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ABSTRACT

We created experimental variation across local markets in China in the share of firms having access to a new loan product, to measure the direct and indirect effects of access to finance. We find that:(1) Access to finance had a large positive direct effect on the performance of treated firms. (2) Access to finance had a similar-sized negative indirect effect on the performance of firms with treated competitors. The two effects offset in the aggregate and imply no detectable gains in producer surplus. (3) Access to finance had a positive direct effect on business practices, service quality, and consumer satisfaction, and a negative effect on price. None of these effects were offset by indirect effects, suggesting net gains in consumer surplus. (4) Two additional indirect effects were active: diffusion of borrowing to firms with treated peers, and diffusion of demand to firms with treated neighbors. (5) Combining several effects in a model-based evaluation, we estimate that the loan had a private return of 74%, most of which was offset by losses to competitors, and a social return of 60%, most of which was driven by gains to consumers.

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1 Introduction

Lack of credit to firms is widely believed to be a growth barrier, and a growing literature documents that credit has positive effects on borrowing firms.[1] But in industry equilibrium, credit can have indirect effects on other actors: on peer firms through business stealing, information diffusion, and other channels; and on consumers through improvements in borrower firms' quality and price. Such industry equilibrium effects can be important for welfare and redistribution, but at present we have limited evidence on their nature and magnitude. Rotemberg (2019) documents a negative indirect effect on peer firms of a subsidy policy in India, and McKenzie and Puerto (2021) document no indirect effect on peer firms of a microenterprise training experiment in Kenya. But we know little about the various indirect effects of credit, on both peer firms and consumers, and about the underlying mechanisms.

To make progress understanding these issues, we conduct a field experiment with 3,173 firms in China, in which we create variation across local markets in the share of firms having access to a new loan product. This design allows us to identify both direct and indirect effects. We show that the loan had a large direct effect on the revenue and profit of treated firms, which however was essentially cancelled by a similar-sized business-stealing effect on competitors, implying that the program had no detectable effect on producer surplus. At the same time, we show that treated firms improved quality and reduced price, and that consumers valued these changes, highlighting the mechanism behind the reallocation and implying that the program had a large positive effect on consumer surplus. We document additional indirect effects driven by the diffusion of information and the diffusion of demand. Combining several effects in a welfare analysis demonstrates the importance of indirect effects for industry outcomes and policy evaluation.

In Section 2 we present our context and experimental design. In 2013, a large bank introduced a new loan product to small and medium enterprises (SMEs) in Jiangxi, China. The loan product was offered in local "markets"—clusters of retail and service firms—and provided better terms than existing alternatives, primarily in that it did not require collateral. In collaboration with the bank, in the summer of 2013 we introduced a randomized treatment to improve access to the new loan

[1] We review the literatures on credit access and indirect effects in more detail below.

product: we had a loan officer visit every treated firm once a month for a year, who explained the terms of the loan and offered help filling the application.

We randomized treatment intensity across 78 markets: in 37 markets we treated 80% of the firms, in 10 markets we treated 50% of the firms, and in 31 markets we treated no firms. This design generated random variation in both the firm's treatment status and the share of its peers that were treated. We complemented the randomized intervention with surveys of our sample of 3,173 firms (average employment 9). We conducted long surveys in 2013 summer, before the intervention (baseline), in 2015 summer, two years after the intervention (midline), and in 2016 summer, one year later (endline). We also conducted a short follow-up survey in 2020 summer to collect data on prices, consumer satisfaction and other outcomes we did not include in our long surveys.

We begin our analysis with three basic facts about the raw data that motivate our model and empirical strategy. First, by endline 28 percentage points more treated than untreated firms borrowed using the new loan product, suggesting that the treatment was successful. Second, the randomized variation in the share of peers treated had a positive effect on the borrowing of untreated firms. This result suggests an indirect effect on borrowing driven by information diffusion. Third, untreated firms in treated markets, relative to pure control firms, have a distribution of sales growth shifted to the left. This result suggests an indirect effect on firm performance driven by business stealing.

Motivated by these facts, in Section 3 we present a simple model of the impacts of borrowing that incorporates both information diffusion and business stealing effects. In the model firms compete monopolistically in local markets. To capture that the loan enables productive investments, we assume that borrowing leads to higher product quality and lower marginal cost. We allow information about the loan to diffuse to untreated firms, and explicitly model the decision to borrow. The main prediction of the model is that the impact of the loan program on (log) revenue can be approximated with a linear function that (i) depends positively on the borrowing status of the firm, representing a direct effect; and (ii) depends negatively on the share of the firm's competitors that borrow, representing a business stealing effect. Information diffusion affects performance only through borrowing and hence does not enter the equation. Because borrowing is endogenous, this equation cannot be taken to the data directly, but we show that it can be estimated—even in the presence of information diffusion—with an instrumental variables (IV) strategy in which the treatment and the share of competitors treated are the instruments. The reduced form of this IV motivates our main empirical specification, a firm fixed effects regression in which the key explanatory variables are the firm's treatment status and the share of the firm's competitors which are treated (both interacted with an indicator for periods after the treatment).

In Section 4 we present our reduced form empirical results. We begin by looking at impacts on main performance measures. When the outcome is log sales, we estimate a significant direct effect of the treatment of about 10 log points, and a significant indirect effect of the share of competitors treated of about -9 log points. The approximately equal magnitude of these coefficients suggests essentially zero impact on market-level revenue, and indicates that the treatment induced a reallocation from firms having treated competitors to firms having the treatment. Similarly, we find a large positive direct and a similar-sized negative indirect effect on profit, suggesting essentially zero impact on producer surplus. And, turning to factors and inputs, we find a similar reallocation for employment and the wage bill, a smaller and less significant reallocation for materials, while—consistent with these firms having low capital intensity—no effect on fixed assets. Overall, the results on main outcomes are consistent with the model's logic of business stealing.

To explore the mechanisms underlying these effects, we turn to intermediate outcomes. We first show that the treatment had a positive direct and negative indirect effect on the number of clients, supporting the demand reallocation interpretation. We then study business practices the loan may have facilitated. We begin with practices that may improve product or service quality: renovation, the introduction of new products, and the share of workers with a high-school degree. For all three we estimate a large and significant direct effect, and a small and less significant indirect effect. We then consider practices that may reduce cost: switching to a new supplier, the stocking period—the average time between restocking, positively related to the amount and hence the supplier's discount—and the use of inventory management techniques. For all three we estimate a large and significant direct effect and an insignificant indirect effect. Finally, we estimate null effects on other borrowing, confirming that firms were genuinely credit constrained (Banerjee and

Duflo 2014). Overall, the results show that firms used the loans to improve quality and reduce cost.

To investigate whether the changes in practices improved consumer satisfaction, we turn to measures of consumer experience. In our 2020 follow-up survey we collected data on price, and on various dimensions of consumer experience measured with the evaluation of a randomly chosen customer. For price we estimate a negative direct effect and no indirect effect. For each dimension of consumer experience, and for an index of overall consumer satisfaction, we estimate a positive direct effect, and a much smaller and less significant indirect effect. We conclude that consumers valued the improved practices enabled by the loan, highlighting a plausible mechanism behind the reallocation.

Our analysis thus far assumed that indirect effects affect all peers with equal intensity. We next explore the heterogeneity of these effects by distance and competition status. First we consider indirect effects on borrowing. We show that the diffusion of borrowing documented above is coming from similar peers not directly competing with the firm: neighboring non-competitors and distant competitors. There is no diffusion from neighboring competitors, suggesting that they withhold information to avoid business stealing effects (Cai and Szeidl 2018, Hardy and McCasland 2021) and highlighting the role of senders' incentives in technology adoption. Next we consider indirect effects on firm performance. Our main result is that treating non-competitor neighbors has a *positive* effect. We present various pieces of evidence that this effect is not explained by the diffusion of borrowing, and conclude that it represents a novel indirect effect: the diffusion of demand from treated stores to non-competitor neighbors, plausibly driven by customers shopping around in the neighborhood. This result provides experimental evidence on a demand externality that may drive the spatial concentration of retail observed around the world as well as in our markets (Leonardi and Moretti 2022).

In Section 5 we combine the direct and various indirect effects. We first estimate the IV regression implied by the model to infer the direct and business stealing effects of the loan, and find, paralleling the reduced-form results, large, significant and nearly offsetting effects. We then use these estimates to evaluate the overall impact of the loan program on welfare and redistribution. Our model implies that we can infer the gain in consumer surplus from the direct effect of the loan on firm revenue, which measures the extent of reallocation, and the elasticity of substitution σ in the market, which governs the welfare gain from a given reallocation. Using a conservative $\sigma = 6$ borrowed from the literature, we find that treating all firms in a market would result in welfare gains of about \$15,000 per firm, or about 17% of profits, largely driven by gains to consumers. Our estimates imply that the private return to capital was about 74% per year, most of which was cancelled by losses to competitors; and the social return to capital was about 60%, most of which was driven by gains to consumers.

A key implication of these results is that accounting for—potentially multiple—indirect effects can be essential for evaluating firm policies. In our setting, the high social return implies that the loan program generated large welfare gains; but only accounting for the direct and indirect effects on firms, while ignoring the effect on consumers, would imply insignificant and small welfare gains. Although our returns may seem high, they are comparable to the private returns of 55-63% found by De Mel, McKenzie and Woodruff (2008) and 105% found by Banerjee and Duflo (2014), and in fact our analysis may help understand why private returns in developing countries are high: these returns depend on the (quality-adjusted) productivity gain enabled by the loan *times* the potential for business stealing, implying that even moderate productivity gains can generate high returns.

In the concluding Section 6 we discuss the external validity and some implications of our analysis. We note that spatially concentrated retail clusters similar to our markets are common worldwide (Leonardi and Moretti 2022). We also compare our results to existing work on indirect effects, and highlight some differences in the design and context—our accounting for business quality and the consumer surplus (compared to Rotemberg 2019), and the absence of an indirect effect driven by treated firms spending their earnings in the market (compared to McKenzie and Puerto 2021)—which help explain the differences between the results. We conclude that indirect effects on peer firms and consumers are plausibly important across a range of contexts.

Our research contributes to two main literatures. Our analysis of access to finance builds on work cleanly documenting credit constraints in firms, especially De Mel et al. (2008) and McKenzie and Woodruff (2008) who experimentally evaluate the impact of cash grants on microenterprises in Sri Lanka and Mexico, and Banerjee and Duflo (2014) who evaluate the impact of a targeted lending program on mid-sized firms in India.[2] Our contribution to this work is the experimental evidence on SMEs, the study of indirect effects and mechanisms, and the welfare evaluation.

Our analysis of indirect effects builds on work studying indirect and equilibrium effects in different contexts. Duflo and Saez (2003) introduced the idea of experimental variation in treatment intensity, a design that has subsequently been used in many domains.[3] There is less work on indirect effects involving firms. Bloom, Schankerman and Van Reenen (2013b) study the spillover and business stealing effects of R&D in the US with observational data. Drexler, Fischer and Schoar (2014) and Calderon, Cunha and De Giorgi (2020) find suggestive evidence of negative indirect effects of financial and business literacy interventions, but do not have the design or statistical power to investigate them more fully. Most closely related to our work, Rotemberg (2019) documents a negative indirect effect on peer firms of a subsidy policy in India, and McKenzie and Puerto (2021) document no indirect effect on peer firms of a microenterprise training experiment in Kenya. Our contribution to these studies is the focus on credit, the experimental evidence on multiple indirect effects on firms and consumers, and the evidence on mechanisms. Also related are Burke, Bergquist and Miguel (2018), Huber (2018), Breza and Kinnan (2021) and Buera, Kaboski and Shin (2021) who study the general equilibrium implications of credit, and Sraer and Thesmar (2020) who propose a method to evaluate the general equilibrium effects of firm policies. Our contribution to their work is the evidence on industry equilibrium effects affecting peer firms and consumers.[4]

[2] Related work exploits shocks and policy variation to measure the impact of bank loans on firm performance, for example Chodorow-Reich (2014), Ponticelli and Alencar (2016) and Brown and Earle (2017). Also related are studies evaluating the impact of microfinance, including Angelucci, Karlan and Zinman (2015), Attanasio, Augsburg, Haas, Fitzsimons and Harmgart (2015), Augsburg, Haas, Harmgart and Meghir (2015), Banerjee, Duflo, Glennerster and Kinnan (2015), Crepon, Devoto, Duflo and Pariente (2015), Tarozzi, Desai and Johnson (2015).

[3] Examples include financial transfers (Angelucci and Giorgi 2009), labor market policies (Crepon, Duflo, Gurgand, Rathelot and Zamora 2013), the adoption of health products (Guiteras, Levinsohn and Mobarak 2019), and others.

[4] We also build on the literature that uses experiments to study private sector development, including Bloom, Eifert, Mahajan, McKenzie and Roberts (2013a), Bruhn, Karlan and Schoar (2018), Atkin, Khandelwal and Osman (2017a), Atkin, Chaudhry, Chaudry, Khandelwal and Verhoogen (2017b), McKenzie (2017) and Brooks, Donovan and Johnson (2018).

2 Context, design, data and basic patterns

2.1 Context and design

Our experimental site is Jiangxi province, located in Southeastern China. Jiangxi is a rapidly growing region of China, with average annual GDP growth in the past five years of 7.6%, and GDP per capita in 2021 of over 8,000 U.S. dollars.

Our experiment was conducted in one city of Jiangxi province, with firms based in the 78 local "markets" in that city. A market is a government-defined geographic cluster of firms, similar to a mall or a bazaar. Each market specializes in a broad product category, such as building materials; most firms in the market sell products in that broad category, but each firm has a more specialized main product (Table A1 lists the broad product categories of our markets). For example, in the market for building materials, firms may specialize selling bricks, or wooden flooring, or painting materials, or stone, among others. There tend to be several firms selling each specialized main product in the market, so that firms have multiple close competitors. Below we refer to the firm's specialized main product as its industry, and the set of peer firms in the market in the same industry as the firm's competitors. The markets are organized: each market has a market office and a manager, who, among other duties, keeps track of all firms.

Partner bank and loan product. We conducted the intervention in collaboration with our partner, a large bank serving both rural and urban areas in China, which has more than 25,000 branches across the country. In 2013, the bank introduced a new loan product to SMEs in Jiangxi province. An important feature of the product was that it was introduced to firms in local markets of the type just described. The markets were considered useful for the bank, because loan officers could rely on the help of the market manager to reduce asymmetric information about borrowers, reducing the screening and the monitoring costs of lending (Banerjee and Duflo 2010).

The new loan product had several features potentially attractive to borrowers. Most importantly, it did not require collateral, whereas—as our informal conversations with firms suggested—most alternative formal loans required either collateral or a guarantor who was a government employee. In addition, the new loan had a standardized application form and the bank committed to make a decision in two weeks. Finally, the monthly interest rate was about 0.7%, which is at about a 15 percent discount relative to existing formal loans in our baseline survey.

The loan worked as follows. Once a borrower was approved, the bank assigned them a credit limit of up to 30% of the value of net assets as computed by the bank, capped at a maximum loan amount of RMB 500,000 (about USD 81,000). Firms could borrow any amount up to their assigned credit limit. They then had to make monthly interest payments, and repay the loan within two years. Taking out a new loan after repayment was possible.

Intervention. In the summer of 2013 we introduced our treatment. Every treated firm was visited every month for a year by a loan officer who provided information about the new loan product, and if the firm decided to apply, provided help with the application process, including filling in the relevant forms. Once a treated firm decided to borrow, the loan officer stopped visiting. Importantly, after a firm—whether treated or untreated—submitted an application, the bank decided on lending independently of us.

We introduced the treatment using a combination of a market-level and a firm-level randomization. Out of our 78 markets, we randomized 37 to have high treatment intensity, 10 markets to have medium treatment intensity, and 31 markets to pure control.[5] In high treatment intensity markets we treated a randomly selected 80% of firms; in medium intensity markets we treated a randomly selected 50% of firms; and in pure control markets we treated no firms. In the market level randomization, we stratified markets by county, and within each county, by whether the number of firms in the market was above or below the median in the county, resulting in 22 strata.[6] In the firm level randomization we stratified by whether the number of employees of the firm was above or below the median in the market. We report balance tests below for both randomizations.

Surveys. The total number of firms in the 78 markets was over 6,000. This was beyond our capacity to survey, thus we randomly selected half of the firms, in each strata of each market, to be included in our survey sample. This gave us 3,173 firms. We conducted three long surveys with these firms: In 2013 summer, before the intervention, a baseline survey; in 2015 summer, two years

[5] To ensure sufficient statistical power for documenting the direct effect of the treatment, we targeted to have 40% of markets in the pure control, 15% of markets in medium intensity treatment, and 45% of markets in the high intensity treatment.

[6] In China a city can consist of several counties.

after the intervention, a midline survey; and in 2016 summer an endline survey. Since the fiscal year in China ends in June, data in the baseline survey refer to the fiscal year before the intervention. We waited two years between the baseline and the midline surveys to give time for the firms to borrow and use the loan. In addition to these long surveys, we conducted a short follow-up survey in the summer of 2020 in which we collected some additional information not covered in our long surveys.[7]

The surveys were conducted by locally hired enumerators in collaboration with the market office and with the bank. A member of the market office guided us and introduced us to the manager of each firm, and a loan officer from the bank was also present to help ensure that the manager considered us trustworthy. These officials then left, and the survey was conducted, in person, with the manager of the firm. In the long surveys we collected information about the following groups of variables. (1) Firm characteristics. Sales, profits, employment, various cost categories, and other balance-sheet variables. We collected two measures of sales: besides the self-reported value which we asked in all three long surveys, in the midline and endline surveys we also collected the book value directly by having the enumerator ask the firm's accountant or manager to physically show the value in the firm's book. (2) Managerial characteristics including demographics. (3) Measures of financial and business activities: borrowing from formal and informal sources, the use of trade credit, the number of suppliers and clients, measures of product introduction, renovation, advertising, and others. In addition, in the endline survey we also asked borrowers what they primarily used the loan for. (4) Spatial networks: we asked each firm to list the four geographically closest neighbors in the market.

