used to calculate predicted repayment uses only baseline characteristics and no stratification cell (locality*week) fixed effects. Panel B regressions include the main effect of the level of predicted repayment, and Panel C regressions include dummies for quintile of predicted repayment main effects. 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 8: Impact of fingerprinting on land use**
**(simple predicted repayment regression)** (For online appendix; not for publication)

| 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 | -0.003 | 0.015 | -0.011 | -0.007 | 0.010 | -0.001 | -0.002 | -0.000 | 0.003 |
| | (0.020) | (0.019) | (0.016) | (0.016) | (0.014) | (0.003) | (0.003) | (0.001) | (0.020) |
| **Panel B** | | | | | | | | | |
| Fingerprint | -0.175 | -0.045 | 0.058 | 0.032 | 0.093 | 0.023 | 0.018 | -0.005 | 0.175 |
| | (.131) | (.086) | (.088) | (.068) | (.087) | (.017) | (.017) | (.006) | (.131) |
| Predicted repayment * fingerprint | 0.210 | 0.072 | -0.085 | -0.048 | -0.102 | -0.029 | -0.025 | 0.006 | -0.210 |
| | (.144) | (.096) | (.102) | (.076) | (.101) | (.020) | (.020) | (.007) | (.144) |
| **Panel C** | | | | | | | | | |
| Fingerprint * Quintile 1 | -0.015 | 0.009 | -0.006 | -0.001 | 0.013 | 0.000 | 0.002 | -0.003 | 0.015 |
| | (.060) | (.043) | (.035) | (.032) | (.035) | (.006) | (.007) | (.003) | (.060) |
| Fingerprint * Quintile 2 | -0.048 | -0.008 | -0.048 | 0.008 | 0.087*** | 0.010 | 0.001 | -0.001 | 0.048 |
| | (.051) | (.042) | (.035) | (.032) | (.034) | (.008) | (.007) | (.004) | (.051) |
| Fingerprint * Quintile 3 | 0.011 | 0.005 | 0.034 | -0.013 | -0.030 | -0.004 | -0.003 | -0.001 | -0.011 |
| | (.046) | (.040) | (.034) | (.028) | (.039) | (.008) | (.007) | (.003) | (.046) |
| Fingerprint * Quintile 4 | 0.016 | 0.025 | 0.014 | -0.029 | -0.021 | -0.002 | -0.005 | 0.002 | -0.016 |
| | (.037) | (.035) | (.032) | (.022) | (.035) | (.008) | (.006) | (.002) | (.037) |
| Fingerprint * Quintile 5 | 0.019 | 0.029 | -0.052* | 0.004 | 0.010 | -0.006 | -0.004 | 0.000 | -0.019 |
| | (.032) | (.033) | (.031) | (.020) | (.030) | (.007) | (.005) | (.001) | (.032) |
| **Observations** | 520 | 520 | 520 | 520 | 520 | 520 | 520 | 520 | 520 |
| **Mean of dependent variable** | 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 stratification cell (location * week of initial club visit) fixed effects. The auxiliary regression used to calculate predicted repayment uses only baseline characteristics and no stratification cell (locality*week) fixed effects. Panel B regressions include the main effect of the level of predicted repayment, and Panel C regressions include dummies for quintile of predicted repayment main effects. 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 were included in follow-up survey in 2009.

**Appendix Table 9: Impact of fingerprinting on agricultural inputs and profits (simple predicted repayment regression)** (For online appendix; not for publication)

