Direct and Indirect Effects of Financial Access on SMEs*

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Abstract

We measure the direct and indirect effects of access to finance using a randomized experiment with 3,100 firms in 78 local markets in China, which created variation in firms' access to a new loan product both within and across markets. Our estimates imply that: (1) Financial access has large positive direct effects. Providing access to a firm increases its revenue by 9 percent, and also significantly increases profits, employment, the number of clients and the use of trade credit. The new loan does not crowd out existing loans. (2) Financial access has large negative indirect effects. Providing access to all of a firm's competitors in the local market reduces its revenue by 7 percent, and also significantly reduces profits, employment, the number of clients and the use of trade credit. (3) In a model matched to the data in which the indirect effect reflects business stealing, treating all firms in a market would generate—despite nearly offsetting direct and indirect effects on firms—sizeable welfare gains to consumers who benefit from competition.

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1 Introduction

Lack of credit is believed to be a major growth barrier. But the overall impacts of access to credit on firms and their consumers, accounting for both direct and indirect effects, are not well understood, especially in the key segment of small and medium enterprises (SMEs). This is partly because few studies investigate the indirect effect on peer firms of any firm-level intervention, but also because SMEs are understudied relative to their importance in the developing world. Our goal in this paper is to measure both the direct and indirect effects of access to finance on SMEs, and to evaluate the resulting gains at the industry level. To this end, we randomize access to a new loan product for firms both within and across local markets in China. This randomization creates variation both in the firm's treatment status and in the share of peer firms treated in its local market, allowing us to measure both direct and indirect effects. We then combine our estimates with a simple model of industry equilibrium to evaluate the resulting gains.

In Section 2 we introduce our context and experimental design. In 2013, the Rural Credit Cooperatives (RCC), a large rural bank, introduced a new loan product to SMEs in Jiangxi, China. A key novelty of the product is that it is offered to firms located in local "markets", that is, clusters of geographically concentrated firms. This allows loan officers to rely on the help of the market leader for information, which reduces the fixed and the monitoring costs of lending. In exchange, RCC is able to offer better terms to borrowers than its competitors, including not requiring collateral for the loan.

In collaboration with RCC, starting in the summer of 2013 we introduced a randomized treatment in improving access to these loans. Our treatment combined information and encouragement: for one year, each treated firm was visited every month by a loan officer, who explained the terms of the loan and offered help in filling the loan application. We randomly assigned the treatment to firms in 78 local markets. We randomized 37 markets to be "majority treatment": in these we treated 80% of firms. We randomized another 10 markets to be "half-half treatment": in these we treated half of the firms. The remaining 31 markets were "control markets" with no treated firms. We surveyed half of the firms in all markets, in total about 3,100 firms (average employment 9), three times: at baseline before the intervention in 2013 summer, at midline two years later in 2015

summer, and at endline in 2016 summer. The treatment worked: treated firms were on average 33 percentage points more likely to borrow from RCC than control firms. We also find evidence consistent with information diffusion: within the pool of untreated firms, regressing borrowing from RCC on the share of treated peers yields a significant elasticity of 0.17.

In Section 3 we present a simple Melitz-style model that guides our main estimation and welfare calculation. In this model monopolistically competitive firms are organized in local markets, and products are more substitutable within than across markets. We formalize access to finance as an increase in firm productivity. In this model, when a random share of firms in a local market are provided access to finance, the impact on firm revenue can be approximated with a linear function of two variables, which correspond to a direct and an indirect effect: The revenue impact is increasing in the firm's own treatment status and decreasing in the share of competitors treated. The direct effect arises because a more productive firm charges a lower price and attracts a higher share of demand. The indirect effect arises because of business stealing: treated competitors charge lower prices and attract some of the demand away from the firm.[1] As long as the elasticity of substitution between markets exceeds one, in our model the coefficient of the direct effect is larger than that of the indirect effect, because there is reallocation not only within but also across markets. The basic version of the model ignores information diffusion about the loan, but we also develop a simple way of including it and show that it leads to straightforward modifications of the main prediction.

In Section 4 we present our main empirical results. Motivated by our simple model, we focus on two key determinants of firm performance: the firm's own treatment status and the share of its competitors in the local market which are treated. For simplicity, in this analysis we ignore diffusion effects.[2] Our basic regression is a firm fixed effects specification in which we interact these variables with *Post*, an indicator for dates after the intervention. This regression effectively measures how the growth rate of firms varies with their own and their peers' treatment status. We cluster standard errors at the market level.

[1] Although in our model higher productivity is manifested only in lower prices, we think of this effect as one representation of generally providing a better service to clients, perhaps via higher quality rather than lower price.

[2] Our model suggests that diffusion effects will bias our estimates downwards, but given the magnitude of diffusion we observe this bias is likely to be small. We are currently working on an estimation framework that explicitly accounts for diffusion.

We begin by looking at impacts on basic firm performance measures. When the outcome is log sales, we estimate that the treatment had a direct effect of about 9 log points, that is, treating a firm increases its sales by more than 9 percent. We also find that an indirect effect of about -7.5 log points, so that treating all of a firm's local competitors reduces the firm's sales by more than 7 percent. Similarly, we find large positive direct and large negative indirect effects for profit, log employment, and the log wage bill. All these effects are statistically significant. We find similar but less significant effects on materials and no effect on fixed assets. And we find a positive direct and a zero indirect effect on survival. Consistent with our model, these results show large positive direct effects and slightly smaller negative indirect effects on firm performance.

We next explore intermediate outcomes related to business activities. We show that the treatment had positive direct and negative indirect effects on the number of clients. This finding is consistent with our model which predicts that demand and production should migrate from untreated to treated firms. We next show that the treatment had positive direct effects on renovation, the introduction of new products, and advertising. These results highlight some ways in which treated firms improved their services, which may have helped them attract clients from their untreated peers.

We then turn to financial outcomes. We find that the treatment had no direct effect on borrowing from other sources. This is a useful result as it rules out the RCC loan crowding out other sources of borrowing, and thus establishes that our firms were genuinely credit constrained (Banerjee and Duflo 2014). We also find that the treatment had positive direct and negative indirect effects on the use of trade credit with both suppliers and clients. A possible reason is that the loan allowed the firm to engage more intensively in using trade credit. And the use of trade credit with clients may be another mechanism through treated firms—by providing better services—attracted clients from untreated firms. Taken together, these findings further connect the empirical analysis with the mechanism of our model.