The 2020 short follow-up survey had three components: a market survey, a firm survey and a consumer survey. In the market survey, we obtained additional information from the market office about the four closest neighbors, which was necessary because the data on this in the long surveys was incomplete and noisy. In the firm survey, we asked retrospective questions about outcomes we did not include in the long surveys, especially the average price of their main product in 2016, and the share of employees with at least high school education in 2016. In the consumer survey, we

[7] There were very few Covid cases in China during our follow-up survey.

randomly picked a customer visiting the firm, and asked her or him to rank it on a scale of 1 to 5 on the following dimensions: product quality, product variety, service quality, shopping environment, value for money, and overall satisfaction.

2.2 Summary statistics

Our full sample consists of 3,173 firms organized in 78 markets. In the average market we observe about 41 firms. Defining a firm's industry with its main product as reported in the baseline survey, we find that in the average market firms are in 4.9 different industries. Since we only surveyed half of the firms in each market, this implies that the average market had about 82 firms in about 5 industries, with around 17 firms in each industry. This suggests that firms operate in a fairly competitive environment.

Table 1 presents summary statics in the baseline survey about firms and managers. Each row corresponds to a separate regression, in which the variable listed in column 1 is regressed on a constant and on four indicators for the different treatment arms: treated firm in a 50% market, untreated firm in a 50% market, treated firm in an 80% market and untreated firm in an 80% market. Thus the coefficient of the constant measures the average of the variable in the "pure control" group of firms in untreated markets, while the other coefficients measure the average difference in the variable relative to the pure control group. We label columns 2-6 accordingly. We cluster standard errors at the market level.

Panel A on firm characteristics shows that in 2013 average firm age was about 6.5 years. Almost 70% of the firms were in retail, with the rest mostly in services and manufacturing. Firms employed on average about 9 workers. The average net profit was about RMB 519,000 (about $84,000), and average revenue was about RMB 3,230,000 ($525,000). Panel B presents managerial characteristics. Almost 60% of managers were men, and in 2013 they were on average 38 years old. About a quarter of them had a college degree. About 15% of managers had a political connection, defined as past working experience in the government. Consistent with the randomization, there is no significant difference between any of the treatment arms in any of these variables.

Table 2 presents summary statics on financial and business activities. Panel A reports on

Table 1: Summary statistics: Firm and manager characteristics

Sample: all baseline, 3,173 firms	Pure Control	Δ Treated 50% Markets	Δ Untreated 50% Markets	Δ Treated 80% Markets	Δ Untreated 80% Markets
Number of firms	1247	222	203	1214	287
Panel A: Firm Characteristics					
Firm age	6.479***	0.697	0.935	-0.310	-0.517
	(0.308)	(1.005)	(0.727)	(0.420)	(0.467)
Sector - Retail (%)	0.682***	0.047	0.027	0.004	-0.041
	(0.057)	(0.089)	(0.103)	(0.072)	(0.090)
Number of employees	8.823***	1.159	0.364	0.015	0.219
	(0.564)	(1.151)	(1.131)	(0.705)	(0.697)
Profit (10,000 RMB)	51.95***	-1.878	-2.483	-0.951	-0.272
	(6.193)	(11.62)	(9.134)	(7.747)	(8.204)
Sales (10,000 RMB)	323.7***	19.06	6.570	2.925	-7.416
	(38.30)	(79.75)	(59.83)	(53.74)	(43.40)
Panel B: Managerial Characteristics					
Gender (1=Male, 0=Female)	0.581***	-0.018	-0.009	-0.002	-0.002
	(0.031)	(0.065)	(0.061)	(0.053)	(0.059)
Age	38.36***	-0.232	0.347	-0.016	0.927
	(0.642)	(1.415)	(1.294)	(1.081)	(1.059)
Education - College	0.246***	0.011	0.025	0.031	0.029
	(0.021)	(0.036)	(0.051)	(0.028)	(0.034)
Political connection (1=Yes, 0=No)	0.148***	0.037	0.015	0.015	0.013
	(0.018)	(0.0400)	(0.031)	(0.025)	(0.027)

Note: Each row reports a separate regression of the outcome variable (column 1) on a constant representing the pure control group, and indicators for treated firms in 50% markets, untreated firms in 50% markets, treated firms in 80% markets, and untreated firms in 80% markets, representing the mean difference relative to the pure control. Standard errors clustered at the market level. *** p<0.01, ** p<0.05, * p<0.1.

borrowing from formal banks. A quarter of firms had a pre-existing loan at baseline. Conditional on having a loan, the average loan amount was about RMB 300,000 ($49,000) and the average monthly interest rate was about 0.9%. Panel B reports data on business connections with suppliers and clients. Firms had about 27 clients per day and about 6 active suppliers. Finally, panel C reports shutdown and attrition by endline. Shutdown is defined as one in a survey wave if we have information that the firm went out of business in or before the fiscal year to which the survey wave refers. Attrition is defined as one in a survey wave if we do not have information about the firm in that wave and we do not know that the firm has shut down, and is typically due to the firm moving away or choosing not to respond. With these definitions, shutdown and attrition are mutually exclusive. We implemented several arrangements to keep attrition low: With the help of the bank and the market office we were able to track most mover firms; the bank phoned

Table 2: Summary statistics: Financial and business activities

Sample: all baseline, 3173 firms	Pure Control	Δ Treated 50% Markets	Δ Untreated 50% Markets	Δ Treated 80% Markets	Δ Untreated 80% Markets
Number of firms	1247	222	203	1214	287
Panel A: Borrowing					
Other Bank Loan (1=Yes, 0=No)	0.253***	0.036	-0.001	-0.027	-0.030
	(0.024)	(0.049)	(0.048)	(0.033)	(0.044)
Loan Size (10,000 RMB)	30.78***	1.271	-4.008	-1.982	-5.531
	(6.737)	(14.28)	(8.919)	(11.12)	(7.769)
Monthly Interest Rate (‰)	9.158***	-0.463	0.332	0.043	0.036
	(0.133)	(0.351)	(0.289)	(0.198)	(0.294)
Panel B: Partnerships					
Number of Clients	27.37***	-0.770	1.232	1.124	2.118
	(1.011)	(1.505)	(2.287)	(1.482)	(1.829)
Number of Suppliers	6.535***	2.091	1.549	-0.244	0.124
	(0.813)	(2.245)	(1.559)	(0.908)	(1.063)
Panel C: Shutdown and Attrition					
Attrition (endline)	0.106***	-0.002	-0.002	0.001	-0.001
	(0.009)	(0.015)	(0.023)	(0.012)	(0.016)
Shutdown (endline)	0.134***	-0.026	-0.031	-0.052*	0.019
	(0.023)	(0.059)	(0.045)	(0.028)	(0.034)

Note: Each row reports a separate regression of the outcome variable (column 1) on a constant representing the pure control group, and indicators for treated firms in 50% markets, untreated firms in 50% markets, treated firms in 80% markets, and untreated firms in 80% markets, representing the mean difference relative to the pure control. The estimations on loan size and monthly interest rate are based on the sample of firms that have borrowed from formal banks at baseline. Standard errors clustered at the market level. *** p<0.01, ** p<0.05, * p<0.1.

managers in advance to arrange the survey; and when the manager was unavailable at arranged time, we attempted to arrange a second meeting. The table shows that the attrition was about 10% by endline, and not significantly different across treatment arms. Finally, the shutdown rate among pure control firms was about 13% by endline, and was significantly lower ($p < 0.10$) among treated firms in 80%-treated markets. This result suggests the treatment may have improved firm performance, but also that differences in shutdown may have induced selection by treatment status, potentially biasing our results. To address this concern, in Appendix Table A2 we present balance tests for the subsample of firms that remain in our data all the way to the 2016 endline, or to the 2020 follow-up, and document no significant differences by treatment status in the baseline characteristics of these firms in either subsample. These results suggest that selective attrition or exit is unlikely to bias our results.

Table 3: Effects on borrowing by endline

Dep. var.:	Borrow with new loan product		
	(1)	(2)	(3)
Treated	0.279***	0.315***	
	(0.034)	(0.034)	
Untreated * Share of Peers Treated		0.178***	
		(0.037)	
Treated * 50% market			0.302***
			(0.057)
Treated * 80% market			0.318***
			(0.039)
Untreated * 50% market			0.112*
			(0.062)
Untreated * 80% market			0.150***
			(0.030)
Constant	0.067***	0.032***	0.031***
	(0.014)	(0.037)	(0.030)
Observations	3173	3173	3173

Note: Dependent variable indicates firm having borrowed using the new loan product by endline. Share of peers treated is the share of other firms in the market which are treated. Standard errors clustered at the market level. *** p<0.01, ** p<0.05, * p<0.1.

2.3 Basic patterns in the data: take-up, spillover, performance

To obtain a high-level understanding of borrowing and firm dynamics in our setting, we present three basic facts that emerge from the raw data. These facts motivate our model and the subsequent empirical analysis.

Take-up. We begin by looking at the borrowing of treated firms. Table 3 presents cross-sectional estimates of take-up. The dependent variable is an indicator for whether the firm has borrowed using the new loan product by the endline survey. Column 1 shows that the probability of borrowing was 28 percentage points higher among treated than among untreated firms, indicating that the treatment succeeded in inducing firms to borrow. Complementing this finding, our data show that the average loan amount borrowed using the new product was RMB 290,000 (about \$47,000), or roughly 9% of average sales. The average monthly interest rate was about 0.73 percent, meaningfully lower than the average interest rate among alternatives shown in Table 2. And, although the bank did not share administrative data with us, they reported that the repayment rate among borrowers in our sample was over 98%, and that about half of borrowers borrowed again after repaying the loan. We conclude that firms found the new loan product attractive, and that the treatment succeeded in providing access to a significant amount of financing.

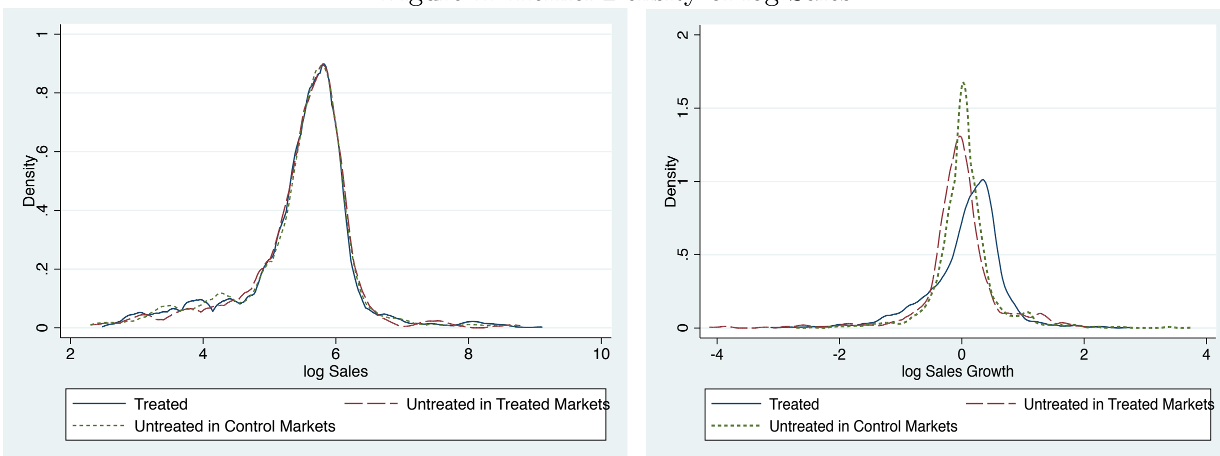
Spillover in borrowing. We next consider the borrowing of untreated firms. Column 2 of Table 3 estimates the indirect effect on borrowing by including the interaction between an indicator for the firm being untreated and the share of other firms in the market which are treated. The estimated coefficient of 0.18 is highly significant, and implies that increasing the share of peers treated from 0% to 100% would increase the likelihood that an untreated firm borrows by 18 percent. Column 3 is a more flexible specification that estimates take-up separately in the five treatment arms. Firms in pure control markets borrowed with 3 percent probability. Relative to these firms, treated firms in the two types of treated markets borrowed with 30 respectively 32 percentage points higher probability. And, relative to pure control firms, untreated firms in the two types of treated markets borrowed with 11 respectively 15 percentage points higher probability. These coefficients are consistent with the parsimonious linear specification of column 2.

The results in Table 3 provide clear evidence of an indirect effect on loan take-up. This effect could arise for two plausible reasons. One is information diffusion: untreated firms may have learned about the loan opportunity from treated peers. Another is cost: since in treated markets the loan officer was present more often, firms may have found it less costly to initiate a loan application. Responses to our survey questions suggest that information diffusion is the likely explanation: the majority of untreated borrowers in treated markets reported that they heard about the program from other firms in the market, rather than from bank officers.[8] Given this evidence, in the rest of the paper we will interpret the indirect effect on borrowing as information diffusion. This interpretation is also consistent with evidence from other contexts documenting the diffusion of information about financial products (Banerjee, Chandrasekhar, Duflo and Jackson 2013, Cai, de Janvry and Sadoulet 2015, Cai and Szeidl 2018).

Firm performance. We next present graphical evidence about the impact of the intervention on firm performance. The left panel in Figure 1 plots the kernel density of log sales at baseline for three different groups of firms: treated firms, untreated firms in treated markets, and untreated

[8] In addition, evidence we present in Section 4.3 shows that much of the spillover in take-up is coming from firms who are not direct competitors, further supporting the information diffusion explanation.

Figure 1: Kernel Density of log Sales



firms in control markets. Consistent with the randomization, these densities are very similar to each other: before the intervention the distribution of log sales was similar in the three groups.

The right panel of the Figure shows, for the same three groups, the kernel density of the *change* in log sales between baseline and endline. There are two salient differences between these densities. First, relative to untreated firms in control markets, treated firms experienced higher growth in sales. This pattern suggests that the intervention had a positive direct effect on the revenue of treated firms: presumably, access to the loan allowed firms to change business practices and expand. Second, relative to untreated firms in control markets, untreated firms in treated markets experienced *lower* growth in sales. This pattern suggests that the intervention had a negative indirect effect on the revenue of untreated firms with treated peers. A natural interpretation is that this is a business stealing effect: as treated firms expanded due to the loan product, they lured away some of the business of their untreated competitors.

In summary, the raw data provide suggestive evidence for two types of indirect effects: information diffusion that affects borrowing, and business stealing that affects firm performance. We now turn to a model that captures both of these effects.

3 Model and empirical strategy

To develop a conceptual framework that can guide our empirical analysis, we build a model of the direct and indirect effects of the loan program. We begin with a model of business stealing; then incorporate take-up and diffusion; and then use the predictions to formulate our empirical strategy.

3.1 A model of business stealing

Our basic model parallels that of Rotemberg (2019) but explicitly incorporates markets and the possibility that the loan enables quality improvements. The main focus of our model is a monopolistically competitive sector which consists of a mass of markets indexed by m, and in each market m a mass n_m of firms indexed by i. Goods purchased in market m aggregate into a composite good

$$Q_m = \left[\int_{i \in m} (h_i \cdot Q_i)^{1-1/\sigma} di \right]^{\frac{\sigma}{\sigma-1}} \tag{1}$$

where h_i is the quality of the product (or service) of firm i. Consumer preferences are given by

$$H + \left[\int Q_m^{1-1/\theta} dm \right]. \tag{2}$$

In these equations σ measures the elasticity of substitution within a market, and θ the elasticity of substitution across markets. We assume $\sigma > \theta > 1$. For simplicity we assume that utility is quasilinear, with H being a numeraire good produced by a perfectly competitive sector and traded at a price normalized to one. One unit of labor produces w units of H, pinning down the wage as w. The aggregate labor supply is L.[9] Firms in the monopolistically competitive sector have constant returns to scale, produce with labor, and take wages as given. The output of firm i is $Q_i = \omega_i L_i$. Firms may differ both in quality (or appeal) h_i and productivity ω_i, and different markets m may have different distributions of firm quality and productivity. Recognizing quality or appeal is important given the finding of Hottman, Redding and Weinstein (2016) that in the U.S. appeal accounts for most of the variation in retail firm size.

We consider the impact on this economy of a loan program that provides loans to a subset of firms in a subset of markets. For now we take the loan assignment to be exogenous and assume

[9] We assume L is sufficiently large that in equilibrium $H > 0$.

that all firms assigned the loan borrow; we will endogenize take-up decision below. We assume that receiving the loan allows for investments in business practices that improve both product quality and firm productivity: quality h_i increases by a factor e^{γ_h}, and productivity ω_i by a factor e^{γ_ω}. We assume $\gamma_\omega, \gamma_h \geq 0$ and let $\gamma_\omega + \gamma_h = \gamma$. We let B_i be an indicator for the borrowing status of firm i, and let Z_m denote the share of firms in market m that borrow.

In the spirit of the potential outcomes approach, it will be helpful to consider counterfactual outcomes that would obtain absent the loan program. We use the convention that variables with tilde represent outcomes absent the loan program, and Δ represents the impact of the program, i.e., $\Delta X = X - \tilde{X}$.

Because the loan assignment need not be random, borrowers may be different from the average firm in their market. To capture this selection, we let λ_m denote the ratio, absent the loan program, of the average revenue of borrowers relative to that of firms in market m:

$$\lambda_m = \frac{\int_{i \in m:\ B_i = 1} \tilde{R}_i di}{Z_m \cdot \int_{i \in m} \tilde{R}_i di}$$

where $\tilde{R}_i$ is the revenue of firm i absent the treatment. Then the following result characterizes the impact of the loan program on firms.

