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Identification Strategy

A Field Experiment on Dynamic Incentives in Rural Credit Markets

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Abstract

How do borrowers respond to improvements in a lender's ability to punish defaulters? This paper reports the results of a randomized field experiment in rural Malawi that examines the impact of fingerprinting borrowers in a context where a unique identification system is absent. Fingerprinting allows the lender to more effectively use dynamic repayment incentives: withholding future loans from past defaulters while rewarding good borrowers with better loan terms. Consistent with a simple model of borrower heterogeneity and information asymmetries, fingerprinting led to substantially higher repayment rates for borrowers with the highest ex ante default risk, but had no effect for the rest of 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).

This paper—a product of the Finance and Private Sector Development Team, Development Research Group—is part of a larger effort in the department to understand credit market imperfections and access to credit more in general. Policy Research Working Papers are also posted on the Web at http://econ.worldbank.org. The author may be contacted at xgine@worldbank.org.

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

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1. Introduction

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

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

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

According to the 2006 Doing Business Report, Malawi ranked 109 out of 129 countries in terms of private credit to GDP, a frequently-used measure of financial development. Malawi also gets the lowest marks in the “depth of credit information index” which proxies for the amount and quality of information about borrowers available to lenders. Using more micro data,

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

74 percent of cash crop farmers in our baseline survey had not borrowed from a bank or microfinance institution in the last 10 years.

In the experiment, smallholder farmers organized in groups of 15-20 members applied for agricultural input loans to grow paprika and were randomly allocated to either a control group or a treatment group where each member had a fingerprint collected as part of the loan application. Unlike conventional ID cards or passports, a fingerprint is an effective personal identifier because it is unique to and embodied in each person, so it cannot be forgotten, lost or stolen. Thus, fingerprinting customers would allow lenders to construct credit histories and use them to withhold new loans from past defaulters. In essence, fingerprinting can make the threat of future credit denial more credible.

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

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

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

contracted crop (paprika), which we interpret as a reduction in moral hazard. When we compare these benefits to fairly conservative costs of implementation, we find that adoption of fingerprinting is cost-effective (the benefit-cost ratio is 2.27).

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

Substantively, our intervention most closely resembles the promise of a future lower interest rate conditional on current loan repayment in Karlan and Zinman (2009), henceforth "KZ", who find evidence of moral hazard and weaker evidence of adverse selection in an experiment with a South African provider of consumer loans. Our experiment differs from KZ's in several important respects. First, our experiment is concerned with lending for productive investment in rural areas, while KZ's involves consumer loans for urban customers. Second, our experiment estimates the impact of manipulating the ability to impose dynamic incentives via a technological innovation. KZ, on the other hand, measures the impact of informing borrowers of the existence of a dynamic incentive. Third, we implement a follow-up survey of borrowers to provide additional insight into the specific behaviors that are changed by the intervention and that result in higher repayment. KZ, by contrast, relies exclusively on the lender's administrative data for analysis and so cannot shed light on what borrower behaviors may have changed.

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

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

By documenting impacts on behaviors related to adverse selection and moral hazard, our findings contribute to a burgeoning empirical literature that tests claims made by contract theory and measures the prevalence of asymmetric information (see Chiappori and Salanie, 2003 for a review). A number of recent papers provide empirical evidence of the existence and impacts of asymmetric information in credit markets, in both developed and developing countries. Ausubel (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 that is taken to US data from

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

the Survey of Consumer Finance and finds evidence consistent with both informational problems.[4]

The paper is also related to a framed experiment conducted by Giné et al. (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 (forthcoming) study the introduction of a credit bureau in Guatemala and find that it did contribute to efficiency in the credit market. Finally, the paper is related to the literature motivated by the rise in personal bankruptcies in the US in the last decades (Livshits et al. 2010).

The remainder of this paper is organized as follows. Section 2 describes the experimental design and survey data and Section 3 presents 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 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

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

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.[5]

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).[6] 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 going forward.

In the absence of fingerprinting, farmer identification relies on the personal knowledge of loan officers (who may also rely on local informants such as village and locality leaders). While loan officers could build up reliable knowledge of borrowers over time, this identification "technology" is imperfect. Loan officers are sometimes promoted and routinely rotated to other localities. Among the 11 loan officers who handle our study areas, the median number of years at the branch is only two, while the median number of years working for the lender is 13.[7] 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.