We also discuss some concerns with measurement and identification. One concern is that experimenter demand effects may drive the results. Contradicting this force, we find no effect of the treatments on the difference between the self-reported and the actual book value of sales. A second

concern is that the surprisingly large negative indirect effect may be the result of some regression misspecification or problem with the market-level randomization. To address this we estimate a placebo version of our main specification in which the outcomes are measured at baseline, i.e., before the intervention. As expected, we find zero direct and indirect effects. A third issue is that our main specification is not fully saturated as it lacks the interaction between the firm's own treatment status and the share of its treated competitors. We estimate this richer specification, and find that the negative indirect effect on untreated firms remains robustly significant, while other coefficients are somewhat closer to zero and often insignificant. However coefficients have the same sign and order of magnitude as our main specification. Moreover, we cannot reject that the indirect effects on treated and untreated firms are identical. We conclude that the results in this specification are similar to our main regressions and that there is not enough power to separately measure indirect effects by the firm's treatment status.

In Section 5 we combine the model and our estimates in a simple welfare calculation. In this calculation we also bring in diffusion.[3] Our model implies that the welfare gain from treating a share S of firms in a market is the sum of the following three terms. (i) The producer surplus, which is the net effect on the profits of all firms in the market, including both direct and indirect effects. (ii) The consumer surplus, which is the product of the revenue of treated firms, and the regression coefficient measuring the direct effect on revenue divided by $(\sigma - 1)$ where σ is the elasticity of substitution between products in a market. Here the regression coefficient measures the extent of reallocation, and is normalized by $\sigma - 1$ because the same degree of reallocation implies lower gains when products are more substitutable. (iii) The gain from diffusion, which is proportional to the sum of producer and consumer surplus and the share of firms to whom loan diffused. Combining our estimates and a value of $\sigma = 6$ implies, when treating all firms in a market, total gains amounting to 16% of total profits in the market (or about USD 12,000 per firm in the market), most of which is coming from the consumer surplus since the direct and indirect profit effects almost cancel. When treating 50% of firms in a market, the implied welfare gain is 10%

[3] This is somewhat inconsistent with our estimation of direct and indirect effects which ignored diffusion, but should still be informative about the order of magnitude of the various effects. We are working on a framework that combines all effects in both the estimation and the welfare calculation.

of total profits in the market. This is more than half of the previous number because diffusion of the loan generates an additional welfare gain amounting to 2% of profits. Even with a fairly conservative $\sigma = 11$ we calculate welfare gains of 10% respectively 6% when treating all of half of the firms in the market. These results suggest that in evaluating firm interventions, finding small aggregate impacts on firms due to offsetting indirect effects may not mean that the intervention is not beneficial, as it may be accompanied by sizable gains to consumers.

Our research builds on and contributes to three main literatures. Most directly we build on the work measuring the impact of credit on firms, especially Banerjee and Duflo (2014) who document large direct effects of finance on mid-sized firms in India using policy variation.[4] To this work we contribute with a randomized experiment, with measuring both direct and indirect effects, and with our focus on a key segment of smaller firms. Also related are the studies evaluating microfinance programs lending to households, which tend to find small effects, including Angelucci, Karlan and Zinman (2015), Attanasio, Augsburg, Haas, Fitzsimons and Harmgart (2015), Augsburg, Haas, Harmgart and Meghir (2015), Crepon, Devoto, Duflo and Pariente (2015), Tarozzi, Desai and Johnson (2015). To these papers we contribute by focusing on firms rather than individuals or one-person microenterprises.

Our research questions are related to work studying indirect and equilibrium effects of various interventions on firms. Bloom, Schankerman and Van Reenen (2013b) study spillover and business stealing effects of R&D in the U.S., McKenzie and Puerto (2017) the direct and indirect effects of training on microenterprises in Kenya, and Rotemberg (2017) the indirect effects of firm subsidies—that provide a wide variety of programs including training, advice, as well as improved access to credit—on mid-sized firms in India. To these papers we contribute by focusing on the direct and indirect effects of finance. Also related are Breza and Kinnan (2018) and Buera, Kaboski and Shin (2017) who explore the general equilibrium implications of microfinance. To their work we contribute by our focus on lending to firms, and by evaluating the implications for industry equilibrium, rather than general equilibrium.

Finally, our methodology and policy results build on a literature that uses experiments to

[4] There is also earlier research that uses cross-country and cross-industry variation, including Rajan and Zingales (1998).

study private sector development. De Mel, McKenzie and Woodruff (2008) randomize cash grants to microenterprises to measure the return to capital. Several papers reviewed in McKenzie and Woodruff (2014) study the effects of business training, while Bloom, Eifert, Mahajan, McKenzie and Roberts (2013a) and Bruhn, Karlan and Schoar (2013) measure the impact of management consulting. McKenzie (2017) evaluates a business plan competition and Brooks, Donovan and Johnson (2016) evaluate a business mentoring program. We contribute to this work by focusing on access to finance and evaluating both its direct and indirect effects.

2 Context, experimental design and data

2.1 Context

Our experimental site is in Jiangxi province in China. We conducted our survey and intervention in collaboration with with the Rural Credit Cooperatives (RCC), a large rural bank in China.

*Loan product.*In 2013 RCC introduced a new loan product to SMEs in Jiangxi. A key novelty of the product is that it is introduced in local "markets", that is, clusters of geographically concentrated firms. Loan officers can rely on the help of the market leader to reduce asymmetric information, which reduces the fixed and the monitoring costs of lending and allowed RCC to offer better terms to borrowers. In particular, the new loan product was standardized and the bank committed to make a decision on funding within two weeks of a loan application; and it did not require collateral from borrowers. Once RCC approves a borrower, it assigns a credit line of up to 30% of the value of net assets. (The maximum credit line is RMB 500,000, about USD 83,000.) The monthly interest rate is about 0.7% which is a 15 percent saving relative to existing formal loans in our survey. Firms must pay interest every month and must repay the loan within two years. RCC decides on lending independently from us.