Proposition 1. *To a first order approximation, the impact of the loan program on the revenue of firm i in market m is*

$$\Delta \log R_i \approx (\sigma - 1)\gamma \cdot B_i - (\sigma - \theta)\gamma\lambda_m \cdot Z_m. \tag{3}$$

All proofs are in Appendix A.1. The impact on revenue is characterized by two terms. The first term represents the positive direct effect of the firm receiving a loan (B_i), while the second term represents the negative business stealing effect of the share of the firm's competitors that receive the loan (Z_m). The logic of the direct effect is that loan-induced improvements in quality and productivity allow the firm to reduce its quality-adjusted price and thereby attract higher demand. The coefficient is $\gamma(\sigma - 1)$, where γ measures the decrease in the quality-adjusted price, and $\sigma - 1$ is the elasticity of firm revenue to price. The logic of the business stealing effect is that the higher the share of competitors who borrow, the lower the quality-adjusted price index in the market representing the strength of competition, and the lower the demand for the product of the firm.

The coefficient is proportional to λ_m because the larger borrowers' average revenue, the higher their absolute impact on the price index. It is also proportional to $\sigma - \theta$ which is the elasticity of firm revenue to the market level price index. This elasticity is lower than the own-price elasticity $\sigma - 1$ governing the direct effect, because a fall in the market-level price index also attracts demand from outside of the market.[10]

3.2 Diffusion, take-up, and other empirically relevant features

We enrich the model by incorporating the indirect effect of information diffusion, as well as other features that facilitate the connection to our empirical analysis: multiple periods, random shocks, the randomized intervention, and imperfect take-up.

We assume that the model is repeated over periods $t = 0, 1, ..., \tau$. Consumers have preferences given by (2) each period, and consume all their income each period. We make relatively weak assumptions about firm dynamics, stated formally in the Appendix: we assume that absent the treatment, for each firm i, the vector of log quality and productivity $\tilde{x}_i^t = (\log \tilde{h}_i^t, \log \tilde{\omega}_i^t)$ evolve according to the sum of (i) a Markov process that depends on firm-level and market-level characteristics and shocks, (ii) a firm-specific idiosyncratic shock, and (iii) a time trend. We also assume that wages w^t, pinned down by the production function for the numeraire good, evolve deterministically.

We model the intervention as follows. A mass M of markets are selected to be treated. Market-level treatment intensities are characterized by intensity levels $0 \leq s_1 \leq s_2 ... \leq s_k$ and associated probabilities $0 \leq q_j \leq 1$ such that $q_1 + ... + q_k = 1$. The intervention is introduced between periods 0 and 1, and treats a randomly chosen share s_j of firms in a randomly chosen share q_j of markets.

The treatment provides information about, and increases the salience of, the loan opportunity, and may reduce the cost of applying. Similarly to the basic model, we assume that every firm which takes up the loan experiences increases in (log) quality and productivity of γ_h and γ_ω, and we also assume that these improvements are permanent. But, differently from our basic model, information about the loan may diffuse to untreated firms, and both treated and untreated firms make a decision about whether to take up.

[10] In our model with quasi-linear preferences the reallocation from outside of the market is coming entirely from the numeraire good.

We model information diffusion in the spirit of the Bass (1969) model by assuming that when a share S_m of firms in market m are treated, an additional share $\phi S_m(1 - S_m)$ learn about the loan opportunity. This share is proportional to the mass of treated firms S_m who can potentially diffuse, and the mass of untreated firms $1 - S_m$ who can potentially receive the information. The parameter ϕ governs the strength of diffusion. We assume that the firms to which information diffuses are a random subset of the untreated firms.

We assume that take-up is imperfect because the firm's manager may not have a sufficiently promising idea that could be developed using the loan. We think that relative to the benefits of the loan the cost of applying is minimal and can be ignored. Treated firms decide to take up in period 1, while informed untreated firms, because diffusion takes time, decide to take up in an exogenously given period s, where $s \geq 1$. We model take-up in a reduced-form fashion that allows for a correlation between firm fundamentals and whether the manager has an idea for the loan. Firm i, if treated, takes up at $t = 1$ with probability $F^T(\tilde{x}_i^1)$, and if untreated but reached by diffusion, takes up at $t = s$ with probability $F^D(\tilde{x}_i^s)$, where the non-decreasing functions F^T and F^D represent the probability that the firm with fundamentals $\tilde{x}_i^t = (\log \tilde{h}_i^t, \log \tilde{\omega}_i^t)$ has an idea.[11]

Denote the probability that in market m a random treated firm borrows by $\mu_m^T = E[F^T(\tilde{x}_i^1)|m]$, and that in market m a random firm accessed by diffusion borrows by $\mu_m^D = E[F^D(\tilde{x}_i^s)|m]$. We assume that $\mu_m^T > \phi\mu_m^D$ for all m, which means that on average getting treated has a higher effect on take-up than getting diffusion from all peers. This assumption will be useful for our IV strategy. Let μ^T and μ^D denote the unconditional average take-up probability of a treated and a diffusion firm.

We let T_i denote the realized treatment status of firm i, and S_m the realized treatment intensity of market m. Because borrowing is now time-dependent, we let B_i^t indicate whether the firm has borrowed in or before period t, Z_m^t denote the share of firms in the market that have borrowed in or before period t, and $\lambda_m^t = \int_{i \in m:\ B_i^t = 1} \tilde{R}_i^t di / (Z_m^t \cdot \int_{i \in m} \tilde{R}_i^t di)$ denote, absent the treatment, the average revenue of borrowers relative to the average revenue of all firms in market m in period t.

[11] At the expense of additional notation we could allow take-up to also depend on market-level characteristics.

Proposition 2. *1. Borrowing in a period $t \geq s$ can be written as*

$$B_i^t = \mu^T \cdot T_i + \mu^D \phi \cdot (1 - T_i) S_m + \eta_i \tag{4}$$

where $E[\eta_i \times (T_i, S_m)] = 0$.

2. Firm revenue in period t, to a first-order approximation, can be written as

$$\log R_i^t \approx (\sigma - 1)\gamma \cdot B_i^t - (\sigma - \theta)\gamma\lambda \cdot Z_m^t + \kappa \cdot Post^t + f_i + \varepsilon_i^t \tag{5}$$

where λ is a non-negative-weighted average of λ_m^t, $Post^t$ is an indicator for $t \geq 1$, f_i are firm-specific effects, and $E[\varepsilon_i^t \times (1_j, Post^t, Post^t \cdot T_i, Post^t \cdot S_m)] = 0$ for all j.

Part 1 characterizes take-up. The first term on the right hand side of (4) shows that, on average, being treated increases the probability of borrowing by μ^T. The second term characterizes diffusion: a non-treated firm ($1 - T_i = 1$) in market m is reached by information diffusion with probability ϕS_m, and conditional on being informed, takes up with average intensity μ^D. The error term η_i reflects both firm-level and market-level idiosyncratic variation in take-up, and is orthogonal to the explanatory variables because of the random treatment assignment.

Part 2 characterizes firm revenue. Equation (5) parallels equation (3) of Proposition 1, but expresses revenue, rather than the treatment effect, and incorporates the additional richness of the model. The direct effect, $(\sigma - 1)\gamma \cdot B_i^t$, is as in the previous result, except that it now accounts for time variation in borrowing. In the business stealing effect, $-(\sigma - \theta)\gamma\lambda \cdot Z_m^t$, a novelty relative to the previous result is that the coefficient λ that accounts for selection is no longer dependent on m (or on t). To obtain this term, using a logic familiar from the study of heterogeneous treatment effects, we move the heterogeneity in the business stealing effect captured by λ_m^t to the error term. The coefficient of the business stealing effect thus represents an average effect, and depends on a weighted average λ which measures average selection.[12] The equation also includes firm and time fixed effects that account for other heterogeneities and dynamics.

[12] Doing this while maintaining the orthogonality condition pins down the weights as functions of the covariance between the share of firms treated and the share of firms who borrow. The non-negativity of this covariance, and hence the weights, is ensured by our assumption that take-up responds more to the treatment than to diffusion.

The main novelty in (5) is that the error term, which captures both firm-level shocks and—as we have just seen—heterogeneity in the strength of the business stealing effect, both of which may be correlated with the right-hand side variables. Indeed, our model allows both firm fundamentals, and the strength of selection λ_m^t, to be correlated with take-up. Importantly, the Proposition states that—essentially because the treatments are randomly assigned— the error term is orthogonal to $Post^t \cdot T_i$ and $Post^t \cdot S_m$. This implies that the coefficients of the direct and business stealing effects can be estimated in an IV strategy, using $Post^t \cdot T_i$ and $Post^t \cdot S_m$ as instruments.

The reduced form equation of this IV strategy takes the familiar difference-in-differences form

$$\log R_i^t = \beta \cdot T_i \cdot Post^t + \delta \cdot S_m \cdot Post^t + \kappa \cdot Post^t + f_i + \epsilon_i^t. \tag{6}$$

In the important special case of no diffusion, we can use the take-up equation (4) to substitute borrowing outcomes with the treatments, resulting in an explicit expression for the coefficients of this reduced-form regression: $\beta = (\sigma - 1)\gamma \cdot \mu^T$ and $\delta = (\sigma - \theta)\gamma\lambda \cdot \mu^T$. Thus, in this special case the reduced-form coefficients are proportional to the second-stage coefficients and serve as measures of the direct and business-stealing effects.

3.3 Empirical strategy

Proposition 2 motivates our empirical strategy. Part 1 of the Proposition provides foundations for our first estimating equation, which documents information diffusion in take-up

$$Borrow_i = const + \mu \cdot Treated_i + (1 - Treated_i) \cdot G(Share\ Peers\ Treated_i) + \eta_i. \tag{7}$$

Here the $G(.)$ function governs how diffusion varies with the treatment intensity, and is assumed to be linear in the model. We already reported estimates of this equation in Table 3 above, using both the linear specification and a more flexible alternative. In that table we defined the set of peers relevant for diffusion to be all firms in the market. Below we also consider alternative definitions based on distance and competition status.

Our second estimating equation documents indirect effects in firm performance

$$y_i^t = \beta \cdot Post^t \times Treated_i + \delta \cdot Post^t \times Share\ Competitors\ Treated_i + \kappa Post^t + Firm\ f.\ e. + \varepsilon_i^t. \tag{8}$$

When the outcome variable y_i^t is log revenue, this specification is the reduced-form regression corresponding to the IV in Part 2 of Proposition 2. As discussed above, within our model, in the case of no diffusion, the reduced-form coefficients have clear interpretation: β is proportional to the direct and δ to the business stealing effect of the loan. With diffusion, the map from reduced-form coefficients to economic forces is more complicated, but even then, $\beta > 0$ and $\delta < 0$ are only possible if the loan has a positive direct and a negative business stealing effect.[13] Moreover, independently of the model, the coefficients have straightforward interpretation as treatment effect estimates: β measures the direct and δ the indirect effect of the treatment. For these reasons, we use (8) as our main estimating equation. In most specifications we define the peer group as competitor firms selling the same main product in the same market, but we also consider alternative definitions.

Our third estimating equation jointly accounts for the diffusion and business stealing effects through the IV regression

$$y_i^t = \zeta \cdot Borrow_i^t + \xi \cdot Share\ Competitors\ Borrow_i^t + \kappa Post^t + Firm\ f.\ e. + \nu_i^t \tag{9}$$

in which $Post^t \times Treated_i$ and $Post^t \times Share\ Competitors\ Treated_i$ are the instruments. When y_i^t is log revenue, this specification is identical to the IV of Proposition 2, providing a way to infer, even with diffusion, the direct and business stealing effects of the loan. We use this regression to confirm the insights obtained from the reduced-form analysis, and for our welfare evaluation.

Before moving to the results, we explain how our approach can account for two different indirect effects using a single source of exogenous variation, treatment intensity. The reason is that the two indirect effects affect different outcome variables in different regressions: diffusion affects take-up in (7), while business stealing affects firm performance conditional on take-up in (9).

[13] Diffusion biases β towards zero because some untreated firms also borrow and experience the direct effect. Thus $\beta > 0$ must imply a positive direct effect. Given that, $\delta < 0$ must imply a negative indirect effect since otherwise diffusion would just amplify the positive direct effect.

4 Reduced-form estimates of direct and indirect effects

In this section we present four sets of results. First, we report reduced-form estimates of the impacts of the intervention on main firm performance measures. Second, we look at intermediate outcomes that provide evidence on mechanisms. Third, we explore the heterogeneity of the indirect effects with respect to distance and competition status, and in the process identify a third indirect effect: diffusion of demand. Finally, we report impacts on market-level outcomes.

4.1 Main effects and specification checks

We begin with regressions showing the impact of the intervention on firm performance. Table 4 reports estimates of our reduced-form estimating equation (8) for a range of leading firm performance measures. In column 1 the outcome is log sales. The coefficient of the interaction between *Treated* and *Post* implies that—holding fixed indirect effects—the treatment increases firm sales by 9.9 log points. The coefficient of the interaction between *Share Competitors Treated* and *Post* implies that treating all the competitors of a firm would reduce its sales by 8.6 log points. Both of these treatment effects are large and highly significant. In column 2 the outcome is profit in levels: because profit may be negative we do not take logs. We estimate a positive and significant direct effect of RMB 126,400 (about $20,000) and a negative and significant indirect effect of RMB 95,000 (about $15,000). These results, consistent with the logic of the model, suggest that the intervention induced a reallocation of demand and profit from firms having many treated competitors to firms having the treatment. And the similar magnitude of the direct and indirect effects suggests that the treatment had little overall effect on market-level revenue or profit.

The remaining columns in the Table focus on factor use, input use and firm survival. Columns 3 and 4 show positive direct and negative indirect effects for log employment and the log wage bill. These results suggest that employment was an important margin of adjustment accommodating the reallocation induced by the treatment. Column 5 reports insignificant and small coefficients for fixed assets, suggesting that the loan was not used for traditional forms of capital. This result is consistent with the fact that most firms in our data are in services and retail and likely have low

Table 4: Direct and indirect effects: Main outcomes

Dep. var.:	log Sales	Profit (10,000 RMB)	log Number of Employees	log Wage Bill	Fixed Assets (10,000 RMB)	log Material Cost	Shutdown
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post*Treated	0.099***	12.64***	0.075**	0.101***	5.468	0.077*	-0.028***
	(0.035)	(3.099)	(0.029)	(0.029)	(4.537)	(0.041)	(0.010)
Post*Share Competitors Treated	-0.086**	-9.478*	-0.066*	-0.069*	-3.013	-0.050	0.001
	(0.041)	(4.802)	(0.038)	(0.037)	(4.558)	(0.047)	(0.018)
Firm FE and Post	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,612	8,612	8,612	8,602	8,612	8,605	8,847

Note: Standard errors clustered at the market level. *** p<0.01, ** p<0.05, * p<0.1.

capital intensity.[14] Column 6 reports effects on material spending. The coefficients are comparable to those for employment, though less significant, and suggest that inputs were another margin of adjustment. Finally, column 7 shows that the treatment had a positive direct effect, and no indirect effect, on survival. The positive direct effect, though formally outside of our model, is consistent with the logic that borrowing improved firm performance. One reason for the lack of an indirect effect may be that business stealing is spread out over multiple competitors and thus has a smaller per-firm impact. Overall, consistent with our model, Table 4 documents positive direct and negative indirect effects of the intervention.

Specification checks. We present two specification checks that provide evidence for internal validity. We first estimate a placebo specification in which we estimate direct and indirect effects in the baseline data. Because the treatment is randomized and takes place after the baseline survey, we expect to find zero effects. Table A3 in the Appendix confirms that both direct and indirect effects are insignificant and small, providing evidence against possible misspecification.

We next explore whether indirect effects are heterogeneous by the firm's treatment status. To do this, we include the interaction of *Share Peers Treated* with both *Treated* and *Untreated*, resulting in a natural "saturated" specification. Table 5 reports the results. The coefficients are insignificant except for the negative indirect effect on untreated firms. The signs and magnitudes of the direct effect, and of the indirect effect on the untreated, are similar to those in our main specification in

[14] As we show below, firms did use the loan for non-traditional forms of capital.

Table 5: Indirect effects by treatment status

Dep. var.:	log Sales	Profit (10,000 RMB)	log Number of Employees	log Wage Bill	Fixed Assets (10,000 RMB)	log Material Cost
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Treated	0.070	7.729	0.032	0.041	0.468	0.052
	(0.087)	(11.47)	(0.054)	(0.052)	(5.880)	(0.124)
Post*Share Competitors Treated*Treated	-0.049	-3.398	-0.013	0.005	3.181	-0.019
	(0.106)	(14.46)	(0.064)	(0.062)	(7.702)	(0.151)
Post*Share Competitors Treated*Untreated	-0.094**	-10.90**	-0.078*	-0.087*	-4.466	-0.057
	(0.045)	(4.230)	(0.044)	(0.044)	(5.175)	(0.054)
Firm FE and Post	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,612	8,612	8,612	8,602	8,612	8,605

Note: Standard errors clustered at the market level. *** p<0.01, ** p<0.05, * p<0.1.

Table 4. The magnitude of the indirect effect on the treated is closer to zero, but not significantly different from that of the indirect effect on the untreated. We conclude that there is not enough power to separately identify differential indirect effects by treatment status, and that we cannot reject our model-implied main specification in favor of this richer specification.

4.2 Intermediate outcomes and mechanisms

To shed light on the mechanisms behind the direct and indirect effects, we investigate impacts on intermediate outcomes. We consider outcomes related to the firms' business practices, as well as to consumers' experience in the stores.