The timeline of the experiment is presented in Appendix Figure 1. Our study sample consists of 249 clubs with approximately 3,500 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

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[5] The Online Appendix can be found in:
http://siteresources.worldbank.org/DEC/Resources/84797-1114437274304/ginegoldbergyangWPSApp092110.pdf

[6] 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.

[7] 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.

visit. The stratification thus ensured that each credit officer handled roughly the same number of treatment and control clubs.[8]

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.[9] 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 laptop computer was now able to identify an individual with only his or her fingerprint. One farmer was chosen at random to have his right thumb scanned again, and the club was shown that the individual's name and demographic information (entered earlier alongside the original fingerprint scan) subsequently was retrieved by the computer program. During these demonstration sessions all farmers whose fingerprints were re-scanned were correctly identified. The control group was not fingerprinted, but as mentioned previously, also received the same training emphasizing the importance of one's credit history and how it influences one's future credit access.[10]

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

[8] There are 16 localities or "extension planning areas" (EPAs) in the study. EPAs are administrative boundaries set up for the delivery of agricultural services by Malawi's agriculture ministry.
[9] Our team of enumerators encountered essentially no opposition to fingerprint collection.
[10] 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.

past variability of income. Summary statistics from the baseline survey are presented in Table 1, and variable definitions are provided in Online Appendix C.[11]

After the completion of the survey, credit history training, and fingerprinting of the treatment group, the names and locations of the members that applied for loans along with their treatment status were handed over to MRFC loan officers so that they could screen and approve the clubs according to their protocols. Among other standard factors, MRFC conditions lending on the club's successful completion of 16 hours of training. MRFC approved loans for 2,063 out of 3,206 customers (in 121 out of 239 clubs). Of the customers approved for loans, some failed to raise the required down payment and others opted not to borrow for other reasons. The final sample consists of 1,147 loan customers from 85 clubs.[12] These loan customers received loan packages with an average value of MK 16,913 (US$117).[13]

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).[14] 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.

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.[15] The formal loan maturity (payment)

[11] These survey data were collected prior to the farmers' being informed about the role of biometrics in the project and their treatment status, to ensure that farmers' survey answers were not influenced by knowledge of the nature of the experiment.

[12] While a natural question at this point is whether selection into borrowing was affected by treatment status, treatment and control groups did not differ in their rates of MRFC loan approval or the fraction of farmers who ended up with a loan (as will be detailed in the results section below).

[13] All conversions of Malawi kwacha to US dollars in this paper assume an exchange rate of MK145/US$.

[14] 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.

[15] 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

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

Overall, we find balance between the two groups in both the full baseline sample and the loan recipient sample. In the full baseline sample, the difference in means for the treatment and control groups is not significant for any of the 11 baseline variables. In the loan recipient sample, for 10 out of these 11 baseline variables, the difference in means between treatment and control groups is not statistically significantly different from zero at conventional levels, and so we cannot reject the hypothesis that the means are identical across treatment groups. For only one variable, the indicator for the study participant being male, is the difference statistically significant (at the 10% level): the fraction male in the treatment group is 6.6 percentage points lower than in the control group.[16]

3. A Simple Model of Borrower Behavior

To study how dynamic incentives affect borrower behavior, Online Appendix D develops a simple model that incorporates both moral hazard and adverse selection. We provide here some

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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.

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

intuition and the main results depending on whether the lender can or cannot use dynamic incentives.

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.[17] Following the credit contract observed in the experiment, the lender 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 small amount is borrowed, production can cover loan repayment even if it fails.

When identification of clients is not possible, borrowers can obtain a fresh loan even if they have defaulted in the past by simply using a different identity. As a result, lenders are forced to offer the same one season contract every period, as they cannot tailor the terms of the contract to individual credit histories. [18]

When biometric technology is available, the lender has the ability to use dynamic incentives by denying credit to past defaulters. In this situation, borrowers face a tradeoff between diverting inputs away from cash crop production but jeopardizing chances of a loan in the future versus ensuring repayment of the current loan and therefore securing a loan in the future. Similarly, by choosing the smaller amount they secure a loan in the future but obtain lower net income in the first period.

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 to divert it all without dynamic incentives but borrow the lower amount and invest it in cash crop production when dynamic

[17] 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.
[18] Though in practice loan officers may recognize clients by sight, loan officers may resign or be transferred and so the new loan officer will not know the clients. Even if loan officers remain on the job, clients could borrow from a different branch or from a different lender altogether.

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. Finally, 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.