Intervention. In the summer of 2013 we introduced our treatment, which combined information about the loan with encouragement to apply. Specifically, every treated firm was visited every month for a year by a loan officer from RCC, who provided information about the loan and help in applying.

We randomized 37 markets to be "majority treatment": in these we treated 80% of firms. We randomized another 10 markets to be "half-half treatment": in these we treated half of the firms. The remaining 31 markets were "control markets" with no treated firms.

Survey. We surveyed half of the firms in all markets, in total about 3,100 firms. We conducted a baseline survey before the intervention in 2013 summer, a midline survey two years later in 2015 summer, and an endline survey in 2016 summer. Because the fiscal year in China ends in June, data in the baseline survey refer to the fiscal year before the intervention.

The surveys were conducted in person with the manager, by locally hired enumerators, in collaboration with CIIT. CIIT officials phoned the firms in advance to arrange the interview, and if the manager was not available at the scheduled time, a CIIT official or our enumerator phoned again to arrange a second meeting.

In the surveys we collected information from firms about the following groups of variables. (1) Firm characteristics. Profits, sales, costs, utility expenses, spending on intermediate inputs, and other balance-sheet variables. For sales we have two measures: besides the self-reported value in the survey we also have the actual book value. To obtain it, in the midline and endline waves, at the conclusion of the survey our enumerators asked the accountant of the firm, or the manager when the firm did not have an accountant or the accountant was not present—to physically show the value in the firm's book. (2) Managerial characteristics including demographics and political connections.

2.2 Summary statistics and loan outcomes

Summary statistics. Table 1 shows basic summary statics on firms and managers. The first three columns report the means for all firms, treated firms, and untreated firms, and standard deviations in parenthesis. The final columns reports the difference between treated and untreated firms, and the p-value in parenthesis. Treated firms include all firms that did, and untreated firms include all firms that did not receive the treatment, irrespective of the treatment intensity of the market. All rows refer to the baseline survey.

In 2013 average firm age was about 6.6 years. Almost 70% of the firms were in retail (with the

Table 1: Firm and manager characteristics

	All Sample	Treatment	Control	Difference
Number of Observations	*3117*	*1404*	*1697*	
Firm age	6.6	6.55	6.63	-0.08
	(4.38)	(4.48)	(4.31)	(0.652)
Sector - Retail (%)	68.68	69.75	67.80	1.94
	(46.39)	(45.95)	(46.74)	(0.2720)
Number of employees	8.96	9.01	8.93	0.09
	(5.63)	(5.93)	(5.38)	(0.692)
Profit (10,000 RMB)	51.60	51.04	52.06	-1.02
	(69.36)	(66.45)	(71.68)	(0.6996)
Sales (10,000 RMB)	335.32	348.17	324.74	23.43
	(474.30)	(526.77)	(426.15)	(0.1949)
Manager age	38.46	38.19	38.69	-0.51
	(8.8)	(8.78)	(8.81)	(0.1295)
Gender (1=Male, 0=Female)	0.57	0.55	0.58	-0.02
	(0.5)	(0.5)	(0.5)	(0.253)
Political connection (1=Yes, 0=No)	0.16	0.16	0.15	0.013
	(0.37)	(0.37)	(0.36)	(0.3661)
Education - College	0.27	0.28	0.26	0.02
	(0.44)	(0.45)	(0.44)	(0.2735)

rest mostly in manufacturing). These firms employed on average 9 workers, but the large standard deviation of employment (5.6) shows that there was much cross-firm heterogeneity. The average net profit was RMB 510,000 (about USD 75,000), and average revenue was RMB 3,350,000 (almost USD 0.5 million). About 15% of managers had a political connection, defined as past working experience in the government. In all these variables, there is no significant difference between treated and untreated firms at baseline, confirming that the randomization is valid.

Table 2 shows summary statics on financial and business activities. 25% of firms had a pre-existing loan at baseline, and conditional on having a loan, the average loan amount was RMB 290,000 (about USD 42,000). About 39% of treated firms, and 7% of untreated firms, took a loan from RCC by endline, suggesting that the treatment did induce borrowing. Firms had 29 clients per day and about 7 active suppliers. And there was some use of trade credit, especially with clients.

The final rows show shutdown and attrition by endline. Shutdown is defined as one if we have

Table 2: Financial and business activities

	All Sample	Treatment	Control	Difference
Loan (1=Yes, 0=No)	0.25	0.24	0.25	-0.01
	(0.43)	(0.43)	(0.43)	(0.7058)
Loan size	29.38	31.77	27.47	4.30
	(85.07)	(104.24)	(65.85)	(0.5104)
Loan interest rate	9.22	9.05	9.37	-0.32
	(6.51)	(5.44)	(7.53)	(0.9115)
RCC loan (follow-up, 1=Yes, 0=No)	0.21	0.39	0.07	0.32***
	(0.41)	(0.49)	(0.26)	(0.0000)
RCC loan size (follow-up)	27.05	28.25	21.57	6.68***
	(20.93)	(20.03)	(23.94)	(0.0000)
RCC loan interest rate	7.38	7.40	7.36	0.03
	(5.27)	(6.08)	(4.38)	(0.9646)
Clients	28.39	28.13	28.61	-0.48
	(18.01)	(17.89)	(18.09)	(0.4847)
Suppliers	6.76	6.66	6.83	-0.18
	(10.36)	(10.45)	(10.29)	(0.6566)
Trade credit - supplier (1=Yes, 0=No)	0.17	0.16	0.17	-0.01
	(0.37)	(0.37)	(0.38)	(0.6166)
Trade credit - client (1=Yes, 0=No)	0.34	0.33	0.35	-0.02
	(0.47)	(0.47)	(0.48)	(0.3193)
Shutdown (endline)	0.10	0.07	0.13	-0.06***
	(0.30)	(0.25)	(0.33)	(0.00)
Attrition (endline)	0.14	0.14	0.15	-0.01
	(0.34)	(0.34)	(0.36)	(0.3239)

information that the firm went out of business in the given survey wave. Attrition is defined as one in a survey wave if we do not have information about the firm in that wave, and can be the result of the firm choosing not to respond, moving away, or shutting down.[5] Shutdown is significantly higher among untreated firms, suggesting that the treatment had an impact on firm performance. We made a considerable effort to keep attrition low. With the help of CIIT we were able to track most mover firms; CIIT phoned 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 relative low at about 15% by endline, and not significantly different between treatment and control firms.

