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

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Abstract

Evaluations of microenterprise credit typically measure effects only on borrowing firms. But what about their customers? If microenterprises sell relatively undifferentiated goods and services, as is often hypothesized, credit may simply reallocate sales across firms and create little consumer benefit. In a randomized controlled trial in Chile, large loans increased treated firms' profits by USD 292 per month, a 13.4% increase. Customer survey data indicate even larger benefits for customers: a gain of USD 494 per month in consumer surplus. Furthermore, using a sample of more than 125,000 non-treated firms operating in the same markets, we find little evidence of business stealing. The welfare gains from credit expansion thus extend well beyond the borrowers themselves.

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. 2025, for a recent overview). Much less is known about the effects on the customers these firms serve or on the nearby firms with which they compete.

On the one hand, much of the academic literature characterizes microenterprises 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, and so on—could facilitate firm growth without yielding meaningful benefits to customers and perhaps at the expense of nearby competitors. A growing firm might attract customers from surrounding businesses, perhaps through marketing or simply by increasing its inventory, without providing access to new or cheaper goods and services.[1]

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, improve service quality, or identify gaps in local markets, introducing goods and services that were previously unavailable or inconvenient to access. More broadly, increases in local income and spending may generate multiplier effects that benefit other firms and households, as related evidence on cash transfers illustrates (e.g. Egger et al. 2022). In this view, microenterprise growth may raise aggregate welfare, rather than merely reallocate sales across firms. This debate has focused on indirect effects on other firms, whereas we assert that examining the impact on the customers of borrowing firms provides critical insight into the nature of competition, product differentiation, and the impact of credit expansions.

We report on a randomized controlled trial (RCT) evaluating a large individual-

[1] See Karnani (2007); Roodman (2012) for policy discussions on this argument.

liability loan offered in collaboration with one of Chile's largest microfinance institutions. The large loan was a secondary loan available to borrowers who already had an outstanding "standard" joint-liability microcredit loan from the lender. The average loan in our experiment provided about USD 2,250 in additional credit beyond the standard loan, which averaged about USD 1,250.[2] The secondary loan generated large, positive effects for the businesses that received it. Businesses assigned to treatment generated an additional USD 599 per month (SE: USD 236) in revenue and USD 292 per month (SE: USD 107) in profits, and borrower household income increased by USD 288 per month (SE: USD 170). These effects represent increases of 13% in business profits and 9% in household income.

The loans also generated large gains for customers. Customer survey data imply that treated firms generated an additional USD 494 per month (SE: USD 224) in consumer surplus. This estimate reflects both an increase in surplus per customer and an increase in the number of customers served. Thus, treated firms appear not only to have grown but also to have done so in ways that created substantial value for the households purchasing from them.

We measure consumer surplus by surveying the businesses' customers. For each customer's most recent purchase from the business, we ask both how much the customer spent and the maximum amount the customer would have been willing to pay for the product or service *at that store*. The difference between these two measures is intended to capture the residual consumer surplus generated by the firm beyond what the customer would have received from the next-best alternative. We estimate a firm's total residual consumer surplus by multiplying average residual consumer surplus among a sample of its customers by the number of customers the business reports serving in a typical week.

Although this measure of consumer surplus is surely noisy, it has several strengths.

[2]Throughout, we apply an exchange rate of 800 Chilean pesos (CLP) per USD, the nominal exchange rate at the onset of the experiment (May 26, 2023). Chile's 2023 purchasing-power-parity (PPP) conversion rate was 433 CLP per USD, implying a nominal-to-PPP exchange-rate ratio of 1.85.

First, customer reports are unlikely to be subject to experimenter-demand effects because customers do not know whether the business is in the treatment or control group or anything about the loan experiment itself. Second, unlike model-based methods of estimating demand, our self-reported measure of consumer surplus does not require firms to sell homogeneous goods. This feature is important in our setting, where firms sell a highly varied set of goods and services.

Because survey-based measurement of consumer surplus is uncommon, we devote a section of the paper to validating the measure. Our measure is highly correlated with customer satisfaction and with customers' stated willingness to recommend the store to a friend. More importantly, it behaves as *residual* surplus should: it declines with the abundance of close substitutes. Customers report less surplus when more same-sector microenterprises operate within 500 meters of the surveyed firm. Moreover, using an independent census of 1,214 chain-supermarket locations that we assemble and validate against official establishment counts, we find that customers of retail firms report less surplus when more supermarkets operate nearby. Finally, consumer surplus is distributed across sectors in ways that accord with economic intuition: sectors providing commodity goods, such as fresh food and groceries, generate little residual consumer surplus on average, whereas more differentiated goods and services, such as beauty and apparel, generate more. Even though groceries are essential, any one grocer is likely to provide little residual surplus because it is a close substitute for another.

How did treated firms create this value? Three sources of evidence—loan applications, firm-reported actions, and customers' stated reasons for shopping—suggest that firms used the credit to improve multiple dimensions of their businesses rather than a single margin. At the time of application, borrowers most commonly planned investments in delivery vehicles, customer-facing improvements such as furnishings and display cases, and cost-reducing assets. In a survey conducted 26 months after randomization, treated firms were 16 percentage points more likely than control firms to report having lowered

prices (22% versus 6%), the only individually statistically significant difference among seven actions. Estimates for service expansion, inventory, and marketing were positive but not statistically significant. Customers of treated firms, in turn, were more likely to cite high-quality service and trust in the proprietor as reasons for shopping there, more likely to say that they preferred the firm to larger stores, and less likely to say that they shopped there merely because it was close to home.

We present a model that formalizes two points central to the empirical analysis. First, under an identifying assumption that the RCT loans do not raise the prices or reduce the quality offered by non-treated firms, the estimated treatment effect on residual consumer surplus at treated firms can be interpreted as a lower bound on the treatment effect on total consumer surplus in the market. Importantly, this assumption does not rule out business stealing. 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, implying zero residual consumer surplus. Business stealing alone therefore does not mechanically generate a positive treatment effect in our customer-survey estimate of consumer surplus.

Second, the framework yields a transparent welfare accounting identity. Under its assumptions, the total welfare effect of the loans is the sum of three components: the loans' impact on treated firms' profits, the effect on consumer surplus, and the spillover effect on the profits of non-treated firms.

Our setting allows us to study these spillovers using unusually rich data. In addition to the firms in the experiment, we observe outcomes and GPS coordinates for more than 125,000 other borrowers from our partner lender who were not part of the experiment but operate in the same local markets. These data allow us to trace the effects of the RCT loans on nearby non-treated firms across distance bands and sector relationships.

The spillover estimates reveal little evidence of meaningful average profit losses among nearby firms. The point estimates for nearby firms' profits are small and statistically in-

distinguishable from zero, including among same-sector firms, where business stealing would be most likely to appear. Because each treated firm is surrounded by many businesses, however, translating these firm-level estimates into aggregate spillovers requires assumptions about the relevant market boundary. Under a benchmark assumption that negative spillovers are confined to same-sector informal firms within 500 meters, even a conservative bound on aggregate losses remains below the combined gains to treated firms and consumers. Allowing spillovers to extend across broader geographic areas or sectors, however, leaves open the possibility of aggregate crowd-out.

Our closest proxy for prices is the gross margin, $\frac{\text{Revenue}-\text{Cost}}{\text{Revenue}}$. In support of the core assumption above, which allows us to map our estimates of residual consumer surplus to total consumer surplus, we find no evidence that nearby firms raised margins in response to treated firms. If anything, several margin estimates are negative, indicating that non-treated firms may have slightly lowered prices as a competitive response. This pattern suggests that our survey-based estimates of consumer surplus are conservative.

Our results imply that the benefits of these loans to customers are similar in magnitude to the benefits captured by the firms themselves, and both are sizable. If these results generalize, impact estimates that measure only enterprise-level gains from enterprise-promoting interventions may miss a substantial share of the total value created. At the same time, the spillover analysis suggests that these gains are not clearly offset by large average losses among nearby non-treated firms. Although we cannot fully rule out economically meaningful aggregate spillovers across the many nearby firms surrounding each treated business, our evidence does not support the view that treated firms grew primarily at the expense of competing firms.

Two further patterns sharpen this interpretation. First, residual consumer surplus is concentrated precisely where close substitutes are scarce, suggesting that microenterprises create value less by offering distinctive products than by making goods and services available nearby and matching local preferences—a natural source of surplus in settings

where travel and search costs make consumer markets highly localized. Second, these firms' customers are substantially poorer than the owners themselves: the 75th percentile of customer household income lies below the 25th percentile of owner household income. Any social welfare function that weights gains to poorer households more heavily would therefore place even greater value on the consumer-side benefits we document.

Our results relate to several streams of research. First, we contribute to a growing body of evidence on the effects of large loans to microenterprises (Bari et al. 2024; Bryan et al. 2024). Second, our study relates to the literature on spillovers from microcredit and other interventions designed to promote firm growth. Several papers document important general-equilibrium effects of microcredit and cash transfers (Kaboski and Townsend 2012; Breza and Kinnan 2021; Egger et al. 2022), including substantial effects of cash and credit on the demand for goods and services and on wages. Closely related work examines effects on untreated competitors. Crépon et al. (2015) and McKenzie and Puerto (2021) find little evidence of business stealing. We contribute to this literature with an unusually large firm-level spillover sample. In addition to our primary sample of firms that applied for the RCT loan, we observe administrative outcomes and GPS coordinates for more than 125,000 nearby non-treated firms. These data allow us to trace spillovers across distance bands and sector relationships. In this setting, we find no evidence of meaningful average business stealing.

Our paper is perhaps closest to Cai and Szeidl (2024), who study access to finance in Chinese retail markets and find that treated firms expand partly at the expense of untreated competitors while consumers benefit through price declines and quality improvements. Two differences are central. First, Cai and Szeidl study substantially larger firms: their baseline firms employ 8.8 workers on average, whereas the businesses in our sample employ about 0.75. This distinction matters for the consumer-welfare question we study. The very small firms in our sample are often thought to sell largely undifferentiated goods and services in crowded local markets, in which case one would expect

firm growth to reallocate sales with little or no residual consumer surplus. Finding sizable consumer-surplus gains is therefore especially informative. Second, Cai and Szeidl recover consumer surplus through a model-based welfare calculation that combines estimated effects on firm borrowing and firm outcomes with a substitution elasticity calibrated using estimates from the retail literature. By contrast, we measure consumer surplus directly by eliciting willingness to pay from customers in short surveys and validate the measure against customer satisfaction, cross-sector patterns of differentiation, and two independent measures of local competition. We discuss the strengths and limitations of this survey-based approach below.

2 Context, Experimental Design, and Data

Our collaborator, one of Chile's largest microfinance institutions, manages a portfolio of around 120,000 active borrowers. Its primary credit product is a joint-liability microloan disbursed to groups of about 20 borrowers. The joint-liability loans range from USD 425 to USD 1,750, average USD 750, and are repaid over about four months. The annual percentage rate (APR) is 49%.

The loan under study—referred to throughout this paper as the "RCT loan"—was new at the time of our experiment. It is an individual-liability loan that may be taken only as a supplement to a joint-liability microloan. RCT loans range from USD 875 to USD 2,500, average USD 2,300, and are repaid over 12 months. Their APR is about 41.5%. The loans are intended to finance fixed assets, expansion of a business's premises, or additional inventory. Borrowers must have successfully completed at least eight joint-liability microloan cycles and made no late payments in the prior year.

Enrollment occurred on a rolling basis from May 2023 to April 2024, with random assignment to treatment occurring roughly every 25–30 days. Data collection also occurred on a rolling basis and continued until December 2025. To enroll in the experiment,

borrowers first had to apply for the RCT loan and receive approval from our partner lender. Approved borrowers were then randomized to either treatment—and thus received the loan—or control. Control-group borrowers were told that they would be eligible to reapply for the loan after two years, but they remained eligible for other, smaller individual-liability loans offered by our partner lender. As we discuss in Section 5, randomization induced an intent-to-treat difference of USD 1,495 per client in total credit received by the treatment group relative to the control group.

Randomization was stratified by loan officer and application batch and was uniform within each stratum.[3] Because the product was new to our partner lender, both demand and the repayment performance of recipients were unknown.[4] The lender therefore constrained the number of RCT loans a given credit officer could handle. Consequently, the probability of treatment assignment varied across strata because credit officers managed different numbers of RCT loans at a given time. These constraints were relaxed as the experiment progressed.[5] In practice, most borrowers had a 50% probability of assignment to treatment. Throughout our regression analyses, we apply inverse-probability weights to account for nonuniform treatment-assignment probabilities.

2.1 Data

We draw on several data sources. To evaluate borrower and business outcomes, we use four surveys conducted, on average, six, nine, 12, and 18 months after randomization. The first and third surveys were conducted by phone, whereas the second and fourth were conducted in person. These surveys cover business performance, household outcomes, and loan use. We also use loan-application data from our partner lender on business outcomes,

[3] An application batch spanned 27 days on average, depending on the rate of loan applications. The minimum was 10 days, and the maximum was 70 days.

[4] In fact, the default rate among RCT loan recipients was substantial, at 14.4%. Moreover, 2.3% of RCT loan recipients defaulted on their standard joint-liability loan, compared with only 0.4% of applicants randomly assigned to the control group.

[5] We describe the precise constraints and how they varied over time in Appendix Section B.

collected each time a borrower renews a loan (recall that borrowers, including those in the treatment group, typically renew their joint-liability microloans every four months). Although collected by our partner lender, these data reflect self-reported business outcomes. They span June 2020 through August 2025. As we show below, our results for business outcomes are stable, albeit less precise, when estimated using only survey data or only loan-application data.

To investigate changes in business practices resulting from the RCT loan, we use a final telephone survey fielded 26 months after randomization. Although similar proportions of treatment- and control-group clients responded, the response rate was only about 20%, so we interpret these data cautiously.

To evaluate consumer surplus, we use two rounds of in-person customer surveys conducted concurrently with the two rounds of in-person business surveys. In each round, a surveyor stationed outside the business approached customers who had just made a purchase.[6] These brief surveys asked 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 residual consumer surplus, one of our key outcomes in the analysis that follows.

For the spillover analysis in Section 6, we use loan-application data for other borrowers of our partner lender who did not apply for the RCT loan. For these "spillover" firms, as well as for the applicant firms, we observe GPS coordinates. We use these data to measure distances between each spillover firm and each applicant firm, which allows us to construct time-varying measures of exposure to nearby treated firms by distance band

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

and sector relationship as the experiment rolled out over time. The spillover analysis uses a monthly panel of spillover firms' sales, costs, profits, and gross margins from January 2023 to December 2025.

Finally, we assemble an external census of large grocery stores to construct a second measure of local competition that is independent of the loan-application data. We identify the 19 major national and regional supermarket chains operating in Chile and use the Google Maps Places API to locate their stores in every city where a sample firm operates, retaining only results that Google classifies as supermarkets or grocery stores. The resulting universe contains 1,214 unique stores.[7]

Table A.1 presents descriptive statistics and balance tests for our sample. The average borrower is 41 years old, and 20% are male. The average business earns about USD 1,800 per month in profits and has about 0.75 employees, while average monthly household income is USD 3,500. Of the businesses in our sample, 25% are in manufacturing, 56% are in retail, and 18% are in services. The only two treatment–control differences that are statistically significant at the 10% level are employment, which is 0.21 employees higher among treatment businesses ($p = 0.061$), and the number of weekly customers, which is 33 higher ($p = 0.060$). An F-test fails to reject joint equality between the two groups ($p = 0.67$). Although not statistically significant, other differences also suggest that treatment-group businesses were slightly larger than control-group businesses: profits were about 6% higher ($p = 0.278$), and household income was 3.8% higher ($p = 0.468$). Accordingly, in alternative specifications, we control for the baseline outcome and a set of firm-size measures: the number of weekly customers, number of employees, monthly profits, and monthly household income. In the appendix, we assess sensitivity to control selection by reporting specifications that use controls selected by PDS LASSO from the

[7]To benchmark coverage, we compare our universe with the Instituto Nacional de Estadísticas (INE) count of supermarkets in April 2026 (CIIU 4711, establishments with at least three cash registers) in the regions where sample firms operate. The INE count totals 1,295 establishments. Our universe is equivalent to 94% of that count, although the two definitions do not coincide exactly.

baseline covariates in Table A.1, as well as specifications that enter all candidate baseline covariates simultaneously.

Table A.2 presents response rates across the survey rounds. Borrower-survey response rates range from 39% to 60%, with no evidence of differential attrition. Ninety-one percent of borrowers enrolled in the study respond to at least one survey, and 95% have at least one observation in our loan-application data. Across the survey and loan-application data, 98% of borrowers are represented at least once. Across the two customer-survey waves, customers from 48% of the businesses are represented in the data, again with no evidence of differential attrition.[8] To address concerns about attrition in any given survey wave, we replicate each key analysis using a single post-randomization outcome equal to the average value across all survey and loan-application waves in which the borrower is represented.

Because we obtained customer data for only 48% of firms, a natural concern is that selection into the customer-data sample differed by treatment status. Table A.3 tests this possibility directly. The outcome is an indicator for whether we collected consumer-surplus data from at least one of a firm's customers in either customer-survey wave. We regress this indicator on treatment, the full set of baseline covariates, and their interactions with treatment, applying inverse-probability weights. Continuous covariates are standardized, and binary covariates are demeaned. Thus, the coefficient on treatment measures the treatment–control difference in the probability that a firm with average baseline characteristics appears in the customer-data sample, while each interaction measures whether the *composition* of firms contributing customer data differs by treatment arm. We find no evidence of differential selection. Treated and control firms were about equally likely to contribute customer data: the difference is 3 percentage points (SE: 4 pp) relative to a control-group mean of 45%. No interaction with a firm-size or demographic covariate

[8]The relatively high rate of missing customer data among businesses reflects the logistical challenges of collecting these data. For many businesses in our sample, surveyors had to travel across the country by bus or plane, making it difficult to coordinate a convenient time for data collection with business owners.

is close to statistical significance, and a joint test of the treatment main effect and all interactions fails to reject the null of no differential selection ($F = 1.31$, $p = 0.194$).

Nevertheless, in Section 5.2.1, we present several robustness tests that address the substantial attrition in this sample. Among other tests, we model selection into the customer-data sample and show that our estimates are robust to reweighting observations to account for this selection. We also compute Lee bounds (Lee 2009) to assess robustness to potential treatment-induced selection. Finally, we conduct a complementary sensitivity analysis of how much smaller the consumer-surplus effect among firms without customer data would need to be, relative to the effect among firms with observed outcomes, to overturn our broad conclusions.

3 A Simple Framework for Residual Consumer Surplus and Welfare

This section provides the conceptual framework for interpreting our consumer-surplus estimates. Our measure comes from a survey of customers exiting the businesses participating in our study. For the product or service just purchased, each customer was asked how much they had spent and the maximum amount they would have been willing to pay at that store.

We interpret a customer's reported maximum willingness to pay as being relative to the customer's next-best alternative. Under this interpretation, the difference between reported willingness to pay and the amount actually paid is residual consumer surplus—the incremental value generated by the sampled firm beyond what the customer would have received from the next-best alternative.