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:

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

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

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

We also examine the interactions between the randomized treatment and a particular baseline characteristic: a measure of the ex-ante probability of repayment. Examining this dimension of heterogeneity is a test of the theoretical model's prediction that the impact of dynamic incentives on repayment is negatively related with the ex-ante repayment rate (what the repayment rate would have been in the absence of dynamic incentives): borrowers who, without the dynamic incentive, would have had lower repayment will see their repayment rates rise more when the dynamic incentive is introduced.[19] To test this question, we estimate regression equations of the following form:

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

D_{ij} is a variable representing the individual's predicted likelihood of repayment (its main effect is included in the vector X_{ij}). The coefficient ρ on the interaction term $B_j * D_{ij}$ reveals the extent to which the impact of biometric identification on repayment varies according to the borrower's predicted repayment.

To implement equation (2) examining heterogeneity in the effect of fingerprinting, we construct an index of predicted repayment. This involves creating what is essentially a "credit score" for each borrower in the sample on the basis of the relationship between baseline characteristics (some of which may not be observable to the lender) and repayment in the control (non-fingerprinted) group. (See Online Appendix E for details on the construction of the predicted repayment variable. Online Appendix Table 1 presents the auxiliary regression results used in construction of the predicted repayment variable.) This index is either interacted linearly with the treatment indicator, or it is converted into indicators for quintiles of the distribution of predicted repayment in the absence of fingerprinting and then interacted with the treatment indicator.[20] In all regression results where the treatment indicator is interacted with predicted repayment, we report bootstrapped standard errors because the predicted repayment variable is a generated regressor.[21]

5. Empirical Results: Impacts of Fingerprinting

[19] 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.

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

[21] We calculate standard errors for regressions in the form of equation (2) from 200 bootstrap replications. In each replication, we re-sample borrowing clubs from our original data (which preserves the original club-level clustering), compute predicted repayment based on the new sample, and re-run the regression in question using the new value of predicted repayment for that replication. See Efron and Tibshirani (1993) for details.

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 7 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 7 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.

5.1 Loan Approval, Take-up, and Amount Borrowed

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

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

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

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

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

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

Because loan officers did not take treatment status into account, it is not surprising that fingerprinting had no effect on loan approval. We also find no effect on loan-take-up by borrowers, perhaps because clubs were formed with the expectation of credit availability and fingerprinting did not act as a strong enough deterrent to borrowing to affect farmers' decisions at the extensive margin. Columns 1 and 2 of Table 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).[22]

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

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

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

Inspecting the coefficients on the interactions of fingerprinting with predicted repayment, it appears that this effect is confined exclusively to borrowers in the lowest quintile of expected repayment. Differences between fingerprinted and non fingerprinted borrowers are small and not significant in quintiles two through four, but in quintile one, where fingerprinted borrowers

[22] 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).

take out loans that are smaller by MK 2,722 (roughly US$19) than those in the corresponding quintile in the control group, the difference is marginally significant (the t-statistic is 1.63).

This result is in accord with the theoretical model's prediction that the "bad" borrowers (those whose repayment rates would be lowest in the absence of dynamic incentives) will respond to the imposition of a dynamic incentive by voluntarily reducing their loan sizes. We view this result – voluntarily lower borrowing amounts on the part of fingerprinted borrowers in the lowest quintile – as evidence that fingerprinting reduces adverse selection in the credit market, albeit on a different margin than is usually discussed in the credit context.

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

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

5.2 Loan Repayment

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

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

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

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

5.3 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 main Table 4, 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 5 examines the average and heterogeneous impact of fingerprinting on land allocation; the dependent variables across columns are fraction of land used in maize (column 1), 7 cash crops (columns 2-8), and all cash crops combined (column 9).[23]

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, Cheetah Paprika) and not other types of crop output. By not producing paprika (or producing less), the borrower is better able to avoid repayment on the loan. Therefore, by

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

improving the lender's dynamic incentives, fingerprinting may discourage such diversion of inputs and land to other crops, as farmers face increased incentives to generate cash profits that are sufficient for loan repayment.

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

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

Inputs used on paprika

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

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

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

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

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

It is worth asking whether the impact of fingerprinting seen in Table 6 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 6's results for input use on the paprika crop only (results are available from the authors on request). This suggests that in the absence of fingerprinting, inputs were not used on other non-paprika crops. (If fingerprinting simply led inputs to be substituted away from non-paprika crops to paprika, the estimated impact of fingerprinting on input use on all crops would be zero.) It therefore seems most likely that fingerprinting made farmers less likely to dispose of the inputs via sale or barter.