Table 3 reports on the patterns of market-level industry structure. In the average market we

[5] Shutdown need not imply attrition: some firms which shut down still answered our survey for the part of the year during which they were active.

Table 3: Industry structure

	mean	stdev
A. In sample		
firms in market	39.96	26.09
industries in market	3.95	2.4
firms in local industry	9.03	9.91
B. In population		
Inverse Herfindahl	14.18	16.22

observe about 40 firms. Defining a firm's industry using the main product of the firm, we find that these firms are on average in 4 industries. Also using the fact that we observe half of the firms in each market, we compute the inverse Herfindahl index of an industry in a local market to be about 14. This suggests that firms operate ina fairly competitive environment, and motivates our model below of a monopolistically competitive industry.

Table 4: Loan Outcomes at Endline

Dep. var.:	RCC loan		
	(1)	(2)	(3)
treatment	0.328***	0.321***	
	(0.0455)	(0.0409)	
employment		0.00233	
		(0.00155)	
gender		0.00553	
		(0.0233)	
education		0.0177	
		(0.0191)	
pol. conn.		0.0468**	
		(0.0185)	
confidence		0.176***	
		(0.0411)	
share peers			0.170***
treated			(0.0397)
Observations	2,658	2,658	1,445

Loan outcomes. Table 4 shows loan outcomes at endline. These report the estimates of a cross-sectional regression using the endline data. The dependent variable is an indicator for whether the firm has borrowed from RCC (i.e., has used the new loan product) by the endline. Column 1

shows that the probability to borrow was 33 percentage points higher among treated than among untreated firms. Thus the treatment worked in inducing firms to borrow. Column 2 shows some other determinants of borrowing.

Column 3 reveals evidence of some indirect effects. It focuses on the subsample of untreated firms, and shows that having a higher share of peers in the local market borrow had a positive effect on the likelihood that a firm borrows. This effect could arise for at least two reasons. One is information diffusion: untreated firms may have learned about the loan opportunity from treated peers. Another is a cost complementarity: since in treated markets the loan officer was more often present, it may have been easier to approach the loan officer to start the application process.

3 Model

To motivate our estimating equations and welfare calculations, here we develop a simple model which captures some key features of our context and intervention. The main focus of our model is a a monopolistically competitive sector that features many markets. We represent the rest of the economy with a numeraire good, which allows us to focus on industry equilibrium effects while setting aside general equilibrium considerations.

In the monopolistically competitive sector there are a continuum of markets m and in each market a continuum of firms i. Goods purchased in market m aggregate into a composite good

$$Q_m = \left[\int q_{i,m}^{1-1/\sigma} di \right]^{\frac{\sigma}{\sigma-1}} \tag{1}$$

and consumer preferences are given by

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

Here σ measures the elasticity of substitution within a market, θ the elasticity across markets, and we assume $\sigma > \theta$. H is the numeraire good produced by a perfectly competitive sector. One unit of labor produces one unit of H. The aggregate labor supply is L. In the monopolistically competitive sector firms have constant returns to scale, produce with labor, and face an exogenous wage. Firms differ in productivity, so that $q_{i,m} = \omega_{i,m} l_{i,m}$.

Consider now a treatment that increases firm productivity by a factor e^γ. Equivalently, the treatment could also be conceptualized as an improvement in product quality, so that the effective quantity entering the the utility function of a consumer who purchases q_i units of a treated firm's output is $q_i \cdot e^\gamma$. The treatment is introduced to a share S_m of firms in market m. The set of treated firms within a market is random. Let T_i be an indicator for whether firm i is treated.

Proposition 1. *To a first-order approximation, the effect of the treatment on the revenue of firm i in market m is*

$$\Delta \log R_i \approx (\sigma - 1)\gamma \cdot T_i - (\sigma - \theta)\gamma \cdot S_m. \tag{3}$$

The first term represents a positive direct effect. As the firm becomes more productive, it charges a lower price and attracts more demand, resulting in an increase in revenue. The second term represents a negative indirect effect. When competitors are treated, they charge lower prices and steal customers for the firm. The higher the share of treated competitors, the stronger this business stealing effect. The coefficient of the direct effect is proportional to $\sigma - 1$: reducing the price reduces revenue on impact (this is the -1), but brings more demand (this is the σ). As long as $\theta > 1$ the coefficient of the indirect effect is smaller: this is because generally lower prices in the market result in a shift in demand away from other markets towards this market.

To evaluate the impact of the treatment on consumers, now consider a policy of treating a share S of firms in a small share of markets.[6] Let R_m be the pre-treatment revenue and Π_m the pre-treatment profit in the average treated market. Denote the coefficient of direct effect in (3) by $\beta = (\sigma - 1)\gamma$ and that of the indirect effect by $\delta = -(\sigma - \theta)\gamma$.

Proposition 2. *To a first-order approximation, the welfare gain from this policy, per treated market, is*

$$S \cdot \left[(\beta + \delta)\Pi_m + \frac{\beta}{\sigma - 1} R_m \right]. \tag{4}$$

Here the first term is the producer surplus and the second term is the consumer surplus. The producer surplus is just the net increase in profits in the market accounting for both direct and indirect effects, and can be computed using the direct and indirect revenue coefficients given that

[6] We treat firms in a positive share of markets because each market is infinitesimal.

profits are a constant fraction of revenue in this model. For an intuition about the consumer surplus, note that holding fixed the current consumption allocation, $S\gamma R_m$ represents the expenditure reduction to the consumer from the price change affecting a share S of firms. Due to the envelope theorem, for a small enough change this is approximately the gain in consumer surplus, and then the definition of β gives the result.