The framework below formalizes this interpretation. It clarifies why a treatment effect on this residual-surplus measure can be interpreted as a treatment effect on total con-

sumer surplus and why business stealing does not by itself invalidate that interpretation. If a treated firm attracts a customer without making that customer better off relative to the next-best alternative, the customer's residual consumer surplus is zero. If the treated firm attracts the customer by offering higher net utility, the measured residual surplus equals the customer's gain. The identifying assumption is therefore not that treated firms do not take customers from other firms; it is that the intervention does not make customers' outside options worse. The main text provides a simplified exposition; Appendix C presents the full model and proofs.

In this section, we assume that customers perfectly understood the survey questions about willingness to pay relative to their next-best alternative. The next section discusses how we elicit and validate residual consumer surplus in practice.

3.1 What the Customer Survey Measures

Consider a customer who purchases from a sampled firm A. Let U_A denote the gross utility the customer receives from the product or service at A, and let p_A denote the price paid. Let B denote the customer's next-best alternative, with gross utility U_B and price p_B.

The maximum amount the customer would be willing to pay at A, relative to the option of buying from B, satisfies

$$U_A - WTP_A = U_B - p_B.$$

Thus

$$WTP_A = p_B + (U_A - U_B).$$

That is, the customer's maximum willingness to pay is the price of the next-best alternative plus the utility difference between the goods from firms A and B. The residual

consumer surplus from the realized transaction is

$$WTP_A - p_A = (U_A - p_A) - (U_B - p_B).$$

This object is the incremental value of buying from the sampled firm rather than from the next-best alternative.

Equivalently, let $u_{ij} \equiv v_{ij} - p_i$ denote the net utility customer j receives from buying from firm i, where v_{ij} represents customer j's gross utility from firm i's good. Normalize the value of purchasing nothing to zero. For any firm i, define the value of customer j's next-best alternative to buying from i as

$$Alt_{ij} \equiv \max \left\{ 0, \max_{k \neq i} u_{kj} \right\}.$$

The residual consumer surplus generated by firm i for customer j is then

$$RCS_{ij} \equiv u_{ij} - Alt_{ij}.$$

Empirically, RCS_{ij} is the customer's reported maximum willingness to pay for the realized transaction minus the amount actually paid. For firm i, total residual consumer surplus is the sum of RCS_{ij} over the customers who purchase from i. In the data, we estimate this firm-level object by multiplying average residual consumer surplus among surveyed customers by the number of customers the firm reports serving.

3.2 Conceptualizing the Experiment

Imagine that firms are partitioned into "applicant firms," which apply for the large loan and for which we have data, and "spillover firms," at which customers can shop but

for which we lack customer data.[9] The experiment randomizes access to the RCT loan among applicant firms. In the conceptual experiment, we compare two ex ante identical economies. In economy $\mathcal{E}$, a given set of applicant firms does not receive the RCT loan. In economy $\mathcal{E}'$, those same applicant firms receive the RCT loan. Spillover firms do not receive a loan in either economy. We use primes to denote all market objects in the economy with the RCT loans. Economy $\mathcal{E}'$ is the treatment economy, and economy $\mathcal{E}$ is the control economy.

Let $\mathcal{B}$ denote the set of all firms in the relevant market, and let $T \subset \mathcal{B}$ denote the applicant firms whose treatment status is changed in the thought experiment. For $i \in T$, the RCT loan may increase the net utility the firm offers to customers, $u_{ij} = v_{ij} - p_i$, by allowing the firm to lower prices or increase quality. That is, for $i \in T$, $u'_{ij} \geq u_{ij}$ for every customer j. For firms $k \notin T$, there is no direct loan treatment, but their outcomes may still differ across the two economies if they respond to treated firms or lose customers to them.

In this conceptual experiment, the treatment effect of the RCT loan on total consumer surplus is the difference in total consumer surplus between economy $\mathcal{E}'$ and $\mathcal{E}$. The rest of this section asks when the treatment effect on residual consumer surplus can be interpreted as a treatment effect on total consumer surplus in the economy, rather than merely as a reallocation of customers from spillover to applicant firms.

3.3 Why Business Stealing Does Not Generate Spurious Consumer Surplus

The key condition for interpreting changes in residual consumer surplus as changes in total consumer surplus is that the intervention does not make customers' outside options

[9] Our theory allows the RCT loan to have spillover effects on applicant firms, but for exposition it is useful to partition firms into those for which we have customer data and those for which we do not. We refer to the latter group as the spillover sample.

worse.

Assumption 1 (Stable Outside Options)**.** *For every firm $k \notin T$ that does not receive the RCT loan and every customer j, the intervention does not change the net utility offered by firm k:*

$$v'_{kj} - p'_k = v_{kj} - p_k.$$

Assumption 1 rules out changes in the price or quality offered by non-treated firms that would change customers' outside options. It does not rule out business stealing. Quantities and profits at non-treated firms may change because customers choose treated firms instead. We discuss relaxations of Assumption 1 below.

To see the role of the assumption, first consider a customer who would shop at treated firm $A \in T$ whether or not A receives the loan (i.e., the customer buys from firm A in both economies, $\mathcal{E}$ and $\mathcal{E}'$). This customer is therefore observed by the econometrician in both economies. Suppose the treatment changes the net utility offered by A from $U_A - p_A$ to $U'_A - p'_A$, while the customer's next-best alternative, B, is unchanged. The change in residual surplus across the two economies is

$$\begin{aligned}&[(U'_A - p'_A) - (U_B - p_B)] - [(U_A - p_A) - (U_B - p_B)] \\ &\quad = (U'_A - p'_A) - (U_A - p_A).\end{aligned}$$

For this non-marginal customer, the outside option cancels. The increase in residual consumer surplus is exactly the increase in the customer's consumer surplus.

Now consider a customer who would have purchased from a spillover firm B absent the intervention in economy $\mathcal{E}$ but instead purchases from treated firm A in $\mathcal{E}'$. In this case, the econometrician does not observe the customer in economy $\mathcal{E}$ because the customer shops at spillover firm B, but does observe the customer in economy $\mathcal{E}'$ because the customer shops at applicant firm A.

However, under Assumption 1, the customer's next-best alternative in economy $\mathcal{E}'$

is firm B, and the assumption further guarantees that the customer's consumer surplus from firm B, $U_B - p_B$, is the same in both economies.[10] Therefore, the customer's gain in consumer surplus in economy $\mathcal{E}'$ relative to economy $\mathcal{E}$ is

$$(U'_A - p'_A) - (U_B - p_B).$$

This is exactly the residual surplus measured at treated firm A in economy $\mathcal{E}'$. If the customer switches but is no better off than under the next-best alternative, then $U'_A - p'_A = U_B - p_B$, and measured residual surplus is zero. The fact that this customer is observed in economy $\mathcal{E}'$ but not in economy $\mathcal{E}$ therefore does not bias our estimate of the treatment effect on total consumer surplus.

The following proposition formalizes this logic.

Proposition 1 (Residual Surplus and Total Consumer Surplus)**.** *Consider first the case of a single treated applicant firm, $T = \{s\}$. Under Assumption 1, the change in residual consumer surplus generated by firm s equals the change in total consumer surplus in the market:*

$$RCS'_s - RCS_s = \Delta CS.$$

More generally, suppose several applicant firms receive the RCT loan. If Assumption 1 is relaxed to allow non-treated firms to weakly improve, rather than requiring them to remain unchanged,

$$v'_{kj} - p'_k \geq v_{kj} - p_k \quad \textit{for every } k \notin T \textit{ and every } j,$$

then the change in residual consumer surplus generated by treated applicant firms is a

[10] More generally, the customer's next-best alternative in economy $\mathcal{E}'$ may be another treated firm that is now preferred to firm B. This would cause the customer's residual consumer surplus in economy $\mathcal{E}'$ to understate the customer's consumer-surplus gain—that is, our estimate of the loan's effect on consumer surplus would be biased downward. We consider this case in the formal treatment of Proposition 1 but, for simplicity, ignore it in the following exposition.

lower bound on the change in total consumer surplus:

$$\sum_{i \in T} (RCS_i' - RCS_i) \leq \Delta CS.$$

The first part of the proposition gives the exact result in the simplest case. For customers who would have purchased from the treated firm even without the loan, a stable outside option implies that any increase in residual surplus is an increase in consumer surplus. For customers who are drawn away from other firms, measured residual surplus is positive only to the extent that the treated firm offers higher net utility than the customer's counterfactual purchase.

The second part of the proposition clarifies how to interpret violations of Assumption 1. If non-treated firms respond by lowering prices or improving quality, then customers' outside options improve. This makes the residual-surplus estimate conservative for two reasons. First, an improved outside option reduces residual surplus measured among customers of treated firms, because treated firms are now being compared to better alternatives. Second, customers who continue shopping at non-treated firms may benefit directly from those lower prices or quality improvements, but these gains are not captured in our survey of customers at treated applicant firms. Thus, if Assumption 1 is violated because non-treated firms become better outside options, our residual-surplus estimates understate the total consumer-surplus effect.

3.4 Competitive Responses and the Role of Assumption 1

The problematic violation of Assumption 1 is the opposite case, in which non-treated firms become worse outside options. For example, if nearby competitors respond to treated firms by raising prices, reducing quality, or exiting, then residual surplus at treated firms could rise partly because the benchmark against which they are compared deteriorated. In that case, the treatment-control difference in residual consumer surplus would no longer isolate

the value created by treated firms.

In Section 6, we examine the loans' spillover effects on nearby non-treated firms. Our closest proxy for prices at these firms is the gross margin, $\frac{\text{Revenue}-\text{Cost}}{\text{Revenue}}$. In support of Assumption 1, we show below that nearby spillover firms do not raise margins in response to the RCT loans. This evidence does not rule out every possible change in the quality of outside options, but it suggests that nearby non-treated firms are not raising prices—the primary price-related threat to Assumption 1.

3.5 Welfare Accounting

In the model formalized in the Appendix, we show that the RCT loan's impact on total welfare takes the following standard form. Let π_i denote the profits of firm i in economy $\mathcal{E}$ and π_i' the corresponding profits in economy $\mathcal{E}'$:

$$\Delta W = \Delta CS + \sum_{i \in T} (\pi_i' - \pi_i) + \sum_{i \notin T} (\pi_i' - \pi_i).$$

The welfare effect of the loans is therefore the sum of three components: the change in consumer surplus, the change in profits among firms that receive the RCT loan, and the spillover effect on profits among firms that do not receive the RCT loan. The empirical analysis estimates the first component using the customer survey, the second using data from applicant firms, and the third using the analysis of nearby spillover firms. Under Proposition 1, the treatment effect on residual consumer surplus provides a lower bound on the consumer-surplus component when non-treated firms respond by lowering prices or improving quality.

4 Measuring and Validating Residual Consumer Surplus

The framework above clarifies the welfare object our customer survey is designed to approximate: residual consumer surplus from a realized transaction at the chosen firm, measured relative to the customer's next-best alternative. Before turning to the experimental estimates, we discuss how this object is measured in practice. We first describe the strengths and limitations of directly eliciting willingness to pay from customers and then present validation exercises that assess whether the resulting measure contains a meaningful signal.

4.1 Comments on Self-Reported Willingness to Pay

Unlike model-driven approaches to estimating demand curves, we elicit willingness to pay directly from customers. This approach has been applied extensively in the development and applied-microeconomics 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 sample firms to supply homogeneous goods, an exogenous supply shifter to facilitate demand-curve estimation, or assumptions about market structure and competition.

However, self-reported willingness to pay has several important limitations. Customers may not understand the question, may be unable to articulate their true willingness to pay for a good or service, or may give context-dependent responses—for example, responses that reflect what they consider a fair market price rather than their willingness to pay (e.g. Thaler 1985; Dizon-Ross and Jayachandran 2022). Customers may also report strategically if they anticipate that their responses will inform future pricing decisions. Our surveyors made clear that individual responses would remain strictly confidential;

however, customers were told that aggregate statistics might be shared with the business owner to help improve the business.

In addition, our measure is elicited ex post: customers are surveyed as they leave the store after completing a purchase. It therefore captures residual surplus from the realized transaction at the chosen firm rather than the ex ante value entering the customer's decision about where to shop. The latter would also include travel and search costs, expected wait times, and uncertainty about whether the desired product or service would be available.

Finally, our measure of consumer surplus is unlikely to be influenced by experimenter-demand effects or social-desirability bias. Customers do not know that the stores at which they shop are participating in an experiment and do not observe treatment assignment, although they may, of course, observe the ways in which the owner has improved the business.

4.2 Validation of the Consumer-Surplus Measure

Given that self-reported willingness to pay is measured with error, we now present several checks that assess whether the measure nevertheless contains a meaningful signal about residual consumer surplus. These checks do not eliminate concerns about reporting error, but they show that the measure covaries with other outcomes in ways that are difficult to reconcile with pure noise or generic survey response patterns.

First, Figure 1 compares our consumer-surplus measure with two simpler, more familiar measures of customer utility: satisfaction with the store's products or services and willingness to recommend the store to friends or family. Customers who report higher residual consumer surplus also report greater satisfaction and a greater likelihood of recommending the store. If reported willingness to pay were largely random or reflected only confusion about the elicitation task, there would be little reason for it to increase

systematically with these independent measures of customer satisfaction.

Second, Figures 2a and 2b test a more specific implication of interpreting the measure as *residual* consumer surplus. Because the object of interest is the value generated by the chosen firm relative to the customer's next-best alternative, measured surplus should be lower when close substitutes are more abundant. Figure 2a shows this pattern using the density of nearby microenterprises: each additional spillover MFI borrower (i.e., a borrower that did not apply for the RCT loan) within 500 meters of the surveyed firm and in the same sector is associated with a USD 0.06 reduction in consumer surplus per transaction (SE: 0.03, $p = 0.02$). Figure 2b repeats the exercise using an independent measure of local competition: the number of large chain supermarkets within 500 meters of the firm. In the retail sample, each additional nearby supermarket is associated with a USD 0.37 reduction in consumer surplus per transaction (SE: 0.17, $p = 0.035$). These relationships are precisely what the residual-surplus interpretation predicts. They would be harder to explain if the measure primarily captured generalized shopper satisfaction, social-desirability bias, or a tendency to report high willingness to pay irrespective of the relevant outside option.

These local-competition patterns also speak to the concern that microenterprises should not generate much residual consumer surplus because they often operate in crowded, weakly differentiated markets. Our evidence does not imply that all microenterprises are highly differentiated, nor that small firms generally dominate larger stores on price, quality, or variety. Rather, it suggests that residual consumer surplus is greatest in precisely the environments where a small firm is most likely to matter: local markets with relatively few close substitutes. This is consistent with evidence from other developing-country retail settings that high travel and search costs can make consumer markets highly localized (e.g. Ramos-Menchelli and Sverdlin-Lisker 2024). In such settings, a microenterprise need not offer a nationally distinctive product to generate surplus. It may create value by making a product or service available nearby, or by offering a better match to local customer

preferences. Conversely, when customers have many same-sector microenterprises or large supermarkets nearby, the incremental value of any one sampled firm over the next-best alternative is lower.

Finally, Table A.4 examines correlates of customer surplus by sector, providing a cross-sector validation of the same logic. Column 1 reports the correlation between sector and the transaction size (price paid) for a customer's most recent purchase; column 2 reports the correlation between sector and the customer's willingness to pay for that purchase; and column 3 reports the correlation between sector and residual consumer surplus—the difference between willingness to pay and transaction size. Column 3 shows that per-customer residual consumer surplus is higher in sectors where goods and services are more likely to be differentiated, including apparel and footwear, beauty and personal-care services, tailoring, and other services. It is lower in sectors where offerings are likely to be more standardized or more easily substituted across nearby firms, including fresh- and specialty-food retail, grocery and convenience retail, and restaurant and prepared-food services. This ranking is not mechanically driven by transaction size or the number of customers served. Instead, it is consistent with the interpretation that our measure captures firm-specific residual value. Essential goods such as groceries may generate substantial total utility, but any individual grocery store may generate little residual surplus if customers have close substitutes nearby. More differentiated goods and services, by contrast, are more likely to generate surplus through the specific firm–customer match.

Taken together, these exercises suggest that the willingness-to-pay data contain meaningful information about residual consumer surplus. The measure is correlated with intuitive measures of customer utility, declines with the availability of close local substitutes, and is systematically higher in sectors where firm-specific differentiation is more plausible. We therefore interpret the survey data as a noisy but informative measure of the residual surplus customers receive from shopping at the sampled firms.

5 Impacts on Treated Firms and Consumer Surplus

Empirical approach. Our main empirical analyses use the following specification:

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

where y_{it} represents an outcome for borrower i at time t; T_i is an indicator for whether borrower i was assigned to treatment; and X_i includes the baseline value of the outcome and baseline firm-size controls: the number of weekly customers, number of employees, monthly profits, and monthly household income. The term γ_t denotes month–year fixed effects, and standard errors are clustered at the borrower level. Throughout, we apply inverse-probability weights by stratum to account for applicants' strata-specific probabilities of assignment to treatment or control.

For robustness, we present analyses using two alternative specifications. First, to address concerns about attrition within any given survey, we pool outcome data across all post-randomization observations:

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

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

$$y_{it} = \alpha + \beta_1 T_i + \beta_2 Post_{it} + \beta_3 \left(Post_{it} \times T_i\right) + X_i\delta + \gamma_t + \varepsilon_{it}, \tag{3}$$

where $Post_{it}$ is an indicator equal to one for observations after borrower i's randomization date. Because we lack baseline data for the consumer-surplus analyses, we cannot replicate them using this approach.

5.1 Firm- and Household-Level Outcomes

We begin by analyzing the experiment's first stage: how much additional credit did treatment-group firms receive? Table 1 presents the results. Panel A reports estimates without additional controls, and Panel B includes firm-size controls.

Treatment-group borrowers received an additional USD 2,232 through the RCT loan (SE: 33; Panel B, column 3), which translated into an additional USD 1,711 in credit from our partner lender (SE: 66; Panel B, column 4). The difference between these figures arises because control-group borrowers remained eligible to apply for additional, smaller loans after assignment. Although not statistically significant, the point estimate suggests that control-group borrowers also received an additional USD 212 in credit from other lenders (SE: 186; Panel B, column 6). In total, treatment-group borrowers received an additional USD 1,495 in credit from all lenders (SE: 200; Panel B, column 1), relative to a control-group mean of USD 2,333.

Table 2 presents our key firm- and household-level outcomes. Once again, Panel A reports estimates without additional controls, and Panel B includes the baseline outcome and firm-size controls. Our preferred specification, which includes firm-size controls, indicates that treatment firms grew substantially. The RCT loan increased firm revenue by USD 599 per month (SE: 236; Panel B, column 1), profits by USD 292 per month (SE: 107; Panel B, column 2), and household income by USD 288 per month (SE: 170; Panel B, column 3). The estimated effect on the total value of business capital—fixed assets plus inventory—is also large, at USD 3,429 (SE: 1,954; Panel B, column 4), although the large standard error suggests that the point estimate may overstate the true effect. We find no effect on employment (Panel B, column 5). Given that the intervention increased total borrowing by USD 1,495 in the first stage, the household-income estimate implies a private monthly return on borrowed capital of 19.2%.

Figure 3 plots treatment and control means over time for firm profits and revenue.[11] The figure suggests that the treatment effect grew somewhat over time.