Business practices. Table 6 studies impacts on the firm's business activities. In column 1 we look at the log number of clients and find a significant positive direct and a significant negative indirect effect. These results are consistent with the logic of business stealing: as borrower firms improve, clients migrate from firms which have many borrower competitors to firms which are borrowers. They are also consistent with the findings of Einav, Klenow, Levin and Murciano-Gorof (2021) who document the importance of customers in accounting for retails sales variation.

The rest of the Table seeks to identify dimensions of improvement that may have driven this reallocation. In columns 2-4 we explore outcomes related to quality. Column 2 reports impacts

Table 6: Direct and indirect effects: Business outcomes

Dep. var.:	log Number of Clients	Renovation	New Product	Quality of Labor	Supplier Change	Stocking Period (unit: month)	Inventory Management	Other Loan Amount
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post*Treated	0.083**	0.243***	0.231***	0.097***	0.114***	0.597***	0.132***	2.154
	(0.032)	(0.020)	(0.018)	(0.025)	(0.025)	(0.086)	(0.022)	(2.611)
Post*Share Competitors Treated	-0.071**	-0.049	-0.047**	-0.026	0.027	-0.034	0.019	-3.242
	(0.034)	(0.030)	(0.019)	(0.030)	(0.032)	(0.112)	(0.027)	(2.174)
Firm FE and Post	Yes	Yes	Yes	No	No	No	No	Yes
Observations	8,612	8,612	8,612	2,781	2,781	2,781	2,781	8,343

Note: Columns 4-7 report cross-sectional regressions using the 2020 follow-up data. In columns 2, 3 and 5 dependent variable is an indicator. In column 4 dependent variable is the share of workers who completed high school, in column 7 it is an indicator for the firm having digitalized inventory records or a designated area for inventory storage. Standard errors clustered at the market level. *** p<0.01, ** p<0.05, * p<0.1.

on renovation. We estimate a large and significant direct effect—the treatment increases the probability of renovation by 24 percentage points—and no indirect effect. Column 3 reports impacts on the introduction of new products. Here too we estimate a large and significant direct effect, again with almost a quarter of treated firms being impacted, and a much smaller indirect effect. Column 4 reports impacts on the quality of labor, measured as the share of workers in 2016 who finished high school. Because we only collected this information in our short 2020 follow-up survey, the specification we estimate is a cross-sectional regression.[15] Here too we find a large positive direct effect and no indirect effect. All three of these intermediate outcomes may have increased the firm's service quality. Importantly, the weak indirect effects suggest market-level improvements in quality, and suggest that quality was one of the drivers of the demand reallocation.

In columns 5-7 we study improvements in inventory and supplier management, which may have affected marginal cost, or equivalently, productivity. Column 5 shows a positive direct effect on the probability of switching to a new supplier. One possible explanation is that the loan allowed the firm to place larger orders, which may have required switching to a different supplier. Consistent with this explanation, column 6 shows a positive direct effect on the stocking period, i.e., the number of months between restocking the store. This may have reduced marginal cost as large orders often

[15] Nevertheless the variable labels in the table are correct, because $Post = 1$ for the year 2016.

come with a discount. And the increased stock may have been stored in the space created by renovation. Column 7 shows an improvement in the quality of inventory management, measured with an indicator that equals one if the firm either digitalized inventory records or had a designated area for inventory storage.[16] These effects could come about for example if the new better-educated workers digitalized records, or the renovation created space for inventory. Improvements in these supplier-related outcomes may have reduced costs and thus increased productivity. The small indirect effects suggest market-wide cost reductions, and suggest that cost was one of the drivers of the demand reallocation. In summary, the results in Table 6 provide evidence that, consistent with the model, the loan enabled investments that improved quality and reduced cost.

These results are in line with our survey of borrowers at endline, in which we asked them to describe what they primarily used the loan for and grouped the answers into categories. The three categories mentioned most frequently were renovation and increase in scale of operations (75%), purchase of inventory or inputs (50%), and starting new projects or introducing new products (34%), in line with our results on renovation, supplier/inventory management, and new product introduction.

Finally, we turn to borrowing from sources other than the new loan product. In column 8 the outcome is the total amount of outstanding other loans. This is an important outcome: as Banerjee and Duflo (2014) note, even firms that are not credit constrained may take advantage of a new loan, which they would then use to pay off an existing loan that has higher interest. In our setting this is not the case: the direct effect is insignificant and small, implying no evidence for crowding out and indicating that the average firm was genuinely borrowing constrained. The indirect effect is also small, showing that the intervention had no detectable effect on other borrowing.

Consumer experience. To investigate whether the loan-induced changes in business practices affected consumer well-being, we look at impacts on various dimensions of consumer experience, including price and quality as experienced by consumers. We collected data on these outcomes in our short follow-up survey in 2020. To measure price, we asked firms to report the average price, in RMB, of their main product in 2016. And to measure experienced quality, we asked a

[16] Separately estimating the impacts on digitization and inventory storage yields similar results. Because answers to the two questions are positively correlated, we decided to group them into a single measure.

random client physically present to evaluate the store along a number of dimensions.[17] Table 7 reports impacts on these outcomes, measured using cross-sectional regressions because the data are only available in one wave. Column 1 shows a negative direct effect on the price of about 5 log points ($p < 0.10$), and an insignificant and small indirect effect. Columns 2-6 show significant improvements in various experienced quality and price-adjusted quality dimensions: advice from sellers, service quality, the shopping environment, value for money, and overall satisfaction. For all these outcomes we observe an indirect effect that is much smaller than the direct effect. The results support our interpretation that the changes in business practices improved consumer experience.

In summary, Tables 6 and 7 suggest that—consistently with our model—the loan enabled firms to improve quality and reduce cost, which increased consumer welfare and led to a reallocation of demand. The results also point to the importance of quality, consistent with the findings of Hottman et al. (2016) that highlight the role of product appeal in explaining retail firm performance. Finally, we note that the negative indirect effects we document on some measures of quality, such as new product introduction and consumer evaluation, are consistent with the model of Aghion, Bloom, Blundell, Griffith and Howitt (2005) which predicts that in highly competitive environments an increase in competition can discourage innovation.

Other outcomes. In Appendix Table A4 we investigate impacts on several other intermediate outcomes. First we look at trade credit, and find a positive direct and a negative indirect effect in its use with both suppliers and clients. One possible explanation is that trade credit tracks the intensity of business activities. Second, we look at advertising spending and find a positive direct but no indirect effect, suggesting that advertising may have contributed to the reallocation. However, the effect is quantitatively small (about RMB 2,100 or $320) so that the role of advertising may have been minor. Third, we find no direct or indirect effects on the markup measured as the ratio of revenue to cost, or on rental payments, suggesting that these channels did not contribute to our findings. We also find no impact on the log number of suppliers of the firm, suggesting that the switch to new suppliers documented above crowded out prior suppliers. Fourth, we find no

[17] We collected the price data for all firms in the baseline sample we could reach by phone or in person, including firms that shut down after 2016. We collected the experienced quality data for firms we found open during our 2020 survey.

Table 7: Direct and indirect effects: Consumer experience

Dep. var.:	log Price	Advice from Sellers	Service Quality	Shopping Environment	Value for Money	Overall Satisfaction
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	-0.052*	0.238***	0.753***	0.991***	0.574***	0.836***
	(0.027)	(0.035)	(0.0950)	(0.0969)	(0.081)	(0.060)
Share Competitors Treated	-0.007	-0.098**	-0.175	-0.345***	-0.211**	-0.231**
	(0.037)	(0.046)	(0.120)	(0.128)	(0.087)	(0.095)
Observations	2,781	1,804	1,804	1,804	1,804	1,804

Note: In column 1 sample is all firms we could reach to collect price data. In columns 2-6, where the outcome is based on consumer evaluation, sample is all firms we found open during the 2020 follow-up. In column 2 outcome is an indicator, in columns 3-6 it is the z-score of the corresponding dimension of consumer evaluation. Standard errors clustered at the market level. *** p<0.01, ** p<0.05, * p<0.1.

direct or indirect effects on an indicator for whether the firm has a different loan, providing further evidence that the new loan product did not crowd out other borrowing.

Experimenter demand. Because all our data come from self-reported surveys, one concern with our findings may be that they are affected by experimenter demand. To address this concern, in Appendix Table A4 we report impacts on the difference between the log of self-reported sales and the log of the book value of sales. The latter was taken by our enumerators directly from the firm's book, and is thus unlikely to be influenced by experimenter demand. Because we only collected the book value of sales in the midline and the endline surveys, in this regression we do not include firm fixed effects. We find no effect on the difference, providing evidence against experimenter demand being a driver of our results.

Heterogeneous effects. In Appendix Table A5 we report estimates of a heterogeneous effect version of our main specification. We find some evidence that direct effects were larger for firms having higher employment and a more-educated manager. These effects seem plausible, and suggest a reallocation within the group of treated firms towards more productive firms.

Table 8: Effects on borrowing by peers' location and competition status

VARIABLES	Borrow with new loan product	
	Treated	Untreated
	(1)	(2)
Share Local Competitors Treated	-0.023	-0.023
	(0.039)	(0.043)
Share Local Non-competitors Treated	0.039	0.100**
	(0.057)	(0.049)
Share Non-local Competitors Treated	0.005	0.112**
	(0.095)	(0.056)
Share Non-local Non-competitors Treated	-0.045	0.061
	(0.146)	(0.076)
Observations	1256	1525

Note: Sample is all firms covered in the 2016 endline survey, which are the firms for which we have the neighbor data. Standard errors clustered at the market level. *** p<0.01, ** p<0.05, * p<0.1.

4.3 Geography and competition

So far we have assumed that all peers in the market induce information diffusion of the same intensity, and all competitors in the market induce business stealing of the same intensity.[18] But plausibly both indirect effects may vary with geographic distance, and may vary differently for competitors and non-competitors. To explore these effects, we use the fact that our data has information on the four closest neighbors of each firm. We call those four neighbors "'local" to the firm, and create four categories of peer firms in the market: local competitors, local non-competitors, non-local competitors and non-local non-competitors. For example, local competitors are those among the four closest neighbors who sell the same product, and non-local non-competitors are firms in the market other then the four closest neighbors who sell a different product. Our key right-hand side variables will be the share of treated firms within each of these groups.[19]

We first investigate heterogeneity by geography and competition status in the information diffusion effect. Table 8 reports regressions of loan take-up on the share of peers treated in each of

[18] The latter conditional on size: our approach incorporates that larger firms generate more business stealing.
[19] We are able to construct these variables even though our sample contains only half of the firms in the market, and hence does not cover about half of the listed neighbors, because we know the treatment status of all firms.

the above-defined four groups. Column 1 focuses on the sample of treated firms, and shows that for them, the likelihood of taking out the loan does not vary with the share treated in any of the groups. This seems plausible: treated firms do not need to rely on information diffusion since they know about the loan directly from the loan officer.

Column 2 reports analogous spillover estimates for the sample of untreated firms. Here the results are more interesting. Beginning with local peers, we find a small and insignificant effect of the share of local *competitors* treated, but a large and significant effect of the share of local *non-competitors* treated. The likely mechanism explaining this difference—shown to be active in different contexts by Cai and Szeidl (2018) and Hardy and McCasland (2021)—is that firms prefer to withhold business-relevant information from their direct competitors. Indeed, our results on business stealing confirm that withholding information about the loan from competitors is in the best interest of the firm. In contrast, for local non-competitors there is no risk of business stealing, and their improvements may attract more shoppers to the neighborhood.

Turning to non-local peers, we find a significant effect of the share of non-local *competitors* treated. This is surprising, as there could be business stealing by these firms as well. A possible explanation is that, because they are located farther away, these peers do not directly compete with the firm, but, because they are in the same business, they do share information. Consistent with this logic, we show below that business stealing by these competitors is weaker. Finally, we find a noisily estimated positive effect of the share of non-local *non-competitors* treated. This is the largest of the four peer groups, so the noisy estimate may imply either that there is no diffusion from these firms, or that there is diffusion from a subset—e.g., who are friends with the manager of the firm—and we are not able to zoom in on that subset.

Taken together, the heterogeneous effects on borrowing support our interpretation that the borrowing spillover reflects information diffusion, and highlight the role of agents' incentives to talk in shaping social learning and technology adoption (Banerjee, Breza, Chandrasekhar and Golub 2018, Chandrasekhar, Golub and Yang 2018).

We next investigate heterogeneity by geography and competition in the indirect effect on firm performance. Panel A of Table 9 reports estimates of the impact on three main outcomes of

Table 9: Effects on main outcomes by peers' location and competition status

	All Sample			Treated and Pure Control		
VARIABLES	log Sales	Profit (10,000 RMB)	log Number of Employees	log Sales	Profit (10,000 RMB)	log Number of Employees
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Treated	0.089**	11.60***	0.079**	0.098	-2.024	0.041
	(0.041)	(2.776)	(0.031)	(0.188)	(10.96)	(0.057)
Post*Share Local Competitors Treated	-0.099*	-11.49**	-0.053	-0.021	-3.065	0.020
	(0.054)	(5.173)	(0.038)	(0.069)	(4.019)	(0.041)
Post*Share Local Non-competitors Treated	0.156***	13.41***	0.056**	0.132**	16.68***	0.015
	(0.046)	(4.416)	(0.027)	(0.053)	(5.291)	(0.024)
Post*Share Non-Local Competitors Treated	-0.065	-9.798	-0.022	0.009	-6.108	-0.0002
	(0.045)	(12.10)	(0.047)	(0.111)	(16.41)	(0.070)
Post*Share Non-Local Non-competitors Treated	0.094	8.412	-0.018	0.035	10.94	-0.042
	-0.062	(15.83)	-0.047	(0.249)	(18.67)	-0.062
Firm FE and Post	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,220	8,220	8,220	6,967	6,967	6,967

Note: Sample is all firms covered in the 2016 endline survey, which are the firms for which we have the neighbor data. All regressions control for the interactions of Post with indicators for the number of local competitors. Standard errors clustered at the market level. *** p<0.01, ** p<0.05, * p<0.1.

the share treated in each of the four different groups. Begin with peers who are competitors. Consistent with the logic that the main competitors of a firm are local, the share of treated among *local* competitors has a significant negative effect on log sales and profits, while the share of treated among *non-local* competitors has a smaller and imprecisely estimated effect on all three outcomes. This result helps rationalize why we observe information diffusion from non-local, but not from local competitors.

The most interesting result of the Table concerns non-competitors. We find, unexpectedly, that the share of local non-competitors treated has a *positive* effect on firm performance. Being positive, and coming from non-competitors, this effect cannot be driven by business stealing. Two other explanations seem possible. First, the effect could be driven by the diffusion of information: treated non-competitors, by inducing borrowing, could make the firm more likely to grow. Second, the effect could be driven by a novel indirect effect based on the diffusion of demand: treated non-competitors may attract more consumers to the neighborhood, who may shop around and increase demand for the firm. Panel B of Table 9 attempts to distinguish between these explanations by estimating the same regression for the subsample of firms who are either treated or in pure control markets. For these firms we do not expect the first channel to be active: treated firms—as we have seen in Table 8—do not, while pure control firms, because they have no treated peers, cannot experience information diffusion. The table shows that the positive indirect effect is preserved in this subsample, providing evidence in favor of the demand diffusion explanation.[20]

In Appendix Table A6 we present two additional results that support and shed further light on the demand diffusion interpretation. First, we show that the share of treated non-competitors does not have a positive effect on our measures of consumer satisfaction. This is inconsistent with the information diffusion explanation, because diffusion-induced borrowing, by the results of Section 4.2, should also increase consumer satisfaction. Second, we present evidence that demand diffusion itself generated business stealing. To show this, we incorporate demand diffusion to our model, and obtain the prediction that when some firms grow because their non-competitor neighbors get treated, their competitors should shrink. We then present regressions showing that firms with a higher share of competitors exposed to demand diffusion do have lower performance. Because these regressions also include the share of local competitors treated, which is correlated with the share of local competitors exposed to demand diffusion, we interpret the results as suggestive. But we still find them interesting, because they are about a second-order indirect effect—from the treatment to demand diffusion to business stealing—suggesting that indirect effects may cumulate over firm networks.[21]

These results provide new experimental evidence on a demand externality that may be an important driver of the spatial concentration of retail establishments commonly observed around the world (Marshall 1920, Fujita and Thisse 1996, Leonardi and Moretti 2022). The demand externality we document acts between non-competitors, but because our markets are fairly specialized, even non-competitors tend to be in the same broadly defined trade, suggesting that this externality can

[20] This is not to say that the information diffusion effect is inactive. In our view it is active but weak, because firms who borrow due to diffusion borrow about 11 months later, and hence the impacts of their loans are probably realized only after the midline survey.

[21] Higher-order indirect effects, albeit of a different kind, play an important role in theories of input-output networks, for example, in Acemoglu, Carvalho, Ozdaglar and Tahbaz-Salehi (2012).

Table 10: Market-level outcomes

Dep. var.:	log Market Revenue	Market Profits	Shutdown Rate	Renovation Rate	Product Intro Rate	Quality of Labor	log Price	Customer Satisfaction
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post*Share Market Treated	0.058	53.41	-0.072**	0.162***	0.146***	0.043	-0.043*	1.020***
	(0.037)	(130.1)	(0.027)	(0.030)	(0.020)	(0.032)	(0.025)	(0.265)
Market FE and Post	Yes	Yes	Yes	Yes	Yes	No	No	No
Observations	234	234	234	234	234	78	78	78

Note: Quality of labor is defined as the share of employees in the market with at least high-school education. Log price is the revenue-weighted average of the firm level log price. Customer satisfaction is measured by the z-score of the market level average of overall customer satisfaction. Standard errors clustered at the market level. *** p<0.01, ** p<0.05, * p<0.1.

help explain the clustering of similar establishments. In some contexts this externality may also generate agglomeration effects, but in our context we were not able to detect such effects.