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

Farm profits

Given these effects of fingerprinting on intermediate farming decisions such as land allocation and input use, what is the effect on agricultural revenue and profits? Columns 1-3 of Table 7 present regression results where the dependent variables are market crop sales, the value of unsold crops, and profits (market sales plus value of unsold crops minus value of inputs used), all denominated in Malawi kwacha. The magnitudes of the overall impacts of fingerprinting on value of sales, unsold harvest, and total profits (Panel A), and in the bottom two quintiles (Panel C) are large and positive, but the effects are imprecisely estimated and none are statistically significantly different from zero.

To help deal with the problem of outliers in the profit figures, column 4 presents regression results where the dependent variable is the natural log of agricultural profits.[24] The effect of fingerprinting in the bottom quintile of predicted repayment is positive but not statistically significant (t-statistic 1.11).

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

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

6. Discussion and Additional Analyses

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

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

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.

6.1 Additional Robustness Checks

Online Appendix F includes regression tables for all results 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 of Table 4), 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 5, 6, and 7).

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 nonborrowers and nonzero effects for borrowers (slightly less than half of individuals in the full sample are borrowers). All in all, we conclude that selection into borrowing is not driving the treatment effect estimates of Tables 5, 6, and 7.

Results with "simple" predicted repayment regression

Results discussed so far examining heterogeneity in treatment effects construct the predicted repayment variable using the regression in column 3 of Online Appendix Table 1. The right-hand-side of this regression contains farmer-level characteristics as well as all interactions between locality and week of initial loan offer fixed effects.

Because the baseline farmer-level characteristics listed in Online Appendix Table 1 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 Online Appendix Table 1, except that (locality)*(week of initial loan offer) 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 locality*(week of initial loan offer) fixed effects. These results reveal that the patterns of treatment effect heterogeneity we emphasize are not simply an artifact of inclusion of the locality fixed effects (and interactions with week of initial loan offer) 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 4 to 7.

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 4 to 7, the 95 percent confidence interval of the coefficients generated in the partitioning exercise does not include a coefficient of zero or 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.

6.2 Evidence for a Reduction in Ex-Post Moral Hazard

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

We investigate this by running regressions where repayment outcomes are the dependent variables, but where we include as independent variables in the regression controls for agricultural profits and the total originally borrowed. Results are reported in Online Appendix Table 15.[25] The profits and total borrowed variables are flexibly specified as indicators for the borrower being in the 1st through 10th decile of the distribution of the variable (one indicator is excluded in each resulting group of 10 indicators, so there so there are 18 additional variables in each regression.)

We cannot reject the hypothesis that fingerprinting has no effect on eventual repayment (columns 4-6) once we control for agricultural profits and original loan size. Coefficient estimates that were previously statistically significant (in Online Appendix Table 4) are now uniformly smaller in magnitude and not statistically significantly different from zero. Indeed, the previously significant coefficients on the fingerprint * quintile 1 interaction across the columns

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

are roughly cut in half. Results are similar for repayment by the due date (columns 1-3), with the exception of the regression for “Balance, Sept. 30” where the linear interaction term and the interaction term with quartile 1 of predicted repayment remain statistically significant at the 5% and 10% levels, respectively. Even in this latter cases, however, the coefficient magnitudes are reduced substantially vis-à-vis the corresponding estimates in Online Appendix Table 4.

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

6.3 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 a decrease in the probability of default (not being fully paid at the loan due date) of roughly 3 percentage points.

7. Benefit-Cost Analysis

The analysis so far has estimated the gains to the financial institution (MRFC) from using fingerprinting to identify new borrowers as part of the process of loan screening. These gains need to be weighed against the costs of fingerprinting. 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 MK475.50 (US$3.28). 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 2.27. (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.[26]

8. Conclusion

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

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

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

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

lender, the credit contract terms could be made more attractive to borrowers, which may further improve repayment.[27]

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

Our results also have implications for microlending practitioners, by quantifying the benefits from exploiting a commercially-available technology to raise repayment rates. Beyond improving the profitability and financial sustainability of microlenders, increased adoption of fingerprinting (or other identification technologies) can bring additional benefits if lenders are thereby encouraged to expand the supply of credit, and if this expansion of credit supply has positive effects on household well-being.[28] 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

[27] 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.

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

lender's dynamic enforcement technology, and so support the use of fingerprints as an identifier in a national credit bureau.