What types of investments did firms make to generate these high returns? The loan applications provide an initial indication: at baseline, borrowers had to specify how they would spend the loan if they received it. We manually classified each applicant's stated plan into up to three of seven exhaustive categories. A firm is counted in every category that its plan covered, so the shares in Figure 4 need not sum to one. Vehicle-related investments (27.4%) were the most common category, reflecting the importance of delivery and inventory logistics. Customer-service improvements such as furnishings, decor, and display cases were the next most common (20.8%), while roughly one in five firms planned cost-reduction investments (20.4%), such as bulk purchasing, storage expansion, or production-process assets. Generic productive assets (16.0%), plans to expand into new products or services (14.6%), and new or expanded premises (8.7%) round out the classification; inventory restocking (4.0%) was least common.

Figure A.1 offers a complementary perspective. Approximately 26 months after randomization, we asked firm owners whether they had taken each of seven concrete business actions since baseline. The figure plots the inverse-probability-weighted share of firms reporting each action separately for the treatment (navy) and control (gray) groups, with 95% confidence intervals. Stars on the treatment bars denote the statistical significance of treatment–control differences from inverse-probability-weighted regressions with standard errors clustered at the firm level. Point estimates are positive for six of the seven actions, but only one difference is statistically significant: 22% of treated firms report having lowered prices in the months following randomization, compared with 6% of control firms, a 16-percentage-point difference ($p < 0.05$). Point estimates for service expansion (+16 pp), inventory increases (+9 pp), and marketing (+9 pp) also suggest more activity

[11] Our loan-application data do not cover the other primary outcomes discussed in this section; thus, we lack sufficiently high-frequency data to construct comparable plots for those outcomes.

among treated firms, but their confidence intervals contain zero. Rates of variety expansion, quality improvement, and operational improvement are essentially identical across the two groups. As noted above, the response rate for this survey was only about 20%. Although response rates and respondent characteristics are balanced across treatment arms, we nevertheless interpret these statistics cautiously.

5.1.1 Robustness of Firm and Household Outcomes

Table A.5 presents difference-in-differences estimates of Specification 3 for the same outcomes as Table 2. Panel A includes firm fixed effects, and Panel B adds the baseline outcome and firm-size controls. Across the board, estimates in both panels are similar to those from our preferred specification. Table A.6 re-estimates the revenue and profit effects using our main Specification 1, restricting the sample to loan-application data (columns 1 and 2) or survey data (columns 3 and 4). The estimates are quantitatively similar to our main results and remain statistically significant at least at the 10% level throughout.

To address attrition in any particular follow-up wave, Table A.7 presents estimates of Specification 2, in which each outcome is averaged across all post-randomization waves in which the borrower is observed. Recall that 98% of borrowers appear in at least one survey or loan-application wave. These estimates are again similar in both magnitude and precision to those in Table 2.

Finally, Figure A.2 examines whether the experiment's main results are sensitive to control selection. It compares specifications with no controls, our firm-size baseline controls, controls selected by PDS LASSO from the baseline covariates in Table A.1, and all candidate baseline covariates entered simultaneously. Panel (a) reports the first-stage outcomes and Panel (b) the business and household outcomes. The estimates are broadly stable across these approaches: the PDS-LASSO estimates closely track our preferred specifications, and the conclusions do not depend materially on the choice of controls.

Appendix Tables A.8 and A.9 report the corresponding PDS-LASSO specifications.

5.2 Consumer Surplus

We now turn to the large loans' downstream effects on firms' customers. Table 3 presents our primary estimates of consumer surplus and its components. Columns 1–3 analyze customer-level outcomes, while columns 4 and 5 present firm-level aggregates. Panel A reports estimates without controls, and Panel B includes the baseline outcome, where relevant, and firm-size controls.

Customers of treated firms report spending an additional USD 5.33 on their most recent transaction (SE: 1.86; Panel B, column 1), relative to a control-group mean of USD 13.68. Similarly, they report an additional USD 6.56 in willingness to pay (SE: 2.38; Panel B, column 2), resulting in a USD 1.21 increase in consumer surplus from their most recent transaction (SE: 0.36; Panel B, column 3), relative to a control-group mean of USD 2.31. In addition to providing more value per customer, treated firms serve 26.7 more customers per week (SE: 10.3; Panel B, column 4), relative to a control-group mean of 94.8. We calculate total weekly consumer surplus by multiplying average consumer surplus across all surveyed customers in both survey waves by the business's reported number of customers per week. We find that treated firms generate an additional USD 123.5 in total consumer surplus per week (SE: 56.1; Panel B, column 5). This is our primary result on the large loan product's downstream effect. Given the first-stage effect of USD 1,495 on total borrowing, this estimate implies a monthly consumer-surplus return on capital of 33.0%—larger than the private return estimated in the previous section.

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 their responses. Customers at treated firms are 8 percentage points more likely to report shopping there because of high-quality customer service (SE: 5 pp; Panel B,

column 3) and 8 percentage points more likely to say that they trust the proprietor. They are also 5 percentage points more likely to say that they prefer the firm to larger stores (SE: 2 pp; Panel B, column 6) and 7 percentage points more likely to say that they prefer to support local firms (SE: 4 pp; Panel B, column 7). In addition, they are 6 percentage points more likely to report liking the firm's prices (SE: 5 pp; Panel B, column 1) and 6 percentage points more likely to report liking the quality of its goods and services (SE: 4 pp; Panel B, column 2), although these differences are not statistically significant. Conversely, customers of treated firms are 10 percentage points less likely to report shopping there merely because it is conveniently located (SE: 5 pp; Panel B, column 5). Because these are post-treatment responses from customers who chose to shop at each firm, the treatment–control differences in Table 4 may reflect changes in customer composition as well as changes in firm practices.

These customer reports align with a broader pattern in the firm-side evidence. The investment plans summarized in Figure 4 span several dimensions relevant to consumers, including logistics and delivery capacity, customer-facing improvements, cost reductions, productive assets, new products or services, and premises expansion. The realized actions in Figure A.1 point in a similar direction: treated firms are more likely than control firms to report lowering prices, expanding services, increasing inventory, and engaging in marketing, although only the price-reduction difference is statistically significant. Taken together, the evidence does not isolate a single dominant channel. Instead, it suggests that treated firms used the additional credit to make a bundle of changes—some potentially lowering costs or prices and others improving availability, service, or the overall customer experience—that could plausibly raise residual consumer surplus. This interpretation is consistent with Table 4, in which customers of treated firms cite several dimensions of value.

Finally, Figure A.3 compares the household-income distributions of business owners and customers. Customers of sample firms are substantially poorer than the firm owners

in our sample. Customers have mean monthly household income of USD 1,488 and median monthly household income of USD 1,250, whereas sample borrowers have mean monthly household income of USD 3,472 and median monthly household income of USD 3,125. The 75th percentile of the customer household-income distribution, USD 1,875, lies below the 25th percentile of the sample borrowers' household-income distribution, USD 2,219.[12] From a social-welfare perspective, customers' lower incomes relative to owners place even greater weight on the consumer-surplus effects.

These income differences strengthen the policy implications of our consumer-surplus results. The loan benefits captured by customers are larger than the private returns captured by borrowers and may also be more socially valuable because customers have comparatively lower incomes.

5.2.1 Robustness and Accounting for Attrition

Table A.11 examines the robustness of the treatment effect on consumer surplus to alternative specifications and assumptions. In all columns, the outcome is total weekly consumer surplus. Column 1 replicates our preferred specification with firm-size controls, corresponding to Panel B, column 5, of Table 3. Column 2 drops the approximately 17% of observations collected remotely by QR code rather than through in-person surveys. Columns 3 and 4 use only observations collected during the first and second in-person customer-survey waves, respectively. Column 5 accounts for selection into the customer-data sample by reweighting firms according to their estimated probability of appearing in the sample. Column 6 constructs total weekly consumer surplus using per-transaction consumer surplus winsorized at the 1st and 99th percentiles, rather than trimmed at the 5th and 95th percentiles as in the preferred specification. Across specifications, es-

[12]Recall, however, that because they are eligible for the large RCT loan, firm owners in our sample are substantially richer than the typical borrower from our microfinance institution. Indeed, Figure A.3 indicates that the household-income distribution of the typical borrower from our partner microfinance institution is similar to that of firm customers in our sample.

timates range from USD 67 to USD 126 per week, and all except those in columns 4 and 6 are statistically significant at the 10% level or better. Further, Panel (c) of Figure A.2 demonstrates robustness of the consumer surplus results to alternative selection of control variables: controls selected by PDS LASSO from the baseline covariates in Table A.1, and all candidate baseline covariates entered simultaneously. Appendix Table A.10 reports the corresponding PDS-LASSO specification.

Tables A.2 and A.3 show no evidence that treatment affected either customer-data coverage or the baseline composition of firms contributing customer data. Nevertheless, we compute Lee bounds (Lee 2009) to adjust our estimated treatment effect on consumer surplus for attrition. The standard Lee procedure requires trimming either the top or the bottom 6.7% of the observed treatment distribution to account for the additional (but not statistically significant) coverage of treatment firms. Table A.12 reports an entirely positive estimated Lee region for total weekly consumer surplus, ranging from USD 103 to USD 253. The estimated Lee region for average consumer surplus per transaction is also entirely positive, ranging from USD 0.22 to USD 1.38. The corresponding 90% Imbens–Manski confidence regions are $[-12.2, 343.4]$ for total weekly consumer surplus and $[-0.7, 2.2]$ for average consumer surplus per transaction (Imbens and Manski 2004).

The Lee bounds address whether treatment-induced selection compromises the treatment–control comparison among firms with observed outcomes, but they do not identify the treatment effect among firms whose outcomes would be missing under either assignment. Under the benchmark assumption of no differential attrition, Appendix E therefore asks how much smaller the treatment effect among firms without consumer-surplus data would have to be to overturn the conclusion that the loans generate positive consumer-surplus effects. Starting from the preferred estimate of USD 123.5 per week, Table A.13 shows that the full-sample estimate remains statistically significant at the 5% level if the effect among firms without these data is at least USD 98.6 per week—a gap of USD 24.9, or approximately 20%. The estimate remains statistically significant at the 10% level if the

effect among firms without these data is at least USD 66.2—a gap of USD 57.3, or approximately 47%. The full-sample point estimate reaches zero only if the effect among firms without these data is USD 226.5 below the observed-firm effect, implying a reversal from a gain of USD 123.5 per week to a loss of USD 103.0 per week.

6 Spillover Effects on Nearby Firms

We next estimate the RCT loans' effects on nearby firms outside our experimental sample, focusing on revenue, costs, profits, and gross margins ($\frac{\text{Revenue}-\text{Cost}}{\text{Revenue}}$). Quantifying these spillovers is important for two reasons. First, spillovers to non-treated firms' profits are the third component of the RCT loans' overall welfare effect in our theoretical framework, alongside the effects on treated firms' profits and consumer surplus. Second, gross margins proxy for prices because, holding other factors fixed, lower prices mechanically reduce gross margins. Evidence on nearby firms' margins is therefore informative about Assumption 1. If nearby firms responded to treated firms by raising margins, our consumer-surplus interpretation would be weakened. Our evidence instead suggests that margins at nearby firms are unchanged or perhaps slightly lower, implying that our consumer-surplus estimates are conservative.

We study these spillovers using loan-application data for borrowers from our partner lender who were not part of the experiment but operate in the same local markets as applicant firms (those that applied for the RCT loan and were randomized to receive it or not). We refer to these nonexperimental borrowers as "spillover firms." For each spillover firm, we observe monthly sales, costs, profits, gross margins, and GPS coordinates. The data are updated each time a borrower renews their loan, yielding an average of 4.8 observations per borrower from January 2023 through December 2025. The sample covers 125,835 unique spillover firms.

Using the distances between each spillover firm and each applicant firm, we classify ex-

posure into three concentric distance bands—0–500 meters, 500 meters–1 kilometer, and 1–2 kilometers—and two mutually exclusive sector categories: same-sector and different-sector, where "sector" refers to a 14-category business classification.[13] The same-sector category is the natural place to look for business stealing, while the different-sector category captures broader local-equilibrium effects.

Table A.14 shows that applicant firms are surrounded by many spillover firms. Within 500 meters, the average applicant firm has 5.9 same-sector spillover neighbors and 58.0 different-sector neighbors.

Empirical approach. For each spillover firm i and month t, we estimate

$$y_{it} = \alpha_i + \sum_{b=1}^{3} \sum_{k \in \{S,D\}} \beta_{bk} \, \text{TreatedCount}_{it}^{bk} + \sum_{b=1}^{3} \sum_{k \in \{S,D\}} \gamma_{bk} \, \text{AllRandomizedCount}_{it}^{bk} + \delta_{c(i)t} + \varepsilon_{it}, \tag{4}$$

where y_{it} is monthly revenue, costs, profits, or gross margin for spillover firm i; α_i denotes firm fixed effects; and $\delta_{c(i)t}$ denotes comuna $\times$ month–year fixed effects, where $c(i)$ indexes firm i's comuna.[14] The index $k \in \{S, D\}$ distinguishes same-sector (S) from different-sector (D) neighbors. The variable $\text{TreatedCount}_{it}^{bk}$ is the inverse-probability-weighted count of treated applicant firms in distance band b and sector channel k that had been randomized by date t, while $\text{AllRandomizedCount}_{it}^{bk}$ is the analogous count of all applicant firms randomized by date t. Because treatment rolled out over 13 randomization rounds, exposure varies over time as nearby applicant firms were sequentially assigned to treatment or control. The density controls ensure that identification comes from random

[13] The 14 sector categories are subgroups of four broad industries, as defined by our partner MFI: retail (apparel and footwear, bakery and pastry, beauty and personal-care retail, gifts and bazaar, grocery and convenience retail, fresh- and specialty-food retail, handicrafts, and other retail); services (beauty and personal-care services, restaurants and prepared-food services, tailoring, and other services); manufacturing; and agriculture.

[14] A comuna is Chile's basic local administrative unit, roughly equivalent to a municipality.

variation in the share of nearby applicant firms assigned to treatment, conditional on the local stock of randomized applicant firms. Standard errors are clustered at the spillover-firm level.

This specification allows spillovers to vary flexibly by distance and sector relationship. The same-sector coefficients are of primary interest because these firms are the closest competitors and therefore the most likely to experience business stealing. The different-sector coefficients allow the RCT loans to affect nearby firms through other channels, such as local-demand spillovers or broader equilibrium responses.

6.1 Average Firm-Level Spillovers

Table 5 reports the main estimates. To examine whether the coarse distance bands mask a sharper effect at close range, Figures A.4a and A.4b re-estimate Equation (4) using ten 100-meter-wide distance bands from 0 to 1 kilometer. They plot the per-treated-neighbor coefficients and 95% confidence intervals separately for same-sector (navy) and different-sector (maroon) neighbors.

Overall, we find little evidence that the RCT loans generated meaningful average business-stealing effects on nearby firms' profits. Two of the three same-sector profit coefficients are negative, but all three point estimates are small, ranging in absolute value from 0.03% to 0.55% of average nearby-firm profits. None is individually statistically significant, and they are not jointly significant ($p = 0.70$). The effects on different-sector firms are similarly small and neither individually nor jointly significant ($p = 0.46$). Appendix Figure A.4a reinforces this pattern at finer spatial resolution: the per-treated-neighbor profit coefficient fluctuates around zero across the ten 100-meter bands, with no systematic decay by distance. In the closest 0–100-meter band—where business stealing would be strongest if present—the same-sector point estimate is positive and statistically indistinguishable from zero.

The margin results are similarly reassuring. We find no evidence that nearby firms raised margins in response to treated firms; if anything, point estimates are uniformly negative across sector channels and distance bands. Same-sector margin coefficients imply declines of roughly 0.17% to 0.47% of the outcome mean, while different-sector estimates are smaller in magnitude, at 0.02% to 0.12% of the mean. None of the individual coefficients is statistically significant at conventional levels, and neither the same-sector nor the different-sector joint test rejects the null of no effect ($p = 0.30$ and $p = 0.70$, respectively). Importantly, the upper bound of the 95% confidence interval for the same-sector margin coefficient is below 0.25 percentage points in every distance band, ruling out meaningful average margin increases per additional treated neighbor. Appendix Figure A.4b confirms that the margin result is not driven by the choice of bands: at 100-meter resolution, coefficients hover near zero across most bands, with only isolated significant estimates at the far edge of the 1-kilometer window that are inconsistent with a business-stealing account.

To the extent that margins proxy for prices, these patterns are consistent with nearby firms modestly lowering rather than raising prices. In the framework of Section 3, this evidence supports Assumption 1. Indeed, if nearby firms respond by lowering prices while maintaining quality, Proposition 1 implies that our survey-based estimates of consumer surplus are conservative.

6.2 Bounding Aggregate Spillovers

These estimates are reassuring about average spillovers on any one nearby firm, but the welfare accounting in Section 3 requires aggregating losses across all potentially affected competitors. Although we observe more than 125,000 nearby firms, our results cannot, by themselves, bound aggregate profit spillovers on non-treated firms if spillovers could affect any firm in the economy. This market-boundary problem is common in related work. Some studies designate a geographically confined market as the relevant spillover boundary (e.g.

McKenzie and Puerto 2021; Cai and Szeidl 2024). These studies provide strong evidence about spillovers within those boundaries, but they do not establish that all affected firms lie within them. For example, if households finance additional purchases at treated firms by reducing expenditures in other sectors or locations, firms outside the study market may experience some profit effects. Thus, mapping estimated firm-level effects into an aggregate producer-side spillover requires assumptions about the economically relevant spillover population.

To bound aggregate spillover losses, we begin by assuming that negative spillovers occur only among same-sector firms within 500 meters of a treated firm. The point estimate in Table 5 for this group is −USD 0.42 per nearby firm per month, and Table A.14 shows that the average treated firm has 5.9 such neighbors in our data. Taking the point estimate literally therefore implies aggregate lost profits of only USD 2.5 per month among these nearby competitors (5.9×0.42). Even the lower endpoint of the 95% confidence interval, −USD 23.0 per firm per month, implies aggregate harm of only USD 135 per treated firm per month (5.89×23.0). This amount is well below the treated-firm profit gain of about USD 292 per month in Table 2.

Of course, not all nearby same-sector firms borrow from our partner lender. As described in Appendix D, we estimate that the average borrower in the lender's administrative data operates in a comuna where the lender accounts for 26.9% of informal microenterprises in commerce, services, and manufacturing.[15] Under the additional assumptions that spillovers are confined to same-sector informal firms and are similar for firms that borrow from our partner lender and those that do not, the point estimate implies an aggregate spillover loss of USD 9 per treated firm ($2.5/0.269 = 9$). If we instead use the upper-bound harm of USD 135 among observed borrowers—corresponding to the

[15]Briefly, we use *Encuesta de Microemprendimiento* microdata to estimate the number of informal microentrepreneurs in commerce, services, and manufacturing in each region. We allocate those regional totals to comunas using each comuna's share of the adult population in the 2024 Census and divide the lender's borrower count in each comuna by this estimated denominator. The 26.9% figure is the estimated exposure of spillover firms scaled by these comuna-level penetration rates.

lower endpoint of the 95% confidence interval for the effect on nearby firms' profits—the loss scales to USD 502 among all affected firms ($135/0.269 = 502$). This amount is large enough that we cannot rule out a full offset of the treated-firm profit gain alone.