4.4 Market-level effects

We now turn to assess the market-level effects of the intervention. Table 10 reports regressions measuring the impact of the share of firms treated on market-level outcomes. Columns 1 and 2 report insignificant effects on market-level revenue and profit. These results are consistent with the similar-sized but opposite-signed direct and indirect firm level effects, and confirm that the loan program led to within-market reallocation but no detectable market-level gains in producer surplus.

The remaining columns report impacts on a set of outcomes for which the firm-level regressions showed a direct effect substantially larger than the indirect effect: the shutdown rate, the renovation rate, the product introduction rate, labor quality, price, and customer satisfaction, the latter measured as a z-score at the market level. Except for labor quality, in each of these outcomes we find significant improvements at the market level.[22] These result further support for our interpretation that—despite the null effects on producer surplus—the treatment generated market wide

[22] Labor quality in the table is an employment-weighted average which measures the labor quality of the average worker. Using the unweighted average instead, which measures the labor quality of the average firm, would give significant results. In addition to these reported results, the market level impacts on all other measures of consumer experience from Table 7 are positive and significant.

gains in price-adjusted quality, and that consumers valued these gains.

5 Combining indirect effects: Estimation and welfare evaluation

We now turn to combine the direct and indirect effects of the loan program. We first present IV estimates that measure the impacts of the loan accounting for both information diffusion and business stealing, then combine the results with the model to evaluate the welfare impact of the program on both firms and consumers, and then discuss the plausibility of some of our key assumptions.

5.1 IV estimates

We begin by measuring the impact of the loan—as opposed to the treatment—on firm performance. In this analysis we ignore the effect of demand diffusion, but below we discuss an extension that incorporates it and explain why doing so has small effects on our results. We also ignore the heterogeneity in information diffusion and business stealing documented in Section 4.3.

Under these assumptions, Proposition 2 implies that we can estimate the direct and indirect effects of the loan using the IV regression (9), in which we instrument borrowing, and the share of competitors who borrow, with the treatment, and the share of competitors who are treated. This IV accounts for the two indirect effects on firms, information diffusion and business stealing, at different stages. Information diffusion is accounted for at the first stage, where the firm's borrowing status can depend on the share of its competitors that are treated; while business stealing is accounted for at the second stage, where the firm's performance can depend on the share of its competitors that borrow. To estimate the IV, we need to define which firm constitutes a borrower in which period. Because using the loan plausibly takes time, we classify untreated borrower firms—which borrow on average 11 months later than treated borrowers—as borrowers only at the endline ($s = Endline$ in the model), effectively assuming that for these firms the impact of the loan is only realized after the midline.[23]

Table 11 reports the results. Columns 1 and 2 show the first stage for both explanatory variables. As expected, both instruments create variation in borrowing, while only the share of competitors

[23] Classifying them as borrowers at midline generates slightly larger direct and indirect effects.

Table 11: Effects of borrowing on main outcomes: IV estimation

	First stage		IV		
Dep. var.:	Borrow (1=Yes, 0=No)	Share Competitors Borrow	log Sales	Profit	log Number of Employees
	(1)	(2)	(3)	(4)	(5)
Post*Treated	0.273***	0.009			
	(0.030)	(0.006)			
Post*Share Competitors Treated	0.091***	0.357***			
	(0.021)	(0.033)			
Borrow			0.318**	40.41***	0.239***
			(0.127)	(9.698)	(0.07)
Share Competitors Borrow			-0.288**	-33.09**	-0.22***
			(0.134)	(12.978)	(0.082)
F-statistics	51.5	58.85			
Firm FE and Post	Yes	Yes	Yes	Yes	Yes
Observations	8612	8612	8612	8612	8612

Note: Borrow is an indicator for borrowing through the new loan product. For treated borrowers it equals one at midline and endline, for untreated borrowers who borrowed later it equals one at endline. Standard errors are clustered at the market level. *** p<0.01, ** p<0.05, * p<0.1.

treated creates meaningful variation in the share of competitors that borrow. The F statistics for the first stages are over 50, suggesting that weak instruments are not a problem. Columns 3-5 show the second stage for three main outcomes. Like in the reduced form regressions, we estimate significant positive direct and significant negative indirect effects which are of comparable magnitude. The coefficients are larger than in the reduced form regressions, since here we evaluate the impact of borrowing, not of the treatment. The estimates imply that borrowing would increase sales by 32 log points, and that increasing the share of competitors that borrow from 0 to 100 percent would reduce sales by 29 log points. We obtain similarly large direct and indirect effects for profit and employment.

These regressions ignore the demand diffusion effect. But including as controls the variables from Table 9 measuring demand diffusion and business stealing from demand diffusion has minor effects on the coefficients of interest (not reported), and a model extension we develop in Appendix A.1.7, which incorporates demand diffusion, implies that to a first-order approximation it has no effect on welfare. Thus omitting demand diffusion likely has small effects on the welfare results.

We conclude that the qualitative findings from the IV are similar to those of the reduced form, validating our approach of using the reduced form in most our analysis. We now turn to use the IV estimates for welfare evaluation.

5.2 Welfare evaluation: Strategy

We define the welfare gain from the loan program as the total improvement in the welfare of market firms and consumers induced by the direct, diffusion and business stealing effects, net of the interest cost of the loan. This measure of the welfare gain would approximate the societal welfare impact of the program if (i) there are no other indirect effects, and (ii) the interest rate is a good measure of the social cost of capital. We discuss both assumptions in detail in Section 5.4 below; for now we note that even if they fail, our definition captures an important component of the societal welfare effect of the program.

We compute the welfare gain using the model of Section 3.2, which omits firm exit and demand diffusion. We discuss below extensions that allow these channels and show that they have small effects on our results. In our model, the change in welfare from the loan program comes from a change in the consumer surplus and a change in the producer surplus. Let $\tilde{R}^t_m$ denote the total revenue of firms in market m in period t absent the treatment, then the following result characterizes the impact of the loan program on the consumer surplus.

Proposition 3. *To a first order approximation, in period $t \geq s$ the impact of a loan program on consumer surplus is*

$$\Delta CS^t \approx \int_m \gamma(S_m\mu^T_m + \phi S_m(1 - S_m)\mu^D_m)\lambda^t_m \tilde{R}^t_m dm. \tag{10}$$

The key to the intuition is that equation (10) expresses the savings from purchasing the same quality-adjusted bundle that would have been purchased absent the intervention, at the new quality-adjusted prices. By the envelope theorem, these savings—which could be spent on any good including the numeraire—are to a first-order approximation equal to the gain in the consumer surplus. The formula expresses these savings as the reduction in spending that results from borrower firms reducing their quality-adjusted prices. In particular, given diffusion and take-up, the share of firms that borrow equals $S_m\mu_m^T + \phi S_m(1-S_m)\mu_m^D$; these firms experience a reduction in their quality-adjusted price of γ; this reduction is relative to total market revenue $\tilde{R}_m^t$; and is amplified because borrower firms are on average λ_m^t times larger than the average firm in the market.

To evaluate welfare impacts using this result, we make two simplifying assumptions. First, we focus on a counterfactual intervention in which a constant share S of firms are treated in markets M. Second, we set aside cross-market heterogeneity and evaluate impacts for the average market. In particular, we approximate the market-level take-up and diffusion intensities μ_m^T and $\phi\mu_m^D$ with their sample averages μ^T and $\phi\mu^D$ estimated in column 2 of Table 3. We measure market revenue absent the treatment, $\tilde{R}_m^t$, by multiplying average firm revenue at midline and endline in pure control markets, $\bar{R}_C$, with the number of firms in the market n_m. And, because at baseline the average revenue of borrowers relative to all firms is about 1.1 and not significantly larger than one, we proxy the market level selection intensity λ_m^t with 1 in all markets.

To estimate the deep parameter γ, note that by Proposition 2 the regression coefficient measuring the direct effect of the loan on revenue is an estimator for $\gamma(\sigma-1)$. We infer γ from this by calibrating σ, based on recent papers estimating the retail elasticity of substitution, Atkin, Faber and Gonzalez-Navarro (2018) who estimate 2.3-4.4, and Dolfen, Einav, Klenow, Klopack, Levin, Levin and Best (2019) who estimate 4.3-6.1. For our main results we set $\sigma = 6$ which is at the high end of these estimates and close to the inverse of the average profit-to-sales ratio in our baseline sample, 5.9, that would be equal to σ in our model. For robustness we also consider $\sigma = 4$ and $\sigma = 8$ in Appendix A.2.

With these assumptions, the impact on consumer surplus of the program in which a share S of firms are treated in all markets $m \in M$, relative to the total mass of firms in treated markets, is

$$\frac{\Delta CS}{\sum_M n_m} \approx \frac{\zeta_R}{\sigma-1}(S\mu^T + \phi\mu^D S(1-S)) \cdot \bar{R}_C \tag{11}$$

where ζ_R is the IV estimate of the direct effect of borrowing on log revenue from Table 11. We can estimate the impact on producer surplus, that is, profits, with a reduced-form approach, using the coefficients of the profit IV regression:

$$\frac{\Delta PS}{\sum_M n_m} = (\zeta_\Pi + \xi_\Pi)(S\mu^T + S(1-S)\phi\mu^D) \tag{12}$$

where ζ_Π and ξ_Π are the direct and indirect profit effects from Table 11. Intuitively, each firm gets the loan with probability $S\mu^T + S(1-S)\phi\mu^D$, and if it does, experiences an average profit gain of ζ_Π; and each firm experiences a business stealing effect from the share of borrowing competitors $S\mu^T + S(1-S)\phi\mu^D$ of magnitude ξ_Π. As above, μ^T and $\phi\mu^D$ are coming from the take-up regression in column 2 of Table 3. These consumer and producer surplus effects are our main measures of the welfare gain from the loan.

Return on capital. To make comparisons with other credit interventions, it is helpful to also compute the return on capital. Following Banerjee and Duflo (2014), we define the private return to capital as the return that business owner would earn from injecting capital into the business. Equivalently, this definition measures the return accumulating to the bank and the borrower firm as a result of the loan. We can analogously define the social return on capital, which is the return that in addition accounts for the effect of the loan on market competitors and consumers.

Our objective is both to compute these returns to capital, and to decompose the social return into the contributions of the private return, business stealing and the consumer surplus. We do this under the assumption that all firms in a market are treated, so that there are no diffusion effects. We proceed in two steps which are explained in detail in Appendix A.1.5. First, we measure the average *yields* of the loan as estimated in our data. We do this by normalizing the components of the welfare gain computed above with the average loan amount, and making adjustments for the interest rate and the default rate which affect the bank's earnings. These yields cannot be directly interpreted as rates of return, because they are measured starting two years after the intervention—midline is two years after baseline—and it is plausible that in the first year yields are lower. In our second step we account for this by assuming that all yields in the first year are zero. We further assume that the yield in year 2 is what we computed, and that the yield in subsequent years depreciates at an annual rate $d = 0.10$.[24] We then compute the internal rate of return associated with this time path of yields. We use this procedure both to compute the private and social return, and to decompose the latter into the contribution of the private return, business

[24] Reduced-form regressions testing for heterogeneous effects over time (not reported) yield small and insignificant differences between effects at midline and endline, suggesting that a 10% depreciation is a conservative choice.

Table 12: Welfare gain estimates

Welfare gain per firm in market	Treat all firms		Treat 50% of firms	
	Share of Profit (%)	USD	Share of Profit (%)	USD
Producer Surplus	4.1	3,566	2.0	1,778
	(4.4)	(3,904)	(2.2)	(1,952)
	[-5, 12]	[-4,263, 10,752]	[-2, 6]	[-2,131, 5,376]
Consumer Surplus	12.7	11,139	6.3	5,565
	(4.6)	(4,022)	(2.3)	(2,011)
	[4, 22]	[3,929, 19,614]	[2, 11]	[1,965, 9,807]
Spillover			2.4	2,087
			(1.3)	(1,144)
			[0, 6]	[316, 4,918]
Total	16.7	14,696	10.7	9,430
	(7.3)	(6,415)	(4.9)	(4,281)
	[3, 32]	[2,724, 28,054]	[2, 21]	[1,508, 18,296]

Note: Bootstrap standard errors in round brackets, and bootstrap bias-corrected percentile confidence intervals in square brackets, are computed by bootstrapping our estimation procedure 1,000 times, drawing markets with replacement. USD values are calculated using the average annual exchange rate rate during midline and endline (6.465).

stealing and the consumer surplus.[25] For confidence intervals we bootstrap the entire procedure. Because the internal rate of return is not defined for negative yields, for the fewer than 1% of draws in which this occurs we make the conservative choice of setting the return equal to the yield.

5.3 Results

We first present the results on the welfare gain and then on the return to capital. Table 12 reports the implied impacts on the consumer surplus, the producer surplus and the total welfare gain. Bootstrapped standard errors and confidence intervals are reported in parentheses. All welfare gains are reported relative to the number of firms in the market (not relative to the number of firms treated) and are thus comparable across different treatment intensities S. The first two columns focus on the impact of treating all firms in the market. Column 1 reports impacts scaled by the profit of the average firm, while column 2 reports them in U.S. dollars. The first row shows that

[25] We compute the return from the private yield; the private yield plus the business stealing yield; and the private yield plus the business stealing yield plus the consumer surplus yield; the increments provide the decomposition.

treating all firms implies an insignificant gain in producer surplus amounting to 4% of profits. The second row shows that treating all firms implies a significant gain in consumer surplus amounting to 13% of profits, or about $11,000 per firm in the market. The total welfare gain is about 17% of profits, or close to $15,000 per firm. Because all firms are treated, we have no information diffusion effects.[26]

The next two columns report the results when 50% of firms are treated. Here we decompose both the consumer surplus and the producer surplus into a term measuring the impact of the loan program absent information diffusion, and another term measuring the additional impact of information diffusion.[27] We label the combined impact on the consumer and producer surplus of information diffusion the spillover effect. As the table shows, the per firm effects on producer and consumer surplus are halved relative to the case when all firms are treated; but now information diffusion generates additional gains which amount on average to 2.4% of firm profits or about $2,100 per firm in the market. These sizeable gains raise the question of whether it may be optimal to treat only a subset of firms and leverage diffusion. In our model, the additional gain in consumer surplus from treating one more firm, even if it is exposed to diffusion from all its peers, is $\gamma(\mu^T - \phi\mu^D)\bar{R}_C$, about $4,000, which is probably much larger than the marginal cost of treatment and suggests that in our setting it is optimal to treat all firms.

These results have two main implications. First, they show that the welfare gain from the loan program was substantial and mainly driven by the consumer surplus. The fact that the incidence of the welfare gain is on consumers, not producers, suggests that policies introduced to improve industrial performance—even if they improve the affected businesses—may not achieve their goal, but may nevertheless generate sizeable welfare gains. The second implication is that accounting for—potentially multiple—indirect effects can be essential for the welfare evaluation of firm policies. In our setting, accounting for the direct and indirect effects on firms, while ignoring the effect on consumers, would imply that the program generated insignificant and small welfare gains; whereas, as we have just seen, also accounting for the effect on consumers implies large and significant welfare

[26] The precision for the total welfare gain would be higher if we imposed the model-implied restriction that the business stealing effect cannot be stronger than the direct effect, i.e., that the market level demand curve slopes down, because it would imply that the producer surplus is negative with very low probability.

[27] The former is obtained by setting ϕ to zero in (11) respectively (12), and the latter is obtained as the residual.

Table 13: Return to capital decomposition

Private Return (%)	74.2
	(12.9)
	[46, 98]
Business Stealing (pp)	-56.3
	(23.4)
	[-104, -13]
Consumer Surplus (pp)	41.9
	(13.6)
	[16, 70]
Social Return (%)	59.8
	(21.8)
	[11, 98]

Note: Bootstrap standard errors in round brackets, and bootstrap bias-corrected percentile confidence intervals in square brackets, are computed by bootstrapping our estimation procedure 1,000 times, drawing markets with replacement. In draws with negative raw yields (<1% of cases), we approximate the internal rate of return with the yield.

gains.[28]

We now turn to the return on capital. Table 13 reports the implied returns and the decomposition. The private return is about 74%. Most of the private return is cancelled by business stealing. The social return is about 60% and mostly driven by gains in consumer surplus. The large gap between the private return to capital and bank deposit rates—which were below the loan interest rate—suggests that some friction limits lending below the privately efficient level. And the fact that the social return of the loan is also very high suggests that due to this friction large potential welfare gains are not realized.

It is useful to compare our results to estimates of the (private) return to capital obtained in other contexts. De Mel et al. (2008) estimate returns of 55-63% for microenterprises in Sri Lanka, while Banerjee and Duflo (2014) estimate a return of 105% for large firms in India. Our private return of 74% for SMEs falls between these estimates.[29] Our analysis may also contribute to understanding

[28] Appendix Table A7 replicates the welfare results for $\sigma = 4$ and $\sigma = 8$, and finds that our qualitative conclusions are robust: the program is estimated to generate large welfare gains mostly driven by the consumer surplus.

[29] Other experimental work in development estimates similarly high (uncompounded) annual returns to capital in

why the return to capital in developing countries is high. In our model, the private yield to borrower firms—of which the private return is a function—is proportional to the loan-induced improvement in quality-adjusted productivity (γ) *times* the potential for business stealing ($\sigma - 1$). Thus business stealing generates an amplification, through which even moderate improvements in productivity can result in high private returns. The same logic of amplification suggests that somewhat lower improvements in productivity should translate into substantially lower returns, thus predicting large differences in returns between developed and developing countries.

