When Microenterprises Grow, are Consumers Better Off? Evidence from Large Loans to Microenterprises in Chile

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March 26, 2026

Abstract

If microenterprises provide undifferentiated goods and services, as is often hypothesized, then policies that expand microenterprises may generate benefits to those microenterprises but not to consumer welfare. We test this via a randomized controlled trial evaluating the impact of large loans delivered to microenterprises in Chile. The loans increased profits of treated firms by USD 217/month, a 12.2% increase, implying a private return on lent capital of 22.0%. Survey data from the customers of participating firms imply that treated firms generated an additional USD 313 of consumer surplus/month, representing an additional 25.8% return on lent capital captured by consumers. In the cross section, we document that larger firms generate more aggregate consumer surplus, and that firms providing more differentiated goods and services generate more consumer surplus. We show that our results strengthen the case for subsidizing some forms of microcredit.

1 Introduction

A large literature has examined the impact of microcredit on the profitability of microenterprises and on various measures of borrower and household well-being (see e.g. Cai et al. 2023, for a recent overview). But what is the impact on the customers that are served by the microenterprises that receive credit? On the one hand, much of the academic literature on microenterprises characterizes them as operating in industries crowded with undifferentiated firms (e.g. McKenzie and Woodruff 2017; Jayachandran 2021; Banerjee et al. 2022). This view might suggest that interventions aimed at increasing microenterprise scale-—through capital, training, market access etc.—could facilitate firm growth without yielding meaningful benefits to customers. A growing firm might attract customers from nearby competitors, perhaps through marketing efforts or even by merely increasing its inventory, without providing access to new or cheaper goods and services.

On the other hand, credit may help entrepreneurs exploit new opportunities to create value for their customers. Growing firms might lower prices through economies of scale, or identify white spaces in their local markets, introducing products or services that were previously unavailable or inconvenient to access. In this view, microenterprises grow not merely by stealing business from one another, but by identifying innovations– whether incremental or large– that raise consumer welfare.

We report on a randomized controlled trial (RCT) evaluating the impact of a large, individual-liability loan, in collaboration with one of Chile's largest microfinance institutions. The large loan was a secondary loan to borrowers who already had an outstanding "standard" joint-liability microcredit loan from the lender. The average loan in our experiment provided about USD 1,800 in additional credit over and above the standard loan (which averaged $1,000). The secondary loan generated large and positive impacts for both the business owners and their customers. Businesses that received credit on average generated an additional USD 441/month (SE: USD 185) more revenue and USD

217/month (SE: USD 82) more profits, implying a 13.8% and 12.4% increase respectively, and corresponding to a 22.0% private return on lent capital. They also generated an additional USD 313/month (SE: USD 173) in consumer surplus, implying an additional 25.8% return on lent capital when accounting not only for the value accrued to the business owner but also their customers.

We measure consumer surplus by surveying the customers of the business owners. For the most recent purchase they made from the business, each customer was asked to report both the amount they spent on that transaction as well as their maximum willingness to pay for the product or service *at this store*. The difference between these two measures is meant to capture the residual consumer surplus generated by the firm, over and above what customers would have enjoyed were they to shop only from competing firms. We compute the total residual consumer surplus generated by a firm by estimating the average residual consumer surplus from a sample of customers and then multiplying it by the number of customers a business owner reports serving in a typical week.

While this measure of consumer surplus is surely recorded with noise, it has several strengths. First, reports from customers are not likely to be subject to experimenter demand bias, as customers do not know whether the business is in the treatment or control group (nor do they know anything about the loan experiment itself). Second, in contrast to some model-based methods of estimating consumer surplus, our self-reported measures of consumer surplus do not require a homogeneous sample-—indeed our sample of microenterprises sells a highly varied set of goods and services.

We validate our measure of consumer surplus in various ways. It is highly correlated with measures of customer satisfaction and likelihood to recommend the store to a friend. And we show that consumer surplus is unevenly distributed across sectors in ways that conform to common intuitions. For instance, sectors that provide commodity goods such as fresh food and groceries generate very little residual consumer surplus on average, while specialized goods and services such as beauty and apparel generate relatively more.

Recall, our key measure is an estimate of how much consumer surplus a business generates relative to a counterfactual where customers could only shop at the competing businesses. Thus, even though groceries are essentials, each grocer is likely to provide little residual consumer surplus, as one grocer is a close substitute for another. In contrast, businesses in sectors like beauty and apparel are more likely to provide goods and services that are differentiated from their competitors, and therefore likely to generate higher residual consumer surplus.

As we formalize in Section 3, our key identifying assumption is that the treatment loans do not change the prices or quality offered by non-treated firms.[1] Under this assumption, any treatment effect we estimate on residual consumer surplus at treated firms is also a treatment effect on total consumer surplus in the market.

Importantly, this assumption does not rule out business stealing. Our survey measures each customer's surplus relative to their next-best alternative. If a treated firm attracts additional customers without lowering prices or improving quality, those marginal customers would report being indifferent between the treated firm and their outside option—that is, a residual surplus of zero. Business stealing alone therefore does not mechanically generate a positive treatment effect in our measure. For our estimates to reflect a genuine increase in total consumer surplus, we require only that the treatment does not worsen customers' outside options—for example, by inducing competing firms to raise prices or reduce quality.

If this assumption holds, our empirical estimates imply that the large loan product increased total consumer surplus by USD 313/month (SE: USD 173) per business. This arises both from an 97 cent (SE: 29 cent) increase in surplus generated per customer, and from a 28 customers/week (SE: 10 customers) increase in total customers served. Correspondingly, customers of treated businesses are more likely to report that they shop there

[1]Through the lens of our model, if a business owner were to stop selling a good or service, that would be akin to raising its price to infinity.

because of improved customer service and because they prefer it over larger competitors, and they are less likely to report shopping there because it is close to their home.

Our results imply that the benefit of these loans to borrowers' customers is of similar magnitude to the benefits to the firms themselves—and both are quite sizable. If these results were to generalize, impact estimates that only measure impact on enterprises from enterprise-promoting interventions may be underestimating the societal benefits of these programs by a factor of roughly two. We conduct a formal analysis that shows that our results imply subsidizing some forms of microcredit would generate a societal benefit.

Our results relate to several streams of research. First, we contribute to a growing body of evidence suggesting the promise of large loans to microenterprises (Bari et al. 2024; Bryan et al. 2024). Second, our study relates to the literature studying spillover impacts of microcredit and other interventions meant to promote firm growth. Several papers have documented important general equilibrium implications of microcredit and cash transfers (Kaboski and Townsend 2012; Breza and Kinnan 2021; Egger et al. 2022), finding substantial impacts of cash and credit on demand for goods and services and wages. Closely related is the work that studies the impact of credit and training on not only the firms that receive them but also their competitors. Crépon et al. (2015) and McKenzie and Puerto (2021) find little evidence of business stealing. In contrast, Cai and Szeidl (2024) studies credit to substantially larger small and medium enterprises and finds evidence of statistically significant negative spillovers to non-treated firms as well as a consumer surplus increase through price declines and quality improvements.

We extend this work by documenting substantial positive spillovers for consumers, studying firms far smaller than those of Cai and Szeidl (2024). This is an important distinction. Not only are there hundreds of millions of microenterprises worldwide—indeed, a third of workers are self-employed in low and middle income countries (Gindling and Newhouse 2014)—but as we argued above, a prevailing narrative dictates that these firms are not likely to create residual consumer surplus. We further complement Cai and

Szeidl (2024), as our measure of consumer surplus is directly elicited from customers, rather than inferred from a model.

2 Context, Experimental Design, and Data

Our collaborator, one of Chile's largest microfinance institutions, manages a portfolio of around 120,000 active borrowers and their primary credit product is a joint-liability microloan disbursed to groups of about 20 borrowers. The joint liability loans range from USD 150 to USD 1400, averaging USD 600, and are repaid over about four months. The interest rate is about 49% APR.

The loan under study—referred to throughout this paper as the "RCT loan"—was new at the time of our experiment. The RCT loan is individual liability, but can only be taken as a supplement to a joint-liability microloan. The RCT loan ranges from USD 700 to USD 2000, averaging USD 1840, and is repaid over 12 months. The interest rate for these loans is about 41.5% APR. The RCT loans are intended for the purchase of fixed assets, expansion of a business' premises, or the broadening of inventory, and a requirement of the loans is that a borrower must have successfully completed at least 8 loan cycles with a joint-liability microloan and have had no late payments in the prior year.

The intake process to the experiment was on a rolling basis from May, 2023 to April, 2024, with random assignment to treatment occurring roughly every 25–30 days. Data collection also was on a rolling basis, lasting until summer of 2025, a year past the final loan was issued under the experiment. For a borrower to be enrolled in the experiment, they needed to first apply for RCT loan and be approved by our partner lender. Conditional on approval, borrowers were randomized into either treatment—thereby receiving the loan—or control. Borrowers in the control group were told that they would be eligible to reapply for the loan after two years. Control borrowers, however, remained eligible for other, smaller individual liability loans offered by our partner lender. As we discuss in

Section 4, the randomization induced an intent-to-treat difference of USD 1196 per client in total credit taken by the treatment group relative to the control group.

Randomization was stratified by loan officer and application batch and was uniformly random within each stratum.[2] However, as the product was new to our partner lender, both demand for the product and repayment performance for those that received it were unknown. The lender needed a constraint on the number of RCT loans a given credit officer could handle. Therefore, this manifested itself as a dynamic constraint in which the probability of treatment assignment varied over stratum, as credit officers varied in the number of RCT loans they were managing at any point in time. As the experiment progressed and the lender observed high repayment rates, these constraints were relaxed[3]. In practice, most borrowers were assigned to treatment versus control with 50% probability. Throughout our regression analyses, we apply inverse probability weights to account for non-uniform probability of assignment to treatment.

2.1 Data

We draw upon several sources of data. To evaluate borrower and business outcomes we utilize four surveys, conducted on average six months after randomization, nine months after randomization, 12 months after randomization, and 18 months after randomization. The first and third of these surveys were conducted by phone, while the second and fourth were conducted in person. These surveys cover business performance, household outcomes, and loan usage. We also utilize administrative data from our partner lender on business outcomes, collected each time a borrower renews a loan (recall, even in our treatment groups, borrowers typically renew their joint-liability microloan every four months). Note, that we refer to these data as "administrative" because they are collected by our

[2]On average, an application batch spanned 27 days, though it depended on the rate of loan applications. The minimum was 10 and the maximum was 70.

[3]We describe the precise constraints and how they varied over time in Appendix Section A

partner lender, but they still reflect self-reported business outcomes. These data span from June, 2020 to August, 2025.

To evaluate customer surplus, we draw upon two in-person survey rounds, conducted concurrently with each of the in-person survey rounds of businesses. In each of these survey rounds, a surveyor was stationed outside of the business and approached customers who had just made a purchase.[4] These brief surveys contained just three questions. (1)"What product or service did you purchase in your most recent transaction?" (2) "How much did you spend in total on this purchase?" and (3) "What is the MAXIMUM you would have been willing to pay at this store for the product or service you just purchased?". As we discuss in the following section, we interpret the difference between the answers to questions 2 and 3 as the customer's consumer surplus, and this forms one of our key outcome variables in the analysis to follow.

Table A.1 presents our survey response rates across the various survey rounds. For our borrower surveys, response rates fluctuate between 39% and 60%, with no evidence of differential attrition. 91% of borrowers enrolled in the study respond to at least one survey. Similarly, 95% of participating borrowers have at least one observation recorded in our administrative data. Across both our surveys and administrative data, 98% of borrowers are represented at least once. Across our two customer survey waves, customers from 48% of our businesses are represented in the data, with no evidence of differential attrition.[5] To account for concerns about attrition in any given survey wave, we replicate each of our key analyses utilizing a single post-randomization outcome reflecting the average value recorded across all survey waves and administrative data waves in which the borrower is represented.

[4]For businesses with infrequent foot traffic, we provided fliers with a QR code that customers could scan to complete the survey remotely. 17% of our customer data come from remote surveys.

[5]The relatively high level of attrition in businesses for which we have customer data is due to the logistical challenges of collecting it. For many businesses in our sample, surveyors had to travel across the country by bus or plane, rendering it difficult to coordinate with business owners about a convenient time for data collection.

Table A.2 presents our sample descriptive statistics and balance check. The average borrower in our sample is 41, and 20% are male. The average business earns about USD 1,450/month in profits, with 0.76 employees, and the average household income is USD 2,800/month. 25% of businesses are in manufacturing, 56% retail, and 18% services. The only two statistically significant difference between treatment and control businesses are that treatment businesses employ 0.22 more employees (p-value=0.057) and have 33 more customers per week (p-value=0.057). An F-test fails to reject equality of the two distributions (p-value=0.65). However, we note that other variables, albeit not significant statistically, are also indicative of slightly larger treatment businesses than control: about 6% higher profit (p-value=0.278) and 3.6% higher household income (p-value=0.486). Accordingly, we control for the baseline level of the outcome variable in each of our primary analyses.[6].

3 A Model of Consumer Surplus Measurement

Our measure of consumer surplus comes from a brief survey, fielded with the customers exiting the premises of the businesses participating in our study. Consumer surplus is derived from the responses to two questions: one eliciting a customer's willingness to pay at this store for the good or service they just purchased, and another asking how much they actually paid. The difference between these two measures is our primary consumer surplus outcome.