However, even this upper bound on harm does not offset the combined treated-firm profit gain of USD 292 and the additional consumer-surplus gain of USD 494 per month ($123.5 \times 4 = 494$) from Table 3. Thus, under the assumption that negative spillovers are confined to same-sector informal firms within 500 meters, even the lower bound of our 95% confidence interval for spillovers implies losses smaller than the point estimate of the combined producer- and consumer-side gains.

This conclusion weakens if negative spillovers are allowed to extend to more firms. For example, applying the lower endpoints of the confidence intervals to all same-sector firms within 1 kilometer implies potential harm of USD 372 per month ($5.89 \times 23.0 + 11.10 \times 21.3 = 372$), enough to offset the estimated treated-firm profit gain even before scaling for the limited coverage of our data. Similarly, allowing spillovers to affect both same- and different-sector firms within 500 meters implies potential harm of USD 296 per month ($5.89 \times 23.0 + 57.99 \times 2.76 = 296$), again enough to offset the producer-profit gain before scaling. Thus, the data are informative against large losses per nearby firm, but aggregate crowd-out can be bounded only under assumptions about the relevant geographic radius and the sectors in which firms are affected.

7 Discussion

This paper asks whether the growth of a microenterprise creates value for its customers. In our experiment, access to a large secondary loan increased recipient firms' profits by approximately USD 292 per month and the residual consumer surplus they generated by approximately USD 494 per month. The consumer-side effect reflects both greater surplus per customer and an increase in the number of customers served. We find little

evidence of meaningful average profit losses among nearby non-treated firms, including those operating in the same sector, although the precision and coverage of our spillover analysis do not allow us to rule out aggregate crowd-out across all potentially affected competitors. Taken together, the results suggest that the loans did more than reallocate sales toward financed firms: they enabled those firms to create substantial value for their customers, with consumers receiving the larger share of the gains we measure.

These findings qualify a common characterization of microenterprises as offering largely undifferentiated goods and services in crowded markets. They do not necessarily imply that growing firms introduce radically new products. Rather, differentiation can be local and relational. When travel and search are costly, a small firm can generate meaningful surplus by making the right product available nearby, lowering its price, or better matching local preferences. Consistent with this interpretation, residual consumer surplus is lower where customers have more close substitutes, whether measured by the density of same-sector microenterprises or the presence of nearby supermarkets. Firms' intended investments, reported actions, and customers' stated preferences likewise point to a bundle of incremental improvements in prices, logistics, availability, service, and the customer experience rather than to a single transformative innovation.

Our findings also bear on a long-running debate over whether the gains attributed to microcredit are overstated because of business stealing. That debate has focused primarily on effects on other firms, although direct evidence remains limited. Because the first generation of randomized microcredit evaluations assigned treatment at the village or neighborhood level, their estimates generally net out spillovers within the unit of randomization; most did not estimate those spillovers separately, as several explicitly acknowledge (Angelucci et al. 2015; Banerjee et al. 2015). Crépon et al. (2015) is a notable exception, using variation in households' propensity to borrow to separate direct effects from externalities and concluding that the results are driven by borrowers. Our spillover analysis adds evidence on this question but does not settle it. Displacement may vary with firm

size and market structure in ways that remain poorly understood, potentially making average spillover tests underpowered or masking concentrated losses.

A central contribution of the paper is therefore to broaden the welfare analysis from recipient and competing firms to consumers. Displacement can coexist with welfare gains if customers benefit through lower prices, greater convenience, more variety, or better service and match quality. In our setting, measured consumer gains exceed treated-firm profit gains, so any unobserved business stealing large enough to overturn the combined gains to treated firms and their customers would have to be substantial. Our data do not rule out such aggregate losses under sufficiently broad market definitions, but we find little evidence of them on average. Despite using different methods—indirect versus direct estimation of consumer welfare—our conclusion echoes Cai and Szeidl (2024). They find that access to finance benefits consumers while imposing negative indirect effects on untreated firms; we also find consumer benefits but little evidence of average profit losses among nearby firms. In both settings, ignoring consumers would materially understate the welfare consequences of improving access to credit for consumer-facing enterprises in low-income countries.

Several limitations discipline the interpretation of these results. Our experimental sample consists of established microfinance borrowers who had successfully completed multiple loan cycles, were approved by the lender, and applied for an unusually large supplemental loan. The estimates therefore identify the effects of relaxing capital constraints for a selected group of experienced entrepreneurs, rather than the effects of expanding credit to the average microenterprise. In addition, our consumer-surplus measure relies on stated willingness to pay for realized transactions and does not capture every component of ex ante consumer utility. Our spillover data cover a large but incomplete set of nearby businesses, and the analysis is not a complete social cost-benefit calculation: it does not quantify, among other components, labor-market impacts or environmental externalities.

Subject to these qualifications, the broader implication is that policies directed at firms

may also function as consumer policies. The welfare consequences of microenterprise growth cannot be understood from recipient-firm outcomes alone; they require tracing value creation through the market—to business owners, competing firms, and consumers. Doing so materially changes the welfare accounting in this setting: the beneficiaries of firm growth include not only the entrepreneurs who receive the loans but also the customers who purchase from them.

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Main Tables and Figures

Table 1: First Stage: Loan Take-Up

	All Lenders	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	1513.30***	0.97***	2238.13***	1742.87***	-0.02	-219.61
	(218.26)	(0.01)	(33.26)	(70.39)	(0.03)	(200.85)
Panel B: With Controls						
Treatment	1495.23***	0.98***	2231.90***	1711.36***	-0.03	-212.07
	(200.26)	(0.01)	(33.12)	(66.13)	(0.03)	(185.83)
Control Group Mean	2333.46	0.00	0.00	1767.96	0.13	547.73
	[2455.42]	[0.00]	[0.00]	[766.45]	[0.34]	[2351.62]
No. Obs	439	541	541	541	439	439
No. Firms	439	541	541	541	439	439

Notes: Each column reports a regression of post-randomization credit outcomes on treatment. Outcomes in columns 2–4 come from partner-MFI administrative data. 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 a lender other than the partner MFI after study enrollment, and the amount of that loan. The outcome in column 1 is the sum of columns 4 and 6 for survey respondents. Controls in Panel B are the baseline number of weekly customers, number of employees, monthly profits, and household income. Control-group means are reported with the standard deviation of the outcome in square brackets. Standard errors, in parentheses, are clustered at the firm level. All regressions are weighted by the inverse probability of treatment assignment within randomization strata. All monetary values are in USD. ***$p \leq 0.01$, **$p \leq 0.05$, *$p \leq 0.10$.

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	1013.05***	447.26***	417.84**	5493.25**	0.15
	(359.68)	(155.60)	(198.25)	(2441.25)	(0.13)
Panel B: With Controls					
Treatment	598.75**	291.72***	287.52*	3428.61*	0.02
	(236.18)	(106.54)	(169.81)	(1954.31)	(0.09)
Control Group Mean	3999.91	2181.32	3165.57	17438.89	0.87
	[3400.54]	[1663.92]	[2048.32]	[26024.27]	[1.43]
No. Obs	2,572	2,567	1,020	1,063	1,087
No. Firms	532	532	481	483	487

Notes: Each column reports a regression of the indicated post-randomization business or household outcome on treatment. Outcomes are pooled across all available post-randomization survey and partner-MFI loan-application observations. Monthly revenue and monthly profits come from both survey and partner-MFI loan-application data; household income, business capital, and employees come from survey data. Business capital is the sum of fixed assets and inventory. All outcomes are winsorized at the 1st and 99th percentiles, with percentiles computed over the full pooled post-randomization sample rather than separately by data wave. Panel A reports regressions of the outcome on treatment with no additional controls. Panel B implements Specification 1. Relative to Panel A, it adds month-year fixed effects, the baseline value of the outcome, and baseline firm-size controls: number of weekly customers, number of employees, monthly profits, and household income. Control-group means are reported with standard deviations in square brackets. Standard errors, in parentheses, are clustered at the firm level. All regressions are weighted by the inverse probability of treatment assignment within randomization strata. Monetary values are in USD per month, except business capital, which is a stock. ***$p\leq0.01$, **$p\leq0.05$, *$p\leq0.10$.

Table 3: Treatment Effects on Consumer Surplus and Its Components

	Customer Level			Business Level	
	Transaction Size	Willingness to Pay	Consumer Surplus	Customers per Week	Total Weekly Consumer Surplus
	(1)	(2)	(3)	(4)	(5)
Panel A: Without Controls					
Treatment	3.52*	4.01	0.74**	53.74***	220.93***
	(1.99)	(2.58)	(0.36)	(17.09)	(61.38)
Panel B: With Controls					
Treatment	5.33***	6.56***	1.21***	26.70***	123.49**
	(1.86)	(2.38)	(0.36)	(10.29)	(56.09)
Control Group Mean	13.68	17.09	2.31	94.76	220.81
	[16.61]	[22.23]	[3.99]	[152.53]	[326.21]
No. Obs	1,466	1,511	1,545	1,052	246
No. Firms	244	245	249	478	246

Notes: Columns 1–3 are customer-level outcomes from customer surveys. Column 1 is transaction size, defined as the amount paid in the customer's most recent transaction at the firm. Column 2 is the maximum amount the customer reported being willing to pay at that store for the same purchase. Column 3 is per-transaction consumer surplus, defined as willingness to pay minus transaction size. Consumer surplus is floored at zero. Columns 4 and 5 are firm-level outcomes. Column 4 is the owner-reported number of customers served in a typical week. Column 5 is total weekly consumer surplus, constructed as the firm-level average of column 3 multiplied by column 4. Outcomes in columns 1–3 are trimmed at the 5th and 95th percentiles; the outcome in column 4 is winsorized at the 1st and 99th percentiles. In all cases, percentiles are computed over the full pooled sample rather than separately by survey wave. Panel A reports regressions of the outcome on treatment with no additional controls. Panel B implements Specification 1. Relative to Panel A, it adds month-year fixed effects, the baseline value of the outcome when available, and baseline firm-size controls: number of weekly customers, number of employees, monthly profits, and household income. Panel B has one fewer observation in columns 1 and 3 because a singleton customer (the only respondent in their month-year cell) is dropped when absorbing the fixed effects. All regressions are weighted by the inverse probability of treatment assignment within randomization strata. Control-group means are reported with standard deviations in square brackets. Standard errors, in parentheses, are clustered at the firm level. Monetary values are in USD: columns 1–3 per transaction and column 5 per week. ***$p \leq 0.01$, **$p \leq 0.05$, *$p \leq 0.10$.

Table 4: Treatment Effects on Customer Preferences

	Reasons Why Customers Prefer the Business								
	Price	Quality	Customer Service	Trust	Location	Prefer Over Big	Support Local	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)	(0.02)	(0.04)	(0.01)	(0.02)
Panel B: With Controls									
Treatment	0.06	0.06	0.08*	0.08*	-0.10**	0.05**	0.07*	-0.00	0.03
	(0.05)	(0.04)	(0.05)	(0.05)	(0.05)	(0.02)	(0.04)	(0.01)	(0.02)
Control Group Mean	0.58	0.59	0.44	0.42	0.50	0.06	0.21	0.01	0.06
	[0.49]	[0.49]	[0.50]	[0.49]	[0.50]	[0.25]	[0.41]	[0.10]	[0.25]
No. Obs	1,615	1,615	1,615	1,615	1,615	1,615	1,615	1,615	1,615
No. Firms	257	257	257	257	257	257	257	257	257

Notes: Outcomes come from customer surveys. Each outcome is an indicator equal to one if the customer cited the given reason in response to the question "Why do you choose to buy this business's products/services over others?" Customers could select multiple reasons, so indicators do not sum to one across columns. Panel A reports regressions of the outcome on treatment with no additional controls. Panel B implements Specification 1. Relative to Panel A, it adds month-year fixed effects and baseline firm-size controls: number of weekly customers, number of employees, monthly profits, and household income. All regressions are weighted by the inverse probability of treatment assignment within randomization strata. Control-group means are reported with standard deviations in square brackets. Standard errors, in parentheses, are clustered at the firm level. ***$p\leq0.01$, **$p\leq0.05$, *$p\leq0.10$.

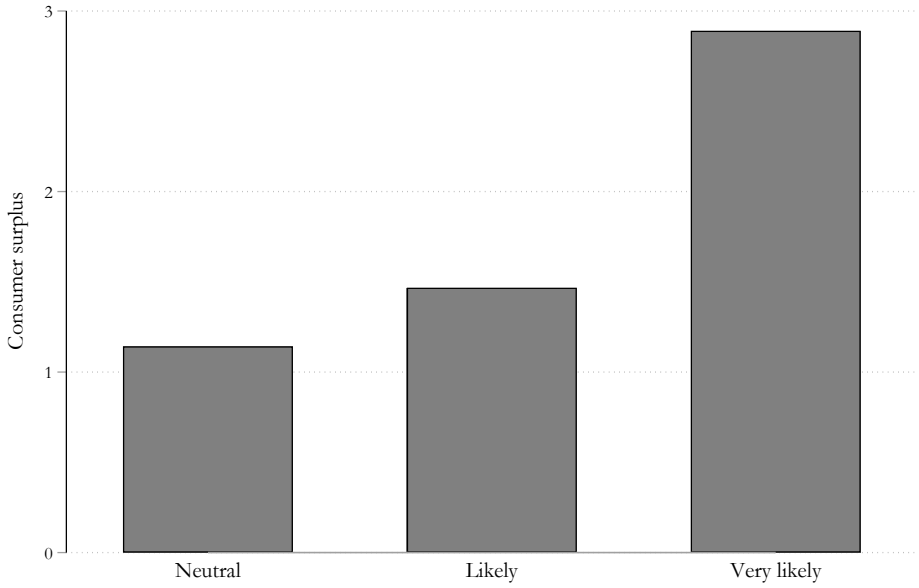
Table 5: Spillover Effects on Nearby Firms

	Revenue	Costs	Profits	Margin×100
	(1)	(2)	(3)	(4)
Same-Sector Treated, 0–500m	-3.75	6.19	-0.42	-0.28
	(19.62)	(10.17)	(11.51)	(0.22)
Same-Sector Treated, 500m–1km	-6.27	3.39	-2.12	-0.10
	(16.75)	(8.33)	(9.78)	(0.16)
Same-Sector Treated, 1km–2km	17.89*	3.70	6.83	-0.13
	(10.09)	(5.00)	(5.90)	(0.10)
Diff-Sector Treated, 0–500m	7.73	2.14	5.41	-0.07
	(7.16)	(3.51)	(4.17)	(0.07)
Diff-Sector Treated, 500m–1km	2.17	1.34	3.29	-0.05
	(5.52)	(2.73)	(3.32)	(0.06)
Diff-Sector Treated, 1km–2km	-1.31	-0.12	-0.25	-0.01
	(3.76)	(1.85)	(2.22)	(0.04)
Outcome Mean	2148.75	863.65	1239.78	60.40
	[1788.77]	[858.73]	[965.35]	[14.07]
No. Obs	571,143	569,695	569,004	568,768
No. Firms	110,235	110,079	110,131	110,346
p-value: Same-Sector Joint = 0	0.35	0.78	0.70	0.30
p-value: Diff-Sector Joint = 0	0.66	0.90	0.46	0.70

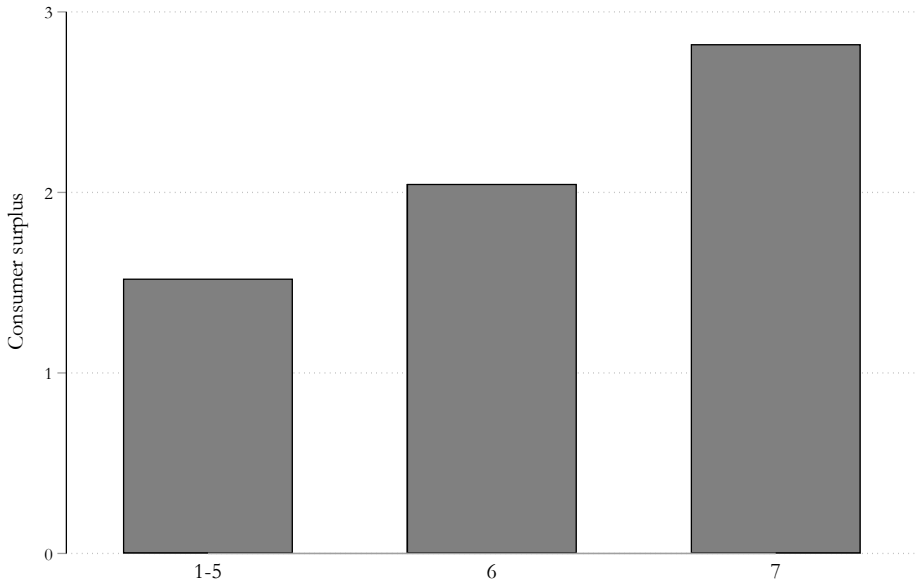
Notes: This table reports estimates of Equation 4. An observation is a spillover firm × month; spillover firms are borrowers of our partner lender outside the experimental sample, observed from January 2023 to December 2025. Outcomes are monthly revenue, costs, and profits (in USD) and gross margin (profit divided by revenue, ×100); each outcome is independently trimmed at the 1st and 99th percentiles. Standard deviations of the outcome are reported in square brackets. Each column regresses the outcome on six treatment-exposure variables—the inverse-probability-weighted count of treated applicant firms randomized by month t in each of three distance bands (0–500m, 500m–1km, 1km–2km) and two sector channels (same sector and different sector, based on the 14-category sector classification)—and six density controls counting all randomized applicant firms (treated and control) in the same band × channel cells. All regressions include firm and comuna × month-year fixed effects. The final two rows report p-values from joint tests that the three same-sector (different-sector) coefficients equal zero. Standard errors, in parentheses, are clustered at the spillover-firm level. ***$p\leq0.01$, **$p\leq0.05$, *$p\leq0.10$.