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Dependent variable: | Seeds (MK) | Fertilizer (MK) | Chemicals (MK) | Man-days (MK) | All Paid Inputs (MK) | KG Manure | Times Weeding | 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) |
| **Panel A** | | | | | | | | | | | |
| Fingerprint | 84.536 | 1037.378 | 357.103 | -408.599** | 1070.419 | 44.863 | 0.048 | 5808.270 | 3571.446 | 11457.127 | 0.043 |
| | (54.312) | (1297.753) | (219.533) | (188.581) | (1523.582) | (37.258) | (0.141) | (9376.512) | (10525.289) | (14071.809) | (0.094) |
| **Panel B** | | | | | | | | | | | |
| Fingerprint | 282.642 | 14092.032** | 800.605 | 391.334 | 15566.614** | 101.494 | 0.421** | 125502.791 | -1179.320 | 96410.326 | 1.257** |
| | (253.064) | (5590.066) | (940.826) | (1444.871) | (6469.535) | (194.348) | (.878) | (62693.17) | (147854.1) | (168129.6) | (.638) |
| Predicted repayment * fingerprint | -241.402 | -15850.798** | -537.673 | -972.848 | -17602.722** | -68.915 | -0.455** | -145347.128 | 5848.375 | -103103.149 | -1.476** |
| | (320.704) | (6394.114) | (1106.397) | (1728.811) | (7558.463) | (231.059) | (1.038) | (68584.33) | (176461.3) | (199752.9) | (.751) |
| **Panel C** | | | | | | | | | | | |
| Fingerprint * Quintile 1 | 205.670** | 2417.000 | 644.153* | -336.456 | 2930.366 | -7.355 | 0.018 | 23135.413 | -6510.534 | 11781.684 | 0.012 |
| | (90.561) | (2457.178) | (351.481) | (449.350) | (2788.066) | (72.893) | (.341) | (24824.16) | (41730.85) | (51026.79) | (.243) |
| Fingerprint * Quintile 2 | 204.141** | 6126.022** | 446.949 | -130.578 | 6646.533** | 125.404 | 0.181 | 35330.984 | -3565.416 | 22466.436 | 0.491* |
| | (103.150) | (2513.384) | (358.592) | (557.157) | (2844.721) | (85.541) | (.320) | (23834.69) | (48903.95) | (55138.88) | (.266) |
| Fingerprint * Quintile 3 | -80.495 | 631.003 | 350.814 | -666.700 | 234.622 | 50.845 | 0.072 | -6890.835 | 64018.007 | 67407.944 | 0.193 |
| | (108.814) | (2508.503) | (407.558) | (591.505) | (2912.471) | (77.011) | (.332) | (22716.73) | (55893.06) | (61100.77) | (.236) |
| Fingerprint * Quintile 4 | 6.115 | -1516.096 | 192.879 | -316.185 | -1633.287 | 19.131 | 0.053 | -5737.961 | -70057.413 | -71136.598 | -0.156 |
| | (102.324) | (2285.521) | (448.384) | (539.082) | (2722.922) | (75.269) | (.311) | (17755.40) | (66463.94) | (70012.55) | (.235) |
| Fingerprint * Quintile 5 | 114.650 | -644.719 | 306.239 | -571.535 | -795.364 | 26.469 | -0.011 | -8414.109 | 28216.613 | 25251.358 | -0.215 |
| | (122.226) | (2285.481) | (407.954) | (545.700) | (2842.941) | (80.906) | (.299) | (14952.61) | (57408.41) | (61067.75) | (.208) |
| **Observations** | 520 | 520 | 520 | 520 | 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 | 65004.30 | 80296.97 | 117779.16 | 11.44 |
| Quintile 1 | 174.13 | 6721.24 | 401.30 | 143.48 | 7440.15 | 97.39 | 1.47 | 60662.57 | 82739.24 | 121222.50 | 11.36 |
| Quintile 2 | 140.00 | 6080.46 | 620.67 | 238.94 | 7080.08 | 39.25 | 1.55 | 89028.25 | 29995.27 | 91652.71 | 11.55 |
| Quintile 3 | 269.90 | 8927.65 | 674.48 | 836.98 | 10709.00 | 105.73 | 2.05 | 57683.74 | 96247.91 | 123242.30 | 11.44 |
| Quintile 4 | 292.07 | 7649.51 | 715.08 | 936.29 | 9592.95 | 93.23 | 2.24 | 61088.27 | 104927.50 | 136467.50 | 11.45 |
| Quintile 5 | 340.18 | 8078.58 | 892.05 | 1065.18 | 10375.99 | 118.13 | 2.28 | 56593.43 | 85817.08 | 115172.50 | 11.39 |
| ***Mean of dependent variable (US $*** | n.a. | n.a. | n.a. | n.a. | n.a. | n.a. | n.a. | 464.32 | 573.55 | 841.28 | n.a. |

Stars indicate significance at 10% (*), 5% (**), and 1% (***)
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 stratification cell (location * week of initial club visit) fixed effects. The auxiliary regression used to calculate predicted repayment uses only baseline characteristics and no stratification cell (locality*week) fixed effects. Panel B regressions include the main effect of the level of predicted repayment, and Panel C regressions include dummies for quintile of predicted repayment main effects. 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 were included in follow-up survey in 2009.