We consider several strengths and limitations of this approach at the end of this section. First, we outline a model to clarify when our survey-based measure of residual consumer surplus identifies changes in total consumer surplus. The key concern is business stealing: if treated firms attract customers away from competitors, does our measure

[6]We also report the mirror analyses controlling for number of weekly customers, number of employees, monthly profits, and monthly household income in appendix Tables A.5, A.6, and A.7.

overstate welfare gains? We show that under a simple no-spillover assumption on non-treated firms, changes in residual consumer surplus at treated firms equal changes in total consumer surplus in the economy.

We consider an economy $\mathcal{E}$ with businesses $b \in \mathcal{B}$ and consumers $c \in \mathcal{C}$. Each business b_i sells a single good γ_i at price p_i. Consumer c_j can consume at most one good and derives value $v_j^i - p_i$ from purchasing and consuming good i. Each consumer c_j purchases the good $\gamma_{\bar{j}}$ that solves $\max_k \gamma_k - p_k$.

Corresponding to our survey question—"what is the most you would be willing to pay for this good at this store?"—consumer j's maximum willingness to pay for $\gamma_{\bar{j}}$ is the utility they derive from this store minus the utility they would obtain from their next best alternative. $WTP_j^{\bar{j}} \equiv \left(v_j^{\bar{j}} - \max_{k \in \mathcal{B} \setminus b_{\bar{j}}} v_j^k - p_k \right)$. Our object of interest is the sum of consumer surpluses derived by all consumers in the economy: $CS \equiv \sum_{c_j \in \mathcal{C}} v_j^{\bar{j}} - p_{\bar{j}}$.

Now, to map to our experiment, consider two apriori identical economies, $\mathcal{E}$, with a set of businesses $\mathcal{B}$ and consumers $\mathcal{C}$, and $\mathcal{E}'$, with businesses $\mathcal{B}'$ and consumers $\mathcal{C}'$. In economy $\mathcal{E}'$, one firm is treated, meaning that for every consumer the treated firm delivers strictly higher utility (through lower prices or higher quality). All other firms are initially identical across the two economies.

Denote the'treated firm b'_T, such that for any consumer indexed by j, $v_j'^T - p'_T > v_j^T - p_T$. That is, in the economy $\mathcal{E}'$, the consumer surplus enjoyed by any consumer c'_j buying the good of business b'_T is higher than the corresponding consumer surplus enjoyed in economy $\mathcal{E}$ by the consumer c_j buying the good from the business b_T. For now, we assume that only one firm is treated in our experiment, but we will return to the case where several firms are treated.

Let $\mathcal{C}_T$ represent the consumers who purchase γ_T in economy $\mathcal{E}$, and similarly $\mathcal{C}'_T$ represents the set of consumers who purchase γ'_T in $\mathcal{E}'$. Our measure of residual consumer surplus for the customers of business b_T, derived from our survey is $CS_T \equiv \sum_{j \in \mathcal{C}_T} WTP_j^T - p_T$, and CS'_T is defined similarly for economy $\mathcal{E}'$.

Our key identifying assumption is that the treatment does not change the prices or quality offered by non-treated firms.

Assumption 1. *There are no spillovers on non-treated firms price or quality: for all* $b_i \in \mathcal{B} \setminus b_T$ *and* $b'_i \in \mathcal{B}' \setminus b'_T$*, and for all* $c_j \in \mathcal{C}$ *and* $c'_j \in \mathcal{C}'$*,* $v^i_j = v'^i_j$*, and* $p_i = p'_i$*.*

This assumption does not require that once treated, firm T does not take customers from the other firms in the economy. In particular, $\mathcal{C}'_i$ can be a strictly larger set than $\mathcal{C}_i$. As our measure of consumer surplus CS'_T captures the value that customers derive from shopping at firm T relative to their next best alternative, our identifying assumption requires only that the treatment loan does not diminish the value that customers would derive from shopping at non-treated firms.

We are now ready to state our main results. Proofs are relegated to Appendix B.

Theorem 1. *Assuming there are no spillovers on non-treated firms price or quality,* $CS'_T - CS_T = CS' - CS$*.*

Our result says that if there are no spillovers of our intervention onto non-treated firms price or quality, then the difference between the residual consumer surplus we measure for the customers of firm b'_T and the customers of b_T is the same as the difference in total consumer surplus between the two economies. This result holds despite the fact that the two sets of customers whose surplus is being measured differs across the two economies; firm b'_T may have strictly more customers than firm b_T.

Intuitively, for any customer purchasing from b'_T, when they report $WTP_j - p_j$, they are factoring into their willingness to pay the utility they would have derived from their next best alternative. If the customer would have purchased from b'_T regardless of the intervention (i.e. the corresponding customer in economy $\mathcal{E}$ would have purchased from b_T), then the customer's outside option is the same, and any increase in their willingness to pay for firm γ'_T reflects a true increase in utility. In contrast, for stolen customers, their

outside option in E' is exactly the firm they would have purchased from in E. Thus, their residual surplus directly measures the true welfare gain from switching.

Our result generalizes to the case where several businesses in $\mathcal{E}'$ are treated, such that for any treated firm b'_T, and any consumer c_j, $v'^T_j - p'_T > v^T_j - p_T$. In this case we have the following result:

Theorem 2. *Assuming there are no spillovers on non-treated firms price or quality,* $\sum_{T\in\mathcal{T}}\{CS'_T - CS_T\} \leq CS' - CS$*, where* $\mathcal{T}$ *is the set of treated firms*

In this case, our measure of the impact on consumer surplus, $\sum_{T\in\mathcal{T}}\{CS'_T - CS_T\}$—derived from eliciting the residual consumer surplus of customers from treated firms and comparing it to the residual consumer surplus of customers from comparable control firms—is a lower bound for the true impact on consumer surplus. When multiple firms are treated, customers at one treated firm may compare it to another treated firm whose quality has also improved. As a result, their reported willingness to pay understates the total welfare gain.

3.1 Comments on Self-Reported Willingness to Pay

In contrast to model driven approaches to estimating demand curves, we directly elicit willingness to pay from customers. This approach has been applied extensively in the development and applied microeconomic literature (e.g. Allcott and Kessler 2019; Berry et al. 2020; Cole et al. 2020; Dizon-Ross and Jayachandran 2023; Maffioli et al. 2023), and has several advantages; it does not require that the sample of firms supplies homogeneous goods, it does not require an exogenous supply shifter to facilitate demand curve estimation, and it does not rely on assumptions about market structure and competition. However, self-reports of willingness to pay suffer from several important limitations; customers may not understand the question, and may not be able to articulate their true willingness to pay for a good or service, and they may furnish responses that are context

dependent, for example that reflect what they consider to be a fair price given the market rather than their willingness to pay (e.g. Thaler 1985; Dizon-Ross and Jayachandran 2022). Also, customers may be strategic in their reports about willingness to pay, if they anticipate that responses will be used to inform future pricing decisions. Our surveyors made clear individual responses would remain strictly confidential, however customers were informed that aggregate statistics may be shared with the business owner, for the purpose of making improvements to their store and business.

To some degree, the noise and bias inherent in self-reported assessments of a consumer's willingness to pay may also extend to real purchasing decisions and influence consumer surplus estimates inferred from revealed-preference approaches. Moreover, as we show below, our results provide several points of reassurance that these measures are not especially noisy. First, our estimates of consumer surplus are correlated with more intuitive—but less quantitatively interpretable—measures of utility, such as responses to the question "On a scale of 1 to 7, with 7 being extremely satisfied and 1 being extremely dissatisfied, how satisfied are you with the products or services that you have purchased from this store?" and "Would you recommend this store to your friends and family members?" We show that treated firms not only produce more consumer surplus for their customers, but also that their customers are more likely to say they shop there because of high quality customer service, and that they prefer the business over larger stores (and they are more likely to say they prefer the price and quality, though not statistically significantly so), and they are less likely to say that they shop at the store because it is close to their home. And we show cross sectionally that consumer surplus is higher for businesses in sub-sectors that produce differentiated goods and services.

Finally, we note that our measure of consumer surplus is not likely to be influenced by experimenter demand effects or social desirability bias, as customers do not know that the stores they are shopping at are participating in an experiment, and do not directly observe treatment assignment, though, of course, they may observe the various ways in

which the business owner has improved the business.

4 Results

4.1 Regression Specifications

Our main empirical analyses estimate the following specification

$$y_{it} = \alpha + \beta T_i + X_i + \gamma_t + \varepsilon_{it} \tag{1}$$

where y_{it} represents our key outcome variables for borrower i at time t, T_i is a dummy variable indicating whether borrower i was assigned to the treatment group, X_i is a vector of controls from Table A.2. X_i always includes the baseline value of the outcome variable, as well as any others selected by PDS LASSO. γ_t is a month x year fixed effect, and standard errors are clustered at the borrower level. Throughout, we apply inverse probability weights by stratum to account for the strata-specific probability of applicants being assigned to treatment or control.

For robustness, we present analyses with two alternative specifications. First, to account for concerns related to attrition within any given survey, we estimate a model where outcome data is pooled across all post randomization data points:

$$y_i = \alpha + \beta T_i + X_i + \varepsilon_i \tag{2}$$

Second, we present our firm-level analyses using a difference in difference specification:

$$y_{it} = \alpha + \beta_1 T_i + \beta_2 Post_t * T_i + X_i + \gamma_t + \varepsilon_{it} \tag{3}$$

where $Post_t$ is a dummy taking a value of 1 for post randomization data waves. As we do not have baseline data for our customer surplus analyses, we cannot replicate them

utilizing this approach.

4.2 Firm- and Household-Level Outcomes

We begin by analyzing the first stage of the experiment: how much additional credit did treatment group firms receive? These results are presented in Table 1; Panel A presents estimates without additional controls, and Panel B includes any controls selected by PDS LASSO from those presented in Table A.2.

Treatment group borrowers received an additional USD 1,791 from the RCT loan (SE: 27, column 3 panel B), which translated to an additional USD 1,346 in credit from our partner lender (SE: 53, column 4 panel B). The difference between these two figures comes from the fact that control group borrowers were eligible to apply for additional, smaller loans, upon being rejected for the RCT loan. Though not statistically significant, the point estimates suggest that control group borrowers also took an additional USD 176 in credit from other lenders (SE: 161, column 6 panel B). In total, treatment group borrowers received an additional USD 1,211 in credit from all lenders (SE: 175, column 1 panel B), compared to the control group mean of USD 1,866.

Table 2 presents our key firm-and household-level outcomes. Once again, Panel A presents estimates without additional controls, and panel B includes controls for the baseline level of the outcome variable as well as those selected by PDS LASSO from those presented in Table A.2. Our evidence indicates that treatment firms grew substantially. Our preferred specification includes firm controls. The estimates indicate that the RCT loan increased firm revenues by USD 441/month (SE: 185 column 1, panel B), increased profits by USD 217/month (SE: 82, column 2, panel B), and increased household income by USD 267/month (SE: 138, column 3, panel B). Our estimate of the loan's impact on the total value of business capital—fixed assets plus inventory— is quite large at USD 2805 (SE: 1513, column 4, panel B), though with such large standard error, the point estimate

may overstate the true impact. We do not find an impact on employment (column 5, panel B). Considering the first-stage impact of our intervention on total borrowing constituted an additional USD 1211, this implies a private return on borrowed capital of 22.0% in terms of household income.

Table A.3 presents the difference in differences estimates in Specification 3, for the same outcomes as in Table 2. Panel A includes firm fixed effects, and Panel B includes controls for the baseline level of the outcome as well as those selected by PDS LASSO from the set presented in Table A.2. Across the board, estimates in both panels are quite similar to our preferred specification.

Finally, for robustness to attrition concerns, Table A.4 presents estimates of Specification 2, where outcomes are averaged across all post-randomization data waves (recall, we have at least one follow-up observation for 96% of our sample). Once again, estimates are quite similar, both in magnitude and precision, to our estimates in Table 2.

Figure 1 plots the treatment effects of the RCT loan over time, for both firm profits and revenues.[7] The figures suggest the treatment effect grew somewhat over time.

4.3 Consumer Surplus

We now turn to our results measuring the downstream impacts of the large loans on firms' customers. Table 3 presents our primary estimates on consumer surplus and its components. Columns 1-3 analyze outcomes at the customer-level, while columns 4 and 5 present firm-level aggregates. Panel A presents the estimates without controls, while Panel B presents estimates controlling for the variables selected by PDS LASSO from those presented in Table A.2.

Customers of treated firms report spending an additional USD 3.34 on their most recent transaction (SE: 1.57, column 1, panel B) relative to a control group mean of

[7] Our administrative data do not cover the other primary outcomes discussed in this section; thus we do not have sufficiently high frequency data to form comparable plots for these outcomes.

USD 10.96. Similarly, they report an additional willingness to pay of USD 3.89 (SE: 2.01, column 2, panel B), resulting in an increase of USD 0.97 in consumer surplus from their most recent transaction (SE: 0.29, column 3, panel B), relative to a control mean of USD 1.85. In addition to providing more value to customers, treated firms also serve 28.1 more customers per week (SE:10.4, column 4 panel B), relative to a control mean of 94.8. Multiplying average customer surplus, reported over all surveyed customers, by the reported weekly number of customers served by the business, we find that treated firms generate an additional USD 78.2 in total customer surplus per week (SE: 43.3, column 5, panel B). This is our primary result on the downstream impact of the large loan product. Considering the first-stage impact of our intervention on total borrowing constituted an additional USD 1211, this implies that the customer-surplus return on capital is 25.8% per month—slightly larger than the private returns estimated in the previous section.