Figure 1: Correlation between Consumer Surplus and Customer Satisfaction and Recommendations

(a) Likelihood of Recommending the Store



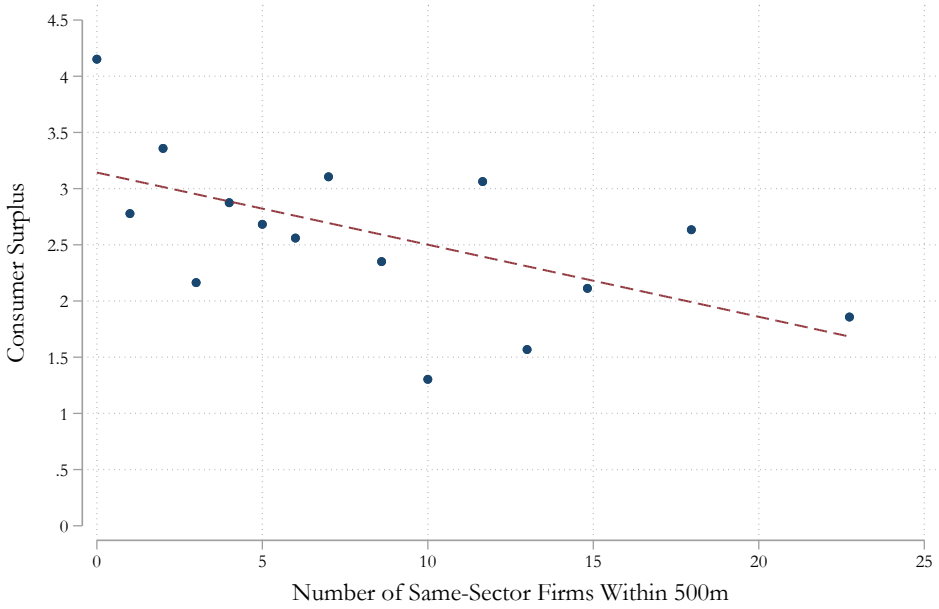
(b) Satisfaction with the Store's Products or Services



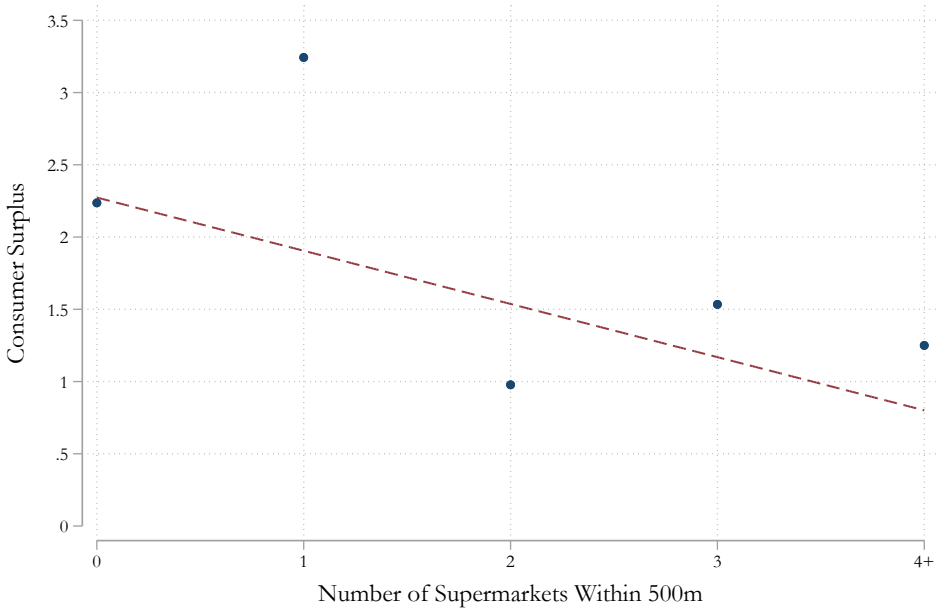
Notes: Each bar reports mean per-transaction consumer surplus, in USD, among customers in the indicated response category. Per-transaction consumer surplus is defined as the maximum amount the customer reported being willing to pay at the store for the purchase minus the amount paid, trimmed at the 5th and 95th percentiles over the pooled customer-survey sample. Panel (a) groups customers according to their reported likelihood of recommending the store to friends or family: Very Unlikely, Unlikely, Neutral, Likely, or Very Likely. Because the first two categories contain relatively few observations, Very Unlikely, Unlikely, and Neutral are combined and labeled Neutral. Panel (b) groups customers according to their satisfaction with the store's products or services, measured on a scale from 1 to 7, with 7 indicating the highest satisfaction. Because categories 1–5 contain relatively few observations, they are combined. The figure pools the two customer-survey waves. The sample comprises 1,546 customer observations from 249 firms.

Figure 2: Consumer Surplus and the Density of Local Competition

(a) Nearby Same-Sector Microenterprises



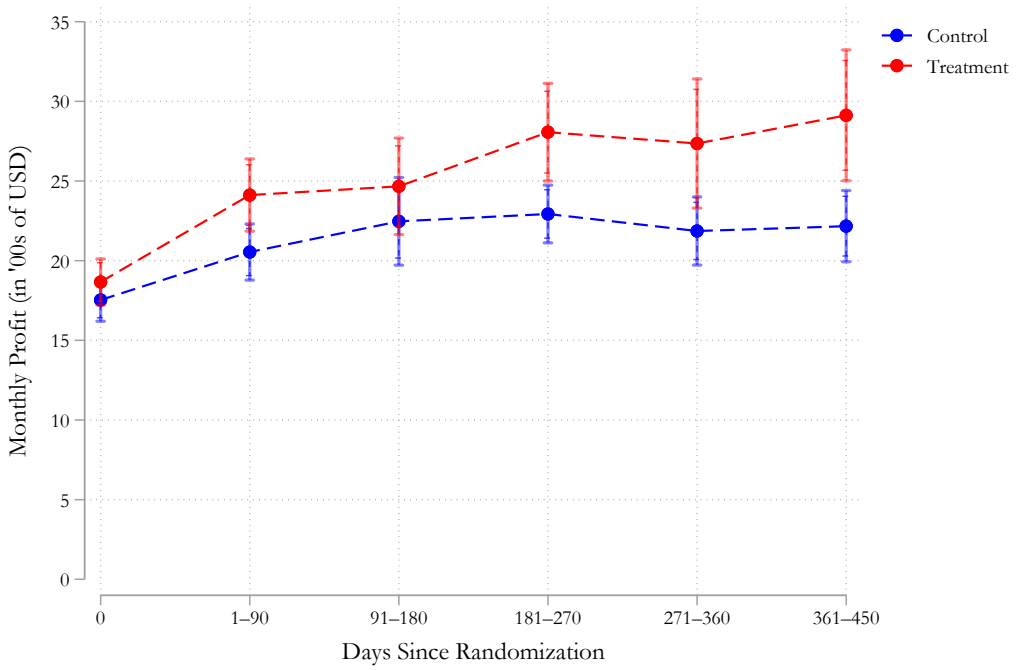
(b) Nearby Supermarkets (Retail Firms)



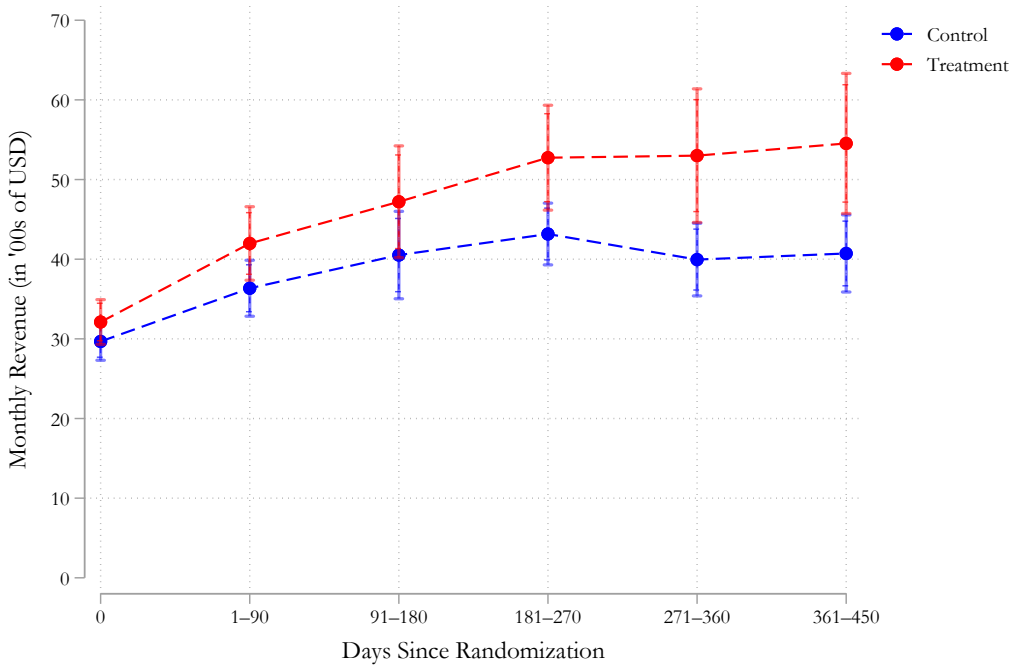
Notes: Both panels are binned scatterplots (binscatters) of customer-level consumer surplus (USD) against a measure of the density of local competition around the customer's chosen store, with a line of best fit estimated on the underlying customer-level data. **Panel (a):** the horizontal axis is the number of non-sample MFI borrowers in the same 14-category sector as the chosen store located within 500 meters of that store. Markers are mean consumer surplus within 20 equal-frequency bins of the count. The line of best fit is from the OLS regression of consumer surplus on the count; slope: -0.064 (SE: 0.029, $p = 0.024$, $N = 1,546$ customer observations). Non-sample firm locations are from the Jan 2023–Dec 2025 loan-application data. **Panel (b):** the horizontal axis is the number of large chain supermarkets within 500 meters of the chosen store, top-coded at 4, computed from the census of 1,214 supermarket locations described in Section 2.1. Markers are mean consumer surplus at each value of the count. The sample is restricted to retail firms (apparel, personal care, fresh and specialty food, gifts and general merchandise, grocery, and other retail), excluding service providers and manufacturers. The line of best fit is from the customer-level OLS regression of consumer surplus on the top-coded count; slope: -0.369 (SE: 0.173, $p = 0.035$, $N = 812$ customers of 119 firms). In both panels, markers are unweighted bin means, the fitted line is estimated on individual customer observations rather than the binned means, and standard errors are clustered at the experimental-firm level.

Figure 3: Evolution of Business Revenue and Profit

(a) Monthly Profit (Hundreds of USD)

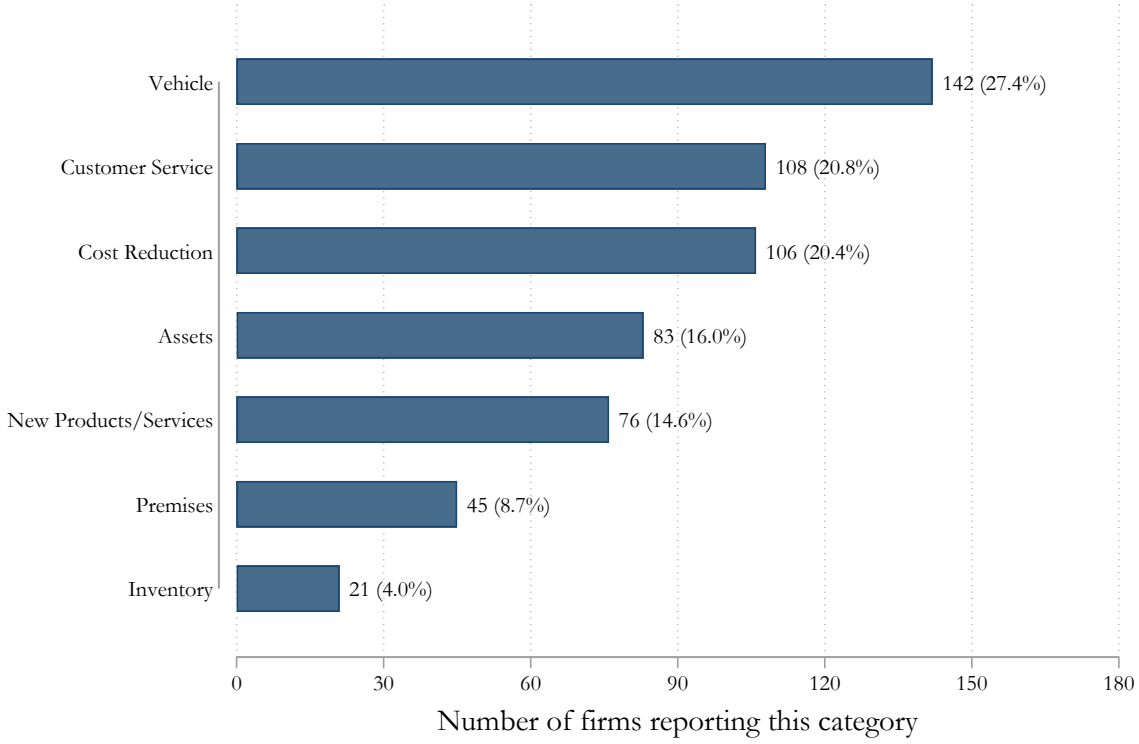


(b) Monthly Revenue (Hundreds of USD)



Notes: Panel (a) plots mean monthly business profit, and Panel (b) plots mean monthly business revenue, separately for the treatment and control groups. Both outcomes are expressed in hundreds of USD. The data come from loan applications and follow-up surveys. Time zero reports the baseline, pre-randomization observation; subsequent observations are grouped into 90-day intervals according to the number of days since randomization: 1–90, 91–180, 181–270, 271–360, and 361–450 days. Profit and revenue are winsorized at the 1st and 99th percentiles, with percentile thresholds computed over the full pooled post-randomization sample rather than separately by period. The shorter inner whiskers denote 90% confidence intervals, and the longer outer whiskers denote 95% confidence intervals. The confidence intervals are constructed using standard errors clustered at the firm level. Other primary outcomes are not displayed because they were not collected in the relatively high-frequency loan-application data.

Figure 4: Distribution of Investment Categories



Notes: The figure shows the distribution of borrower-stated loan-use categories, based on a manual classification of loan applications. Each applicant's stated plan was classified into up to three of seven exhaustive categories. A firm is counted in every category covered by its plan, so the shares need not sum to one. Bar labels report the number of firms and the share of classified applicants. Category definitions: **Vehicle** = delivery vehicles, motorcycles, or transport equipment; **Customer Service** = customer-facing improvements such as decor, air conditioning, furniture, display cases, or premises remodeling to improve the customer experience; **Cost Reduction** = bulk purchasing, storage expansion, or assets that lower unit production costs; **Assets** = generic productive assets not tied to a customer-facing or cost-reduction objective; **New Products/Services** = assets or inventory intended to expand the range of goods or services offered; **Premises** = new premises or expansion of existing premises for productive rather than customer-facing purposes; **Inventory** = restocking of existing inventory. The sample comprises 519 loan applicants with at least one classified loan-use tag.

Internet Appendix for "When Microenterprises Grow, Are Consumers Better Off? Evidence from Large Loans to Microenterprises in Chile"

Dean Karlan, Natalia Rigol, and Benjamin N. Roth

A Appendix Tables and Figures

Table A.1: Balance Table

	Control Group Mean	Treatment - Control Difference	No. Obs
	(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)	
Household Members	3.81	-0.03	541
	[1.35]	(0.13)	
Household Income (USD)	3507.56	131.77	532
	[1980.71]	(181.56)	
Panel B: Business Outcomes			
Monthly Revenue (USD) †	2967.49	233.98	541
	[1969.07]	(186.59)	
Monthly Profits (USD) †	1753.46	108.82	541
	[1112.15]	(100.20)	
Number of Weekly Customers	118.52	32.58*	528
	[170.04]	(17.28)	
Number of Employees	0.76	0.21*	532
	[1.16]	(0.11)	
Value of Business Capital (USD)	17617.22	3174.91	532
	[24036.25]	(2201.21)	
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		0.79	
p-value		0.671	

Notes: Column 1 reports the control-group mean. Column 2 reports the treatment-control difference. Column 3 reports the number of non-missing observations. Value of capital is the sum of durable assets and inventory. Control-group standard deviations are reported in square brackets. Robust standard errors are reported in parentheses. The F-statistic and p-value report a joint test of equality of baseline covariates across treatment arms. Variables marked with † come from loan-application data. Continuous baseline variables are winsorized at the 1st and 99th percentiles. All monetary values are in USD. ***$p \leq 0.01$, **$p \leq 0.05$, *$p \leq 0.10$

Table A.2: Survey Completion Rates

	Control Group Mean	Treatment Difference
	(1)	(2)
Panel A: Borrower Business-Outcome Data		
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-Survey Data		
Follow Up II: Number of customers surveyed	379	128
Follow Up II: Proportion of Businesses Represented	0.25	0.03
	[0.43]	(0.04)
Follow Up IV: Number of customers surveyed	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)
No. Firms	269	272

Notes: Column 1 reports the control-group mean. Column 2 reports the treatment-control difference. Panel A reports completion or availability rates for borrower business-outcome data: baseline survey, follow-up survey waves, any follow-up survey, any post-randomization partner-MFI administrative observation, and either source. Panel B reports customer-survey coverage. Rows labeled "Number of customers surveyed" report raw counts of customer respondents by treatment arm. Rows labeled "Proportion of Businesses Represented" are indicators equal to one if at least one customer of the business was surveyed in the indicated customer-survey wave or in any customer-survey wave. Control-group standard deviations are reported in square brackets. Standard errors for treatment-control differences, in parentheses, are clustered at the firm level. All regressions are weighted by the inverse probability of treatment assignment within randomization strata. ***$p \leq 0.01$, **$p \leq 0.05$, *$p \leq 0.10$.

Table A.3: Attrition into the Customer-Data Sample

	Has Consumer Surplus Data
	(1)
Treatment	0.03
	(0.04)
Treatment · Age	-0.07
	(0.05)
Treatment · Male	-0.15
	(0.11)
Treatment · Post-Secondary Education	0.02
	(0.10)
Treatment · HH Members	-0.03
	(0.04)
Treatment · HH Income (USD)	0.01
	(0.04)
Treatment · Monthly Revenue (USD)	-0.08
	(0.11)
Treatment · Monthly Profits (USD)	0.07
	(0.11)
Treatment · Number of Weekly Customers	0.06
	(0.05)
Treatment · Number of Employees	0.02
	(0.05)
Treatment · Value of Business Capital (USD)	0.08
	(0.05)
Treatment · Sector: Manufacturing	0.02
	(0.11)
Treatment · Sector: Agriculture	0.50
	(0.31)
Treatment · Sector: Services	-0.03
	(0.12)
Control Group Mean	0.45
	[0.50]
No. Obs	541
No. Firms	541
F-stat	1.31
p-value	0.194

Notes: This table tests for differential selection into the customer-data sample. The outcome is an indicator equal to one if consumer-surplus data were collected from at least one customer of the firm in either customer-survey wave. The table reports a linear probability model regressing this indicator on treatment, baseline covariates, and treatment-by-covariate interactions. Covariate main effects are included but not reported. Continuous covariates are standardized to mean zero and unit variance; binary covariates are demeaned. Interaction coefficients therefore report how the treatment-control difference in customer-data coverage changes with a one-standard-deviation increase in a continuous covariate or a one-unit increase in a binary covariate. The Treatment coefficient is the differential probability for a firm with average baseline characteristics. Retail is the omitted sector category. Missing covariate values are set to zero and flagged with missing-data indicators; those indicators and their treatment interactions are included but not reported. The F-statistic and p-value report a joint test that the Treatment coefficient and all reported interaction terms equal zero. The control-group mean is the IPW-weighted share of control firms with customer data, with its standard deviation reported in square brackets. Standard errors, in parentheses, are clustered at the firm level. The regression is weighted by the inverse probability of treatment assignment within randomization strata. $^{***}p\leq0.01$, $^{**}p\leq0.05$, $^{*}p\leq0.10$.