**<u>Appendix Table 10</u>: Auxiliary regression predicting loan repayment, no fixed effects**
(For online appendix; not for publication)

| | (1) | (2) |
|---|---|---|
| Dependent variable: | Fraction Paid by Sept. 30 | Fraction Paid by Sept. 30 |
| Male | 0.080 | 0.074 |
| | (0.073) | (0.071) |
| Married | -0.071 | -0.080 |
| | (0.060) | (0.065) |
| Age | 0.004 | |
| | (0.001)*** | |
| Years of education | -0.005 | |
| | (0.005) | |
| Risk taker | -0.078 | -0.072 |
| | (0.041)* | (0.043)* |
| Days of Hunger in previous season | 0.001 | 0.000 |
| | (0.002) | (0.001) |
| Late paying previous loan | -0.058 | -0.045 |
| | (0.071) | (0.067) |
| Standard deviation of past income | -0.000 | -0.000 |
| | (0.000) | (0.000) |
| Years of experience growing paprika | 0.005 | 0.004 |
| | (0.013) | (0.012) |
| Previous default | 0.088 | 0.062 |
| | (0.163) | (0.169) |
| No previous loan | -0.012 | -0.009 |
| | (0.062) | (0.061) |
| Constant | 0.729 | 1.006 |
| | (0.114)*** | (0.108)*** |
| Locality * week of initial loan offer fixed effects | -- | -- |
| Dummy variables for 5-year age groups | -- | Y |
| Dummy variables for each year of education | -- | Y |
| Observations | 563 | 563 |
| R-squared | 0.05 | 0.08 |

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 11: 95% confidence interval of Q1*Treatment interaction term from partitioning exercise**
(For online appendix; not for publication)

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| ***Panel A: Corresponds to Table 3, Col. 4-9*** | | | | | | | | | |
| Dependent variable: | Balance, Sept. 30 | Fraction Paid by Sept. 30 | Fully Paid by Sept. 30 | Balance, Eventual | Fraction Paid, Eventual | Fully Paid, Eventual | | | |
| Coefficient: Fingerprint * Quintile 1 | -10844.701*** | 0.506*** | 0.549*** | -7249.271** | 0.327** | 0.408*** | | | |
| Bootstrapped standard error | (2622.283) | (.125) | (.144) | (2918.825) | (.135) | (.156) | | | |
| 95 percent confidence interval using half of control group in 1st stage | [-11284.34, -5382.259] | [.271, .527] | [.301, .623] | [-7999.921, -2937.949] | [.138, .37] | [.187, .519] | | | |
| ***Panel B: Corresponds to Table 4*** | | | | | | | | | |
| Dependent variable: Fraction of land used for... | Maize | Soya/Beans | Groundnuts | Tobacco | Paprika | Tomatoes | Leafy Vegetables | Cabbage | All cash crops |
| Coefficient: Fingerprint * Quintile 1 | -0.087 | 0.002 | 0.005 | -0.007 | 0.077 | 0.008 | 0.004 | -0.003 | 0.087 |
| Bootstrapped standard error | (.074) | (.058) | (.050) | (.050) | (.049) | (.007) | (.013) | (.003) | (.074) |
| 95 percent confidence interval using half of control group in 1st stage | [-.177, .041] | [-.043, .073] | [-.056, .068] | [-.097, .06] | [-.002, .124] | [-.003, .012] | [-.015, .008] | [-.008, .001] | [-.041, .177] |
| ***Panel C: Corresponds to Table 5, Col. 1-7*** | | | | | | | | | |
| Dependent variable: | Seeds (MK) | Fertilizer (MK) | Chemicals (MK) | Man-days (MK) | All Paid Inputs (MK) | KG Manure | Times Weeding | | |
| Coefficient: Fingerprint * Quintile 1 | 214.555*** | 5852.606 | 384.382 | 114.901 | 6566.444 | 56.139 | 0.406 | | |
| Bootstrapped standard error | (82.610) | (4058.444) | (339.435) | (207.522) | (4262.700) | (124.425) | (.329) | | |
| 95 percent confidence interval using half of control group in 1st stage | [99.4, 288.319] | [1066.13, 9849.334] | [91.782, 955.366] | [-160.601, 222.7] | [1482.077, 10720.66] | [-106.88, 158.155] | [-.184, .848] | | |
| ***Panel D: Corresponds to Table 5, Col. 8-11*** | | | | | | | | | |
| Dependent variable: | 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) | | | | | |
| Coefficient: Fingerprint * Quintile 1 | 32123.244 | 168.559 | 25730.854 | 0.434 | | | | | |
| Bootstrapped standard error | (39966.77) | (33675.88) | (53903.61) | (.359) | | | | | |
| 95 percent confidence interval using half of control group in 1st stage | [3835.673, 96839.79] | [-127201.3, 46065.56] | [-91420.59, 112410.6] | [-.072, .98] | | | | | |