Figures 2 provide some reassurance that customers understood our willingness to pay question. Namely, we asked customers to respond to the more intuitive questions about their level of satisfaction with the firm's products and services, as well as their likelihood to recommend the store to a friend.[8] Both figures demonstrate that our measure of consumer surplus is highly correlated with responses to these questions.

How do treated firms generate additional customer surplus? Customers were asked "Why do you choose to buy this business's products/services over others?" Table 4 analyzes customer responses. Customers at treated firms are 9 percentage points more likely to report that they shop at the firm because of high quality customer service (SE: 4pp, column 3, panel B), and 4 percentage points more likely to say they prefer it over larger stores (SE: .02, column 7, panel B). They are also 3 percentage points more likely to report that they like the prices charged by the firm (SE: .05, column 1, panel B), and 4 percentage points more likely to report that they like the quality of the firm's goods

[8]Customers were asked: "On a scale of 1 to 7, with 7 being extremely satisfied and 1 being extremely dissatisfied, how satisfied are you with the products or services that you have purchased from this store?" and "Would you recommend this store to your friends and family members?"

and services (SE: .05, column 2, panel B), though these are not statistically significant. Conversely, customers of treated firms are 8 percentage points less likely to report that they shop there merely because it is conveniently located (SE: .05, column 5, panel B).

Another hint about the mechanism underlying the consumer surplus impacts comes from the loan applications for borrowers in our sample, who had to specify how they would spend the loan if they received it. The distribution of these responses are categorized in Figure A.1. 23.5% reported that they would spend the loan remodeling their business premises, thereby either improving its appeal to customers or expanding its productive capacity, 28.3% said they would invest in a vehicle with the modal purpose being for delivery and inventory transport, 43.1% said they would invest in fixed assets not belonging to either of the aforementioned categories, and 12.6% said they would invest in inventory, either to expand product and service offerings or to leverage economies of scale. Unfortunately, due to sample limitations, we are not powered to conduct heterogeneity analyses along these investment intentions.

How is consumer surplus distributed across the economy? Recall, our measure of consumer surplus captures the residual consumer surplus generated by a firm, taking as given the goods and services offered by competitors. Table A.8 and A.9 examine the cross-sectional distribution of consumer surplus, pooling both treatment and control firms. Table A.8 examines how various measures of business size correlate with consumer surplus. Interestingly, we find a strong negative relationship between the number of weekly customers served and consumer surplus *at the individual level*, suggesting that smaller stores create more value for each individual customer (column 3). This relationship reverses, however, at the aggregate level. Larger firms generate more aggregate consumer surplus, as they serve more customers; every measure of firm size is positively correlated with aggregate consumer surplus (column 4).

Table A.9 examines correlates of customer surplus by subsector. Column 3 indicates that businesses specializing in apparel and footwear, beauty and personal care services,

tailoring, and "other services" all provide higher than average consumer surplus, at the individual level, while businesses specializing in fresh food and specialty retail, grocery and convenience retail, and restaurants and prepared food services all provide lower than average consumer surplus at the individual level. While we do not have a rigorous mapping, these categorizations intuitively line up with the theory that businesses providing more differentiated goods and services provide higher consumer surplus than those that provide relatively undifferentiated goods and services. Column 4 does not demonstrate substantial variation across subsectors in total consumer surplus delivered, which largely reflects substantial heterogeneity in firm size within each subsector.

Finally, Figure A.2 presents a graphical comparison of the income distribution of business owners and customers. The figure indicates that firm customers are substantially poorer than the firm owners in our sample. Firm customers have a mean income of USD 1368/month (median USD 1000/month), while firm owners have a mean income of USD 2,400/month (median USD 2500/month). The 75th percentile of the customer household income distribution is lower than the 25th percentile firm owner household income distribution.[9]

That firm customers are substantially poorer than the borrowers in our sample may strengthen the policy implications of our customer surplus results. Not only are the benefits to the loan, captured by customers, slightly larger than the private returns captured by the borrowers, but the returns to customers may be more socially valuable given their comparatively lower incomes.

[9]Recall, however, that by virtue of being eligible for the large RCT loan, firm owners in our sample are substantially richer than the typical borrower at our microfinance institution. Indeed, Figure A.2 indicates that the distribution of household income for the typical borrower at our partner microfinance institution is quite similar to that of firm customers in our sample.

5 Examining the Case for Subsidizing the RCT Loans

Our findings suggest that the RCT loan generates considerable social benefit, accruing both to the borrower and their customers. We now explore the degree to which these findings strengthen the case, all else equal, for subsidizing the RCT loan and similar credit products. Neither the impact of these loans on borrower profits nor on consumer surplus is internalized by a profit-maximizing lender. Thus, if unprofitable loans—those with ex-ante low repayment likelihood—have high social impact, a subsidy may be warranted.

We begin our empirical exercise by building a predictive model of borrower default for the RCT loans, to assess which loans our partner lender would be likely to grant in the future in the absence of a subsidy. Our baseline data contain many variables with which to predict repayment, including several that are motivated by theory: pledgeability of capital (proxied by baseline assets), profitability of the business, and volatility in expected returns. We measure volatility through a subjective expectations module in the loan application, which elicits the borrower's assessment of the likelihood of good, bad, and normal profit realizations, conditional on receiving the loan, and the anticipated level of profits in each state of the world.

We build our predictive model by estimating

$$y_i = \alpha + \beta X_i + \varepsilon_i \tag{4}$$

where y_i represents various measures of lateness or default, and X_i is a vector of controls including those referenced above as well as a battery of demographic and business characteristics including business sector, intended investment type, prior repayment trouble, and age and gender of the borrower. A full list of the variables included in the model is presented in Appendix C. The unit of observation is a loan, and the sample is all RCT loans given to borrowers in the treatment group. We estimate the model via LASSO using the STATA command "lasso linear."

The model is presented in Table A.11. In Column 1, the outcome variable is a dummy taking a value of 1 if the borrower was never late during their loan cycle, in Column 2 the outcome is a dummy taking a value of 1 if the borrower was never more than 15 days late during their loan cycle, and in Column 3 the outcome is a dummy taking a value of 1 if the borrower was never more than 90 days late during the loan cycle. 90 day lateness is our partner lender's threshold for writing off a loan and declaring it to be in default, but all three measures represent material losses to our lender. 14% of the treatment loans entered default, while 28% were at least 1 day late. Conditional on entering default, the average amount defaulted on was USD787, and conditional on a loan ever being at least 1 day late, the expected amount of default (i.e. the portion unpaid conditional on subsequently reaching 90 days late) is USD420.

As indicated in Table A.11, the model is far more predictive for 1 day lateness than 90 day lateness, with the model for 15 day lateness performing in the middle. The model for 1 day lateness (Column 1) utilizes many baseline variables. In line with the theory discussed above, borrowers with more baseline assets and lower volatility of expected returns are predicted to have higher repayment. The model also selects a measure of profitability—anticipated earnings conditional on not receiving the RCT loan—which is positively predictive of repayment. The model performs reasonably well, with an AUC of 0.79 and a Brier score of 0.16. The model for 15 day lateness (Column 2) selects somewhat fewer variables, and has a more modest performance, with an AUC of 0.73 and a Brier Score of 0.13. Finally, the model for 90 day lateness selects no variables and has no predictive power.

As an additional check on the strength of these predictive models, Table A.12 reports the results of estimating specification 4, where X represents the repayment probability prediction from estimating the above model and the sample is once again all borrowers who received RCT loans in our treatment group. A borrower with 1-standard-deviation higher predicted chance of never falling late has a 23 percentage point higher chance of

never falling late (from 72% to 95%, Column 1). For 15 days late, the analogous estimate is 11 percentage points (from 83% to 94%, Column 2). These indicate substantial predictive capacity. Figure A.3 plots the results of a non-parametric regression of late repayment on predicted repayment, corroborating the results of Table A.12.

For the exercise to follow, we rely primarily on the model for 1 day lateness, which has the best performance, but for robustness we also include a variant of the analysis relying on 15 day lateness. We reiterate that both of these are material outcomes from the perspective of our partner lender. Even in the absence of substantial transaction costs accruing from late payments, a borrower who is at least 1 day late at any point in their loan has an expected default (amount outstanding at 90 days lateness) of USD420, and a borrower who is at least 15 days late at any point in their loan has an expected default of USD700.

Next, we estimate the relationship between the treatment effects of the RCT loan and predicted repayment probability. If borrowers with relatively low predicted repayment probability experience low impact from the loan, then there is no misalignment between the interests of the lender and the social planner, and no case for subsidizing unprofitable loans.

We estimate

$$y_{it} = \alpha + \beta_1 T_i + \beta_2 p_i + \beta_3 T_i * p_i + X_i + \gamma_t + \varepsilon_{it} \tag{5}$$

where p_i is the predicted probability of a borrower not falling late on her loan and all other variables are as defined in the description of Specification 1. Estimates are presented in Table A.13, with Panel A corresponding to predictions for 1 day lateness, and Panel B corresponding to predictions for 15 day lateness. Columns 1 and 2 present results for treatment effects on monthly household income, Columns 3 and 4 present results for treatment effects on weekly consumer surplus, and Columns 5 and 6 present results for

treatment effects monthly household income plus monthly consumer surplus. Overall, we do not find evidence of heterogeneous treatment effects based on predicted repayment probability, with the estimate of β_3 rarely reaching statistical significance across specifications.

How do these estimates translate to social impact and repayment performance across various portfolios that a lender might select? We consider repayment performance in Figure A.4. The x-axis corresponds to predicted late payment values. The y-axis corresponds to the realized repayment performance of all borrowers in the treatment group who had a predicted repayment likelihood above a given x-value. This represents the repayment performance of a portfolio resulting from giving loans to all borrowers with a predicted repayment value above a given threshold. Figure A.5 presents analogous plots for social impact. As in the previous figure, the x-axis corresponds to predicted late payment values. The blue dotted line corresponds to the average predicted impact on household income from a portfolio of borrowers all of whom had a predicted repayment likelihood above a given x-value. Here, the predicted impact on household income is assigned to each borrower in the sample based on the estimates from specification 5. Similarly, the black dotted line corresponds to the predicted average impact on household income plus consumer surplus from a portfolio of borrowers all of whom had a predicted repayment likelihood above a given x-value.

Together Figures A.5 and A.4 plot the frontier of repayment performance and impact. Moving to the right in either figure would correspond to increasing the portfolio by making marginally riskier loans, with the y-axis depicting the resulting marginal decline in repayment performance in Figure A.4, and the resulting change in average impact in Figure A.5.

Finally, we consider several concrete portfolios, and ask whether the marginal impact arising from relaxing the lending criteria would justify a subsidy to cover the corresponding additional non-payment. Table A.14 considers four hypothetical portfolios constructed by

selecting the n borrowers with the highest predicted repayment scores such that the portfolio has 95%, 90%, 85% and 80% on-time payment (Panel A) or < 15 day late payment (Panel B). Column 3 presents the late-repayment probability of the marginal borrowers in the portfolio, who are present in the current row but not in the more conservative portfolio one row above. Column 4 considers the same marginal borrowers and indicates the average impact of giving an RCT loan to these borrowers in terms of the expected increase in their monthly household income. And Column 5 considers the corresponding impact in terms of monthly household income plus consumer surplus.

The results indicate that a subsidy may be warranted, though perhaps only once the social surplus captured by consumers is accounted for. For concreteness, we make this point with reference to the riskiest portfolio—the 80% repayment rate portfolio for 15 day lateness (last row, Panel B). The marginal borrowers who are present in this portfolio but not in the marginally more conservative portfolio one row above, have a predicted repayment rate of 81.6% - 84.5%, where these estimates and all subsequently reported ranges correspond to the 95% confidence interval. A back of the envelope calculation suggests that these borrowers will default on average on USD108.7 - USD128.5, where this calculation is the product of the 15 day lateness rate, and the fact that the average borrower who is 15 days late eventually defaults on USD700.

Now suppose that the lender required a subsidy of this expected default amount in order to be willing to make the loan[10] A social planner considering only the impact of these loans on household income of the borrower, as is common in the microcredit literature, would likely conclude that these marginal loans are not worth subsidizing, as the 95% confidence interval for the average impact of these loans on household income ranges from USD-84.47/month to USD416.1/month, with a point estimate of USD165.8/month. However, once accounting for the additional expected impact accruing to customers, the

[10]This is an approximation. The required subsidy could be somewhat lower if the lender did not require a full loan guarantee, but could also be higher if they required a subsidy to cover the additional labor required for tracking down delinquent borrowers.

case for subsidy is considerably strengthened. The 95% confidence interval for the average impact of these loans on the sum of household income and consumer surplus ranges from USD-49.5/month to USD 1109.4/month with a point estimate of USD530.0/month. Column 2 of Table A.14 presents analogous calculations for the required subsidy to marginally expand the lender's portfolio, so that the reader can examine the cost-benefit analysis of marginally expanding the portfolio at every level of lender conservatism.

In summary our analysis suggests that some marginally unprofitable loans from the lender's perspective may have sufficiently large social impact to justify a subsidy. Importantly, this conclusion hinges on the very large impact of these loans on consumer surplus—our study's main novel finding. An analyst considering only the loans impact on the borrower's household income, as is commonly done in the research literature, may have reached the opposite conclusion.