5.4 Discussion of assumptions for welfare evaluation

Other indirect effects. Our welfare results assume that there are no indirect effects other than those we consider in the analysis. Omitted indirect effects may include effects on competitors from outside the market, effects on suppliers, and general equilibrium effects through income or the wage. On outside competitors we expect at most weak effects: since there are no detectable impacts on market-level revenue, consumers do not seem to be reducing spending elsewhere. On suppliers, since we observe some switching, we do expect a reallocation effect. To the extent that treated firms are switching to higher-quality suppliers, this effect is likely to be welfare-enhancing. As to business owners' income, because the impact on the producer surplus is roughly zero, we expect at most a small effect driven by differences in the propensity to spend. Finally, concerning the wage, because the direct and indirect effects on employment roughly cancel, we expect at most small effects. In summary, we believe that the omitted indirect effects are either approximately zero or marginally positive, suggesting that our welfare evaluation may slightly underestimate the impact of the program.

Interest rate as cost of capital. Our measure of the welfare gain net of interest payments approximates the societal welfare impact of the program under the assumption that the interest rate measures the cost of capital. This is a natural assumption for evaluating the private gain from the loan program: since presumably the bank makes some profit on the loans, the interest rate is a plausible upper bound for their private cost of capital. However, for welfare evaluation the

microenterprises: McKenzie and Woodruff (2008) estimate 120-396% in Mexico, Fafchamps, McKenzie, Quinn and Woodruff (2014) estimate 180% in Ghana, and Field, Pande, Papp and Rigol (2013) estimate 156% in India.

relevant measure is not the private but the social cost of capital, which may well be different. For example, if the bank, instead of lending to our firms, uses the capital to buy government securities, that itself could have indirect effects on other actors. Because we have no evidence on the social cost of capital, our preferred interpretation of the estimates is that they measure the welfare gain from lending through this particular program. With this interpretation, comparing our (private and social) returns to those in alternative uses of the capital would lead to valid conclusions about the desirability of these different uses of capital.

Alternative loan allocation. A related question, motivated by Bertrand, Schoar and Thesmar (2007) and Sraer and Thesmar (2020) who find that bank lending sometimes inefficiently subsidizes under-performing firms, is whether a different loan allocation could have led to even larger welfare gains. This question is not the focus of the present paper, but our expectation is that better allocations may exist. Indeed, heterogeneous effect regressions show that loan impacts were higher for firms that were larger or had a more educated manager (Table A5), suggesting that targeting the loan to such firms could have increased allocational efficiency.

Omitted effects: default, exit, demand diffusion. We now discuss some effects that were not incorporated in the welfare analysis. In the welfare gain calculation we ignored the impact of loan default. Although we do not have direct data on default, the bank informed us that repayment rates for the two-year loan were over 98%, suggesting a default rate of less than 1% per year. Because the bank acts as a for-profit lender in the market, given this low rate we find it plausible that interest payments covered the bank's cost of capital including losses from default. Thus our approach of computing the welfare gain net of interest payments accounts for default as well.[30]

The analysis ignored firm exit. In Appendix A.1.6 we develop a model extension that incorporates exit, and highlights two new effects: (1) Because some borrowers exit, the effects of the program on consumer and producer surplus are reduced over time. (2) Borrowing increases survival, which, to the extent that borrowers are larger than average, increases the surplus. In the data the exit rate of borrowers is low, which implies that the first channel has small impact on our welfare results. In addition, at baseline borrowers are only slightly and insignificantly larger

[30] Even if the interest payments do not cover default, the 1% default rate implies that the quantitative impact of default on the welfare gain is small.

than the average firm, suggesting that the second effect is also plausibly small; moreover, ignoring it implies that our results likely underestimate the true welfare effect.

Finally, the analysis ignored demand diffusion. In Appendix A.1.7 we develop a model extension that formalizes diffusion as a random reallocation of demand which is not driven by improvements in quality or productivity. We show that such a reallocation, to a first-order approximation, has no effect on welfare, because the expected marginal utility of consuming the goods that experience a demand increase is the same as that of the goods that experience a demand decrease.

6 Conclusion

We estimated the direct and indirect effects of a loan program. We found that borrower firms provided higher quality at a lower price, that consumers valued these gains and reallocated their demand to borrowers, and that the net impact of these changes was a statistical null effect on producer surplus but a large increase in consumer surplus. We also found indirect effects operating through the diffusion of information and demand. We now discuss some caveats with and implications of these results.

We begin with external validity. A natural concern is that in our specialized retail markets business stealing effects may be especially strong. However, such settings seem fairly common in both developing and advanced countries (Jensen 2007, Hardy and McCasland 2021, Leonardi and Moretti 2022). Moreover, to our minds the important question for external validity is not whether the zero net effect on firms generalizes, but whether business stealing and other indirect effects can meaningfully influence industry performance. To think about this question, it is helpful to compare our results to the other studies of indirect effects cited in the Introduction.

Rotemberg (2019), like us, finds a large business stealing effect. However, in contrast to our results on improvements in quality and cost, he does not find evidence on improvements in firm productivity. Possible explanations for this difference may be that he studies a subsidy, not a loan program, or that his data do not allow measuring impacts on product quality. A second difference is that Rotemberg (2019) does not explore impacts on consumers. But the reallocation he documents suggests that consumers do benefit from lower prices or higher quality, indicating that there may be an impact on consumer surplus in his context as well. McKenzie and Puerto (2021), similarly to us, document improvements in business practices in response to their business training program. However, in contrast to us, they do not find evidence of business stealing effects. One key difference between their context and ours is that their business owners spent some of their income in the local market. This effect is likely absent in our specialized markets, and because it predicts that the extra revenue earned by treated businesses may be spent on untreated peers, it can help close the gap between their results and ours. Finally, Drexler et al. (2014) and Calderon et al. (2020) both find suggestive evidence for business stealing effects, consistent with our results. We conclude that comparisons with the literature are consistent with indirect effects on peer firms and consumers being active in other contexts.

Our results have implications for firm-level impact evaluations and for industrial policy. Concerning impact evaluations, our results suggest that a positive direct effect on firms, especially if these firms sell directly to consumers, likely reflects a gain in consumer surplus: after all, there must be a reason consumers increase their purchases at the treated firm. But the impact on producer surplus is less clear, as the direct effect may be partially offset by losses at untreated firms. Concerning industrial policy, our results suggest that firm policies introduced to improve industry outcomes such as employment—even if they increase employment at treated firms—may not achieve their goal due to business stealing effects; but at the same time these policies may increase consumer surplus and aggregate welfare.

Finally, our results provide new evidence that innovations which improve aggregate welfare can create economic losers, a force that may be important for understanding the development process. In particular, our analysis suggests that—because in our model business stealing impacts all firms, including treated non-borrowers—the loan program had a negative effect on the *majority* of firms in treated markets, so that if firms could have voted on the program while fully anticipating its implications, they should have voted it down. Of course this is purely hypothetical as in our context such a collective action was not possible. But our evidence does suggest that economic losers are an important consequence of development, so that there may be contexts in which they act as a development barrier.

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A Appendix

A.1 Model: assumptions, proofs and extensions

A.1.1 Assumptions about firm dynamics

We say that a distribution over a finite-dimensional Euclidean space is smooth if it has a continuous density. Let k_m be a vector of market-specific characteristics which summarizes the initial distribution of firm quality and productivity in the market, and is drawn from a smooth distribution K. Firm level latent characteristics b_i^t is a two-dimensional vector which characterizes the dynamics of realized quality and productivity. The initial value of this vector of latent characteristics, b_i^0, is drawn for each firm i from a smooth distribution $B^0(k_m)$ that depends continuously on k_m. The evolution of latent characteristics is given by $b_i^t = B(b_i^{t-1}, \eta_{bi}^t, \eta_{bm}^t)$ where the η_{bi}^t vector of firm-level shocks are i.i.d. across firms and over time and drawn from a smooth distribution, while the η_{bm}^t vector of market-level shocks are i.i.d. across markets and over time and drawn from a smooth distribution. The B function is differentiable with a continuous derivative. Realized log quality and productivity absent the intervention evolve as $\tilde{x}_i^t = b_i^t + g^t + \eta_{xi}^t$, where g^t is 2-dimensional vector representing a deterministic time trend, and η_{xi}^t is a 2-dimensional vector i.i.d. across firms and over time and drawn from a smooth distribution.

We assume that all these variables have well-defined means and variances. We also assume that the continuum law of large numbers holds (Sun 2006), so that cross-sectional averages in a realization are equal to the analogously-defined expected values of the same variables. We use the imprecise but convenient notation that the expectations operator denotes both the expectation in the probabilistic model, which is useful for some of the derivations, and the average across firms and over time in a realization, which is useful for analyzing the identifying assumptions.

A.1.2 Proof of Proposition 1

We first solve the model without making any assumptions about the treatment. Normalize the price of H to one. Start with the optimal allocation in market m given budget E_m. Maximizing

(1) subject to the budget constraint $\int_{i\in m} P_i Q_i di = E_m$ implies

$$Q_i = Q_m h_m^{\sigma-1} P_i^{-\sigma} l_m^{-\sigma}$$

for $i \in m$ where l_m is the multiplier on the constraint. Expressing $(h_i Q_i)^{1-1/\sigma}$ from this and integrating over i gives

$$\frac{1}{l_m} = \left(\int_{i\in m} \left(\frac{P_i}{h_i}\right)^{1-\sigma} di \right)^{\frac{1}{1-\sigma}}$$

which is the quality-adjusted price index for market m, and which we denote by P_m. It then follows directly that

$$\frac{h_i Q_i}{Q_m} = \left(\frac{P_i/h_i}{P_m}\right)^{-\sigma} \tag{13}$$

for $i \in m$ so that the quality-adjusted relative quantity relates with elasticity $-\sigma$ to the quality-adjusted relative price of a product in market m. Moreover rearranging this implies that $\int_{i\in m} P_i Q_i di = P_m Q_m$, justifying our definition of P_m.

Now consider maximization across markets. Given a budget E that we assume is sufficiently large that in the unconstrained optimum there is positive consumption of the numeraire, the consumer's problem can be rewritten as

$$E + \int Q_m^{1-1/\theta} dm - \int P_m Q_m dm$$

which implies the first-order condition

$$(1 - 1/\theta) Q_m^{\frac{-1}{\theta}} = P_m.$$

From this we can express demand for composite good m as

$$Q_m = \left(\frac{\theta}{\theta - 1}\right)^{-\theta} P_m^{-\theta}. \tag{14}$$

From these and the previous equations, given prices P_i and qualities h_i all quantities can be expressed.

Now consider firms' price-setting. Because a firm is small relative to the market, it does not take into account its impact on the price indices. As a result, maximizing profits $P_i Q_i - w L_i$ subject to the demand curve (13) yields the usual constant markup

$$P_i = \frac{\sigma}{\sigma - 1} \cdot \frac{w}{\omega_i}.$$

From (13) and (14), firm revenue can be written as

$$P_i Q_i = \left(\frac{\theta}{\theta - 1}\right) \left(\frac{P_i}{h_i}\right)^{1-\sigma} P_m^{\sigma-\theta} \tag{15}$$

for $i \in m$. This equation can be used to characterize revenue both with and without the treatment.

Effect of loan program on revenue. As (15) shows, the loan program can affect firm revenue in two ways: through the firm's quality-adjusted price P_i/h_i and through the market's quality-adjusted price index P_m. Given our assumption that the loan acts by changing quality and cost, so that for a borrower firm i, $\Delta \log h_i = \gamma_h$ and $\Delta \log \omega_i = \gamma_\omega$, the program's effect on the firm's log quality-adjusted price is $\Delta \log(P_i/h_i) = -(\gamma_\omega + \gamma_h) = -\gamma$.

To characterize the program's effect on the price index P_m, note that

$$\begin{aligned} P_m = \left(\int_{i\in m} \left(\frac{P_i}{h_i}\right)^{1-\sigma} di\right)^{\frac{1}{1-\sigma}} &= \left(\int_{i\in m} \left(\frac{\tilde{P}_i}{\tilde{h}_i}\right)^{1-\sigma} di\right)^{\frac{1}{1-\sigma}} \cdot (1 - \lambda_m Z_m + \lambda_m Z_m e^{\gamma(\sigma-1)})^{\frac{1}{1-\sigma}} \\ &= \tilde{P}_m \cdot (1 - \lambda_m Z_m + \lambda_m Z_m e^{\gamma(\sigma-1)})^{\frac{1}{1-\sigma}} \end{aligned} \tag{16}$$

where we used that borrowers represent a share Z_m of firms in market m, that $(\tilde{P}_i^t/\tilde{h}_i^t)^{1-\sigma}$ is proportional to revenue and—absent the treatment—treated firms' revenue is on average λ_m times that of the average firm, and that the loan multiplies $\tilde{P}_i/\tilde{h}_i$ by $e^{-\gamma}$. It follows that

$$\Delta \log P_m = \frac{\log(1 - \lambda_m Z_m + \lambda_m Z_m e^{\gamma(\sigma-1)})}{1-\sigma}. \tag{17}$$

At $\gamma = 0$ the term on the right-hand side equals zero and has derivative in γ of $-\lambda_m Z_m$. Thus to a first order approximation

$$\Delta \log P_m \approx -\gamma \lambda_m Z_m.$$

Substituting this to (15) implies that the change in firm revenue in response to the treatment is

$$\Delta P_i Q_i \approx (\sigma - 1)\gamma \cdot B_i - (\sigma - \theta)\gamma\lambda_m \cdot Z_m \tag{18}$$

for $i \in m$, as claimed.

A.1.3 Proof of Proposition 2

Take-up. We introduce the notation that for any variable X, the conditional expectation $E[X|t,m]$ means conditioning on characteristics specific to m (that is, the realized k_m) and t (that is, market-level shock realizations up to and including t) absent the treatment, while for example the conditional expectation $E[X|t,m,T_i,S_m]$ means also conditioning on the treatment assignment (T_i, S_m). Let $\bar{x}_i = (\tilde{x}_i^1, \tilde{x}_i^s)$ denote the vector of firm characteristics absent the treatment that influence whether firm i borrows either in period 1 due to the treatment or in period s due to diffusion. The probability that a firm with characteristics $\bar{x}_i$ in market m is a borrower in period t, conditional on its treatment status, is

$$E[B_i^t|t,m,\bar{x}_i,T_i,S_m] = T_i \cdot F^T(\tilde{x}_i^1) \cdot 1_{\{t\geq 1\}} + \phi(1-T_i)S_m \cdot F^D(\tilde{x}_i^s) \cdot 1_{\{t\geq s\}}. \tag{19}$$

We note here that this equation as well as the rest of the derivation would work identically if F^T and F^D were also dependent on the market-level characteristics k_m. Observe that

$$E[F^T(x_i^1)|T_i,S_m] = E[F^T(x_i^1)] = \mu^T$$

and

$$E[F^D(x_i^s)|T_i,S_m] = E[F^D(x_i^s)] = \mu^D$$

because each firm in M is equally likely to get a particular treatment assignment (T_i, S_m). Taking the conditional expectation of (19) with respect to T_i and S_m in period t, it follows that for $t \geq s$,

$$E[B_i^t|t,T_i,S_m] = \mu^T \cdot T_i + \phi\mu^D \cdot (1-T_i)S_m$$

and hence

$$B_i^t = \mu^T \cdot T_i + \phi\mu^D \cdot (1-T_i)S_m + \eta_i^t$$

where $E[\eta_i^t|T_i,S_m] = 0$, as claimed in the Proposition.

Averaging equation (19) across all firms in market m implies

$$E[Z_m^t|t,m,S_m] = S_m E[F^D(x_i^1)|m] \cdot 1_{\{t\geq 1\}} + \phi(1-S_m)S_m E[F^T(x_i^s)|m] \cdot 1_{\{t\geq s\}},$$

and because $Z_t^m = E[Z_m^t | t, m, S_m]$, we can write

$$Z_m^t = S_m \cdot \mu_m^T \cdot 1_{\{t \geq 1\}} + \phi(1 - S_m) S_m \cdot \mu_m^D \cdot 1_{\{t \geq s\}} \tag{20}$$

where $\mu_m^T = E[F^D(x_i^1)|m]$ and $\mu_m^D = E[F^T(x_i^s)|m]$ are the average take-up rates due to treatment respectively diffusion in market m.

Firm revenue. We can write

$$\log P_i^t Q_i^t = \Delta \log P_i^t Q_i^t + \log \tilde{P}_i^t \tilde{Q}_i^t \tag{21}$$

where, as before, tilde denotes outcomes absent the intervention. Rewriting the second term by applying Proposition 1 to period t, firm revenue is

$$\log R_i^t \approx \log \tilde{R}_i^t + (\sigma - 1)\gamma \cdot B_i^t - (\sigma - \theta)\gamma \lambda_m^t \cdot Z_m^t.$$

We now show that $\log \tilde{R}_i^t$ is orthogonal to the residualized instruments defined by projecting the instruments on the firm fixed effects and $Post^t$ and taking the residual. To see why, first note that the residualized instruments can be computed as $Post^t \cdot (T_i - \bar{S})$ and $Post^t \cdot (S_m - \bar{S})$ where $\bar{S} = E[S_m] = E[T_i]$ is the average treatment intensity. The required orthogonality conditions are then

$$E[\log \tilde{R}_i^t \cdot Post^t \cdot (T_i - \bar{S})] = 0$$

and

$$E[\log \tilde{R}_i^t \cdot Post^t \cdot (S_m - \bar{S})] = 0$$

which both hold because T_i and S_m are randomly assigned independently of the $\tilde{R}_i^t$ realizations. Now decompose $\log \tilde{R}_i^t$ into the sum of a component spanned by the firm effects and $Post^t$, and another component, denoted ν_i^t, orthogonal to the firm effects and $Post^t$. Because $\log \tilde{R}_i^t$ is orthogonal to the residualized instruments, and by construction so is the first component, it follows that so is ν_i^t. But then ν_i^t, which is orthogonal to the firm effects and $Post^t$ is also orthogonal to the instruments themselves. Thus we can write

$$\log R_i^t \approx f_i + \kappa \cdot Post^t + (\sigma - 1)\gamma \cdot B_i^t - (\sigma - \theta)\gamma \lambda_m^t \cdot Z_m^t + \nu_i^t \tag{22}$$

where $E[\nu_i^t \times (1_j, Post^t, Post^t \cdot T_i, Post^t \cdot S_m] = 0$ holds for all j.