Notes: The coefficients reported in this table are for the interaction between the indicator for being in the bottom quintile of predicted repayment and being assigned to have a fingerprint collected when applying for a loan. They correspond to the coefficients for bottom quintile in Panel C of Tables 3, 4, and 5. The standard errors are the bootstrapped standard errors reported in those tables. The confidence intervals are from 1000 replications of each regression where one half of the control group was randomly chosen for inclusion in the first stage regression, and the remaining half of the control group plus the full treatment group was preserved for inclusion in the second stage regression.

**Appendix Table 12: 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 | -266.318 | 0.010 | 0.001 | 201.469 | -0.012 | -0.003 |
| | (768.031) | (0.040) | (0.066) | (559.701) | (0.030) | (0.052) |
| **Panel B** | | | | | | |
| Fingerprint | -9659.780* | 0.422 | 0.412 | -5190.059 | 0.198 | 0.207 |
| | (4698.647) | (.237) | (.298) | (5140.761) | (.237) | (.280) |
| Predicted repayment * fingerprint | 11231.990** | -0.493 | -0.493 | 6443.577 | -0.251 | -0.252 |
| | (4903.271) | (.246) | (.326) | (5378.012) | (.247) | (.308) |
| **Panel C** | | | | | | |
| Fingerprint * Quintile 1 | -8276.641* | 0.372* | 0.333 | -5247.917 | 0.221 | 0.240 |
| | (4308.034) | (.204) | (.246) | (4236.123) | (.189) | (.220) |
| Fingerprint * Quintile 2 | 2691.144 | -0.150 | -0.159 | 2402.793 | -0.135 | -0.138 |
| | (2204.118) | (.115) | (.164) | (1919.401) | (.102) | (.148) |
| Fingerprint * Quintile 3 | -410.867 | 0.031 | 0.042 | -210.122 | 0.021 | 0.037 |
| | (1038.583) | (.058) | (.105) | (910.565) | (.052) | (.096) |
| Fingerprint * Quintile 4 | 432.743 | -0.017 | -0.073 | 650.240 | -0.026 | -0.067 |
| | (872.418) | (.050) | (0.104) | (818.118) | (.047) | (.095) |
| Fingerprint * Quintile 5 | 361.559 | -0.012 | -0.003 | 614.725 | -0.021 | 0.015 |
| | (1004.290) | (.055) | (.115) | (857.018) | (.046) | (.096) |
| **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 |
| **Comparison to Appendix Table 4** | | | | | | |
| Difference in Panel B interaction terms | 6355.944** | -0.312** | -0.394* | 4019.440 | -0.197* | -0.224 |
| | (2852.902) | (.137) | (.205) | (2485.785) | (.116) | (.183) |
| Difference in Quintile 1 interaction terms | -4326.144** | 0.201** | 0.284** | -2836.847 | 0.128 | 0.163 |
| | (2050.553) | (.100) | (.139) | (1758.450) | (.084) | (.127) |

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 stratification cell (location * week of initial club visit) fixed effects. Panel B regressions include the main effect of the level of predicted repayment, and Panel C regressions include dummies for quintile of predicted repayment main effects. 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.