6 Discussion

We study the impact of a large loan, targeted to microfinance borrowers, and show that it has large impact both on the businesses themselves as well as on their customers. Our impact estimates on borrowers household income imply a private return of lent capital of 22.0% per month. Our impact estimates on customer surplus imply an additional social return of 25.8% per month.

Our results shed light on the value created by microenterprises. While a common narrative dictates that microenterprises provide undifferentiated goods and services, and that the growth of a microenterprise primarily benefits the business owner, our study strongly suggests the opposite. More than half of the value created by the large loans we study was captured by consumers. If these results were to generalize to other interventions targeting microenterprise growth, the benefits of these programs may be substantially understated. Of course, our study does not quantify the impact of the loans on other components of

social welfare, such as that of competing firms and environmental externalities. Nevertheless, the theoretical framework in Section 3 emphasizes that our consumer surplus results are not confounded by the possibility of business stealing. Our results therefore strengthen the case of subsidizing some forms of microcredit.

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A Main Tables and Figures

Table 1: First Stage - Loan Take-Up

		Partner MFI			Outside Partner MFI	
	Total Credit Taken Amount	Took RCT Loan	RCT Loan Amount	Total Credit Taken Amount	Took Loan	Total Credit Taken Amount
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Without controls						
Treatment	1210.64***	0.97	1790.50***	1378.06***	-0.02	-175.69
	(174.61)	(.)	(26.61)	(55.80)	(0.03)	(160.68)
Control group mean	1866.77	0.00	0.00	1430.32	0.13	438.18
	[1964.34]	[0.00]	[0.00]	[597.76]	[0.34]	[1881.29]
No. Obs	439	538	538	538	439	439
Panel B: With controls						
Treatment	1210.64***	0.97	1790.50***	1345.92***	-0.02	-175.69
	(174.61)	(.)	(26.61)	(52.77)	(0.03)	(160.68)
Control group mean	1866.77	0.00	0.00	1430.32	0.13	438.18
	[1964.34]	[0.00]	[0.00]	[597.76]	[0.34]	[1881.29]
No. Obs	439	538	538	538	439	439

Notes: ***$p \leq 0.01$, **$p \leq 0.05$, *$p \leq 0.10$. Outcomes in columns 2-4 come from MFI administrative data. The outcomes in Columns 5 and 6 come from survey data (hence the lower number of observations): respondents are asked whether they took a loan from someone other than the partner MFI after the beginning of the study and the amount of the loan. The outcome in Column 1 is the sum of Columns 4 and 6 for those who answered the survey. There were 7 treatment group borrowers who were offered the RCT loan and did not take it. PDS-LASSO controls use all business baseline characteristics presented in Table A.2 as controls. Standard deviation reported in square brackets and robust standard errors in parentheses. Standard errors are clustered at the borrower level, and all regressions use IPW. All monetary values are in USD.

Table 2: Treatment Effects on Business and Household Finances

	Monthly Revenue	Monthly Profit	Monthly HH Income	Value of Business Capital	Employees
	(1)	(2)	(3)	(4)	(5)
Panel A: Without controls					
Treatment	810.44**	357.81**	334.27*	4394.60*	0.15
	(287.74)	(124.48)	(158.60)	(1953.00)	(0.13)
Control group mean	3199.93	1745.05	2532.45	13951.11	0.87
	[2720.43]	[1331.13]	[1638.65]	[20819.42]	[1.43]
No. Obs	2,572	2,567	1,020	1,063	1,087
Panel B: With controls					
Treatment	440.70**	216.83***	267.08*	2804.88*	0.03
	(184.75)	(82.25)	(138.05)	(1513.47)	(0.09)
Control group mean	3199.93	1745.05	2532.45	13951.11	0.87
	[2720.43]	[1331.13]	[1638.65]	[20819.42]	[1.43]
No. Obs	2,572	2,567	1,020	1,063	1,087

Notes: ***$p \leq 0.01$, **$p \leq 0.05$, *$p \leq 0.10$. Outcomes in Columns 1 and 2 come from loan application data and survey data. Outcomes in Columns 3–5 come from survey data. Outcomes are top and bottom coded at the 1st and 99th percentiles. Controls in Panel B include month-year fixed effects and PDS-LASSO controls using all the variables presented in Table A.2. Every regression in Panel B forces a control for the baseline value of the outcome variable. Standard deviation reported in square brackets and robust standard errors in parentheses. Standard errors are clustered at the borrower level, and all regressions use IPW. All monetary values are in USD.

Table 3: Treatment Effects on Consumer Surplus

	Customer level			Business level	
	Transaction Size	Willingness to Pay	Consumer Surplus	Number of Customers	Total Consumer Surplus
	(1)	(2)	(3)	(4)	(5)
Panel A: Without controls					
Treatment	2.86	3.21	0.61*	53.74**	176.02***
	(1.58)	(2.06)	(0.29)	(17.09)	(49.25)
Control group mean	10.96	13.67	1.85	94.76	179.94
	[13.27]	[17.78]	[3.19]	[152.53]	[268.31]
No. Obs	1,484	1,511	1,543	1,052	247
Panel B: With controls					
Treatment	3.34**	3.89*	0.97***	28.06***	78.17*
	(1.57)	(2.01)	(0.29)	(10.40)	(43.27)
Control group mean	10.96	13.67	1.85	94.76	179.94
	[13.27]	[17.78]	[3.19]	[152.53]	[268.31]
No. Obs	1,484	1,511	1,542	1,052	247

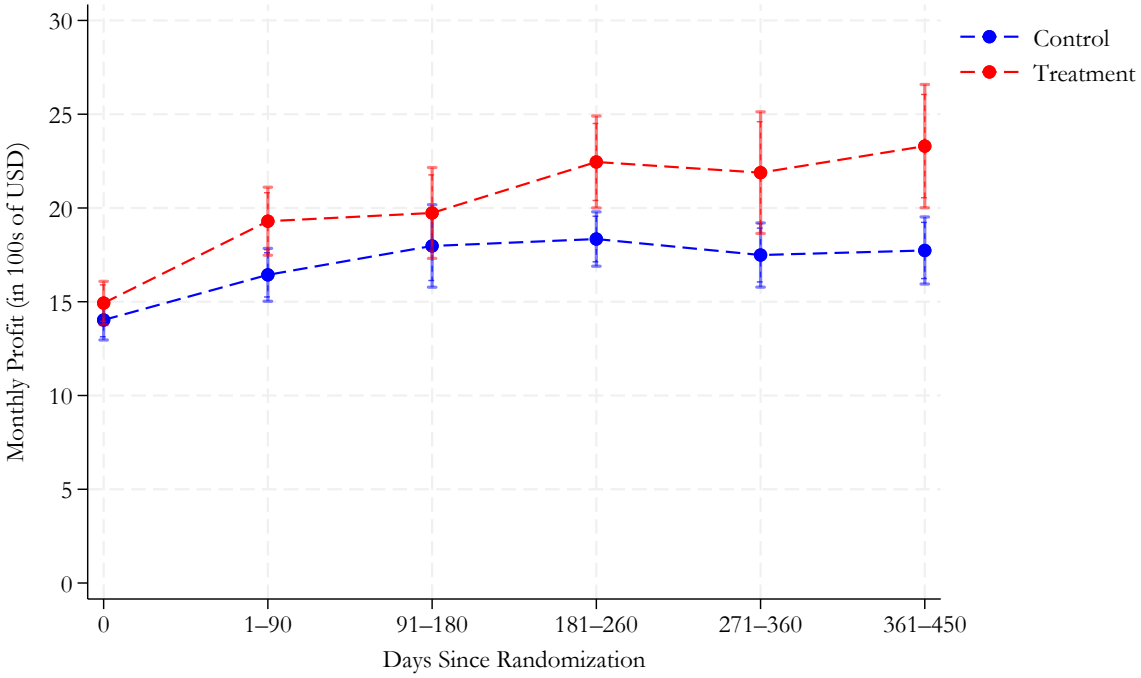
Notes: ***$p\leq0.01$, **$p\leq0.05$, *$p\leq0.10$. Outcomes in Columns 1–3 come from customer surveys. The outcome in column 4 comes from business surveys and is measured at the weekly level. The outcome in Column 5 is constructed by taking the average consumer surplus (Column 3) at the firm level and multiplying by the number of customers (Column 4). Controls include month-year fixed effects and PDS-LASSO controls, which use all business baseline characteristics presented in Table A.2 as controls. In Column 4, we also force the control for baseline number of clients (we cannot do this for the other outcomes as we did not collect baseline customer data). Outcomes in Columns 1–3 are trimmed at the 5th and 95th percentiles. The outcome in Column 4 is top and bottom–coded at the 1st and 99th percentiles. Standard deviation reported in square brackets and robust standard errors in parentheses. Standard errors are clustered at the borrower level, and all regressions use IPW. All monetary values are in USD.

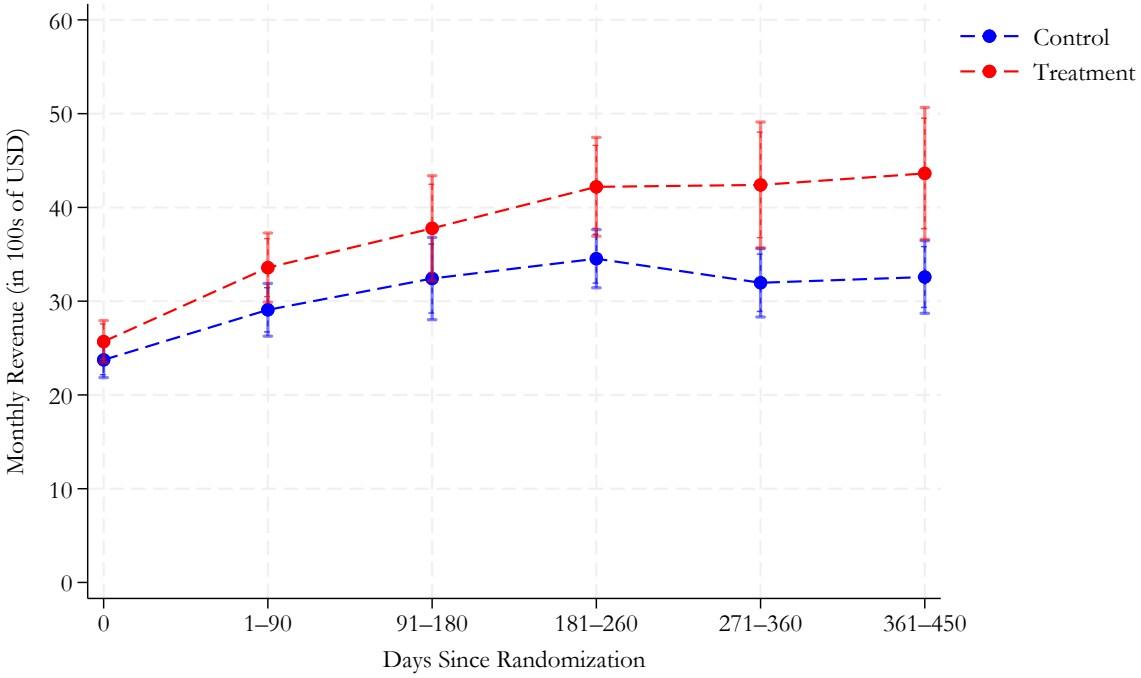
Table 4: Treatment Effects on Customer Preferences

	Reasons Why Customers Prefer the Business								
	Price	Quality	Customer Service	Trust	Location	Support Local	Prefer Over Big	Variety	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Without controls									
Treatment	0.03	0.04	0.09**	0.06	-0.08*	0.04	0.04*	-0.00	0.02
	(0.05)	(0.05)	(0.04)	(0.05)	(0.05)	(0.04)	(0.02)	(0.01)	(0.02)
Control group mean	0.58	0.59	0.44	0.42	0.50	0.21	0.06	0.01	0.06
	[0.49]	[0.49]	[0.50]	[0.49]	[0.50]	[0.41]	[0.25]	[0.10]	[0.25]
No. Obs	1,615	1,615	1,615	1,615	1,615	1,615	1,615	1,615	1,615
Panel B: With controls									
Treatment	0.03	0.04	0.09**	0.06	-0.08*	0.04	0.04*	-0.00	0.02
	(0.05)	(0.05)	(0.04)	(0.05)	(0.05)	(0.04)	(0.02)	(0.01)	(0.02)
Control group mean	0.58	0.59	0.44	0.42	0.50	0.21	0.06	0.01	0.06
	[0.49]	[0.49]	[0.50]	[0.49]	[0.50]	[0.41]	[0.25]	[0.10]	[0.25]
No. Obs	1,615	1,615	1,615	1,615	1,615	1,615	1,615	1,615	1,615

Notes: ***$p\leq0.01$, **$p\leq0.05$, *$p\leq0.10$. Outcomes in this table come from customer surveys. Controls include month-year fixed effects and PDS-LASSO controls using all the variables presented in Table A.2. Every regression forces a control for the baseline value of the outcome variable. Standard deviation reported in square brackets and robust standard errors in parentheses. Standard errors are clustered at the borrower level, and all regressions use IPW.