The expression of the proposition is a convenient representation because β and δ correspond to regression coefficients in the empirical analysis and hence are directly measurable.[7] The consumer surplus is the direct effect normalized by $\sigma - 1$: the direct effect measures the extent of reallocation, and is normalized because the same amount of reallocation reflects lower welfare gains when products are more substitutable.

Now consider a simple extension of the model that incorporates diffusion of information about the treatment. Suppose that due to diffusion a random share $\phi \cdot S_m$ of untreated firms also borrow. This modification has two implications. First, equation (3) needs to be modified. Since some untreated firms also borrow, T_i needs to be replaced by an indicator for borrowing and S_m needs to be replaced by the share of borrower firms $S_m(1 + (1 - S_m)\phi)$.[8] Second, the welfare expression changes: S needs to be increased to $S(1 + (1 - S)\phi)$ to account for the welfare effects of diffusion-induced borrowing.

[7] An alternative way of measuring the producer surplus is to directly read off the direct and indirect profit effects from a regression in which profits are the outcome.

[8] These changes also imply that estimating (3) in an economy that has diffusion would result in an underestimate of β because some firms classified as untreated actually borrow; and an underestimate (too negative estimate) of δ because the indirect effect generated by untreated borrowers is assigned to the share of firms treated. Both of these biases act towards lowering the implied welfare gain relative to the true welfare gain.

4 Direct and Indirect Effects on Firms

4.1 Main Effects and Specification Checks

Estimating equation. In this analysis we ignore diffusion effects (we bring them back in Section 5) and motivated by the model use the following estimating equation:

$$\begin{aligned} y_{it} = \kappa \cdot Post_t + \beta \cdot Post_t \times Treatment_i \\ + \delta \cdot Post_t \times Share\ Competitors\ Treated_i \\ + Firm\ f.\ e. + \varepsilon_{it}. \end{aligned} \quad (5)$$

Here i indexes firms, t indexes years, and y_{it} is an outcome variable such as log sales. $Treatment_i$ is an indicator for the firm itself receiving the treatment, and $Share\ Competitors\ Treated_i$ is the share of the firm's competitors in the local market which are receiving the treatment. $Post_t$ is an indicator for the midline or endline survey waves. The firm fixed effects take out time-invariant heterogeneity, including whether the firm is treated or untreated, and the treatment status of the market. This specification is analogous to the one used by De Mel et al. (2008).

Our coefficients of interest are β, which measures the direct effect of the treatment; and δ, which measures the indirect effect of the treatment. Intuitively, β is identified as the additional average growth in the outcome variable in a treated versus an untreated firm; and δ is identified as the additional growth when more versus fewer peers of the firm are treated. Because the treatment can induce correlated errors within a local market, for inference we cluster standard errors at the market level.

Main effects. Table 5 shows the results for a range of firm performance measures. Start with column 1 in which the outcome is log sales. The coefficient of the uninteracted *Post* variable shows that average sales growth from before to after the intervention was about 4 log points among untreated firms in control markets. The coefficient of the interaction between *Treated* and *Post* implies that—holding fixed indirect effects—a treated firm on average experiences an additional sales growth of about 9 log points. And the coefficient of the interaction between *Share Competitors Treated* and *Post* implies that treating all the competitors of a firm would reduce

Table 5: Direct and Indirect Effects: Main Outcomes

Dep. var.:	log sales	profit	log employment	log wage bill	fixed assets	log materials	shutdown
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
post	0.039**	4.32**	0.042***	0.064***	2.54*	0.023	0.088***
	(0.017)	(1.65)	(0.013)	(0.013)	(1.49)	(0.022)	(0.014)
post*treatment	0.091***	11.34***	0.071***	0.083***	5.47	0.080**	-0.037***
	(0.027)	(3.36)	(0.023)	(0.023)	(4.19)	(0.039)	(0.010)
post*share	-0.075**	-9.47*	-0.065**	-0.066**	-3.01	-0.049	-0.000
competitors treated	(0.034)	(5.57)	(0.030)	(0.031)	(4.30)	(0.044)	(0.021)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,220	8,220	8,220	8,214	8,220	8,213	8,220
Number of firms	2,781	2,781	2,781	2,779	2,781	2,781	2,781

its growth by 7.5 log points. Both of these treatment effects are significant. We find similar results for profits in column 2. Here too the direct effect is large and positive at RMB 110,000 (or USD 16,000) while the indirect effect is large and negative at RMB 95,000 (USD 14,000). These results show large impacts for two key business-relevant outcomes.

The remaining columns focus on factors, inputs and firm survival. Columns 3 and 4 show similar patterns—with coefficients comparable to those of log sales—for log employment and the log wage bill. These results suggest that borrowing firms expanded by hiring workers. Column 5 reports insignificant and small coefficients for fixed effects suggesting that the loan was not used for traditional forms of capital. Column 6 reports affects on material spending. Here the coefficients are comparable to those for sales, though less significant. Finally, column 7 reveals that the treatment had a positive direct effect, and no indirect effect, on survival. Overall we conclude that Table 5 shows large positive direct and large negative indirect effects of financial access.

Specification checks. To confirm the validity of our results, and more fully explore the nature of indirect effects, here we report some specification checks. We begin with a placebo specification in which we estimate direct and indirect effects at baseline. This is a cross-sectional regression using only baseline data in which the key dependent variables are *Treatment* and *Share Competitors Treated*. Because the treatment is randomized both within and across markets, and takes place after the

Table 6: Specification Check: Effects at Baseline

Dep. var.:	log sales	profit	log employment	log wage bill	fixed assets	log materials
	(1)	(2)	(3)	(4)	(5)	(6)
treatment	-0.0118	0.190	-0.0312	-0.0245	0.873	-0.0180
	(0.0452)	(4.449)	(0.0359)	(0.0382)	(2.809)	(0.0548)
share competitors treated	0.0465	-2.113	0.0311	0.0257	0.00536	0.0514
	(0.0565)	(5.541)	(0.0453)	(0.0483)	(3.518)	(0.0684)
Observations	2,781	2,781	2,781	2,779	2,781	2,781

baseline survey, we expect to find zero effects in this regression. Thus it is a useful test to isolate possible regression misspecifications or problems with the market level randomization. Table 6 reports the results: as expected we estimate zero direct and indirect effects at baseline, providing internal consistency to our results.