Table A.4: Correlation of Consumer Surplus and Business Sector

	Correlation		
	Transaction Size	Willingness to Pay	Consumer Surplus
	(1)	(2)	(3)
Subsector			
Apparel and Footwear	0.172***	0.182***	0.268***
Bakery and Pastry	0.049	0.019	0.048
Beauty and Personal Care Services	0.103	0.089	0.154**
Beauty and Personal Care Retail	-0.035	-0.043	-0.045
Fresh and Specialty Food Retail	-0.077	-0.098	-0.158**
Gifts and Bazaar	-0.074	-0.093	-0.079
Grocery and Convenience Retail	-0.274***	-0.255***	-0.193***
Handicrafts	0.052	0.057	0.010
Restaurants and Prepared Food Services	-0.096	-0.078	-0.170***
Tailoring	0.206***	0.185***	0.150**
Other Manufacturing	0.200***	0.276***	.
Other Retail	-0.094	-0.025	-0.068
Other Services	0.123*	0.086	0.138**
No. Firms	244	245	249

Notes: Each cell reports the correlation between an indicator for the row's business subsector and the outcome listed in the column. Transaction size, willingness to pay, and per-transaction consumer surplus are customer-level outcomes averaged at the firm-level. Transaction size, willingness to pay, and per-transaction consumer surplus are trimmed at the 5th and 95th percentiles. Missing cells indicate that the correlation could not be estimated because of insufficient non-missing data. ***$p \leq 0.01$, **$p \leq 0.05$, *$p \leq 0.10$.

Table A.5: 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	623.65**	308.04**	315.29	3026.07	-0.15
	(248.80)	(119.92)	(214.42)	(1929.58)	(0.10)
Post	874.88***	317.73***	-346.47**	-409.99	0.13*
	(120.86)	(67.57)	(133.99)	(1225.22)	(0.08)
Panel B: With Controls					
Post × Treatment	704.87***	308.46**	315.22	3096.63	-0.07
	(255.51)	(122.01)	(215.48)	(2028.76)	(0.10)
Treatment	47.26	37.54	-23.85	464.70	0.06**
	(51.04)	(31.71)	(85.72)	(468.83)	(0.03)
Post	842.54***	328.34***	-360.91***	-325.65	0.08
	(127.22)	(68.91)	(138.65)	(1304.63)	(0.08)
Control Group Mean	3402.03	1939.11	3288.42	17524.71	0.83
	[3056.27]	[1886.72]	[2084.16]	[25571.62]	[1.35]
No. Obs	7,441	7,436	1,552	1,595	1,619
No. Firms	541	541	540	540	540

Notes: This table estimates Specification 3, the difference-in-differences analogue of Table 2. The coefficient on $Post \times Treatment$ is the estimated treatment effect. Columns 1 and 2 add pre-randomization partner-MFI loan-application data, beginning in June 2020. Columns 3-5 use the baseline survey as the pre-randomization observation. Panel A includes firm fixed effects. Panel B includes treatment, Post, month-year fixed effects, the baseline outcome where not already included, and baseline firm-size controls: number of weekly customers, number of employees, monthly profits, and monthly household income. All outcomes are winsorized at the 1st and 99th percentiles, with percentiles computed over the full pooled post-randomization sample rather than separately by data wave. Control-group means are reported with standard deviations in square brackets. Standard errors, in parentheses, are clustered at the firm level. All regressions are weighted by the inverse probability of treatment assignment within randomization strata. Monetary values are in USD per month, except business capital, which is a stock. ***$p \leq 0.01$, **$p \leq 0.05$, *$p \leq 0.10$.

Table A.6: Treatment Effects on Business Finances by Data Source

	Loan Application Data		Survey Data	
	Monthly Revenue	Monthly Profit	Monthly Revenue	Monthly Profit
	(1)	(2)	(3)	(4)
Panel A: Without Controls				
Treatment	838.35**	455.28***	1259.29***	433.80**
	(324.40)	(162.62)	(481.81)	(188.96)
Panel B: With Controls				
Treatment	506.29**	312.16***	733.50**	254.99*
	(203.71)	(110.75)	(354.64)	(149.00)
Control Group Mean	3952.70	2269.92	4065.09	2058.10
	[2811.11]	[1570.34]	[4078.54]	[1780.05]
No. Obs	1,495	1,495	1,077	1,072
No. Firms	517	517	487	486

Notes: Relative to Table 2, this table uses only partner-MFI loan-application data for columns (1)—(2) and only post-randomization survey observations for columns (3)—(4). Panel A reports regressions of the outcome on treatment without additional controls. Panel B includes month-year fixed effects and controls for the baseline value of the outcome where available and for baseline firm size: number of weekly customers, number of employees, monthly profits, and monthly household income. All outcomes are winsorized at the 1st and 99th percentiles, with percentiles computed over the full pooled post-randomization sample rather than separately by data wave. Control-group means are reported with standard deviations in square brackets. Standard errors, in parentheses, are clustered at the firm level. All regressions are weighted by the inverse probability of treatment assignment within randomization strata. Monetary values are in USD per month. ***$p \leq 0.01$, **$p \leq 0.05$, *$p \leq 0.10$.

Table A.7: Treatment Effects on Average Post-Randomization 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	900.35***	457.80***	473.43**	5847.88**	0.13
	(326.79)	(142.26)	(188.37)	(2445.01)	(0.13)
Panel B: With Controls					
Treatment	443.08**	294.11***	299.36*	3194.27*	-0.06
	(206.96)	(98.23)	(159.38)	(1751.84)	(0.10)
Control Group Mean	3941.97	2109.50	3121.44	17052.09	0.90
	[2904.50]	[1316.56]	[1847.77]	[23087.59]	[1.32]
No. Obs	532	532	481	483	487
No. Firms	532	532	481	483	487

Notes: Relative to Table 2, this table collapses each outcome to one post-randomization observation per firm by averaging across all available post-randomization survey and partner-MFI loan-application data. Panel A reports regressions of the averaged outcome on treatment without additional controls. Panel B controls for the baseline value of the outcome where available and for baseline firm size: number of weekly customers, number of employees, monthly profits, and monthly household income. Outcome construction follows the same cleaning rules as in Table 2. Control-group means are reported with standard deviations in square brackets. Standard errors, in parentheses, are clustered at the firm level. All regressions are weighted by the inverse probability of treatment assignment within randomization strata. Monetary values are in USD per month, except business capital, which is a stock. ***$p \leq 0.01$, **$p \leq 0.05$, *$p \leq 0.10$.

Table A.8: First Stage: Loan Take-Up with PDS-LASSO Controls

	All Lenders	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	1513.30***	0.97***	2238.13***	1699.93***	-0.02	-219.61
	(218.26)	(0.01)	(33.26)	(66.42)	(0.03)	(200.85)
Control Group Mean	2333.46	0.00	0.00	1767.96	0.13	547.73
	[2455.42]	[0.00]	[0.00]	[766.45]	[0.34]	[2351.62]
No. Obs	439	541	541	541	439	439
No. Firms	439	541	541	541	439	439

Notes: This table repeats the first-stage analysis in Table 1 using PDS-LASSO controls. Each column reports a regression of the indicated post-randomization credit outcome on treatment. Outcomes in columns 2-4 come from partner-MFI administrative data. Outcomes in columns 5 and 6 come from survey data, which accounts for the lower number of observations: respondents were asked whether they took a loan from a lender other than the partner MFI after study enrollment and, if so, the amount of that loan. The outcome in column 1 is the sum of columns 4 and 6 for survey respondents. PDS-LASSO controls are selected from the baseline characteristics reported in Table A.1. Control-group means are reported with standard deviations in square brackets. Standard errors, in parentheses, are clustered at the firm level. All regressions are weighted by the inverse probability of treatment assignment within randomization strata. All monetary values are in USD. ***$p \leq 0.01$, **$p \leq 0.05$, *$p \leq 0.10$.

Table A.9: Treatment Effects on Business and Household Finances with PDS-LASSO Controls

	Monthly Revenue	Monthly Profit	Monthly HH Income	Value of Business Capital	Employees
	(1)	(2)	(3)	(4)	(5)
Treatment	554.18**	271.92***	333.84*	3506.09*	0.03
	(231.01)	(102.84)	(172.57)	(1891.83)	(0.09)
Control Group Mean	3999.91	2181.32	3165.57	17438.89	0.87
	[3400.54]	[1663.92]	[2048.32]	[26024.27]	[1.43]
No. Obs	2,572	2,567	1,020	1,063	1,087
No. Firms	532	532	481	483	487

Notes: This table repeats the analysis in Table 2 using PDS-LASSO controls. Each column reports the treatment effect on the indicated post-randomization business or household outcome. Outcomes are pooled across all available post-randomization survey and partner-MFI loan-application observations. Controls include month-year fixed effects and covariates selected by PDS-LASSO from the baseline characteristics reported in Table A.1. The baseline value of the outcome is included where available. Outcomes are winsorized at the 1st and 99th percentiles. Control-group means are reported with standard deviations in square brackets. Standard errors, in parentheses, are clustered at the firm level. All regressions are weighted by the inverse probability of treatment assignment within randomization strata. Monetary values are in USD per month, except business capital, which is a stock. ***$p \leq 0.01$, **$p \leq 0.05$, *$p \leq 0.10$.

Table A.10: Treatment Effects on Consumer Surplus with PDS-LASSO Controls

	Customer level			Business level	
	Transaction Size	Willingness to Pay	Consumer Surplus	Customers per Week	Total Weekly Consumer Surplus
	(1)	(2)	(3)	(4)	(5)
Treatment	4.14**	4.86*	1.17***	28.06***	100.25*
	(1.97)	(2.51)	(0.36)	(10.40)	(53.90)
Control Group Mean	13.68	17.09	2.31	94.76	220.81
	[16.61]	[22.23]	[3.99]	[152.53]	[326.21]
No. Obs	1,466	1,511	1,545	1,052	246
No. Firms	244	245	249	478	246

Notes: This table repeats the analysis in Table 3 using PDS-LASSO controls. Columns 1-3 are customer-level outcomes from customer surveys. Column 1 is transaction size, defined as the amount paid in the customer's most recent transaction at the firm. Column 2 is the maximum amount the customer reported being willing to pay at that store for the same purchase. Column 3 is per-transaction consumer surplus, defined as willingness to pay minus transaction size. Consumer surplus is floored at zero. Columns 4 and 5 are firm-level outcomes. Column 4 is the owner-reported number of customers served in a typical week. Column 5 is total weekly consumer surplus, constructed as the firm-level average of column 3 multiplied by column 4. Outcomes in columns 1-3 are trimmed at the 5th and 95th percentiles; the outcome in column 4 is winsorized at the 1st and 99th percentiles. Controls include month-year fixed effects and covariates selected by PDS-LASSO from the baseline characteristics reported in Table A.1. Control-group means are reported with standard deviations in square brackets. Standard errors, in parentheses, are clustered at the firm level, including in the customer-level regressions. All regressions are weighted by the inverse probability of treatment assignment within randomization strata. Monetary values are in USD: columns 1-3 per transaction and column 5 per week. ***$p\leq0.01$, **$p\leq0.05$, *$p\leq0.10$.

Table A.11: Robustness: Treatment Effects on Total Weekly Consumer Surplus

	Main Specification	In-Person Only	Wave II Only	Wave IV Only	Selection Reweighted	Winsorized Consumer Surplus
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	123.49**	125.78*	126.47*	119.38	108.77**	67.00
	(56.09)	(64.40)	(66.72)	(72.31)	(50.90)	(201.19)
Control Group Mean	220.81	203.26	321.25	171.73	220.81	484.23
	[326.21]	[327.26]	[387.76]	[291.28]	[326.21]	[1795.62]
No. Obs	246	216	135	180	239	253
No. Firms	246	216	135	180	239	253
Wave II	Yes	Yes	Yes	No	Yes	Yes
Wave IV	Yes	Yes	No	Yes	Yes	Yes
Includes Remote Surveys	Yes	No	Yes	Yes	Yes	Yes
IPW	Yes	Yes	Yes	Yes	No	Yes
Selection Reweighting	No	No	No	No	Yes	No

Notes: The outcome in all columns is total weekly consumer surplus at the firm level, constructed as the firm-level average consumer surplus per customer multiplied by the owner-reported number of customers served in a typical week. Consumer surplus is willingness to pay minus transaction size and is floored at zero. Column 1 replicates the preferred specification from Table 3, column 5, Panel B. Column 2 excludes customer observations collected remotely by QR code or online survey. Columns 3 and 4 use customer-survey data from the first and second in-person customer-survey waves separately. Column 5 reweights observations by the inverse predicted probability of customer-survey participation rather than by the treatment-assignment inverse-probability weights. Column 6 winsorizes per-transaction consumer surplus at the 1st and 99th percentiles rather than trimming it at the 5th and 95th percentiles. Unless otherwise noted, regressions include month-year fixed effects, baseline firm-size controls, and inverse-probability weights by randomization stratum. Control-group means are reported with standard deviations in square brackets. Standard errors, in parentheses, are clustered at the firm level. Monetary values are in USD per week. ***$p \leq 0.01$, **$p \leq 0.05$, *$p \leq 0.10$.

Table A.12: Lee Bounds on Treatment Effects on Consumer Surplus

	Observed-Sample Difference in Means	Estimated Lee Region	90% Imbens–Manski Conf. Interval	Trimmed Share	No. Firms
	(1)	(2)	(3)	(4)	(5)
Total weekly consumer surplus (USD)	220.93	[103.11, 252.60]	[-12.20, 343.35]	0.067	246
	(61.38)			*treatment*	
Consumer surplus per customer, firm mean (USD)	1.06	[0.22, 1.38]	[-0.71, 2.18]	0.073	249
	(0.50)			*treatment*	

Notes: This table reports ordinary Lee (2009) trimming bounds for the treatment effect on consumer surplus. Total weekly consumer surplus requires both customer-survey data and a post-randomization customer count. Consumer surplus per customer is first averaged within each firm, so each firm contributes one equally weighted observation; this differs from the customer-level regression in column 3 of Table 3. Column 1 is computed without covariates or month-year fixed effects and is reported with its standard error in parentheses; for total weekly consumer surplus it equals the estimate in column 5, Panel A, of Table 3. The entry in italics below the trimmed share indicates which arm was trimmed. The confidence regions follow Imbens and Manski (2004) and are based on 1,000 firm-level bootstrap replications. All estimates are weighted by the inverse probability of treatment assignment within randomization strata. The bounds identify the average treatment effect for firms whose outcome would be observed under either assignment. Monetary values are in USD per week in the first row and USD per transaction in the second.

Table A.13: Sensitivity of the Consumer-Surplus Estimate to Attrition

	Maximum gap $\Delta = \tau_R - \tau_M$	Implied effect, τ_M
	(1)	(2)
Significant at the 5% level	24.86	98.63
Significant at the 10% level	57.27	66.22
Positive point estimate	226.47	−102.98

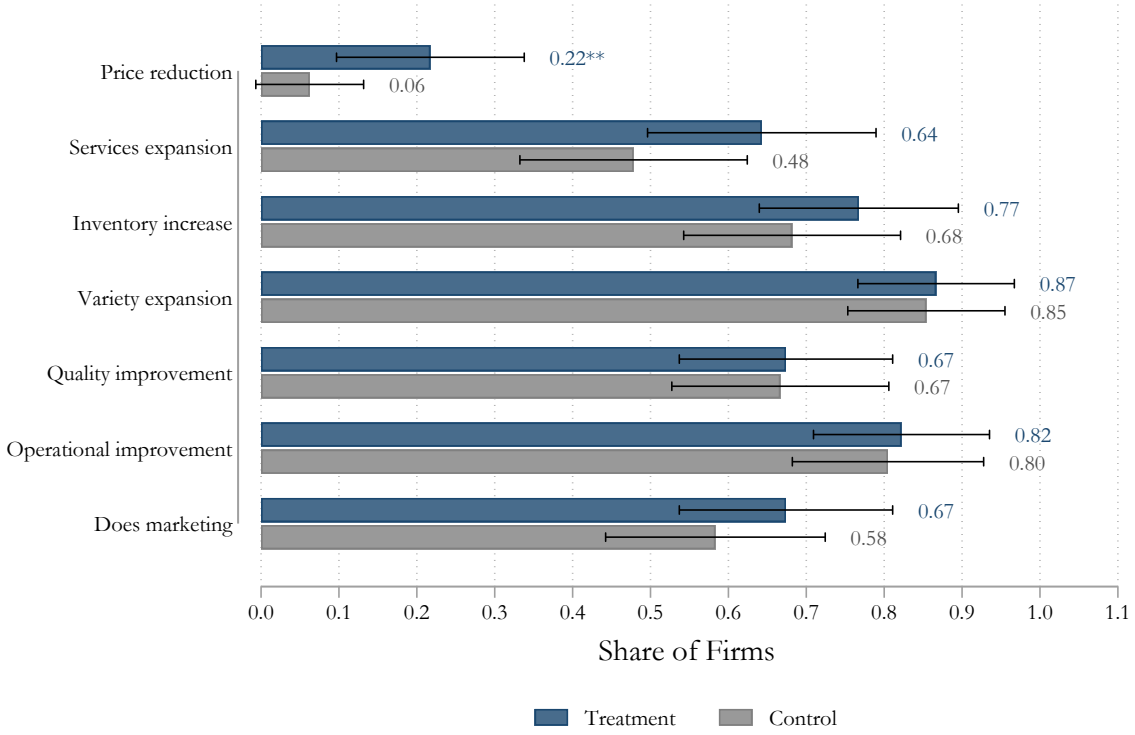
Notes: Each row reports how much smaller the average treatment effect on total weekly consumer surplus among firms without customer data would have to be, relative to the effect among firms with customer data, before the conclusion in the left column is overturned. Column 1 reports the largest treatment-effect gap $\Delta = \tau_R - \tau_M$ consistent with each conclusion, and column 2 the implied effect among firms without customer data; $\Delta = 0$ corresponds to equal effects in the two groups. Here, τ_R denotes the average treatment effect among firms represented in the customer-data sample, and τ_M denotes the average treatment effect among firms without customer data. The derivation and inference conventions are described in Appendix E. Calculations use the preferred estimate from column 5, Panel B, of Table 3 ($\widehat{\tau}_R = 123.49$, $SE = 56.09$) and the observed share $\widehat{\pi} = 246/541$. Monetary values are in USD per firm per week.