Substituting average selection. In the second stage equation (22) the business stealing term involves λ_m^t rather than λ. For any fixed λ rewrite the equation as

$$\log P_i^t Q_i^t \approx f_i + \kappa \cdot Post^t + (\sigma - 1)\gamma \cdot B_i^t - (\sigma - \theta)\gamma\lambda \cdot Z_m^t + (\sigma - \theta)\gamma(\lambda - \lambda_m^t) \cdot Z_m^t + \nu_i^t.$$

We show below that with appropriate choice of λ the new term $(\lambda - \lambda_m^t) \cdot Z_m^t$ is orthogonal to the residualized instruments. This implies that $(\lambda - \lambda_m^t) \cdot Z_m^t$ can be decomposed into a component spanned by the firm and time effects and another component, denoted u_i^t, orthogonal to the firm and time effects and the instruments. It then follows that after replacing λ_m by λ, and changing the fixed effects, the orthogonality conditions for the IV will hold with the new error term $\varepsilon_i^t = u_i^t + \nu_i^t$.

Since the residualized instruments are $Post^t \cdot (T_i - \bar{S})$ and $Post^t \cdot (S_m - \bar{S})$, the required orthogonality conditions are

$$E[(\lambda - \lambda_m^t) Z_m^t \cdot (S_m - \bar{S}) Post^t] = 0$$

and

$$E[(\lambda - \lambda_m^t) Z_m^t \cdot (T_i - \bar{S}) Post^t] = 0.$$

Because in the second condition all terms except T_i depend only on m and t, and because the mean of T_i in market m is S_m, the first condition implies the second:

$$E[E[(\lambda - \lambda_m^t) Z_m^t \cdot (T_i - \bar{S}) Post^t] | t, m, S_m] = E[(\lambda - \lambda_m^t) Z_m^t \cdot (S_m - \bar{S}) Post^t].$$

The first condition would hold if

$$\lambda = \frac{E[\lambda_m^t \cdot Z_m^t (S_m - \bar{S}) Post^t]}{E[Z_m^t \cdot (S_m - \bar{S}) Post^t]}, \tag{23}$$

which gives us a set of candidate weights

$$a_m^t = E[Z_m^t (S_m - \bar{S}) Post^t | t, m]$$

where defining the weights as these conditional expectations does not change the weighted average because λ_m^t depends only on characteristics specific to t and m. This definition of λ ensures that the orthogonality conditions hold. We also need to verify that a_m^t are non-negative. To do this, recall from (20) that

$$Z_m^t = S_m \mu_m^T \cdot 1_{\{t \geq 1\}} + \phi(1 - S_m) S_m \mu_m^D \cdot 1_{\{t \geq s\}}.$$

Using this, we can rewrite a_m^t as

$$a_m^t = \mu_m^T \cdot 1_{\{t \geq 1\}} E[S_m(S_m - \bar{S})] + \phi \mu_m^D \cdot 1_{\{t \geq s\}} E[(1 - S_m) S_m (S_m - \bar{S})] \tag{24}$$

where we used that the distribution of S_m conditional on t and m is the same as its unconditional distribution due to the random assignment. Now note that $E[S_m(S_m - \bar{S})]$ is the variance of S_m and hence non-negative. Given this, our assumption that $\mu_m^T > \phi \mu_m^D$ implies that the sign of a_m^t when $t \geq 1$ is not lower than the sign of

$$E[(S_m + (1 - S_m) S_m) \cdot (S_m - \bar{S})],$$

which is the covariance between $S_m + (1 - S_m) S_m$ and S_m, and since both are non-decreasing functions of S_m, is non-negative.

A.1.4 Proof of Proposition 3

We first develop a characterization of welfare in any period that is valid with or without the treatment. Let $E_d = \int_m P_m Q_m dm$ denote expenditure on the differentiated products and Π denote total profits by all firms producing differentiated products. The consumer's budget constraint is

$$wL + \Pi = E = H + E_d. \tag{25}$$

We assume throughout that L is sufficiently large so that $H > 0$. Then the consumer's maximized utility, or welfare, can be written as

$$wL + \Pi + \int Q_m^{1-1/\theta} dm - \int P_m Q_m dm.$$

Since L is fix, the treatment can affect welfare by changing the producer surplus Π or the consumer surplus

$$CS = \int Q_m^{1-1/\theta} dm - \int P_m Q_m dm.$$

From (14) we can express the consumer surplus as

$$CS = \left(\frac{\theta}{\theta-1}\right)^{1-\theta}\int_m P_m^{1-\theta}dm - \left(\frac{\theta}{\theta-1}\right)^{-\theta}\int_m P_m^{1-\theta}dm = \frac{1}{\theta-1}\left(\frac{\theta}{\theta-1}\right)^{-\theta}\int_m P_m^{1-\theta}dm$$

To use this formula for period t, recall that

$$P_m^t = \tilde{P}_m^t(1-\lambda_m^t Z_m^t + \lambda_m^t Z_m^t e^{\gamma(\sigma-1)})^{\frac{1}{1-\sigma}} \tag{26}$$

thus

$$\Delta CS^t = \frac{1}{\theta-1}\left(\frac{\theta}{\theta-1}\right)^{-\theta}\int_m (\tilde{P}_m^t)^{1-\theta}[(1-\lambda_m^t Z_m^t + \lambda_m^t Z_m^t e^{\gamma(\sigma-1)})^{\frac{1-\theta}{1-\sigma}} - 1]dm$$

and differentiating the last term with respect to γ at $\gamma = 0$ gives $(\theta-1)\lambda_m^t Z_m^t$ and hence

$$\Delta CS^t \approx \frac{1}{\theta-1}\left(\frac{\theta}{\theta-1}\right)^{-\theta}\int_m (\tilde{P}_m^t)^{1-\theta}(\theta-1)\gamma\lambda_m^t Z_m^t dm = \gamma\int_m \tilde{R}_m^t \lambda Z_m^t dm.$$

Finally, using (20), for $t \geq s$ we can write

$$\Delta CS^t \approx \int_m [S_m\mu_m^T + \phi(1-S_m)S_m\mu_m^D]\lambda_m^t\gamma\tilde{R}_m^t dm.$$

A.1.5 Return on capital

We proceed in two steps. First we measure different yield components relevant for the midline and endline using our estimates. We measure the average private yield in the midline and endline with the profit direct effect IV estimate ζ_Π as

$$\textit{Private yield} = \frac{\zeta_\Pi}{\textit{Avg loan}} + \textit{Interest rate} - \textit{Default rate}.$$

The first term is the yield accumulating to borrowers as profits, while the second and third terms measure the net yield accumulating to the bank. To compute the social yield, this private yield needs to be adjusted by business stealing and the consumer surplus. We measure the contribution of business stealing with the profit indirect effect IV estimate ξ_Π as

$$\textit{Business stealing yield} = \frac{\xi_\Pi}{\textit{Avg loan}}$$

where the business stealing effect of the loan on profits is normalized by the average loan size. And we measure the contribution of the consumer surplus as

$$\textit{Consumer surplus yield} = \frac{\frac{\zeta_R}{\sigma-1}\bar{R}_C}{\textit{Avg loan}}$$

where the gain in consumer surplus from the loan is normalized by loan size.

In our second step, we assume that the yield in year 1 are zero, the yield in year 2 is as computed above and denoted by y, and the yield in year $t \geq 2$ is $y(1-d)^{t-2}$ where d is the depreciation rate. We then compute the internal rate of return by noting that with a discount rate ρ, the present discounted value of this payment stream is

$$\frac{y}{1+\rho}\frac{1}{\rho+d}$$

which needs to be equal to 1 if ρ is the internal rate of return. This yields a quadratic equation that gives $\rho = [(4y+1+d^2-2d)^{1/2}-d-1]/2$. We solve for ρ when y is the private yield; when y is the private yield plus the business stealing yield; and when y is the private yield plus the business stealing yield plus the consumer surplus yield. The resulting ρ values, and the increments between them, give us the private return, the additional contribution of business stealing, the additional contribution of consumer surplus, and the social return. We conduct this calculation using an annual depreciation rate of $d = 0.10$.

A.1.6 Exit

Our theoretical analysis so far has assumed that all firms stay alive and stay in the sample. But in practice some firms leave because they exit the market, and some firms do not answer the survey even if they stay in the market. We now discuss the impacts of exit and attrition on our empirical approach and welfare calculations.

Regression estimates. Let A_i^t be an indicator for whether firm i is in the sample in period t. Thus $A_i^t = 0$ represents both exit and attrition. Let $\bar{\varepsilon}_i^t$ denote the residualized variable obtained by projecting ε_i^t on the firm effects and *Post* conditional on $A_i^t = 1$, and taking the residual. Then the identifying condition for (5), conditional on observations with $A_i^t = 1$, becomes

$$E[\bar{\varepsilon}_i^t \times (Post^t \cdot T_i, Post^t \cdot S_m)|A_i^t = 1] = 0.$$

This condition is not testable, but we can test the related condition at baseline

$$E[\varepsilon_i^0 \times (T_i, S_m)|A_i^\tau = 1] = 0$$

that is, whether the characteristics of firms that stay in the sample are correlated with the treatment variables at baseline. Table A2 below shows no such correlation at baseline for several key outcomes, providing evidence against exit or attrition inducing bias in our estimates.

Welfare effects. Conditional on the coefficient estimates being correct, attrition does not affect the welfare calculations as long as the firms that do not answer the survey behave as described by our model. But exit is outside the model, and we need to incorporate it to characterize its impact on welfare.

Motivated by the result that the treatment reduces the probability of exit, assume that each period $t \geq 1$ a share x_B of borrowers and a share x_N of non-borrowers exit. Assume that exiting borrowers as well as exiting non-borrowers are drawn randomly from the set of borrowers respectively non-borrowers in the market. This assumption is consistent with the data: at baseline the average revenue of exiting firms is not significantly different from that of all firms, and the average revenue of exiting borrowers is not significantly different from that of all borrowers.[31] For simplicity we assume that the exit rate of firms borrowing due to information diffusion—which only borrow in period $s \geq 1$—is already x_B starting from period 1. Since we model exit, we also need to model entry. Suppose that exiting firms are replaced by firms that are drawn from the distribution of firms in the market absent the intervention. Assume that replacement firms do not borrow. Finally, for ease of notation, in the derivation below we ignore depreciation, but it can be easily added by discounting γ with powers of $1-d$ where d is the depreciation rate.

Now consider an intervention in which a share S of firms are treated in a set of markets M. As in the main text, abstract away from cross-market heterogeneity. Similarly to the main text, assume the market-level selection coefficients λ_m^t are all identical to some value λ^0, but, differently from the main text, allow λ^0 to be different from one. Let $Z = S\mu^T + S(1-S)\phi\mu^D$ denote the share of borrowers in the market absent exit.

In a period $t \geq s$ we can write the market level price index as

$$P_m^t = \tilde{P}_m^t(1 + \lambda^0(e^{\gamma(\sigma-1)} - 1)Z(1-x_B)^t + [(1-x_B)^t - (1-x_N)^t]Z(\lambda^0 - 1))^{\frac{1}{1-\sigma}}.$$

[31] When borrowers are larger than the average firm, our assumption would imply that exiting borrowers are larger than the average exiting firm, which seemingly contradicts with our finding that treated and untreated surviving firms are similar at baseline. But borrower firms are only slightly and insignificantly larger than the average firm.

There are two novelties relative to the analogous expression (26) in the proof of Proposition 3. First, the share of borrowers in period t is no longer Z, but is instead $Z(1-x_B)^t$ due to exit. These firms charge a quality-adjusted price which is reduced by factor e^{γ}. Second, a share $[(1-x_B)^t-(1-x_N)^t]Z$ of firms survived to period t because of borrowing. These firms are on average λ^0 times as large as the average firm, explaining the last term in the expression.

Using the previous formula, we take a first-order approximation of the impact of the intervention on the price index raised to $1-\theta$, around the point $\gamma = 0$ and $\lambda^0 = 1$:

$$(P_m^t)^{1-\theta} - (\tilde{P}_m^t)^{1-\theta} \approx (\tilde{P}_m^t)^{1-\theta} \frac{1-\theta}{1-\sigma} \left[Z(1-x_B)^t(\sigma-1)\gamma + [(1-x_B)^t - (1-x_N)^t]Z(\lambda^0-1) \right].$$

This implies that

$$\begin{aligned}
\Delta CS^t &= \frac{1}{\theta-1}\left(\frac{\theta}{\theta-1}\right)^{-\theta} \int_m (P_m^t)^{1-\theta} - (\tilde{P}_m^t)^{1-\theta} dm \\
&\approx \left(\frac{\theta}{\theta-1}\right)^{-\theta} \int_m (\tilde{P}_m^t)^{1-\theta} dm \cdot \left[Z(1-x_B)^t\gamma + \frac{[(1-x_B)^t - (1-x_N)^t]Z(\lambda^0-1)}{\sigma-1} \right] \\
&= \int_m \tilde{R}_m^t dm \cdot \left[Z(1-x_B)^t\gamma + \frac{[(1-x_B)^t - (1-x_N)^t]Z(\lambda^0-1)}{\sigma-1} \right].
\end{aligned}$$

Finally, approximating revenue absent the intervention with revenue in control markets $\bar{R}_C$ yields

$$\frac{\Delta CS^t}{\sum_M n_m} \approx \frac{\zeta_R}{\sigma-1} Z \cdot \bar{R}_C (1-x_B)^t + [(1-x_B)^t - (1-x_N)^t] Z \frac{\lambda^0-1}{\sigma-1}\bar{R}_C. \tag{27}$$

In this formula, the first term is that in (11) modified by $(1-x_B)^t$: the reduction in quality-adjusted prices is coming only from borrowers, whose share declines with exit. The second term is new and represents the impact of the reduction in the exit rate induced by borrowing. This reduction implies that a share $(1-x_B)^t - (1-x_N)^t$ of borrowers are saved from being replaced by average firms. To the extent that these firms have above-average revenue (λ^0-1), their quality adjusted price must be lower ($1/(\sigma-1)$ is the elasticity of price to revenue), so that their survival reduces the quality-adjusted price index.

Finally consider the producer surplus. A back-of-the envelope approach to incorporate the impact of exit is to compute

$$\frac{\Delta PS}{\sum_M n_m} \approx (\zeta_\Pi + \xi_\Pi) Z (1-x_B)^t. \tag{28}$$

The novelty is $(1 - x_B)^t$: the share of borrowers—which generates both the direct and business stealing effect—is declining due to exit. This approach ignores the possible effect that the survival of larger-then-average borrowers may increase the producer surplus, and that the presence of those borrowers may intensify competition and reduce the producer surplus. Because borrowers are only slightly and insignificantly larger, we ignore these forces.

To operationalize these formulas, assume that $\lambda^0 = 1$, and because the share of borrowers that exit by endline is 1.95%, that $x_B = 0.07$ per year. Then the impact of exit on the producer surplus and the consumer surplus by the endline is less than 2 percent. We ignore this effect in the text.

Return to capital. To adjust the calculation of return to capital to exit, we need to bring back depreciation. Formally, depreciation is almost equivalent to exit, as both forces discount future yields. The difference is that we assume depreciation starts only after period 2, whereas exit also operates in periods 1 and 2. Thus incorporating an annual exit rate of 0.007 roughly amounts to discounting the annual yield by twice this amount and increasing the depreciation rate by 0.007. The combined impact of these changes is roughly a 2 percent reduction in the returns. We ignore this effect in the main text.

A.1.7 Demand diffusion

Model. We model the demand externality for the special case in which (i) there is no diffusion ($\phi = 0$), and (ii) there is no heterogeneity in λ ($\lambda^t_m = \lambda^0$). This special case is not a bad approximation of reality, and it makes the analysis simple and transparent. We assume that the intervention generates an indirect effect in which a share D_m of firms in market m experience demand diffusion. We further assume that demand diffusion is governed by the share of non-competitors in the same marketplace which are treated, and can be written as $D_m = aS_m + v_m$ where v_m captures sampling variation and is independent of all realizations absent the intervention. The assumption that demand diffusion depends directly on the share who are treated, rather than the share who borrow, simplifies the logic, as otherwise we would need to instrument the latter with the former.

Experiencing demand diffusion means that the firm's perceived quality is shifted by a factor e^{χ} where $\chi \geq 0$. This is compensated for by a shift in the perceived quality of all competitors in market m of $e^{-D_m\chi}$ so that to a first-order approximation average quality is unchanged. Let X_i be an indicator for whether firm i experiences the demand externality, then the perceived quality $\bar{h}_i$ of firm i satisfies $\log \bar{h}_i = \log h_i + \chi X_i - \chi D_m$. We assume that the demand externality is a mistake, so that actual quality is still given by h_i. We use the convention that bar denotes qualities and prices misperceived by the consumer. We do not include bars in the notation for quantities.

Estimating equation. The price index $\bar{P}_m$ for market m satisfies

$$\begin{aligned}\bar{P}_m &= \left(\int_{i\in m}\left(\frac{P_i}{\bar{h}_i}\right)^{1-\sigma} di\right)^{\frac{1}{1-\sigma}} \\ &= \left(\int_{i\in m}\left(\frac{P_i}{h_i}\right)^{1-\sigma} di\right)^{\frac{1}{1-\sigma}} \cdot \left(D_m e^{\chi(1-D_m)(\sigma-1)} + (1-D_m)e^{-\chi D_m(\sigma-1)}\right)^{\frac{1}{1-\sigma}}.\end{aligned}$$

The log of the second term at $\chi = 0$ is zero, and its derivative with respect to χ is

$$\frac{1}{1-\sigma}\frac{\partial}{\partial\chi}\log(D_m e^{\chi(1-D_m)(\sigma-1)} + (1-D_m)e^{-\chi D_m(\sigma-1)}) = D_m(1-D_m)(\sigma-1) - D_m(1-D_m)(\sigma-1) = 0$$

thus, to a first order approximation in χ, $\log \bar{P}_m \approx \log P_m$.