As is the case with all field experiments, it is important to replicate this study in other contexts to gauge the external validity of the results. In addition to conducting similar studies in other rural sub-Saharan African contexts, it is also crucial to gauge the extent to which impacts of fingerprinting-enabled dynamic incentives are different in urban areas or areas with greater access to microcredit, for example. As mentioned above, the effects of fingerprinting on repayment could very well rise over time, and so future studies should monitor effects beyond a single loan cycle. Future work should also make sure to examine responses by the lender, such as changes in the credit contract, approval rates or in loan officer monitoring. While in our case loan officers did not behave differently towards treated borrowers, in other contexts, perhaps under different loan officer incentives, this may not be the case. We view these and related questions as promising areas for future research.

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Table 1: Summary statistics

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

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

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

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

Table 3: Impact of fingerprinting on loan approval, loan take-up, and amount borrowed

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

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

Table 4: Impact of fingerprinting on loan repayment

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

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

Table 5: Impact of fingerprinting on land use

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent variable: Fraction of land used for…	Maize	Soya/Beans	Groundnuts	Tobacco	Paprika	Tomatoes	Leafy Vegetables	Cabbage	All cash crops
Panel A									
Fingerprint	0.001	0.015	-0.012	-0.004	0.005	-0.001	-0.003	-0.001	-0.001
	(0.019)	(0.019)	(0.016)	(0.016)	(0.014)	(0.003)	(0.003)	(0.001)	(0.019)
Panel B									
Fingerprint	-0.009	-0.025	-0.025	-0.033	0.079	0.009	0.006	-0.003	0.009
	(.092)	(.094)	(.060)	(.062)	(.064)	(.010)	(.015)	(.004)	(.092)
Predicted repayment * fingerprint	0.013	0.049	0.016	0.036	-0.092	-0.013	-0.011	0.003	-0.013
	(.101)	(.105)	(.068)	(.066)	(.073)	(.013)	(.016)	(.005)	(.101)
Panel C									
Fingerprint * Quintile 1	-0.061	-0.013	-0.008	-0.012	0.083	0.005	0.007	-0.002	0.061
	(.066)	(.063)	(.052)	(.050)	(.051)	(.008)	(.012)	(.003)	(.066)
Fingerprint * Quintile 2	0.065	0.019	-0.014	-0.019	-0.035	-0.005	-0.010	-0.002	-0.065
	(.052)	(.042)	(.041)	(.030)	(.037)	(.008)	(.008)	(.002)	(.052)
Fingerprint * Quintile 3	-0.012	0.002	-0.009	0.004	0.009	0.008	-0.002	-0.001	0.012
	(.044)	(.045)	(.033)	(.022)	(.038)	(.008)	(.007)	(.002)	(.044)
Fingerprint * Quintile 4	0.008	0.015	-0.026	0.009	-0.003	-0.002	-0.003	0.002	-0.008
	(.041)	(.040)	(.034)	(.021)	(.037)	(.009)	(.007)	(.003)	(.041)
Fingerprint * Quintile 5	-0.005	0.043	-0.001	-0.001	-0.018	-0.012	-0.005	-0.002	0.005
	(.044)	(.040)	(.036)	(.023)	(.034)	(.009)	(.006)	(.003)	(.044)
Observations	520	520	520	520	520	520	520	520	520
Mean of dependent 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 locality * week of initial loan offer fixed effects, baseline characteristics (male, five-year age categories, one-year education categories, and marriage), and baseline risk indicators (dummy for self-reported risk-taking, days of hunger in the previous season, late payments on previous loans, standard deviation of income, years of experience growing paprika, dummy for default on previous loan, and dummy for no previous loans). 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.

Table 6: Impact of fingerprinting on agricultural inputs used on paprika crop

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

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

Table 7: Impact of fingerprinting on revenue and profits

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

Stars indicate significance at 10% (*), 5% (**), and 1% (***) levels.
Notes: Each column presents estimates from three separate regressions: main effect of fingerprinting in Panel A, linear interaction with predicted repayment in Panel B, and interactions with quintiles of predicted repayment in Panel C. All regressions include locality * week of initial loan offer fixed effects, baseline characteristics (male, five-year age categories, one-year education categories, and marriage), and baseline risk indicators (dummy for self-reported risk-taking, days of hunger in the previous season, late payments on previous loans, standard deviation of income, years of experience growing paprika, dummy for default on previous loan, and dummy for no previous loans). 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. Value of unsold harvest computed using regional prices.