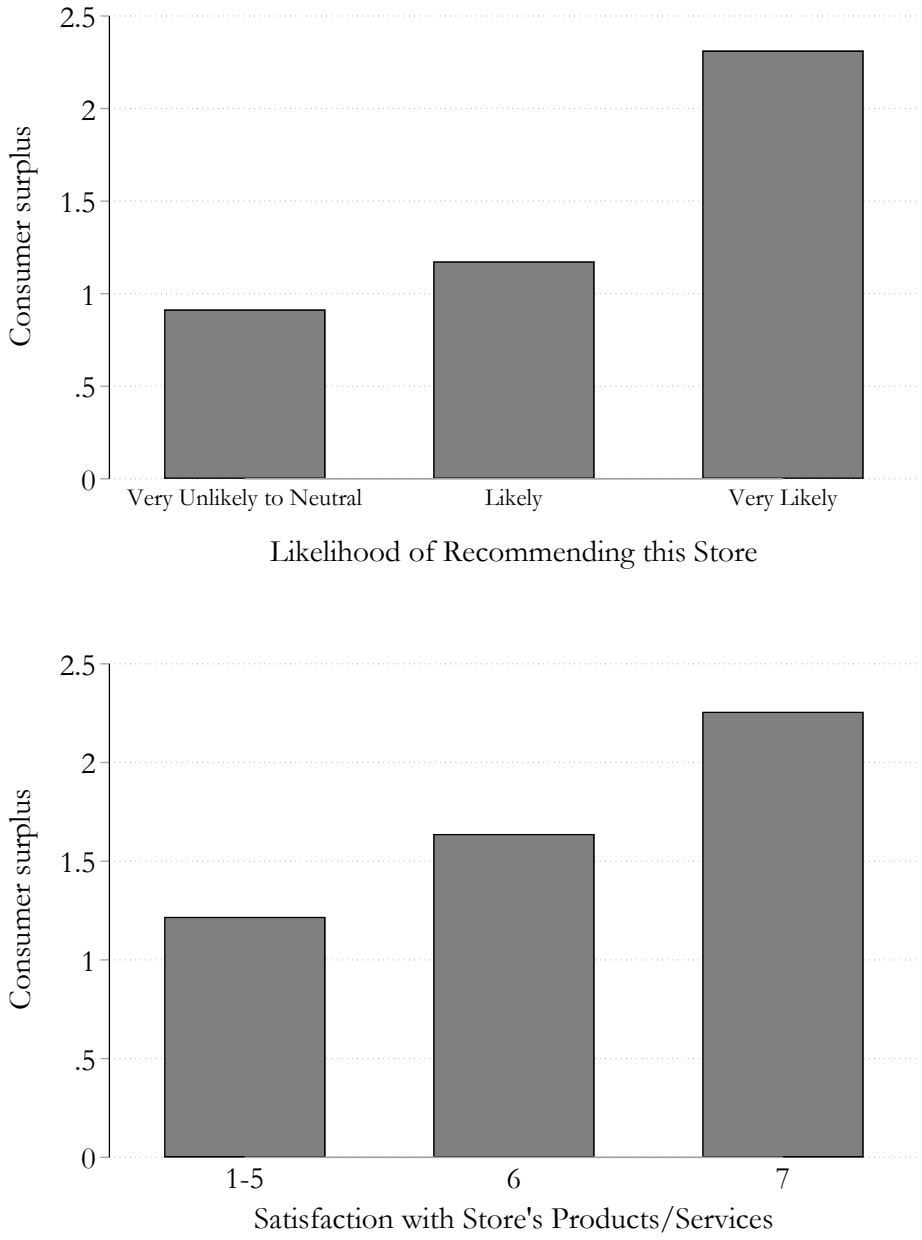
Figure 1: Evolution of Business and Household Finances





Notes: In this figure, we plot the evolution of monthly business profits and revenues for the treatment and control group. The data for this figure come from the loan applications data as well as the survey data. Observations are pooled by time to randomization. The values at 0 are baseline (pre-intervention) values. The figure shows the means for each time frame as well as the 90% and 95% confidence intervals of the means (the whiskers). We are unable to plot other outcome variables since they are not collected in the relatively high-frequency loan application data.

Figure 2: Correlation of Consumer Surplus and Customer Satisfaction/Recommendation



Notes: In this figure, we plot the correlation between consumer surplus and customer satisfaction/recommendation. Customers are asked how likely they are to recommend the store to their friends and family (Very Unlikely, Unlikely, Neutral, Likely Very Likely) and how satisfied they are with the store's products/services (on a scale of 1-7, with 7 being the highest level of satisfaction). In the top figure, we pool answers Very Unlikely, Unlikely, Neutral since there are relatively few observations in the first two categories. In the bottom figure we club values 1-5 for the same reason. The dots represent the average consumer surplus value for each category of answer. The dotted likes represent the lines of best fit.

Appendix Tables and Figures

Table A.1: Survey Completion Rates

	Control	Treatment
	Mean	Difference
	(1)	(2)
Panel A: Business Outcomes		
Baseline	1.00	0.00
	[0.00]	(0.00)
Follow Up I (6m)	0.55	-0.05
	[0.50]	(0.04)
Follow Up II (9M)	0.54	-0.04
	[0.50]	(0.04)
Follow Up III (12M)	0.39	-0.00
	[0.49]	(0.04)
Follow Up IV (18M)	0.61	-0.05
	[0.49]	(0.04)
Any Follow Up Survey	0.92	-0.03
	[0.27]	(0.03)
Any Loan Application	0.96	-0.01
	[0.20]	(0.02)
Any Follow Up Survey or Any Loan Application	0.99	-0.01
	[0.11]	(0.01)
Panel B: Customer Value		
Follow Up II: Number of customers	379	128
Follow Up II: Proportion of Businesses Represented	0.25	0.03
	[0.43]	(0.04)
Follow Up IV: Number of customers	397	-37
Follow Up IV: Proportion of Businesses Represented	0.34	-0.00
	[0.48]	(0.04)
Proportion of Businesses Represented in Any CS Survey	0.45	0.05
	[0.50]	(0.04)
Total Businesses	269	272

Notes: ***$p \leq 0.01$, **$p \leq 0.05$, *$p \leq 0.10$. Standard deviation reported in square brackets and robust standard errors in parentheses. The second column presents the difference between the treatment group and control group.

Table A.2: Balance Table

	Control	Treatment	Total
	Mean	Difference	N
	(1)	(2)	(3)
Panel A: Demographics and HH Outcomes			
Age	41.25	0.03	538
	[9.81]	(0.85)	
Male	0.20	-0.00	541
	[0.40]	(0.03)	
Post-Secondary Education	0.29	0.04	534
	[0.45]	(0.04)	
HH Members	3.81	-0.03	541
	[1.35]	(0.13)	
HH Income (USD)	2806.05	101.35	531
	[1584.57]	(145.49)	
Panel B: Business Outcomes			
Monthly Income (USD) †	2373.99	187.18	541
	[1575.25]	(149.27)	
Monthly Profits (USD) †	1402.77	87.06	541
	[889.72]	(80.16)	
Number of Weekly Customers	118.52	33.07*	527
	[170.04]	(17.32)	
Number of Employees	0.76	0.22*	531
	[1.16]	(0.11)	
Value of Business Capital (USD)	14093.78	2551.70	531
	[19229.00]	(1764.46)	
Sector: Manufacturing	0.25	-0.02	541
	[0.43]	(0.04)	
Sector: Retail	0.56	0.03	541
	[0.50]	(0.04)	
Sector: Services	0.18	-0.01	541
	[0.39]	(0.03)	
Sector: Agriculture	0.01	-0.00	541
	[0.11]	(0.01)	
F test			
F-stat		0.81	
p-value		0.651	

Notes: ***$p\leq0.01$, **$p\leq0.05$, *$p\leq0.10$. Standard deviation reported in square brackets and robust standard errors in parentheses. The second column presents the difference between the treated group minus the control group. † This variables comes from loan application data. The baseline characteristics are top coded at the 1st and 99th percentiles. All monetary values are in USD.

Table A.3: Differences-in-Differences Specification

	Monthly Revenue	Monthly Profit	Monthly HH Income	Value of Business Capital	Employees
	(1)	(2)	(3)	(4)	(5)
Panel A: With firm fixed effects					
Post · Treatment	498.92*	246.43*	252.23	2420.86	-0.15
	(199.04)	(95.94)	(171.54)	(1543.66)	(0.10)
Post	699.91***	254.19***	-277.18*	-327.99	0.13
	(96.69)	(54.06)	(107.19)	(980.17)	(0.08)
Control group mean	2721.62	1551.29	2630.73	14019.77	0.83
	[2445.02]	[1509.37]	[1667.33]	[20457.29]	[1.35]
No. Obs	7,441	7,436	1,493	1,538	1,566
Panel B: With PDS-LASSO controls					
Post · Treatment	566.95***	249.76**	271.24	2477.49	-0.07
	(204.43)	(97.68)	(172.59)	(1613.85)	(0.10)
Post	671.72***	260.94***	-291.07***	-286.97	0.08
	(101.30)	(55.13)	(110.91)	(1037.33)	(0.08)
Treatment	23.69	22.00	-13.51	376.08	0.07***
	(41.07)	(25.94)	(66.84)	(397.95)	(0.03)
Control group mean	2721.62	1551.29	2630.73	14019.77	0.83
	[2445.02]	[1509.37]	[1667.33]	[20457.29]	[1.35]
No. Obs	7,441	7,436	1,551	1,594	1,618

Notes: ***$p\leq0.01$, **$p\leq0.05$, *$p\leq0.10$. In this table, we estimate Specification 3; it is the difference-in-differences analogue of Table 2. In Columns 1-2, we add pre-randomization loan application data (as early as June, 2020). In Columns 3-5, the pre-randomization data point comes from the baseline survey. Panel A includes firm fixed effects. Panel B controls include month-year fixed effects and PDS-LASSO controls using all the variables presented in Table A.2. Every regression forces a control for the baseline value of the outcome variable. Outcomes are top and bottom coded at the 1st and 99th percentiles. Standard deviation reported in square brackets and robust standard errors in parentheses. Standard errors are clustered at the borrower level, and all regressions use IPW.

Table A.4: Treatment Effects on Average Business and Household Finances

	Monthly Revenue	Monthly Profit	Monthly HH Income	Value of Business Capital	Employees
	(1)	(2)	(3)	(4)	(5)
Panel A: Without controls					
Treatment	720.28**	366.24**	378.74*	4678.31*	0.13
	(261.44)	(113.81)	(150.69)	(1956.01)	(0.13)
Control group mean	3153.58	1687.60	2497.15	13641.67	0.90
	[2323.60]	[1053.25]	[1478.21]	[18470.07]	[1.32]
No. Obs	532	532	481	483	487
Panel B: With controls					
Treatment	334.94**	220.45***	234.84*	2805.39**	-0.04
	(161.50)	(75.44)	(127.62)	(1389.84)	(0.10)
Control group mean	3153.58	1687.60	2497.15	13641.67	0.90
	[2323.60]	[1053.25]	[1478.21]	[18470.07]	[1.32]
No. Obs	532	532	481	483	487

Notes: ***$p\leq0.01$, **$p\leq0.05$, *$p\leq0.10$. In this table, we estimate Specification 2. Relative to Table 2, in this table the outcome variables are the average of all post-randomization observations by respondent. Controls include month-year fixed effects and PDS-LASSO controls using all the variables presented in Table A.2. Every regression forces a control for the baseline value of the outcome variable. Standard deviation reported in square brackets and robust standard errors in parentheses. Standard errors are clustered at the borrower level, and all regressions use IPW.

Table A.5: First Stage - Loan Take-Up with Baseline Controls

		Partner MFI			Outside Partner MFI	
	Total Credit Taken Amount	Took RCT Loan	RCT Loan Amount	Total Credit Taken Amount	Took Loan	Total Credit Taken Amount
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	1196.18***	0.98***	1785.81***	1354.77***	-0.03	-169.65
	(160.21)	(0.01)	(26.48)	(52.48)	(0.03)	(148.67)
Control group mean	1866.77	0.00	0.00	1430.32	0.13	438.18
	[1964.34]	[0.00]	[0.00]	[597.76]	[0.34]	[1881.29]
No. Obs	439	538	538	538	439	439

Notes: ***$p\leq0.01$, **$p\leq0.05$, *$p\leq0.10$. Outcomes in columns 2-4 come from MFI administrative data. The outcomes in columns 5 and 6 come from survey data (hence the lower number of observations): respondents are asked whether they took a loan from someone other than the partner MFI after the beginning of the study and the amount of the loan. The outcome in column 1 is the sum of columns 4 and 6 for those who answered the survey. Controls included are Number of Weekly Customers, Number of Employees, Monthly Profits and HH Income and month-year fixed effects. Standard deviation reported in square brackets and robust standard errors in parentheses. Standard errors are clustered at the borrower level, and all regressions use IPW. All monetary values are in USD. Last update in 26 Mar 2026.

Table A.6: Treatment Effects on Business and Household Finances with Baseline Controls

	Monthly Revenue	Monthly Profit	Monthly HH Income	Value of Business Capital	Employees
	(1)	(2)	(3)	(4)	(5)
Treatment	467.84**	233.37***	230.02*	3309.52*	0.02
	(197.84)	(85.24)	(135.85)	(1802.68)	(0.09)
Control group mean	3199.93	1745.05	2532.45	13951.11	0.87
	[2720.43]	[1331.13]	[1638.65]	[20819.42]	[1.43]
No. Obs	2,572	2,567	1,020	1,063	1,087

Notes: ***$p\leq0.01$, **$p\leq0.05$, *$p\leq0.10$. Outcomes are top and bottom coded at the 1st and 99th percentiles. Controls included are Number of Weekly Customers, Number of Employees, Monthly Profits and HH Income and month-year fixed effects. Standard deviation reported in square brackets and robust standard errors in parentheses. Standard errors are clustered at the borrower level, and all regressions use IPW. Last update in 26 Mar 2026.