We next explore whether indirect effects are heterogeneous by the firm's treatment status. Specifically, we decompose the *Share Peers Treated* variable into its interaction with *Treated* respectively with *Untreated*. From a reduced-form econometric perspective this is a natural "saturated" specification.

Table 7 reports the results. While the signs of the coefficients of interest are generally as in Table 5, the coefficients are generally insignificant except for the negative indirect effects on untreated firms. Mechanically, the reason many coefficients are insignificant here is that the indirect effects we estimate on treated firms, though generally negative, are not as negative as those we estimate on untreated firms. However, given the standard errors we are not able to reject the hypothesis that these two indirect effects are equal. We conclude that there is not enough power to separately identify the heterogeneous indirect effects, but that the signs of the coefficients in this richer specification are consistent with those we reported as our main results.

Finally we investigate whether our definition of competitors is the empirically relevant peer group. To do this, we re-estimate our main specification but also include the share of non-competitors who are treated, interacted with *Post*. Appendix Table A1 reports the results. Although the two indirect effect coefficients are generally insignificant, probably because they are

Table 7: Indirect Effects by Treatment Status

Dep. var.:	log sales	profit	log employment	log wage bill	fixed assets	log materials
	(1)	(2)	(3)	(4)	(5)	(6)
post	0.0412**	4.593***	0.0436***	0.0659***	2.828*	0.0242
	(0.0163)	(1.660)	(0.0137)	(0.0141)	(1.536)	(0.0222)
post*treatment	0.0573	6.471	0.0395	0.0425	0.468	0.0557
	(0.0862)	(10.87)	(0.0555)	(0.0554)	(5.670)	(0.124)
post*share competitors	-0.0336	-3.430	-0.0266	-0.0150	3.181	-0.0190
treated*treated	(0.109)	(14.46)	(0.0650)	(0.0648)	(7.165)	(0.151)
post*share competitors	-0.0852**	-10.89**	-0.0740**	-0.0772**	-4.466	-0.0557
treated*untreated	(0.0378)	(4.331)	(0.0340)	(0.0345)	(4.898)	(0.0499)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,220	8,220	8,220	8,214	8,220	8,213
Number of firms	2,781	2,781	2,781	2,779	2,781	2,781

strongly correlated, it is the *Share Competitors Treated* coefficient which remains large and close to the analogous coefficient in our main specification, while the *Share Non-Competitors Treated* coefficient generally much smaller and close to zero. These results suggest that our definition of competitors is plausible.

4.2 Intermediate Outcomes and Mechanisms

We now turn to impacts on various intermediate outcomes that may have contributed to firm growth. Table 8 explores outcomes related to the firm's business activities. Columns 1 and 2 show impacts on the log number of suppliers and clients. The impacts on suppliers are insignificant, but the impacts on clients are significant and large, suggesting the reallocation of clients from untreated firms to their treated competitors.

Columns 3-5 focus on specific business-related activities that may help understand the above patterns. We estimate significant positive direct effects for store renovation, the introduction of new products and advertising spending. These results suggest that treated firms improved the service provided to their clients, which may have helped them attract more clients. Renovation may have been particularly important: in our survey of borrowers 76% mentioned renovation and

Table 8: Business Outcomes

Dep. var.:	log num suppliers	log num clients	renovation	new product	advertising cost	informal	log sales diff
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
post	0.022	0.046***	0.106***	0.06***	0.05	0.002	-0.000
	(0.019)	(0.013)	(0.016)	(0.01)	(0.047)	(0.003)	(0.002)
post*treatment	0.052	0.084***	0.243***	0.231***	0.21**	-0.054***	-0.003
	(0.035)	(0.031)	(0.02)	(0.017)	(0.082)	(0.008)	(0.007)
post*share	-0.042	-0.072**	-0.049*	-0.047***	0.037	0.011	0.009
competitors treated	(0.036)	(0.033)	(0.028)	(0.018)	(0.066)	(0.012)	(0.009)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,212	8,220	8,220	8,220	8,220	8,220	5,167
Number of firms	2,781	2,781	2,781	2,781	2,781	2,781	2,766

the increase in the scale of operations as a primary use of the loan.

Column 6 shows that the treatment induced firms to improve their status from a small-scale "informal" business type to a regular enterprise.[9] This result suggests that the loan can have positive effects on improving formality. Finally, in column 7 the outcome is the difference between the log of self-reported sales and the log of the book value of sales (which our enumerators took directly from the firm's book). There is no direct or indirect effect on this difference, suggesting that experimenter demand effects are unlikely to drive the main results.

Table 9 explores various financial outcomes. Column 1 shows large direct effects and insignificant but positive indirect effects on the amount borrowed from RCC. The direct effect is unsurprising given our earlier finding that the treatment did induce a large share of firms to borrow. The zero indirect effect could be a net of two forces: a positive information diffusion effect as we documented earlier, and the general negative indirect effect.

Columns 2 and 3 focus on borrowing from other sources. These are key outcomes: as Banerjee and Duflo (2014) emphasize, even firms that are not credit constrained may take advantage of an attractive loan opportunity, but for such firms the new loan should crowd out existing loans. The table shows this not to be the case in our setting: the direct effects are insignificant and small both

[9] The small-scale business type can hire at most 8 workers but does not need to pay taxes.

Table 9: Financial Outcomes

Dep. var.:	RCC loan amount	other loan	other loan amount	trade credit supplier	trade credit client
	(1)	(2)	(3)	(4)	(5)
post	1.22***	0.022	3.03**	0.039***	0.036***
	(0.40)	(0.018)	(1.22)	(0.0076)	(0.008)
post*treatment	8.61***	0.025	2.15	0.068***	0.075***
	(1.29)	(0.025)	(1.89)	(0.014)	(0.020)
post*share	1.63	-0.022	-3.24*	-0.041**	-0.072***
competitors treated	(1.13)	(0.035)	(1.73)	(0.017)	(0.022)
Firm FE	Yes	Yes	Yes	Yes	Yes
Observations	8,220	8,220	8,343	8,220	8,220
Number of firms	2,781	2,781	2,781	2,781	2,781

for the firm having another loan and for the loan amount. Thus the new RCC loan was taken out in addition to existing loans, confirming that the firms in our setting were borrowing constrained. The indirect effect here are also small and slightly negative, consistent with the generally negative indirect effects we found so far.