By (15) in the proof of Proposition 1, firm revenue is

$$P_i Q_i = \left(\frac{\theta}{\theta-1}\right)\left(\frac{P_i}{\bar{h}_i}\right)^{1-\sigma} \bar{P}_m^{\sigma-\theta}. \tag{29}$$

Since $\log \bar{P}_m \approx \log P_m$, substituting in $\bar{h}_t$ implies that in period $t \geq 1$

$$\Delta \log R_i^t \approx (\sigma-1)\gamma\mu^T \cdot T_i - (\sigma-\theta)\gamma\mu^T\lambda \cdot S_m + (\sigma-1)\chi \cdot X_i - (\sigma-1)\chi \cdot D_m$$

and hence revenue is

$$\begin{aligned}\log R_i^t \approx &(\sigma-1)\gamma\mu^T \cdot Post^t \cdot T_i - (\sigma-\theta)\gamma\lambda\mu^T \cdot Post^t \cdot S_m \\ &+ (\sigma-1)\chi \cdot Post^t \cdot X_i - (\sigma-1)\chi \cdot Post^t \cdot D_m + \kappa \cdot Post^t + f_i + \varepsilon_i^t.\end{aligned} \tag{30}$$

Here ε_i^t is defined exactly as before, and in this special case with no λ heterogeneity only depends on realizations absent the intervention. Thus ε_i^t is orthogonal to $Post^t \cdot T_i$ and $Post^t \cdot S_m$, and is also orthogonal to $Post^t \cdot X_i$ and to $Post^t \cdot D_m$ because X_i is randomly assigned conditional on D_m and $D_m = aS_m + v_m$ with v_m independent of realizations absent the intervention. This equation motivates our empirical approach to estimating the direct and indirect effects induced by demand diffusion.

Welfare. To characterize the welfare effect of demand diffusion, note that due to consumer optimization in the presence of misperception

$$\frac{\bar{h}_i Q_i}{Q_m} = \left(\frac{P_i/\bar{h}_i}{P_m}\right)^{-\sigma} \tag{31}$$

which implies that

$$(h_i Q_i)^{\frac{\sigma-1}{\sigma}} = \left(\frac{\bar{h}_i}{h_i}\right)^{\frac{(\sigma-1)^2}{\sigma}} Q_m^{\frac{\sigma-1}{\sigma}} \left(\frac{P_i/h_i}{P_m}\right)^{1-\sigma}. \tag{32}$$

The true (not misperceived) utility from consuming goods in market m is then

$$\begin{aligned} Q_m^{\frac{\sigma-1}{\sigma}} \int_{i\in m} & \left(\frac{\bar{h}_i}{h_i}\right)^{\frac{(\sigma-1)^2}{\sigma}} \left(\frac{P_i/h_i}{P_m}\right)^{1-\sigma} di \\ &= Q_m^{\frac{\sigma-1}{\sigma}} \int_{i\in m} \left(\frac{P_i/h_i}{P_m}\right)^{1-\sigma} di \cdot (D_m e^{\chi(1-D_m)\frac{(\sigma-1)^2}{\sigma}} + (1-D_m)e^{-\chi D_m \frac{(\sigma-1)^2}{\sigma}}) \end{aligned} \tag{33}$$

because of the random assignment of demand diffusion. Because of a logic parallel to establishing that $\log \bar{P}_m \approx \log P_m$, the log of the second term, to a first-order approximation, is zero, implying that demand diffusion, to a first order approximation, has no impact on the utility enjoyed from consuming goods in market m. Because the market price index is also approximately the same as without it, demand diffusion has approximately no impact on spending either. Because it has approximately no impact on the market level utilities and spending, demand diffusion has approximately no impact on the consumer surplus. And because profits in the differentiated markets are proportional to revenue which equals spending on these markets, demand diffusion has approximately no impact on the producer surplus either.

A.2 Additional empirical results

Markets. Table A1 reports the distribution of markets across broad industry categories.

Baseline balance of firms that remain in sample. Table A2 reports balance tests at baseline of firms that remain in the sample up to the 2016 endline survey (columns 1-4) or up to the 2020 follow-up survey (columns 5-8). Similarly to Tables 1 and 2, these tables report the results of

Table A1: Distribution of broad product categories across markets

Industry	**Number of markets**
Building materials	15
Furniture	9
Cloth and shoes	12
Food	11
Electronics	8
Vehicle	6
Textile	5
Daily necessities	4
Entertainment suppliers and toys	3
Industrial park	4
Hardware	1
Total	78

regressing the dependent variable on a constant and on four indicators of different treatment arms: treated firm in a 50% market, untreated firm in a 50% market, treated firm in an 80% market and untreated firm in an 80% market. There are no significant differences across treatment arms, suggesting that selection in exit or attrition is unlikely to meaningfully affect our estimates.

Table A2: Baseline balance of firms that remain in sample

Sample:	Firms in Sample up to 2016 Endline				Firms in Sample up to 2020 Follow-up			
Dep. var., at baseline:	Sales	Profit	Number of employees	Number of Clients	Sales	Profit	Number of employees	Number of Clients
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated * 50% market	35.89	-2.302	1.305	-1.962	11.22	-5.390	1.139	-1.894
	(84.73)	(11.96)	(1.241)	(2.048)	(88.83)	(12.98)	(1.096)	(1.700)
Untreated * 50% market	10.70	-3.050	0.030	0.878	3.096	-5.875	0.325	-0.230
	(56.16)	(8.339)	(1.011)	(2.204)	(77.99)	(10.25)	(1.086)	(2.553)
Treated * 80% market	3.785	-1.100	0.016	0.416	-0.715	-2.652	-0.061	-0.321
	(51.43)	(7.661)	(0.699)	(1.631)	(70.93)	(10.81)	(0.863)	(1.655)
Untreated * 80% market	-15.00	-0.494	0.095	2.653	-29.74	-4.453	-0.059	1.916
	(39.33)	(7.115)	(0.662)	(1.980)	(56.32)	(9.801)	(0.812)	(2.015)
Constant	324.6***	51.75***	8.741***	28.62***	344.0***	54.36***	8.877***	29.39***
	(35.81)	(5.882)	(0.551)	(1.106)	(53.34)	(8.829)	(0.702)	(1.181)
Observations	2,482	2,482	2,482	2,482	1,804	1,804	1,804	1,804

Note: Baseline balance tests of firms that do not exit or attrit. Dependent variables are measured at baseline. In columns 1-4 sample is all firms that we surveyed in the baseline, midline and endline long surveys. In columns 5-8 sample is the subset of firms that we also surveyed in the 2020 follow up. Standard errors clustered at the market level. *** p<0.01, ** p<0.05, * p<0.1.

Indirect effects at baseline. Table A3 reports a placebo specification in which outcomes *at baseline* are regressed on the treatment and the share of competitors treated. Because the treatment took place after the baseline survey we expect no significant impacts. This is what we find, providing an additional balance test and evidence against regression misspecification.

Table A3: Specification check: Effects at baseline

Dep. var.:	log Sales	Profit (10,000 RMB)	log Number of Employees	log Wage Bill	Fixed Assets (10,000 RMB)	log Material Cost
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	-0.008	1.413	0.003	0.006	1.274	-0.027
	(0.041)	(4.047)	(0.038)	(0.040)	(2.552)	(0.051)
Share Competitors Treated	-0.024	-3.885	-0.031	-0.032	0.181	0.012
	(0.052)	(5.123)	(0.048)	(0.051)	(3.302)	(0.064)
Observations	3,173	3,173	3,173	3,167	3,173	3,173

Note: Standard errors clustered at the market level. *** p<0.01, ** p<0.05, * p<0.1.

Other business outcomes. Table A4 reports our main reduced form specification with other business practices as the outcome variables. The results are discussed in Section 4.2.

Table A4: Direct and indirect effects: Other business outcomes

Dep. var.:	Trade Credit Supplier	Trade Credit Client	Advertising Cost	log Markup	log Rent	log Number of Suppliers	Other Loan	log Reported - log Book Sales
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post*Treated	0.068***	0.075***	0.210*	0.018	0.048	0.051	0.025	-0.003
	(0.014)	(0.020)	(0.106)	(0.024)	(0.099)	(0.034)	(0.025)	(0.002)
Post*Share Competitors Treated	-0.041**	-0.072***	0.037	-0.035	0.011	-0.041	-0.022	0.003
	(0.018)	(0.023)	(0.075)	(0.029)	(0.162)	(0.035)	(0.035)	(0.003)
Firm FE and Post	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Observations	8,222	8,221	8,220	8,612	8,220	8,716	8,612	5,167

Note: In column 4 markup is defined as the ratio of revenue to cost, the latter measured as total wage and material spending. In column 8 the sample only contains observations from the mid- and endline surveys, in which enumerators took the book value of sales directly from the firm's book. In that regression we do not include firm fixed effects or Post. Standard errors clustered at the market level. *** p<0.01, ** p<0.05, * p<0.1.

Heterogeneous effects. To explore heterogeneous effects by some baseline firm characteristic X_i, we extend our main specification (8) by including interactions of X with all right-hand-side variables. The coefficients of interest and the triple interactions corresponding to heterogeneity in the direct and in the indirect effects. Table A5 shows these interactions for three characteristics X. The general pattern in the table is that estimates are not as robustly and consistently significant as in our main effects, but the direct effects—which are stronger—tend to reveal plausible patterns.

Table A5: Heterogeneous effects on main outcomes

Dep. var.:	log Sales	Profit (10,000 RMB)	log Number of Employees	log Material Cost	log Number of Clients
	(1)	(2)	(3)	(4)	(5)
A. X=log Employment					
Post*Treated*X	0.077**	-4.954	0.189***	0.070	0.055
	(0.034)	(7.908)	(0.054)	(0.048)	(0.055)
Post*Share Competitors Treated*X	-0.067	3.824	-0.169**	0.030	-0.055
	(0.060)	(13.00)	(0.070)	(0.069)	(0.062)
B. X=Education					
Post*Treated*X	0.174*	5.371	0.018	0.148*	0.068
	(0.096)	(7.002)	(0.079)	(0.087)	(0.061)
Post*Share Competitors Treated*X	-0.052	10.65	-0.057	-0.030	-0.037
	(0.110)	(9.715)	(0.104)	(0.113)	(0.071)
C. X=Political Connection					
Post*Treated*X	0.121	17.16	-0.052	0.196	-0.063
	(0.108)	(13.00)	(0.071)	(0.124)	(0.072)
Post*Share Competitors Treated*X	-0.062	-11.13	0.116	-0.144	0.112
	(0.136)	(19.57)	(0.097)	(0.153)	(0.094)
Firm FE	Yes	Yes	Yes	Yes	Yes
Observations	8,612	8,612	8,612	8,612	8,612

Note: Each panel reports heterogeneous effect regressions with respect to a different variable, denoted X. All regressions control for Post and its interactions with treatment status, the share of competitors treated, and X. Standard errors clustered at the market level. *** p<0.01, ** p<0.05, * p<0.1.

Panel A reports heterogeneous effects by firm size, measured with log employment at baseline. The direct effects suggest that larger firms benefitted more from the treatment, which seems plausible for example if firm size partly reflects productivity or quality. Panel B shows heterogeneity by the manager's education and the larger direct effects for more educated managers are again consistent with this interpretation. Panel C reports heterogeneity by the manager's political connection: here the effects are insignificant. These estimates are suggestive of some reallocation taking place

Table A6: Demand diffusion: Additional results

VARIABLES	Service Quality	Shopping Environment	Value for Money	Overall Satisfaction	log Sales	Profit	log Number of Employees
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated	0.763***	0.998***	0.594***	0.854***			
	(0.103)	(0.100)	(0.086)	(0.068)			
Share Local Competitors Treated	-0.258*	-0.250**	-0.171	-0.140			
	(0.136)	(0.100)	(0.105)	(0.084)			
Share Local Non-Competitors Treated	0.119	0.021	0.040	0.030			
	(0.143)	(0.103)	(0.082)	(0.085)			
Post*Treated					0.090**	9.770***	0.080**
					(0.040)	(3.045)	(0.031)
Post* Share Local Competitors Treated					-0.096*	-12.11*	-0.049
					(0.055)	(6.550)	(0.038)
Post*Share Local Non-Competitors Treated					0.160***	13.30***	0.060**
					(0.046)	(4.637)	(0.027)
Post*Share Local Competitors with High Share of Local Non-competitors Treated					-0.088	-12.61**	-0.098**
					(0.084)	(5.312)	(0.045)
Firm FE and Post	No	No	No	No	Yes	Yes	Yes
Observations	1,804	1,804	1,804	1,804	8,220	8,220	8,220

Note: Columns 1-4 use the endline data, include indicators for the number of local competitors, and include the share of non-local competitors and non-local non-competitors treated. Columns 5-7 use the full panel for firms covered in the endline survey, include the interactions of Post with indicators for the number of local competitors, and with the share of non-local competitors treated and the share of non-local non-competitors treated. The share of local competitors with high share of local non-competitors treated is defined using the above-median share of local non-competitors treated. Standard errors clustered at the market level. *** p<0.01, ** p<0.05, * p<0.1.

within the group of treated firms towards more productive firms. The indirect are imprecisely estimated in all three panels.

Demand diffusion: additional results. Table A6 reports additional results on the demand diffusion effect. Columns 1-4 show that the share of local non-competitors treated does not significantly increase consumer satisfaction, providing further evidence that the effect is not driven by the diffusion of borrowing. Columns 5-7, based on equation (30), explore the business stealing effect generated by demand diffusion by including the interaction of *Post* with the share of local competitors being exposed to demand diffusion, the latter measured as the firm having a higher-than-median share of local non-competitors treated. We find a significant negative effect for two of the three outcomes, profit and the log number of employees, providing evidence consistent with demand diffusion generating business stealing effects. But because the new variable is correlated with the share of competitors treated which measures the business stealing effect of the treatment, we interpret these results as suggestive.

Welfare and return to capital: different elasticities. To show the sensitivity of our welfare results to the elasticity of substitution we replicate Tables 12 and 13 using $\sigma = 4$ and $\sigma = 8$. Table A7 reports the welfare estimates. The impact on the producer surplus is unchanged because it is computed directly from the regression coefficients. The impact on the consumer surplus, as well as the total effect, is larger for $\sigma = 4$, and smaller for $\sigma = 8$, but the qualitative patterns are similar to the results in the main text. In the conservative $\sigma = 8$ case the total welfare impact is a significant USD 7,400 per firm, which amounts to about 8% of profits. Table A8 reports the estimated returns to capital. Here too, the results are qualitatively similar to those in the main text.

Table A7: Robustness of welfare gain estimates

Welfare gain per firm in market	Treat all firms		Treat 50% of firms	
	Share of Profit (%)	USD	Share of Profit (%)	USD
Panel A: Sigma=4				
Producer Surplus	4.1	3,566	2.0	1,779
	(4.4)	(3,904)	(2.2)	(1,952)
	[-5, 12]	[-4,263, 10,752]	[-2, 6]	[-2,132, 5,376]
Consumer Surplus	21.1	18,579	10.5	9,246
	(7.6)	(6,703)	(3.8)	(3351.690)
	[7, 37]	[6,550, 32,690]	[4, 19]	[3,275, 16,345]
Spillover			3.6	3,170
			(1.8)	(1,598)
			[1, 8]	[716, 7,227]
Total	25.1	22,101	16.1	14,177
	(9.9)	(8,741)	(6.7)	(5,859)
	[7, 46]	[5,997, 40,631]	[4, 30]	[3,687, 26,742]
Panel B: Sigma=8				
Producer Surplus	4.1	3,566	2.0	1,779
	(4.4)	(3,904)	(2.2)	(1,952)
	[-5, 12]	[-4,263, 10,752]	[-2, 6]	[-2,132, 5,376]
Consumer Surplus	9.0	7,951	4.5	3,962
	(3.3)	(2,873)	(1.6)	(1,436)
	[3, 16]	[2,807, 14,010]	[2, 8]	[1,404, 7,000]
Spillover			1.9	1,673
			(1.1)	(902)
			[0, 5]	[125, 3,981]
Total	13.1	11,535	8.4	7,396
	(6.3)	(5,518)	(4.2)	(3,666)
	[1, 25]	[710, 22,381]	[0, 17]	[424, 14,974]

Note: Bootstrap standard errors in round brackets, and bootstrap bias-corrected percentile confidence intervals in square brackets, are computed by bootstrapping our estimation procedure 1,000 times, drawing markets with replacement. USD values are calculated using the average annual exchange rate rate during midline and endline (6.465).

Table A8: Robustness of return to capital decomposition

	Sigma=4	Sigma=8
Private Return (%)	74.2	74.2
	(12.9)	(12.9)
	[46, 98]	[46, 98]
Business Stealing (pp)	-56.3	-56.3
	(23.4)	(23.4)
	[-104, -13]	[-104, -13]
Consumer Surplus (pp)	62.8	31.7
	(19)	(10.7)
	[26, 102]	[12, 55]
Social Return (%)	80.8	49.6
	(25.3)	(20.6)
	[29, 127]	[5, 86]

Note: Bootstrap standard errors in round brackets, and bootstrap bias-corrected percentile confidence intervals in square brackets, are computed by bootstrapping our estimation procedure 1,000 times, drawing markets with replacement. In draws with negative raw yields (<1% of cases), we approximate the internal rate of return with the yield.