Table A.7: Treatment Effects on Consumer Surplus with Baseline Controls

	Customer level			Business level	
	Transaction Size	Willingness to Pay	Consumer Surplus	Number of Customers	Total Consumer Surplus
	(1)	(2)	(3)	(4)	(5)
Treatment	4.26***	5.25***	0.97***	26.70***	98.79**
	(1.49)	(1.91)	(0.29)	(10.29)	(44.87)
Control group mean	10.94	13.67	1.85	94.76	176.65
	[13.29]	[17.78]	[3.19]	[152.53]	[260.97]
No. Obs	1,466	1,511	1,545	1,052	246

Notes: ***$p\leq0.01$, **$p\leq0.05$, *$p\leq0.10$. Data in columns 1-3 come from customer surveys. Data in column 4 come from business surveys. Outcomes in columns 1-3 are trimmed at the 5th and 95th percentiles. The outcome in column 4 is top-coded at the 1st and 99th percentiles. Controls included are Number of Weekly Customers, Number of Employees, Monthly Profits and HH Income and month-year fixed effects. Standard deviation reported in square brackets and robust standard errors in parentheses. Standard errors are clustered at the borrower level, and all regressions use IPW. Last update in 26 Mar 2026.

Table A.8: Correlation of Consumer Surplus and Business Size

	Corr			
	TS	WTP	CS	TCS
	(1)	(2)	(3)	(4)
Baseline characteristics				
Monthly Revenue †	-0.066	-0.077	-0.034	0.356***
	(0.303)	(0.230)	(0.598)	(0.000)
Monthly Profit †	-0.010	-0.009	-0.019	0.257***
	(0.879)	(0.891)	(0.769)	(0.000)
HH Income	-0.062	-0.056	-0.054	0.261***
	(0.336)	(0.384)	(0.399)	(0.000)
Weekly Number of Customers	-0.255***	-0.265***	-0.153**	0.398***
	(0.000)	(0.000)	(0.016)	(0.000)
Employees	-0.052	-0.096	0.081	0.459***
	(0.427)	(0.137)	(0.206)	(0.000)
Value of Assets	-0.009	-0.004	0.044	0.381***
	(0.896)	(0.950)	(0.488)	(0.000)
Value of Inventory	0.010	0.042	0.090	0.440***
	(0.873)	(0.518)	(0.161)	(0.000)

Notes: ***$p \leq 0.01$, **$p \leq 0.05$, *$p \leq 0.10$. This table estimates the correlation between the residuals of the baseline business characteristics and the residuals of the total consumer surplus, and its components, controlling for subsectors. TS stands for transaction size, WTP for willingness to pay, CS for consumer surplus, and TCS for total consumer surplus. Baseline characteristics are top-coded at the 1st and 99th percentiles. Transaction size, willingness to pay, and consumer surplus are trimmed at the 5th and 95th percentiles. † This variables comes from Loan Application Data.

Table A.9: Correlation of Consumer Surplus and Business Sector

	Corr			
	TS	WTP	CS	TCS
	(1)	(2)	(3)	(4)
Subsector				
Apparel and Footwear	0.168***	0.182***	0.262***	0.007
Bakery and Pastry	0.059	0.019	0.045	-0.032
Beauty and Personal Care Services	0.102	0.089	0.175***	-0.075
Beauty and Personal Care Retail	-0.035	-0.043	-0.046	-0.046
Fresh and Specialty Food Retail	-0.078	-0.098	-0.159**	-0.013
Gifts and Bazaar	-0.075	-0.093	-0.081	-0.015
Grocery and Convenience Retail	-0.275***	-0.255***	-0.196***	-0.078
Handicrafts	0.055	0.057	0.011	0.103
Restaurants and Prepared Food Services	-0.096	-0.078	-0.174***	0.144**
Tailoring	0.198***	0.185***	0.145**	-0.076
Other Manufacturing	0.200***	0.276***	.	.
Other Retail	-0.094	-0.025	-0.070	0.021
Other Services	0.122*	0.086	0.144**	0.054

Notes: ***$p \leq 0.01$, **$p \leq 0.05$, *$p \leq 0.10$. This table estimates the correlation between the business subsector and the total consumer surplus, and its components. TS stands for transaction size, WTP for willingness to pay, CS for consumer surplus, and TCS for total consumer surplus. Transaction size, willingness to pay, and consumer surplus are trimmed at the 5th and 95th percentiles.

Table A.10: Default

	15 Days	90 Days	Amount Defaulted
	(1)	(2)	(3)
Panel A: BC loans			
Treatment	0.00	0.00	1.11
	(0.00)	(.)	(2.27)
Control group mean	0.00	0.00	3.94
	[0.06]	[0.00]	[31.13]
No. Obs	1,743	1,743	1,743
Panel B: All loans			
Treatment	0.03***	0.03***	25.85***
	(0.01)	(0.00)	(4.55)
Control group mean	0.01	0.00	5.34
	[0.10]	[0.04]	[37.31]
No. Obs	2,707	2,707	2,707
PDS-LASSO Controls	Yes	Yes	Yes

Notes: ***$p \leq 0.01$, **$p \leq 0.05$, *$p \leq 0.10$. Controls include month-year fixed effects and PDS-LASSO controls using all the variables presented in Table A.2. Standard deviation reported in square brackets and robust standard errors in parentheses. Standard errors are clustered at the borrower level, and all regressions use IPW.

Table A.11: Repayment Prediction

	0 days	15 days	90 days
	(1)	(2)	(3)
Intention to Purchase: Remodeling	0.09		
	(0.06)		
Intention to Purchase: Inventory	0.12*		
	(0.06)		
Monthly Profits (USD)	-0.00		
	(0.00)		
Monthly Revenues (USD)	-0.00		
	(0.00)		
Value of Assets (USD)	0.00***		
	(0.00)		
Sector: Manufacturing	-0.02		
	(0.09)		
Sector: Retail	0.06	0.11**	
	(0.07)	(0.05)	
Age	0.01***	0.00**	
	(0.00)	(0.00)	
Female	0.10		
	(0.08)		
Expected Earnings Without CI	0.00*	0.00*	
	(0.00)	(0.00)	
Var(Expected Earnings With CI)	-0.00***		
	(0.00)		
Total loans	0.01		
	(0.00)		
Average Days of Delinquency	-0.05**	-0.06**	
	(0.02)	(0.02)	
FE Zone 2	-0.15**	-0.07	
	(0.07)	(0.06)	
FE Zone 3	0.14**	0.11**	
	(0.05)	(0.04)	
HH Members		-0.01	
		(0.01)	
Constant	0.19	0.58***	0.86***
	(0.14)	(0.13)	(0.02)
Treatment group mean	0.72	0.83	0.86
	[0.45]	[0.38]	[0.35]
No. Obs	265	265	265
R^2	0.21	0.10	-0.00
$adj R^2$	0.16	0.08	-0.00
AUC	0.79	0.73	0.50
Brier Score	0.16	0.13	0.12

Notes: ***$p\leq0.01$, **$p\leq0.05$, *$p\leq0.10$. Standard Deviation reported in square brackets and robust standard errors in parentheses. Standard errors are clustered at the borrower level. Estimates are from a OLS regression model. Control variables are selected via LASSO.

Table A.12: Repayment Prediction

	0 days	15 days
	(1)	(2)
Predicted Repayment Probability	0.23***	0.11***
	(0.02)	(0.03)
Treatment group mean	0.72	0.83
	[0.45]	[0.38]
No. Obs	265	265

Notes: ***$p \leq 0.01$, **$p \leq 0.05$, *$p \leq 0.10$. Standard Deviation reported in square brackets and robust standard errors in parentheses. Standard errors are clustered at the borrower level.

Table A.13: RCT using administrative and survey data - Heterogenity

	HH Income		TCS		HH Inc + 4 · TCS	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 0 days						
Treatment · Predicted Repayment Probability	-175.52	-52.42	-77.56*	-60.33	-551.73	-328.50
	(210.49)	(164.59)	(45.77)	(37.18)	(420.82)	(335.79)
Predicted Repayment Probability	-66.98	85.09	12.95	22.64**	-66.50	149.71
	(95.77)	(102.15)	(11.94)	(11.09)	(105.42)	(147.39)
Treatment	388.06**	251.28*	184.23***	82.59*	1661.85***	930.78***
	(172.35)	(143.75)	(50.59)	(44.36)	(376.11)	(280.70)
Control group mean	2532.45	2532.45	176.65	176.65	3016.40	3016.40
	[1638.65]	[1638.65]	[260.97]	[260.97]	[1884.06]	[1884.06]
No. Obs	1,020	1,020	246	246	566	566
Panel B: 15 days						
Treatment · Predicted Repayment Probability	264.92	94.25	88.38	24.95	750.87*	335.28
	(168.95)	(149.08)	(58.52)	(44.85)	(402.52)	(324.44)
Predicted Repayment Probability	-49.66	-35.72	10.56	15.21	-30.69	-34.32
	(61.75)	(62.86)	(11.35)	(9.66)	(85.59)	(88.24)
Treatment	307.56*	263.99*	157.18***	72.22	1373.59***	879.22***
	(156.82)	(138.26)	(50.92)	(44.62)	(344.40)	(265.46)
Control group mean	2532.45	2532.45	176.65	176.65	3016.40	3016.40
	[1638.65]	[1638.65]	[260.97]	[260.97]	[1884.06]	[1884.06]
No. Obs	1,020	1,020	246	246	566	566
Month-Year FE	No	Yes	No	No	No	No
PDS-LASSO Controls	No	Yes	No	Yes	No	Yes

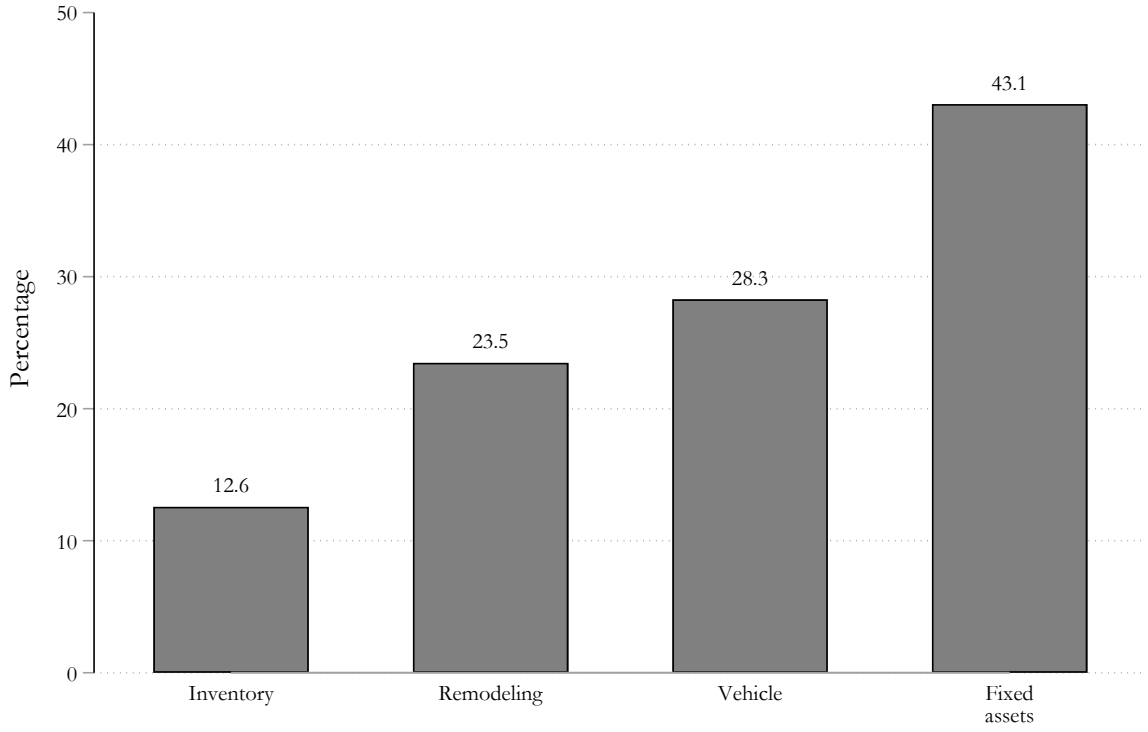
***$p\leq0.01$, **$p\leq0.05$, *$p\leq0.10$. Standard Deviation reported in square brackets and robust standard errors in parentheses. Controls include month-year fixed effects and PDS-LASSO controls, which use all business baseline characteristics presented in Table A.2 as controls forcing to include the baseline value of the outcome variable. Outcomes are top coded at the 1st and 99th percentiles. Standard errors are clustered at the borrower level, and all regressions use IPW. Predicted Repayment Probability (Standardized - 0 days).