Columns 6 and 7 show positive direct effects and negative indirect effects on the use of trade credit with both suppliers and clients. One possible explanation for some of these results is that the loan enabled the use of trade credit, which may have contributed to attracting more clients. Taken together, our results are consistent with a story in which treated firms steal business from untreated firms by offering better services to customers. This story is essentially what our simple model formalizes.

Heterogeneous effects. To explore heterogeneous effects by some baseline firm characteristic X_i, we extend our main specification (5) 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 10 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.

Panel A reports heterogeneous effects by firm size, measured with log employment at baseline.

Table 10: Heterogeneous effects

Dep. var.:	log sales	profit	log employment	log materials	log num clients
	(1)	(2)	(3)	(4)	(5)
A. log employment					
post*treatment*X	0.047	7.82	0.083**	0.097*	0.054
	(0.038)	(8.451)	(0.042)	(0.05)	(0.055)
post*share	-0.035	7.916	-0.102*	0.0077	-0.045
competitors treated*X	(0.064)	(14.90)	(0.056)	(0.074)	(0.062)
B. education					
post*treatment*X	0.185**	9.030	0.001	0.148*	0.065
	(0.078)	(7.734)	(0.054)	(0.087)	(0.061)
post*share	-0.079	6.565	-0.05	-0.03	-0.033
competitors treated*X	(0.097)	(10.50)	(0.072)	(0.113)	(0.071)
C. Political connection					
post*treatment*X	-0.087	7.220	-0.075	-0.044	-0.161**
	(0.084)	(12.72)	(0.047)	(0.116)	(0.073)
post*share	-0.089	-10.99	0.035	-0.136	0.051
competitors treated*X	(0.1)	(18.74)	(0.059)	(0.138)	(0.094)

The direct effects suggest that larger firms benefitted more from the treatment, which seems plausible for example if firm size partly reflects managerial ability. 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 point estimates for the direct effect are generally negative, suggesting that connected people may be worse in using the loan. These direct effect estimates seem overall plausible. The indirect effects in all three panels are very imprecisely estimated and do not allow us to draw conclusions.

Market-level outcomes. Table 11 reports market level regressions. These estimate the impact of the share of treated firms in the market on market-level revenue and profits. The estimates are insignificant and close to zero. This is consistent with our finding that the direct and indirect effects have offsetting magnitudes, and suggest that treating a set of firms in the market results in within-market reallocation but does not result in large market-level gains in producer surplus.

Table 11: Market-level outcomes

Dep. var.:	log market revenue	market profits
	(1)	(2)
post	0.051**	147.7*
	(0.022)	(74.86)
post*share market	0.022	23.03
treated	(0.038)	(129.3)
Market FE	Yes	Yes
Observations	234	234
Number of markets	78	78

5 Welfare Calculations and Counterfactuals

We can use the estimates for model-based calculations of welfare impacts. Our approach to measure welfare effects is to combine the reduced-form regression estimates with a welfare expression that builds on Proposition 2. To capture key features of the data we make two modifications to the model. First, we incorporate diffusion in the welfare calculation. As we noted in Section 3, diffusion can also bias the estimates of β and δ, and for simplicity here we ignore this bias and treat the estimated coefficients as the true parameters. Because the bias is downwards, this implies that our welfare results are lower bounds for the actual welfare effect. Second, in the model, but not in the data, a treated firm always borrows. This creates a problem because we do not observe the diffusion of information about the loan (which is how we conceptualize the treatment), only the diffusion of borrowing. To incorporate take-up in a simple way, we assume that a treated firm borrows with an exogenous constant probability p_B which can then be measured with the elasticity of take-up to the treatment in Table 4 as 0.33. This implies that we can estimate the diffusion coefficient ϕ as the elasticity of take-up among the untreated to the share of peers treated divided by p_B which gives $0.17/0.33 = 0.52$. Thus treating all of its peers would turn an untreated firm into an "informed" firm with probability 0.52 and into a borrower with probability $0.52 \cdot 0.33 = 0.17$.

With these assumptions, the welfare effect of treating a share S of firms in a market can be

Table 12: Welfare estimates

Welfare gain per	Treat all firms		Treat 50% of firms	
firm in market	share of profit	USD	share of profit	USD
Panel A: sigma=6				
producer surplus	3.6%	2,700	1.8%	1,300
consumer surplus	12.4%	9,300	6.2%	4,700
spillover			2.1%	1,600
total	16%	12,000	10%	7,600
Panel B: sigma=11				
producer surplus	3.6%	2,700	1.8%	1,400
consumer surplus	6.2%	4,700	3.1%	2,300
spillover			1.3%	1,000
total	9.8%	7,400	6.2%	4,600

written as

$$(S + S(1-S)\phi) \cdot \left((\beta_\Pi + \delta_\Pi) + \frac{\beta_R}{\sigma - 1} R \right) \tag{6}$$

where β_Π and δ_Π are the direct and indirect profit effects, β_R is the direct revenue effect, and R is average revenue. Here $\beta_\Pi + \delta_\Pi$ is the profit effect per firm and thus represents the producer surplus; $\beta_R/(\sigma-1)R$ is the consumer surplus per firm; and the factor $S + S(1-S)\phi$ is the "effective" share of treated firms including diffusion effects.

To evaluate this formula, given the estimate of ϕ described above, we only need to set σ, the elasticity of substitution between products in the industry. We work with two benchmark values: $\sigma = 6$ which is close to the ratio of revenue to profits in our sample (6.48) that would equal to σ in our model; and $\sigma = 11$ which is at the high end of existing estimates for the elasticity of substitution. We consider two scenarios: one in which we treat 50% of firms in the market and another in which we treat all firms in the market.