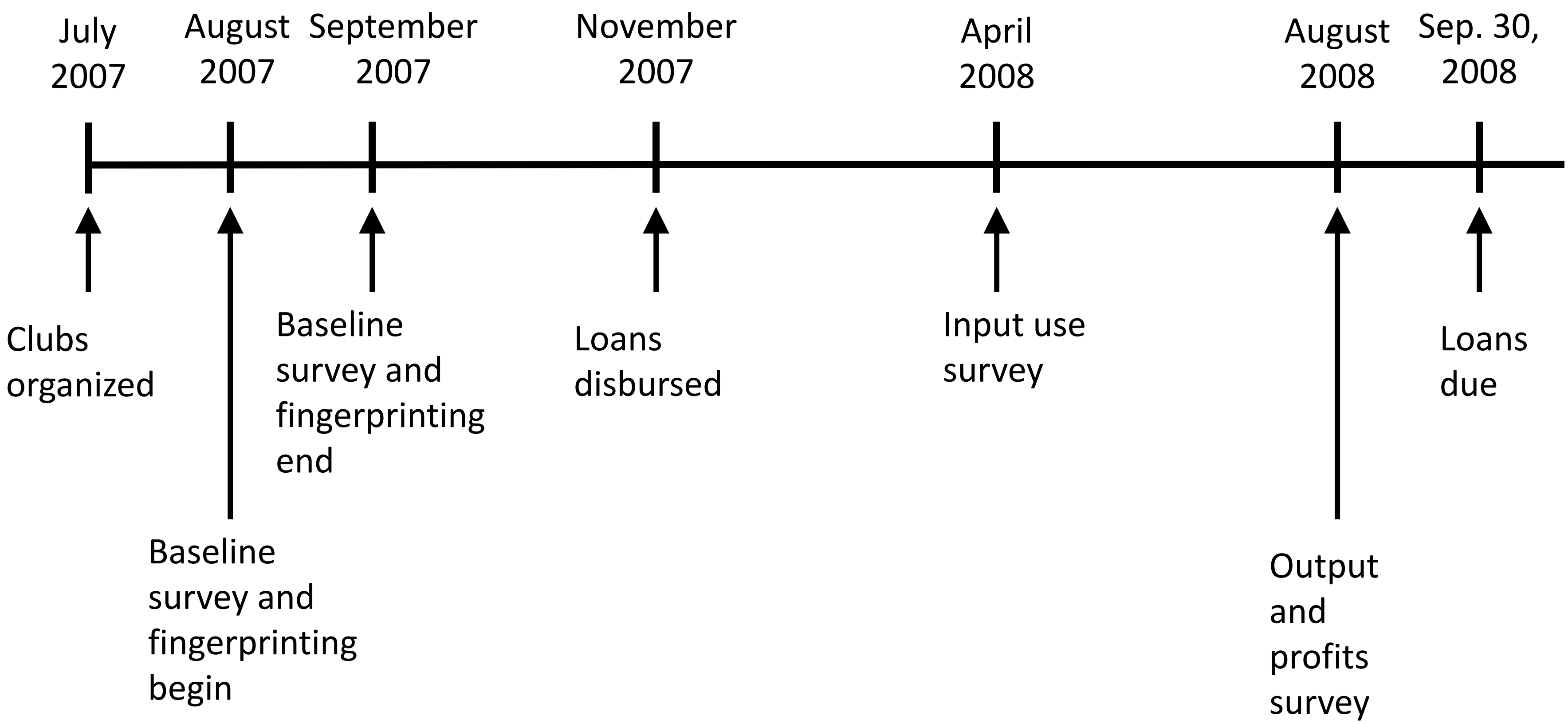
**<u>Appendix Table 13</u>: Benefit-cost analysis**

| | | |
|---|---|---|
| **Benefit** | | |
| (a) Increase in repayment due to fingerprinting in Quintile 1 | 4,905.00 | 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)] | 490.50 | 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)]** | 281.30 | Malawi kwacha |
| **(n) Benefit-cost ratio [ = (d) / (l)]** | 2.34 | |