Table A.14: Social Benefits by Borrower Portfolio

	Portfolio Repayment Rate (PRR)	Required Subsidy for Marginal Borrowers	PRR for Marginal Borrowers	ATE for Marginal Borrowers (HH Inc)	ATE for Marginal Borrowers (HH Inc + 4 · TCS)
	(1)	(2)	(3)	(4)	(5)
Panel A: 0 days					
	95%	11.71	97.21%	149.76	294.58
		(2.77 - 20.66)	(95.08% - 99.34%)	(-435.12 - 734.64)	(-915.37 - 1504.53)
	90%	31.66	92.46%	202.04	622.21
		(23.60 - 39.72)	(90.54% - 94.38%)	(-131.93 - 536.01)	(-55.78 - 1300.20)
	85%	51.01	87.85%	240.29	861.91
		(45.72 - 56.30)	(86.59% - 89.11%)	(26.31 - 454.27)	(408.22 - 1315.60)
	80%	73.27	82.55%	267.06	1029.61
		(68.90 - 77.63)	(81.51% - 83.59%)	(16.77 - 517.35)	(450.14 - 1609.08)
Panel B: 15 days					
	95%	15.53	97.78%	370.12	1256.77
		(2.80 - 28.27)	(95.96% - 99.60%)	(-46.97 - 787.21)	(362.97 - 2150.57)
	90%	53.04	92.42%	292.63	981.09
		(41.57 - 64.52)	(90.78% - 94.06%)	(-41.34 - 626.60)	(303.10 - 1659.08)
	85%	86.28	87.67%	232.99	768.93
		(76.55 - 96.01)	(86.28% - 89.06%)	(19.01 - 446.97)	(315.24 - 1222.62)
	80%	118.61	83.05%	165.82	529.97
		(108.67 - 128.55)	(81.63% - 84.47%)	(-84.47 - 416.11)	(-49.50 - 1109.44)

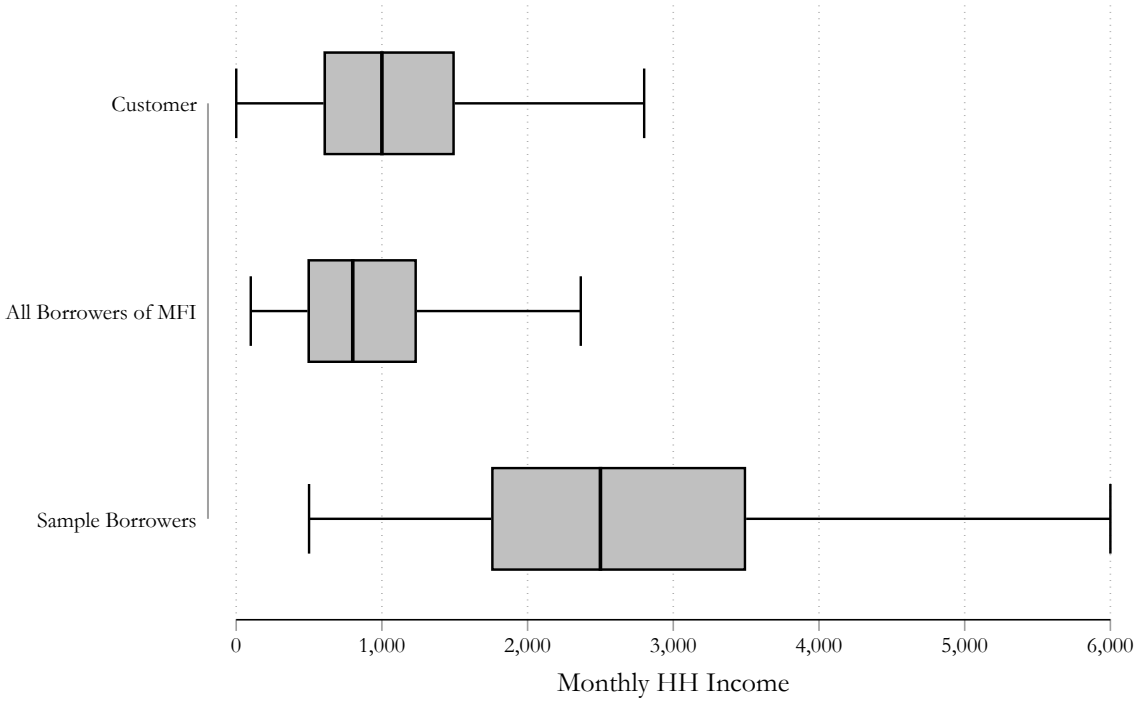
Portfolio Repayment Rate is the percentage of borrowers in the portfolio who do not experience delinquency. Lateness is defined using the '0 day' definition. This table includes only treated borrowers.

Figure A.1: Distribution of Investment Categories



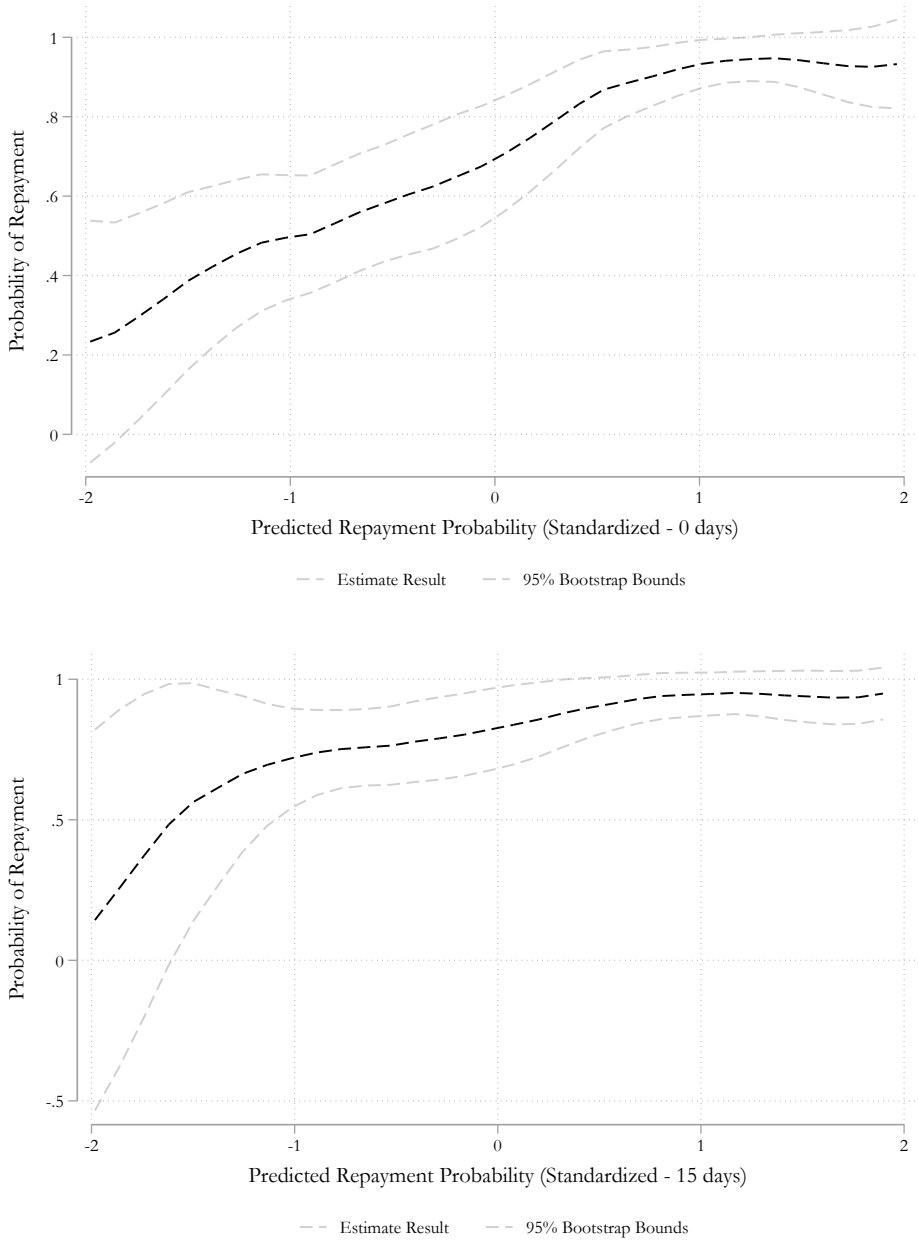
Notes: In this figure, we plot the categories that treatment and control respondents said they would invest their loans in if they were to receive the RCT loan. These data were collected at baseline.

Figure A.2: Distribution of Household Income by Group



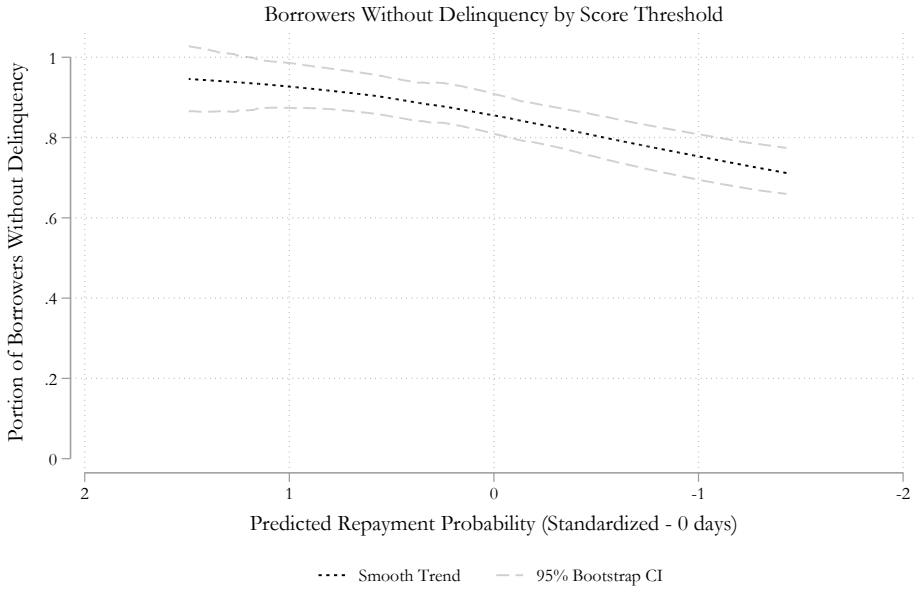
Notes: In this figure, we plot the distribution of monthly household income for 3 groups: customers of sample businesses (from customer surveys), all borrowers of our partner MFI, and sample participants (at baseline). The latter two outcomes come from administrative self-reported data. Moments of the customer income distribution: USD 600 (25th percentile), USD 1000 (median), USD 1500 (75th percentile), USD 1368 (mean). Moments of the income distribution of all partner MFI borrowers: USD 490 (25th percentile), USD 800 (median), USD 1240 (75th percentile), USD 960 (mean). Moments of the sample income distribution: USD 1750 (25th percentile), USD 2500 (median), USD 7500 (75th percentile), USD 2400 (mean).

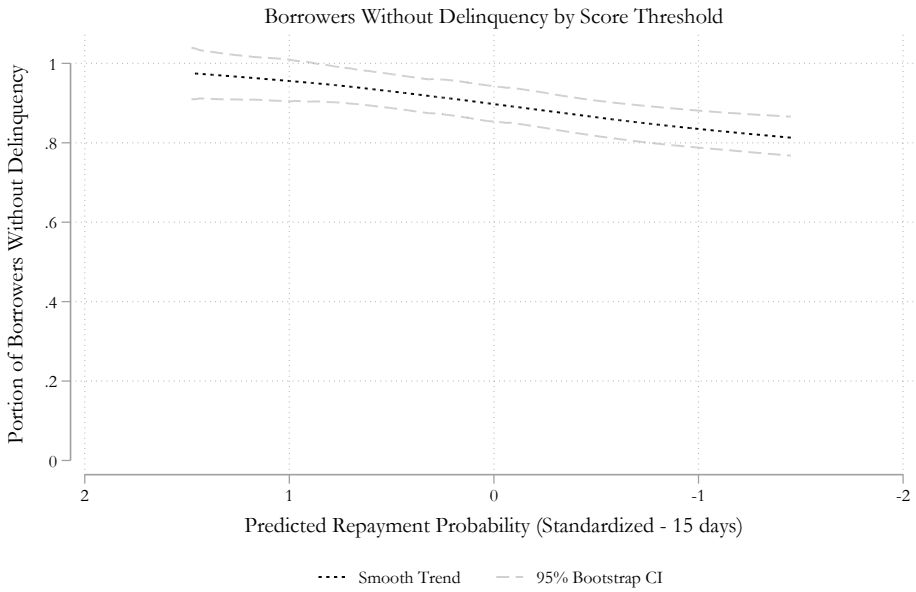
Figure A.3: Late Repayment on Predicted Repayment



Notes:

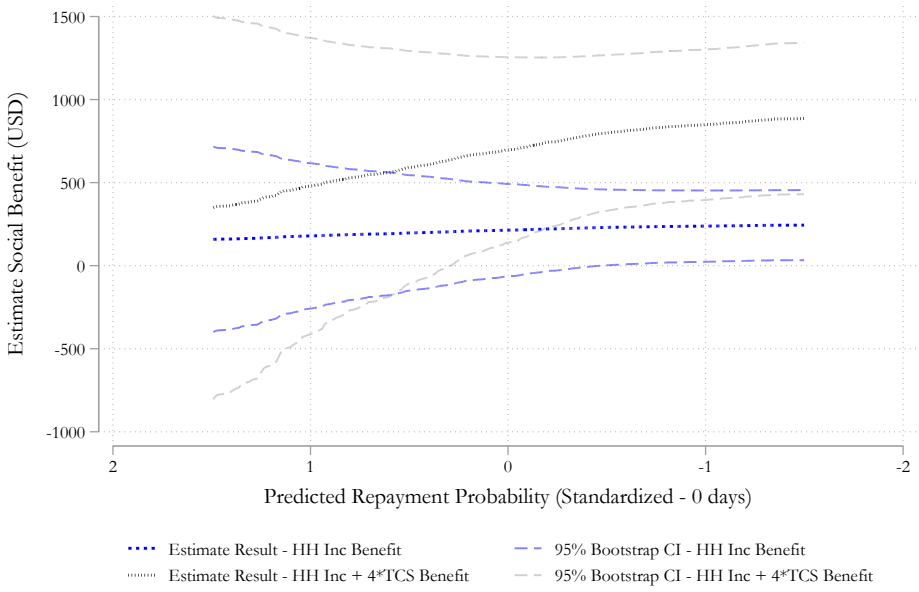
Figure A.4: Predicted Repayment by Portfolio