Table A.14: Neighbors Summary

	Same Sector (1)	Different Sector (2)
Neighbors within [0m, 500m]	5.89	57.99
Neighbors within (500m, 1km]	11.10	107.43
Neighbors within (1km, 2km]	27.24	279.61

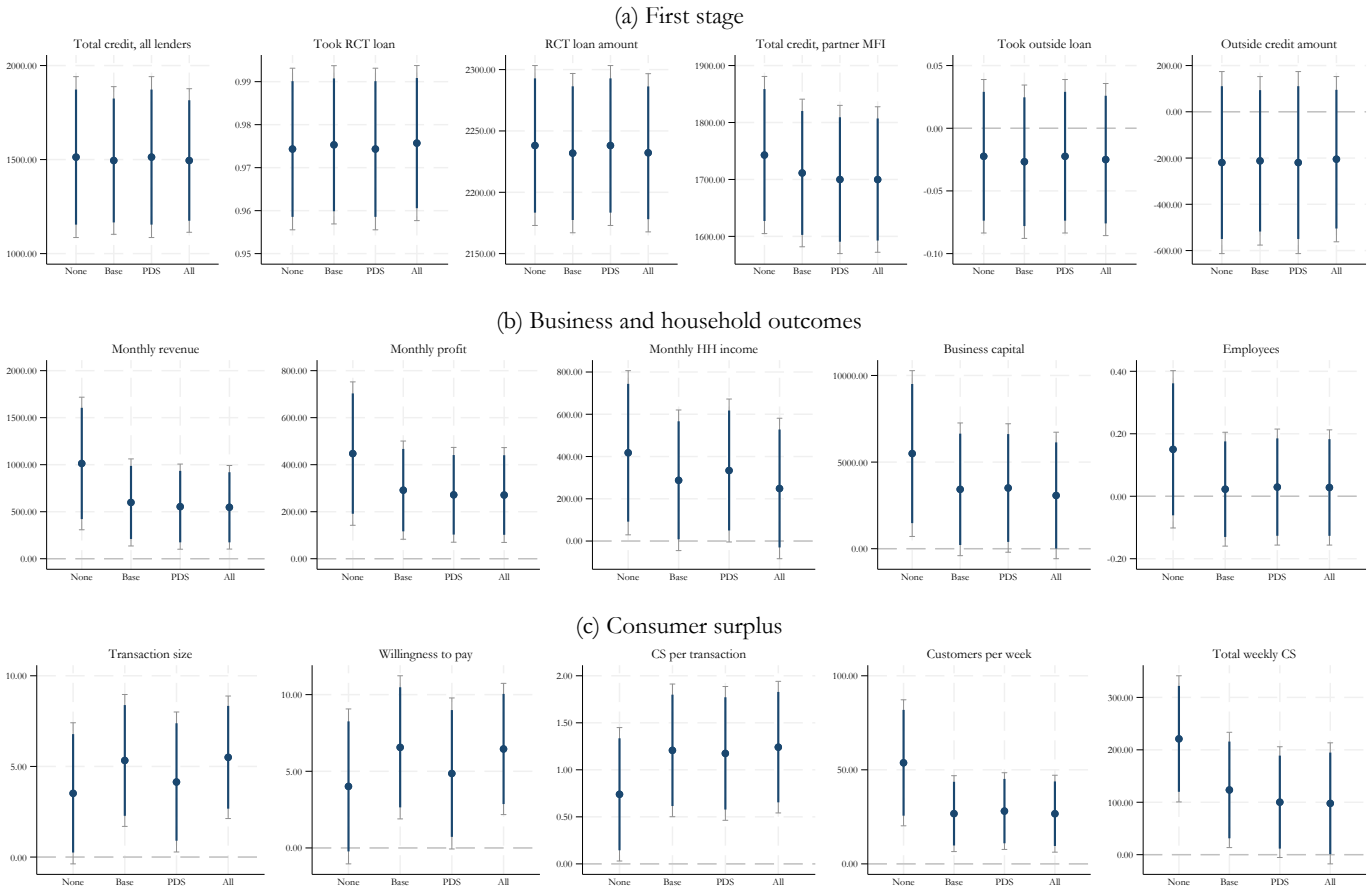
Notes: Each cell reports the average number of non-sample partner-MFI borrowers located within the indicated distance band around an experimental-sample firm. Distance bands are concentric rings: 0-500 meters, 500 meters-1 kilometer, and 1-2 kilometers. Same-sector neighbors are non-sample firms in the same 14-category sector as the experimental-sample firm; different-sector neighbors are all other non-sample firms. The non-sample firm universe is the spillover sample used in Section 6 and spans January 2023 through December 2025.

Figure A.1: Firm Actions Reported at 26 Months, by Treatment Status



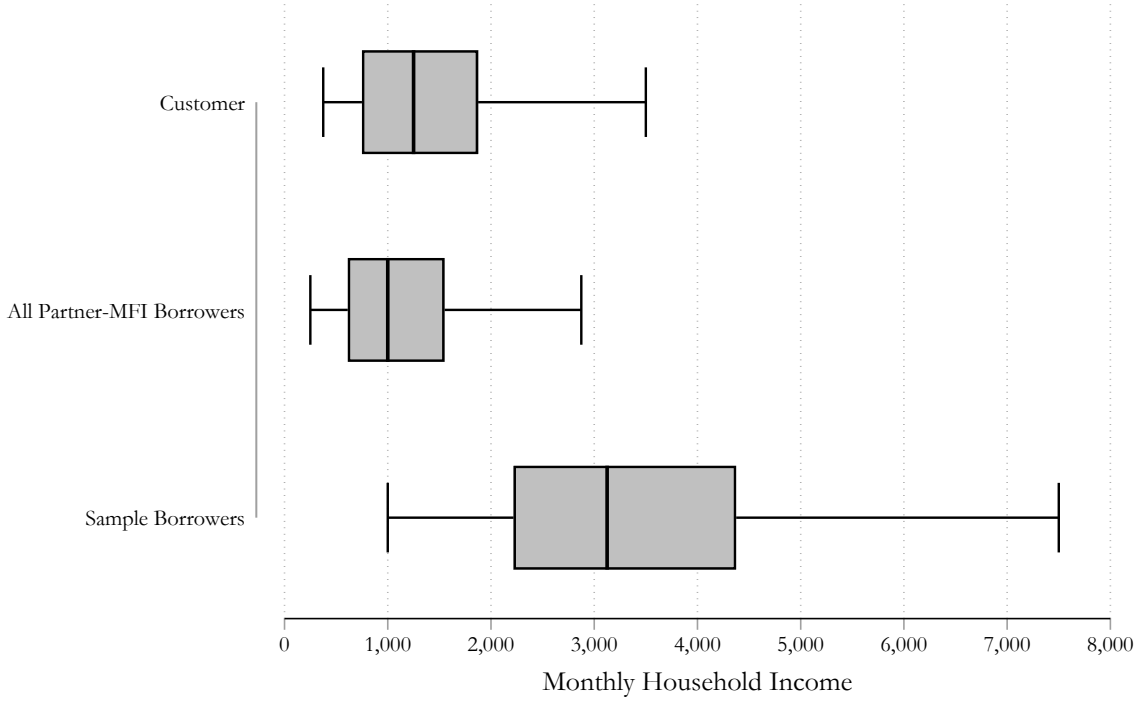
Notes: For each of seven binary firm-action outcomes elicited approximately 26 months after randomization, the figure plots the inverse-probability-weighted share of firms reporting the action separately for the treatment (navy) and control (gray) groups. The weights account for variation across randomization strata in the probability of assignment to treatment. Numeric labels at the ends of the bars report the weighted means, and whiskers denote 95% confidence intervals. Stars on the treatment bars denote the statistical significance of the inverse-probability-weighted treatment–control differences; standard errors are clustered at the firm level. ***$p \leq 0.01$, **$p \leq 0.05$, *$p \leq 0.10$. The analysis includes the 95 firms that responded to the survey (46 treatment and 49 control firms).

Figure A.2: Robustness of Treatment Effects to Control Selection



Notes: Each panel plots the estimated treatment effect on the indicated outcome under four specifications: no controls (None), corresponding to Panel A of the relevant table; the firm-size baseline controls of Panel B (Base); controls selected by PDS LASSO (PDS), as in Tables A.8, A.9, and A.10; and all candidate baseline covariates offered to PDS LASSO, entered simultaneously (All). Panel (a) reports the first-stage outcomes of Table 1, panel (b) the business and household outcomes of Table 2, and panel (c) the consumer-surplus outcomes of Table 3; outcome definitions, samples, and fixed effects follow the corresponding table specification. Estimates are in levels, and each subpanel has its own scale. Thick spikes show 90% confidence intervals, and thin capped lines show 95% confidence intervals, based on standard errors clustered at the firm level. All regressions are weighted by the inverse probability of treatment assignment within randomization strata.

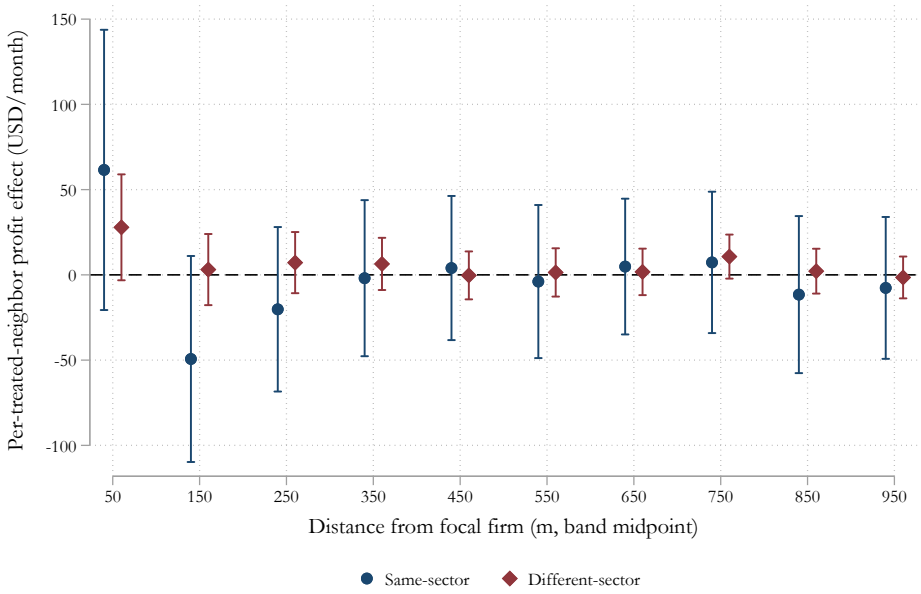
Figure A.3: Distribution of Household Income by Group



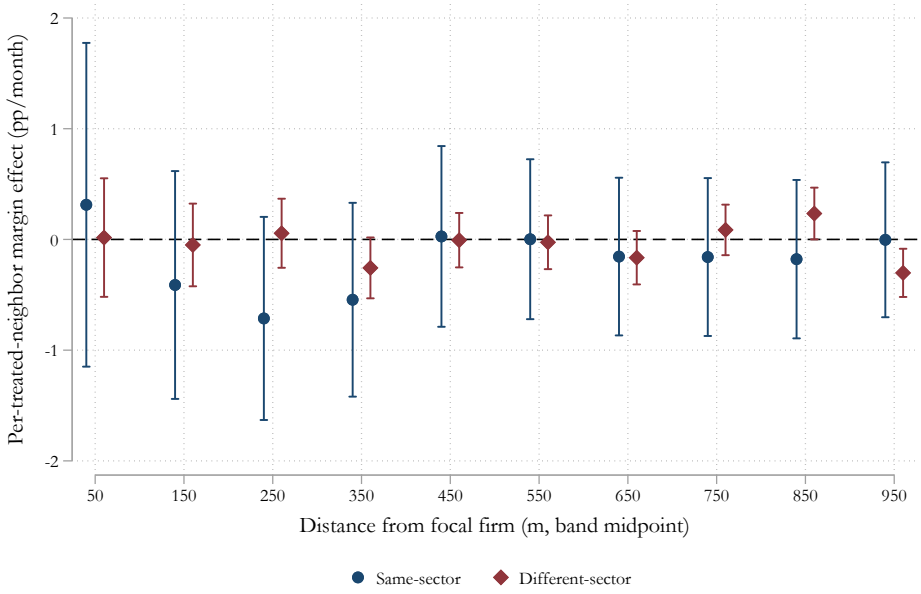
Notes: The figure plots the distribution of monthly household income for three groups: customers of sample businesses (from customer surveys), all borrowers from our partner MFI, and sample participants at baseline. Income for all partner-MFI borrowers comes from self-reported loan-application data. For customers, the distribution has a 25th percentile of USD 750, median of USD 1,250, 75th percentile of USD 1,875, and mean of USD 1,488. For all partner-MFI borrowers, the corresponding values are USD 613, USD 1,000, USD 1,550, and USD 1,157. For the experimental sample, they are USD 2,219, USD 3,125, USD 4,375, and USD 3,472.

Figure A.4: Per-Treated-Neighbor Spillovers by 100-Meter Distance Band

(a) Monthly Profits (USD)



(b) Margin × 100



Notes: Panel (a) plots effects on monthly profits, in USD, and Panel (b) plots effects on gross margin, defined as profit divided by revenue and multiplied by 100. Each marker reports the coefficient on the inverse-probability-weighted number of treated experimental neighbors in one of ten 100-meter-wide concentric bands (0–100 m, 100–200 m, . . . , 900 m–1 km); its horizontal position is the midpoint of the corresponding band. Coefficients are reported separately for same-sector neighbors (navy circles) and different-sector neighbors (maroon diamonds), based on the 14-category sector classification. Thus, each coefficient is the estimated effect of one additional treated experimental neighbor in the indicated distance band and sector channel. Each regression includes density controls for the total number of randomized experimental neighbors, treated or control, in every distance-band–sector-channel cell, as well as firm and comuna × month–year fixed effects. Whiskers denote 95% confidence intervals constructed using standard errors clustered at the spillover-firm level. The sample comprises partner-MFI borrowers outside the experimental sample observed monthly from January 2023 through December 2025. Profits and margins are independently trimmed at the 1st and 99th percentiles.

B Randomization Process

Applicants deemed eligible for the large loan were randomized in 13 rounds based on when they submitted their applications. Recall that all borrowers eligible for the larger loan must also have had a standard joint-liability loan from our partner lender and thus already had an assigned loan officer.

In rounds 1–4, 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, no more than two treated borrowers could be assigned to that officer, reserving the third treatment slot for subsequent rounds. In round 5, the cap increased to five treated borrowers per loan officer. In rounds 6–13, the restriction was removed entirely.

Within each randomization-round $\times$ loan-officer stratum, borrowers were randomly assigned to treatment or control with uniform probability, subject to the capacity constraints described above.

C Formal Model and Proofs

This appendix formalizes the framework in Section 3. The main text presents the intuition in terms of residual consumer surplus, business stealing, and the role of stable outside options. Here we provide a simple model that delivers the welfare accounting identity and the two formal results underlying Proposition 1 in the main text.

C.1 Environment

There is a finite set of households $j \in \{1, \ldots, J\}$ and a finite set of firms $i \in \mathcal{B}$. Each household consumes a numeraire good $x_j \geq 0$ and can purchase at most one unit from one firm. Let

$$a(j) \in \mathcal{B} \cup \{\emptyset\}$$

denote household j's choice, where $a(j) = \emptyset$ means that the household purchases nothing.

If household j buys from firm i, it receives gross utility v_{ij} and pays price p_i. Utility is quasi-linear:

$$U_j = x_j + \mathbf{1}\{a(j) \in \mathcal{B}\} v_{a(j)j}.$$

Household j has endowment income y_j and owns shares in firms. Let $\theta_{ji} \in [0, 1]$ denote household j's ownership share in firm i, with

$$\sum_{j=1}^{J} \theta_{ji} = 1 \quad \text{for every firm } i \in \mathcal{B}.$$

The household budget constraint is

$$x_j + \mathbf{1}\{a(j) \in \mathcal{B}\} p_{a(j)} = y_j + \sum_{i \in \mathcal{B}} \theta_{ji} \pi_i.^{16}$$

[16] Here, we assume that households are small, so household j's purchase of good $a(j)$ does not relax its own budget constraint through its effect on firm $a(j)$'s profits.

Define net utility from buying from firm i as

$$u_{ij} \equiv v_{ij} - p_i,$$

and define the value of purchasing nothing as

$$u_{\emptyset j} \equiv 0.$$

Substituting the budget constraint into household utility, the terms $y_j + \sum_i \theta_{ji}\pi_i$ are common across purchase options. Household j therefore chooses an option that maximizes net utility:

$$a(j) \in \arg\max_{i \in \mathcal{B} \cup \{\emptyset\}} u_{ij}.$$

Ties are broken according to an arbitrary fixed rule.

Let q_i denote the quantity sold by firm i:

$$q_i = \sum_{j=1}^{J} \mathbf{1}\{a(j) = i\}.$$

Firm i's profits are π_i. These profits may reflect revenues, variable costs, and fixed costs associated with any investment undertaken by the firm. The exact profit function is not important for the consumer-surplus results below; the welfare accounting identity only requires that firm profits accrue to households through the ownership shares θ_{ji}.

We compare two economies. In the unprimed economy $\mathcal{E}$, the firms in $T \subset \mathcal{B}$ do not receive the RCT loan. In the primed economy $\mathcal{E}'$, the firms in T receive the RCT loan. Firms outside T do not receive the loan, but their outcomes may nevertheless differ across the two economies if they respond to treated firms. We therefore allow prices, gross

utilities, quantities, and profits to carry primes for all firms:

$$v'_{ij}, \quad p'_i, \quad u'_{ij} \equiv v'_{ij} - p'_i, \quad q'_i, \quad \pi'_i.$$

The outside option of purchasing nothing is unchanged, so $u'_{\emptyset j} = u_{\emptyset j} = 0$. We assume that firms $i \in T$ receiving the loan in economy $\mathcal{E}'$ have $u'_{ij} \geq u_{ij}$ for every customer j.[17]

The main identifying condition in Section 3 is stable outside options.

Assumption 2 (Stable Outside Options)**.** *For every firm $k \notin T$ that does not receive the RCT loan and every customer j, the intervention does not change the net utility offered by firm k:*

$$u'_{kj} = u_{kj}.$$

Equivalently,

$$v'_{kj} - p'_k = v_{kj} - p_k.$$

This is the same condition stated as Assumption 1 in the main text. It allows non-treated firms' quantities and profits to change because customers reallocate across firms. It rules out only changes in the net utility that non-treated firms offer to customers.

C.2 Consumer Surplus and Residual Consumer Surplus

Total consumer surplus in economy $\mathcal{E}$ is

$$CS = \sum_{j=1}^{J} \max_{i \in \mathcal{B} \cup \{\emptyset\}} u_{ij}.$$

[17] We abstract from the financial-intermediation side of the intervention. Because utilities are quasilinear, net payments under the loan contract are transfers between borrowers and the lender and therefore cancel in the aggregate; we consequently do not model these financial flows separately.

Equivalently, since households choose the option that maximizes net utility,

$$CS = \sum_{j=1}^{J} \mathbf{1}\{a(j) \in \mathcal{B}\} \left(v_{a(j)j} - p_{a(j)}\right).$$

Define CS' analogously in economy $\mathcal{E}'$, and let

$$\Delta CS \equiv CS' - CS.$$

For any firm i and household j, define the value of household j's next-best alternative to purchasing from firm i as

$$Alt_{ij} \equiv \max_{\ell \in \mathcal{B} \cup \{\emptyset\}, \ell \neq i} u_{\ell j}.$$

The maximum willingness to pay for firm i's product, relative to this next-best alternative, is

$$WTP_{ij} = v_{ij} - Alt_{ij}.$$

Residual consumer surplus from buying from firm i is therefore

$$RCS_{ij} \equiv WTP_{ij} - p_i = u_{ij} - Alt_{ij}.$$

This is the surplus generated by firm i for household j over and above the household's next-best alternative.

Let

$$C_i \equiv \{j : a(j) = i\}$$

denote the set of customers who buy from firm i in economy $\mathcal{E}$, and define C_i' analogously in economy $\mathcal{E}'$. Total residual consumer surplus generated by firm i in economy $\mathcal{E}$ is

$$RCS_i = \sum_{j \in C_i} RCS_{ij} = \sum_{j \in C_i} \left(u_{ij} - Alt_{ij}\right),$$

with RCS_i' defined analogously in economy $\mathcal{E}'$.