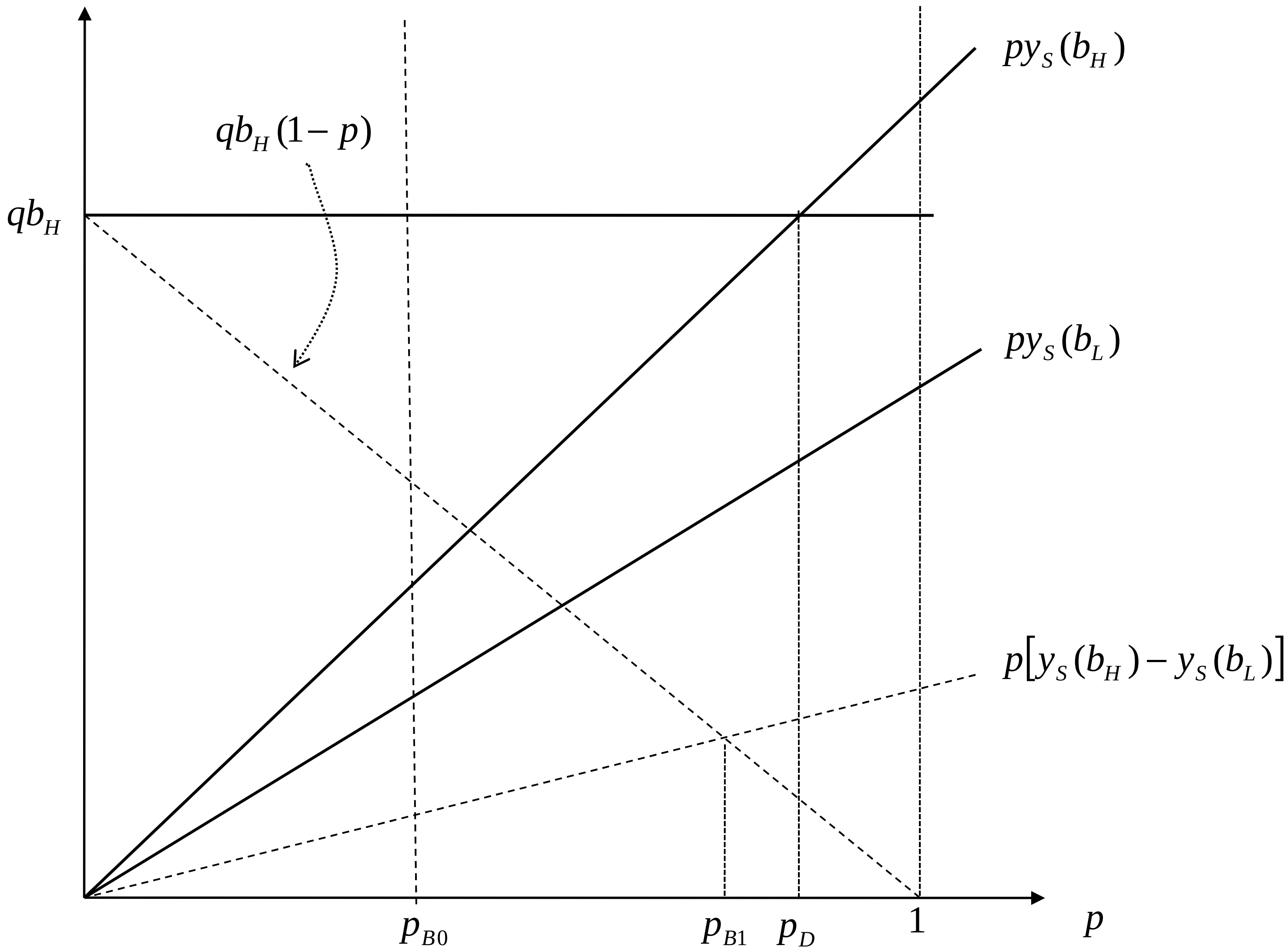
*Assumptions:*

| | | |
|---|---|---|
| Exchange rate: | 145 | MK/US$ |
| Loan size | 15,000 | Malawi kwacha |
| Increase in share of loan repaid due to fingerprinting in Quin | 32.7% | |
| Cost per equipment unit (laptop computer + fingerprint scan | 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 |

Appendix Figure 1: Experimental Timeline



Appendix Figure 2: Optimal behavior as a function of $p$



**Appendix Figure 3**: Borrower behavior under various theoretical cases, with and without dynamic incentives

Case (i): $qb_H > y_S(b_H)$

| | Without Dynamic Incentives | | With Dynamic Incentives | |
|---|---|---|---|---|
| | $p < P_{B1}$ | $p \geq P_{B1}$ | $p < P_{B1}$ | $p \geq P_{B1}$ |
| 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): $y_S(b_H) > qb_H > y_S\,(b_L)$

| | Without Dynamic Incentives | | | With Dynamic Incentives | | |
|---|---|---|---|---|---|---|
| | $p < P_{B1}$ | $P_{B1} \leq p < P_D$ | $p \geq P_D$ | $p < P_{B1}$ | $P_{B1} \leq p < P_D$ | $p \geq P_D$ |
| 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): $y_S(b_L) > qb_H$

| | Without Dynamic Incentives | | | With Dynamic Incentives | | |
|---|---|---|---|---|---|---|
| | $p < P_D$ | $P_D \leq p < P_{B0}$ | $p \geq P_{B0}$ | $p < P_D$ | $P_D \leq p < P_{B0}$ | $p \geq P_{B0}$ |
| 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 | $p$ |