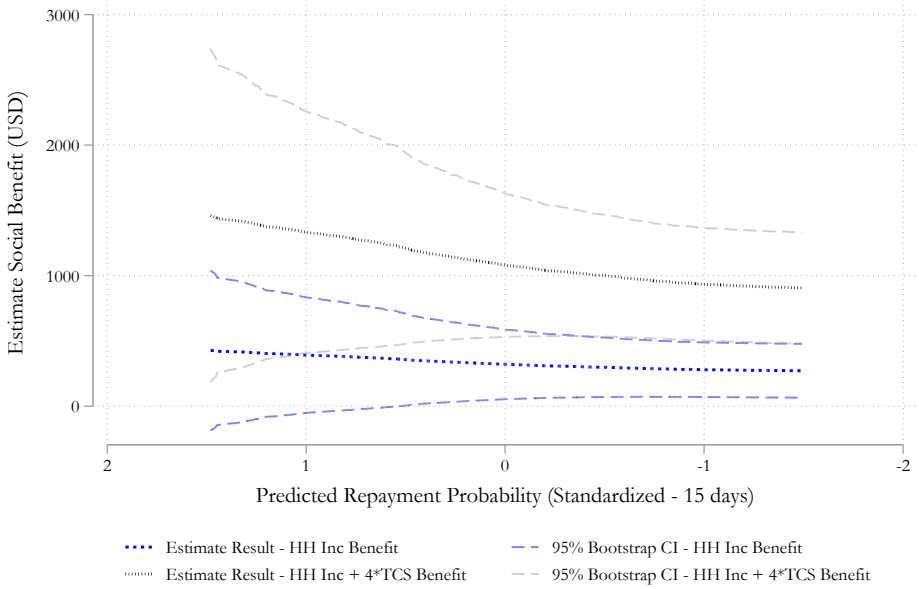




Notes:

Figure A.5: Social Benefit





Notes:

Randomization Process

Loan applicants who were deemed eligible for the large loan were passed randomized in 13 rounds, based on the time at which they submitted their loan application. Recall that all borrowers eligible for the larger loan must also have had a standard joint liability loan with our partner lender, and thus already had a loan officer assigned to them.

From rounds 1 to 4, our microfinance partner imposed a cap of three treated borrowers per loan officer. In the first round in which a loan officer had a borrower in the sample, a maximum of two treated borrowers could be assigned to the loan officer, reserving the third treatment slot for subsequent rounds. Beginning in round 5, the cap was increased to five treated borrowers per loan officer. From rounds 6 to 13, this restriction was fully lifted, removing any limit on the number of treated borrowers that could be assigned to a given loan officer.

Within a randomization round x loan officer, borrowers were randomly assigned to the treatment and control groups with uniform probability, where the probability of assignment to the treatment group was dictated by the by the capacity constraints outlined above.

B Proofs of Theorems 1 and 2

Proof of Theorem 1. Consider the consumers $c_i \in \mathcal{C}'_T$, and consider two cases.

Case 1: $c_i \in \mathcal{C}_T$ (i.e. consumer c_i buys good γ_T in both economy $\mathcal{E}$ and $\mathcal{E}'$. In this case, the residual consumer surplus of c_i is part of the sum in CS'_T and CS_T.

In economy $\mathcal{E}'$, the residual consumer surplus of c_i is $WTP_i'^T - p'_T = \left(v_i'^T - \max_{k \in \mathcal{B}' \setminus b_T} v_i'^k - p'_k\right) - p'_T$. And in economy $\mathcal{E}$, the residual consumer surplus of c_i is $WTP_i^T - p_T = \left(v_i^T - \max_{k \in \mathcal{B} \setminus b_T} v_i^k - p_k\right) - p_T$.

By assumption, $\max_{k \in \mathcal{B}' \setminus b_T} v_i'^k - p'_k = \max_{k \in \mathcal{B} \setminus b_T} v_i^k - p_k$, as only firm T was treated.

Hence the difference between the residual consumer surplus of c_i in economy $\mathcal{E}'$ and that of c_i in economy $\mathcal{E}$ is $v_i'^T - p'T - \left(v_i^T - p_T\right)$, which is also the difference in total consumer surplus for c_i across the two economies.

Case 2: : $c_i \notin \mathcal{C}_T$ (i.e. consumer c_i buys good γ_T in economy $\mathcal{E}'$ and buys some other good γ_0 in economy $\mathcal{E}$. In this case, the residual consumer surplus of c_i is part of the sum in CS'_T, but not CS_T.

In economy $\mathcal{E}'$, the residual consumer surplus of c_i is $WTP_i'^T - p'_T = \left(v_i'^T - \max_{k\in\mathcal{B}'\setminus b_T} v_i'^k - p'_k\right) - p'_T$.

By assumption, $\max_{k\in\mathcal{B}'\setminus b_T} v_i'^k - p'_k = v_i^O - p_O$. Hence the residual consumer surplus of c_i in economy $\mathcal{E}'$ is $v_i'^T - p'_T - \left(v_i^O - p_O\right)$, which is the difference in total consumer surplus for c_i across the two economies.

It is easily verified that for any $c_i\mathcal{C}'_T$, consumer surplus is unaffected. Thus, $CS'_T - CS_T = CS' - CS$ $\square$

Proof of Theorem 2. Consider the consumers $c_i \in \sum_{T\in\mathcal{T}} \mathcal{C}'_T$, and consider two cases.

Case 1: $c_i \in \sum_{T\in\mathcal{T}} \mathcal{C}_T$ (i.e. consumer c_i buys a good from a firm in $\mathcal{T}$ in both economy $\mathcal{E}$ and $\mathcal{E}'$. In this case, the residual consumer surplus of c_i is part of the sum in $\sum_{T\in\mathcal{T}} CS'_T$ and $\sum_{T\in\mathcal{T}} CS_T$.

Denote the firm from which c_i buys in economy $\mathcal{E}'$ as b'_{T1} and the firm from which c_i buys in economy $\mathcal{E}$ as b_{T2}, recognizing that $T1$ may equal $T2$. In economy $\mathcal{E}'$, the residual consumer surplus of c_i is $WTP_i'^{T1} - p'_{T1} = \left(v_i'^{T1} - \max_{k\in\mathcal{B}'\setminus b_{T1}} v_i'^k - p'_k\right) - p'_{T1}$. And in economy $\mathcal{E}$, the residual consumer surplus of c_i is $WTP_i^{T2} - p_{T2} = \left(v_i^{T2} - \max_{k\in\mathcal{B}\setminus b_{T2}} v_i^k - p_k\right) - p_{T2}$.

In the following paragraph we show that $\max_{k\in\mathcal{B}'\setminus b_{T1}} v_i'^k - p'_k \geq \max_{k\in\mathcal{B}\setminus b_{T2}} v_i^k - p_k$. From here it immediately follows that the difference between the residual consumer surplus of c_i in economy $\mathcal{E}'$ and that of c_i in economy $\mathcal{E}$ is greater than $v_i'^T - p'_T - \left(v_i^T - p_T\right)$, which is the difference in total consumer surplus for c_i across the two economies.

To see that $\max_{k\in\mathcal{B}'\setminus b_{T1}} v_i'^k - p'_k \geq \max_{k\in\mathcal{B}\setminus b_{T2}} v_i^k - p_k$, we once again consider two cases:

$T1 = T2$ and $T1 \neq T2$. Let $O1 \equiv \text{argmax}_{k \in \mathcal{B}' \setminus b_{T1}} v_i'^k - p_k'$, and $O2 \equiv \text{argmax}_{k \in \mathcal{B} \setminus b_{T2}} v_i^k - p_k$, where O stands for "outside option". If $T1 = T2$ then $v_i'^{O1} - p_{O1}' \geq v_i'^{O2} - p_{O2}' \geq v_i^{O2} - p_{O2}$ where the first inequality holds by revealed preference and the second inequality holds by assumption. If $T1 \neq T2$ then $v_i'^{O1} - p_{O1}' \geq v_i'^{T2} - p_{T2}' \geq v_i^{T2} - p_{T2} \geq v_i^{O2} - p_{O2}$, where the first and third inequalities hold by revealed preference, and the second holds by assumption.

Case 2: : $c_i \notin \sum_{T \in \mathcal{T}} \mathcal{C}_T$ (i.e. consumer c_i buys good from a firm in $\mathcal{T}$ in economy $\mathcal{E}'$ and buys a good from some other firm $b_{T2} \notin \mathcal{T}$ in economy $\mathcal{E}$. In this case, the residual consumer surplus of c_i is part of the sum in $\sum_{T \in \mathcal{T}} CS_T'$, but not $\sum_{T \in \mathcal{T}} CS_T$.

Denote the firm from which c_i buys in economy $\mathcal{E}'$ as b_{T1}'. In economy $\mathcal{E}'$, the residual consumer surplus of c_i is $WTP_i'^{T1} - p_{T1}' = \left(v_i'^{T1} - \max_{k \in \mathcal{B}' \setminus b_{T1}} v_i'^k - p_k'\right) - p_{T1}'$.

In the next paragraph we show that $\max_{k \in \mathcal{B}' \setminus b_{T1}} v_i'^k - p_k' \geq v_i^{T2} - p_{T2}$. From here it immediately follows that the residual consumer surplus of c_i in economy $\mathcal{E}'$ is less than $v_i'^{T1} - p_{T1}' - \left(v_i^{T2} - p_{T2}\right)$, which is the difference in total consumer surplus for c_i across the two economies.

To see that $\max_{k \in \mathcal{B}' \setminus b_{T1}} v_i'^k - p_k' \geq v_i^{T2} - p_{T2}$, let $O1 \equiv \text{argmax}_{k \in \mathcal{B}' \setminus b_{T1}} v_i'^k - p_k'$. Then we have $v_i'^{O1} - p_{O1}' \geq v_i'^{T2} - p_{T2}' \geq v_i^{T2} - p_{T2}$, where the first inequality holds by revealed preference, and the second holds by assumption.

Finally, is easily verified that for any $c_i \notin \sum_{T \in \mathcal{T}} \mathcal{C}_T'$, consumer surplus is unaffected. Thus, $\sum_{T \in \mathcal{T}} CS_T' - \sum_{T \in \mathcal{T}} CS_T \leq CS' - CS$ $\square$

C LASSO Controls to Estimate Repayment

The following list presents the baseline variables used in the Lasso procedure to select the optimal set of control variables for our model. These covariates capture key characteristics of treated borrowers, such as their business profile, financial history, and demographic attributes, and are used to estimate predicted repayment rates.

To identify the optimal controls for the repayment model, we implemented a linear Lasso regression using the Stata lasso linear command. The selection procedure was restricted to the subsample of borrowers who received the RCT loan. Standard errors were clustered at the borrower level.

- Age
- Post-Secondary Education
- Female
- Baseline HH Members
- Sector: Manufacturing
- Sector: Retail
- Sector: Services
- Sector: Agriculture
- FE: Zone 1
- FE: Zone 2
- FE: Zone 3
- FE: Zone 4
- Baseline Monthly Profits (USD)
- Baseline Monthly Revenues(USD)
- Baseline Value of Inventory (USD)
- Baseline Value of Assets (USD)
- Baseline Value of Inventory (USD) - Missing
- Baseline Value of Assets (USD) - Missing
- Intention to Purchase With CI: Vehicle
- Intention to Purchase With CI: Remodeling
- Intention to Purchase With CI: Inventory
- Intention to Purchase With CI: Other Fixed Assets
- Expected Probability of a Bad Scenario
- Expected Probability of a Normal Scenario
- Expected Probability of a Good Scenario
- Expected Earning Without CI
- Expected Earning With CI
- Earning Variance Without CI
- Earning Variance With CI
- Expected Probability of a Bad Scenario - Missing
- Expected Probability of a Normal Scenario - Missing
- Expected Probability of a Good Scenario - Missing
- Expected Earning Without CI - Missing
- Expected Earning With CI - Missing
- Earning Variance Without CI - Missing
- Earning Variance With CI - Missing
- Most Frequent Loan Schedule Prior to Randomization - Weekly
- Most Frequent Loan Schedule Prior to Randomization - Bi-monthly
- Most Frequent Loan Schedule Prior to Randomization - Monthly
- Total Number of Loans Prior to Randomization
- Average Loan Size Prior to Randomization

- Loan Size SD Prior to Randomization
- Average Lateness Prior to Randomization
- Lateness SD Prior to Randomization
- Average Delinquency Amount Prior to Randomization
- Delinquency Amount SD Prior to Randomization
- Maximum Days of Lateness Prior to Randomization
- Total Delinquency Amount Prior to Randomization