C.3 Welfare Accounting

Proposition 2 (Welfare Decomposition)**.** *Total welfare can be written as*

$$W = \sum_{j=1}^{J} y_j + CS + \sum_{i \in \mathcal{B}} \pi_i.$$

Therefore,

$$\Delta W = \Delta CS + \sum_{i \in T} (\pi_i' - \pi_i) + \sum_{i \notin T} (\pi_i' - \pi_i).$$

Proof. Summing household budget constraints gives

$$\sum_{j=1}^{J} x_j + \sum_{j=1}^{J} \mathbf{1}\{a(j) \in \mathcal{B}\} p_{a(j)} = \sum_{j=1}^{J} y_j + \sum_{j=1}^{J} \sum_{i \in \mathcal{B}} \theta_{ji} \pi_i.$$

Since $\sum_j \theta_{ji} = 1$ for every firm i, this becomes

$$\sum_{j=1}^{J} x_j + \sum_{i \in \mathcal{B}} p_i q_i = \sum_{j=1}^{J} y_j + \sum_{i \in \mathcal{B}} \pi_i.$$

By definition,

$$W = \sum_{j=1}^{J} x_j + \sum_{j=1}^{J} \mathbf{1}\{a(j) \in \mathcal{B}\} v_{a(j)j}.$$

Using

$$\sum_{j=1}^{J} \mathbf{1}\{a(j) \in \mathcal{B}\} p_{a(j)} = \sum_{i \in \mathcal{B}} p_i q_i,$$

substitute the budget identity into welfare:

$$\begin{aligned} W &= \sum_{j=1}^{J} y_j + \sum_{i \in \mathcal{B}} \pi_i - \sum_{j=1}^{J} \mathbf{1}\{a(j) \in \mathcal{B}\} p_{a(j)} + \sum_{j=1}^{J} \mathbf{1}\{a(j) \in \mathcal{B}\} v_{a(j)j} \\ &= \sum_{j=1}^{J} y_j + \sum_{i \in \mathcal{B}} \pi_i + \sum_{j=1}^{J} \mathbf{1}\{a(j) \in \mathcal{B}\} \left(v_{a(j)j} - p_{a(j)}\right) \\ &= \sum_{j=1}^{J} y_j + \sum_{i \in \mathcal{B}} \pi_i + CS. \end{aligned}$$

Taking differences between $\mathcal{E}'$ and $\mathcal{E}$ gives

$$\Delta W = \Delta CS + \sum_{i \in \mathcal{B}} (\pi_i' - \pi_i) = \Delta CS + \sum_{i \in T} (\pi_i' - \pi_i) + \sum_{i \notin T} (\pi_i' - \pi_i).$$

□

C.4 Residual Surplus with One Treated Sample Firm

Proposition 3 (Exact Identification with One Treated Sample Firm)**.** *Suppose $T = \{s\}$ and Assumption 2 holds. Then*

$$RCS_s' - RCS_s = \Delta CS.$$

Proof. Fix household j. Let

$$b_j \equiv \max_{\ell \in \mathcal{B} \cup \{\emptyset\}, \ell \neq s} u_{\ell j}$$

denote the value of the best alternative to firm s in economy $\mathcal{E}$. Since s is the only treated sample firm and Assumption 2 holds, every alternative to s is unchanged in economy $\mathcal{E}'$. Thus the value of the best alternative to s in economy $\mathcal{E}'$ is also b_j.

Household j's contribution to residual consumer surplus at firm s in economy $\mathcal{E}$ is

$$\mathbf{1}\{a(j) = s\}(u_{sj} - b_j).$$

Because household j chooses s only when $u_{sj} \geq b_j$, this contribution can be written as

$$(u_{sj} - b_j)_+,$$

where $(z)_+ \equiv \max\{z, 0\}$. Similarly, household j's contribution to residual consumer surplus at firm s in economy $\mathcal{E}'$ is

$$(u'_{sj} - b_j)_+.$$

Thus household j's contribution to $RCS'_s - RCS_s$ is

$$(u'_{sj} - b_j)_+ - (u_{sj} - b_j)_+.$$

Household j's consumer surplus in economy $\mathcal{E}$ is

$$\max\{u_{sj}, b_j\} = b_j + (u_{sj} - b_j)_+,$$

and household j's consumer surplus in economy $\mathcal{E}'$ is

$$\max\{u'_{sj}, b_j\} = b_j + (u'_{sj} - b_j)_+.$$

Therefore household j's change in consumer surplus is also

$$(u'_{sj} - b_j)_+ - (u_{sj} - b_j)_+.$$

The equality holds household by household. Summing over households gives

$$RCS'_s - RCS_s = \Delta CS.$$

□

This result formalizes the business-stealing intuition in the main text. If a household would have purchased from another firm absent the treatment but purchases from s after treatment, then b_j is the household's counterfactual consumer surplus. The household's measured residual surplus at s is $u'_{sj} - b_j$, which is exactly the household's gain. If $u'_{sj} = b_j$, the household is merely reallocated across firms and contributes zero residual consumer surplus.

C.5 Residual Surplus as a Lower Bound

The exact equality above need not hold when several sample firms receive the loan at the same time. A customer of one treated sample firm may have another treated sample firm as the next-best alternative. In that case, residual surplus at the chosen treated firm nets out some of the value created by the intervention, because the outside option may itself have improved. The same conservative logic applies if non-treated firms respond by lowering prices or improving quality.

Proposition 4 (Residual Surplus as a Lower Bound)**.** *Suppose that non-treated firms weakly improve rather than deteriorate,*

$$u'_{kj} \geq u_{kj} \quad \textit{for every } k \notin T \textit{ and every } j.$$

Then

$$\sum_{i \in T} (RCS'_i - RCS_i) \leq \Delta CS.$$

Proof. The assumption implies that every market option weakly improves between economies $\mathcal{E}$ and $\mathcal{E}'$:

$$u'_{ij} \geq u_{ij} \quad \text{for every } i \in \mathcal{B} \text{ and every } j.$$

The outside option of purchasing nothing is unchanged, $u'_{\emptyset j} = u_{\emptyset j} = 0$.

Fix household j. We compare household j's contribution to

$$\sum_{i \in T} (RCS'_i - RCS_i)$$

with its contribution to ΔCS. There are four cases.

Case 1: household j buys from a treated sample firm in both economies. Let $a(j) = h \in T$ and $a'(j) = i \in T$. The household's contribution to the change in residual consumer surplus is

$$\left(u'_{ij} - Alt'_{ij}\right) - \left(u_{hj} - Alt_{hj}\right).$$

We claim that

$$Alt'_{ij} \geq Alt_{hj}.$$

Let k attain Alt_{hj} in economy $\mathcal{E}$. If $k \neq i$, then k is available in the maximization defining Alt'_{ij}. Since every option weakly improves,

$$Alt'_{ij} \geq u'_{kj} \geq u_{kj} = Alt_{hj}.$$

If instead $k = i$, then $Alt_{hj} = u_{ij}$. Since household j chose h in economy $\mathcal{E}$, $u_{hj} \geq u_{ij}$. Firm h is available in the maximization defining Alt'_{ij}, so weak improvement implies

$$Alt'_{ij} \geq u'_{hj} \geq u_{hj} \geq u_{ij} = Alt_{hj}.$$

Thus $Alt'_{ij} \geq Alt_{hj}$. Therefore

$$\left(u'_{ij} - Alt'_{ij}\right) - \left(u_{hj} - Alt_{hj}\right) \leq u'_{ij} - u_{hj}.$$

The right-hand side is household j's change in consumer surplus in this case.

Case 2: household j buys from a treated sample firm in economy $\mathcal{E}'$, but not in economy $\mathcal{E}$. Let $a'(j) = i \in T$ and $a(j) \notin T$. The household's contribution to the change in residual consumer surplus is

$$u'_{ij} - Alt'_{ij}.$$

The option $a(j)$ is available as an alternative to i in economy $\mathcal{E}'$. If $a(j) = \emptyset$, its value is zero in both economies. If $a(j) \in \mathcal{B} \setminus T$, weak improvement implies $u'_{a(j)j} \geq u_{a(j)j}$. Hence

$$Alt'_{ij} \geq u_{a(j)j}.$$

Therefore

$$u'_{ij} - Alt'_{ij} \leq u'_{ij} - u_{a(j)j},$$

which is household j's change in consumer surplus.

Case 3: household j buys from a treated sample firm in economy $\mathcal{E}$, but not in economy $\mathcal{E}'$. Let $a(j) = h \in T$ and $a'(j) \notin T$. The household's contribution to the change in residual consumer surplus is

$$-\left(u_{hj} - Alt_{hj}\right) \leq 0.$$

Because every option weakly improves, household j's realized consumer surplus cannot fall between $\mathcal{E}$ and $\mathcal{E}'$. Thus household j's contribution to ΔCS is weakly positive, and the residual-surplus contribution is no greater than the consumer-surplus contribution.

Case 4: household j does not buy from a treated sample firm in either economy. The household contributes zero to the change in residual consumer surplus generated by

treated sample firms. Since every option weakly improves, the household's contribution to ΔCS is weakly positive.

In every case, household j's contribution to the change in residual consumer surplus generated by treated sample firms is no greater than its contribution to ΔCS. Summing over households gives

$$\sum_{i \in T} (RCS_i' - RCS_i) \leq \Delta CS.$$

□

Proposition 4 covers the conservative cases emphasized in the main text. First, even if non-treated firms' net utilities are unchanged, the inequality can be strict when several sample firms are treated simultaneously, because one treated sample firm may enter another treated sample firm's outside-option set. Second, if non-treated firms respond to treated firms by lowering prices or improving quality, then customers' outside options improve. This both lowers measured residual surplus at treated firms and creates gains for customers who continue shopping at non-treated firms. Both forces make the survey-based residual-surplus estimate conservative. By contrast, if non-treated firms raise prices, reduce quality, or exit, outside options deteriorate and the lower-bound result need not hold.

Proposition 1 in the main text follows directly from Propositions 3 and 4. The first part of Proposition 1, which considers the case $T = \{s\}$ under stable outside options, is exactly Proposition 3. The second part follows from Proposition 4: when several sample firms receive the RCT loan, if treated sample firms weakly improve and non-treated firms weakly improve rather than deteriorate, then the change in residual consumer surplus generated by treated sample firms is a lower bound on the change in total consumer surplus.

D Estimating Lender Coverage of Potential Spillover Firms

To assess how much of the potential spillover-firm universe is represented in our lender's administrative data, we construct a comuna-level estimate of the number of informal microenterprises operating in the broad sectors most relevant to our sample. The object is not the total number of firms in Chile but a proxy for the number of nearby small, informal enterprises that could plausibly compete with the firms in our experiment. We therefore focus on informal microentrepreneurs in commerce, manufacturing, and "other" sectors. This restriction retains the broad sector groups that map most closely to the retail, service, and manufacturing firms in the experiment. Because the EME "commerce" category is broader than retail and its "other" category includes but is broader than services, our analysis may yield a conservative proxy for the relevant universe of informal microenterprises.

We use three inputs. First, we use the lender's administrative count of borrowers by comuna, denoted B_c. Second, we use anonymized respondent-level records from the public-use files of the eighth *Encuesta de Microemprendimiento* (EME), conducted by Chile's Instituto Nacional de Estadísticas (INE) and Ministry of Economy.[18] The EME's target population consists of own-account workers and employers who own a microenterprise with up to ten workers, including the owner. Third, we use comuna-by-age population data from the 2024 Census to allocate regional EME totals to comunas because the EME does not provide comuna-level estimates.[19]

The calculation proceeds in two steps. First, using the EME microdata, we estimate

[18]The eighth EME's documentation and public-use data are available from INE and the Ministry of Economy. See, for example, the EME user manual and methodological documentation: `https://www.economia.gob.cl/wp-content/uploads/2025/12/manual-de-usuario-de-bbdd-eme-8.pdf` and `https://www.economia.gob.cl/wp-content/uploads/2025/12/diseno-muestral-eme-8.pdf`.

[19]The 2024 Census results portal provides downloadable comuna-level population tables: `https://censo2024.ine.gob.cl/resultados/`.

the number of informal microenterprises in commerce, manufacturing, and "other" in each region. Let i index EME respondents, w_i denote the EME expansion factor, and $r(i)$ denote the respondent's region. We define

$$M_r = \sum_i w_i \mathbf{1}\{r(i) = r\} \mathbf{1}\{\texttt{num_micro}_i = 1\} \mathbf{1}\{\texttt{informalidad}_i = 1\} \mathbf{1}\{\texttt{c1_caenes_1d_red}_i \in \{2, 4, 6\}\}.$$

The sector codes $\{2, 4, 6\}$ correspond to manufacturing, commerce, and other sectors. We use the EME expansion factor `factor_eme`. Summing M_r across regions yields a national total of 739,019 informal microentrepreneurs in manufacturing, commerce, and other sectors.

Second, we allocate each regional total M_r to comunas using each comuna's share of the region's adult population. Let A_c be the 2024 Census population age 15 or older in comuna c, computed by summing all five-year age groups except 0–4, 5–9, and 10–14. Let

$$A_r = \sum_{c \in r} A_c$$

be the corresponding adult population in region r. For each comuna c in region r, our estimated number of informal commerce/manufacturing/other microenterprises is

$$\widehat{M_c} = M_r \times \frac{A_c}{A_r}.$$

This allocation allows the total number of relevant microenterprises to vary across regions according to the EME while assuming that, within a region, the density of relevant informal microenterprises is proportional to the adult population.

We then merge $\widehat{M_c}$ with the lender's borrower count, B_c, by comuna name. Define comuna-level lender penetration as

$$\widehat{p_c} = \frac{B_c}{\widehat{M_c}}.$$

To construct the penetration rate relevant to the spillover bound in Section 6, let O_c denote the number of observed experimental-firm–spillover-firm links in which the firms are in the same sector and within 500 meters of one another and the spillover firm is located in comuna c. A spillover firm near multiple experimental firms contributes one link for each pairing. We assume that the average loss per exposed link, ℓ, is constant across comunas and the same for lender and nonlender firms. Under these assumptions, $O_c/\widehat{p}_c$ estimates the total number of relevant links in comuna c, and the effective penetration rate is

$$\widehat{p}_{\text{eff}} = \frac{\sum_c O_c}{\sum_c O_c/\widehat{p}_c}.$$

Equivalently, this is the ratio of observed aggregate losses, $\ell \sum_c O_c$, to estimated aggregate losses among all relevant firms, $\ell \sum_c O_c/\widehat{p}_c$. In our data, $\widehat{p}_{\text{eff}} = 0.269$, or 26.9 percent. We therefore scale observed aggregate spillover losses by $1/\widehat{p}_{\text{eff}}$.

E Sensitivity of the Consumer-Surplus Estimate to Attrition

As shown in Table A.2, customers from approximately 48% of firms are represented in at least one customer-survey wave. Under the assumption that treatment does not affect which firms enter the customer-data sample, random assignment identifies a valid treatment effect among the firms represented in that sample. Table A.3 finds little evidence that treatment affected either the probability or composition of selection into the customer-data sample, providing support for this assumption.

However, we cannot identify the treatment effect for the full experimental sample, because the average effect among firms without customer data remains unknown. We therefore take the respondent-sample effect as our starting point and parameterize how much smaller the average treatment effect among firms without consumer-surplus data would have to be to overturn our full-sample conclusions.

E.1 Stable Surveyability and Treatment-Effect Decomposition

For firm i, let $Y_i(t)$ denote total weekly consumer surplus under assignment $t \in \{0, 1\}$, and let $R_i(t)$ indicate whether the firm would enter the consumer-surplus analysis under assignment t.

Assumption 3 (Stable Surveyability)**.** *For every firm i,*

$$R_i(1) = R_i(0).$$

Assumption 3 states that there is no differential selection by treatment into the sample of firms for which we have consumer-surplus data. The similar coverage rates and composition of observed firms in Tables A.2 and A.3 provide support for this assumption.

Under Assumption 3, write the common surveyability indicator as R_i and define

$$\tau_R \equiv \mathbb{E}[Y_i(1) - Y_i(0) \mid R_i = 1], \qquad \tau_M \equiv \mathbb{E}[Y_i(1) - Y_i(0) \mid R_i = 0].$$

Let $\pi \equiv \Pr(R_i = 1)$. The treatment effect in the full experimental sample is then

$$\tau = \pi\tau_R + (1 - \pi)\tau_M.$$

Define the responder–nonresponder treatment-effect gap as

$$\Delta \equiv \tau_R - \tau_M.$$

Thus, $\Delta = 0$ corresponds to equal average treatment effects, while $\Delta > 0$ allows the effect to be smaller among firms without consumer-surplus data. Substituting $\tau_M = \tau_R - \Delta$ gives

$$\tau(\Delta) = \tau_R - (1 - \pi)\Delta,$$

with estimator

$$\widehat{\tau}(\Delta) = \widehat{\tau}_R - (1 - \widehat{\pi})\Delta.$$

Column 5 of Panel B in Table 3 reports $\widehat{\tau}_R = 123.49$, with $\mathrm{SE}(\widehat{\tau}_R) = 56.09$. The primary outcome is available for 246 of the 541 randomized firms, so $\widehat{\pi} = 246/541 = 0.455$.[20] Therefore,

$$\widehat{\tau}(\Delta) = 123.49 - 0.545\Delta.$$

For each fixed value of Δ, this is an affine transformation of $\widehat{\tau}_R$ with unit slope.

[20]The 48% figure in Table A.2 refers to firms for which at least one customer contributed data. The slightly smaller share here reflects the 246 firms for which all components of total weekly consumer surplus are available.

Treating the realized sample share $\widehat{\pi}$ as fixed,

$$\mathrm{SE}[\widehat{\tau}(\Delta)] = \mathrm{SE}(\widehat{\tau}_R) = 56.09.$$

E.2 Breakdown Values

The estimated full-sample effect reaches zero when

$$\Delta^{\text{zero}} = \frac{\widehat{\tau}_R}{1-\widehat{\pi}} = \frac{123.49}{295/541} = 226.47.$$

At this value, the implied treatment effect among firms without consumer-surplus data is

$$\widehat{\tau}_M = 123.49 - 226.47 = -102.98.$$

Thus, overturning the sign of the point estimate requires the treatment effect to change from a gain of approximately USD 123 per week among observed firms to a loss of approximately USD 103 per week among unobserved firms.

For a conventional two-sided test at significance level α, statistical significance is lost when

$$\Delta^{\text{sig}}_{\alpha} = \frac{\widehat{\tau}_R - z_{1-\alpha/2}\,\mathrm{SE}(\widehat{\tau}_R)}{1-\widehat{\pi}}.$$

Using standard-normal critical values gives

$$\Delta^{\text{sig}}_{0.10} = \frac{123.49 - 1.645(56.09)}{295/541} = 57.27$$

at the 10% level and

$$\Delta^{\text{sig}}_{0.05} = \frac{123.49 - 1.960(56.09)}{295/541} = 24.86$$

at the 5% level. These correspond to treatment effects among unobserved firms of

USD 66.22 and USD 98.63 per week, respectively. Table A.13 summarizes the results.

Eliminating the positive point estimate therefore requires a large reversal of the treatment effect among firms without customer-surplus data. Statistical significance at the 5% level would be preserved if the treatment effect among unrepresented firms were about 20% smaller than that among represented firms; significance at the 10% level would be preserved if the effect among unrepresented firms were as much as 47% smaller.