Table 12 reports the results. We begin with Panel A in which $\sigma = 6$. The first two columns show that treating all firms in the market implies, on a per firm basis, a gain in producer surplus amounting to 3.6% of profits and a gain in consumer surplus amounting to 12.4% of profits. The dollar equivalents are about USD 2,700 and USD 9,300, for a total welfare gain of USD 12,000 for each treated firm. The next two columns report the results when 50% of firms are treated. Here

the per firm effects are halved relative to the case when all firms are treated; but diffusion generates additional gains in the form of spillovers. The sum of the producer and consumer surplus generated by spillovers, normalized by the number of treated firms, amounts to 2.1% of firm profits or USD 1,600. Panel B shows the analogous results for $\sigma = 11$. The estimates are smaller but the basic patterns remain.

These results show that the treatment has a small effect on the producer surplus due to offsetting direct and indirect effects; but it has a relatively large effect on the consumer surplus. The model implies this large effect because the direct effect of the treatment is large, which in the model must mean that consumers value the improved services resulting from the treatment. The results also show sizeable welfare gains from diffusion, and thus suggest that policies which treat only a subset of firms in a market and rely on information diffusion may be more cost-effective.

Return on capital. The model also allows us to infer the return on capital. Here it is useful to distinguish between the private return to capital which is what accumulates to the borrower firm, and the social return which also takes into account the effect of the loan on competitors and consumers. For simplicity, we compute these returns under the assumption that all firms in a market are treated, so that there are no diffusion effects. We can write the private return as

$$\text{Private return} = \frac{\beta_\Pi}{p_B \cdot \text{loan size}} + \text{loan interest}. \tag{7}$$

Here the first term is the direct profit effect for the average firm, normalized by the average loan amount per firm which also accounts for the probability of borrowing conditional on the treatment. This term represents the gains after making interest payments; the second term adds back the loan interest.

The social return is

$$\text{Social return} = \text{Private return} + \frac{\delta_\Pi}{p_B \cdot \text{loan size}} + \frac{\beta_R}{\sigma - 1} R \frac{1}{p_B \cdot \text{loan size}}. \tag{8}$$

Here the second term represents business stealing and the third term represents the consumer surplus, both measured per firm and relative to the average loan per firm.

Table 13 reports the implied private and social returns for the benchmark values of $\sigma = 6$ and $\sigma = 11$. The private return is about 135% and does not depend on σ as it is a function of the profit

Table 13: Return on capital

	sigma=6	sigma=11
private return	135%	135%
social return	100%	64%

effect. These high returns are comparable to estimates of the private return in other contexts: for example, Banerjee and Duflo (2014) estimate 105% while De Mel et al. (2008) estimate 60%. We find that the social return is quite a bit lower than the private return: about 100% when $\sigma = 6$ and 64% when $\sigma = 11$. This is essentially because the business stealing effect negates most of the private return so that much of the social return is the consumer surplus.

Comparison to difference-in-differences. In comparing our results to other papers, it is useful to characterize how the standard difference-in-differences (DID) methodology is influenced by indirect effects. Assume that the true model features indirect effects

$$y_i = \beta \cdot T_i + \delta \cdot S_m + \varepsilon_i$$

but the econometrician ignores this and simply compares treated and untreated firms:

$$E[y_i|T = 1] - E[y|T = 0] = \beta + \delta \cdot (E[S_m|T = 1] - E[S_m|T = 0]) .$$

If the treatment is unconditionally randomized then the second term is zero and the comparison yields a consistent estimate of the direct effect. But if T and S_m are positively correlated, then—given that $\delta < 0$—the second term is negative and the comparison underestimates the direct effect. Such positive correlation between T and S_m may arise when the research design uses policy variation in which treated firms are more similar to each other than to untreated firms: in such cases we might expect that treated firms have a higher share of treated competitors than do control firms.

6 Conclusion

Taken together, our results suggest that financial access had large positive direct effects on firms and large negative indirect effects on their competitors; these effects were further enhanced by diffusion effects; and that while the positive and negative effects approximately cancel out at the firm level, they generate sizable gains to consumers.

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A Appendix

Proof of Proposition 1. Normalize the wage and the price of H to one. Begin with the consumer's problem, and let $P_m = \int P_{m,i} Q_{m,i} di / Q_m$ be the price of the composite good in market m. The consumer's objective function can be written as

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

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

$$Q_m = \left(\frac{\theta}{\theta - 1} \right)^{-\theta} P_m^{-\theta}. \tag{9}$$

Now consider demand within a market. For a fixed budget B_m over market m, maximizing Q_m given prices $P_{i,m}$ yields the first order condition

$$Q_m^{\frac{1}{\sigma}} Q_{m,i}^{\frac{-1}{\sigma}} = \lambda_m P_{m,i}$$

where λ_m is the multiplier over the budget constraint. Manipulating and integrating this condition yields $\lambda = 1/P_m$. It follows that

$$\frac{Q_{i,m}}{Q_m} = \left(\frac{P_{i,m}}{P_m} \right)^{-\sigma} \tag{10}$$

and

$$\frac{P_{i,m} Q_{i,m}}{P_m Q_m} = \left(\frac{P_{i,m}}{P_m} \right)^{1-\sigma} \tag{11}$$

and also

$$P_m^{1-\sigma} = \int P_{i,m}^{1-\sigma} di. \tag{12}$$

From these and the previous equations, given prices $P_{i,m}$, 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 $P_{i,m} Q_{i,m} - w L_{i,m}$ subject to the demand curve (10) yields the usual constant markup

$$P_{i,m} = \frac{\sigma}{\sigma - 1} \cdot \frac{w}{\omega_{i,m}}. \tag{13}$$

Now consider firm revenue. Combining (11) and the optimal price (suppressing m from the notation where it does not cause confusion)

$$\log P_i Q_i = \log P_m Q_m + (\sigma - 1) \log P_m - (\sigma - 1) \log \left(\frac{\sigma}{\sigma - 1} \right) - (\sigma - 1) \log w + (\sigma - 1) \log \omega_i. \quad (14)$$

Using (9) we can express this further as

$$\log P_i Q_i = +(\sigma - \theta) \log P_m - \theta \log \left(\frac{\theta}{\theta - 1} \right) - (\sigma - 1) \log \left(\frac{\sigma}{\sigma - 1} \right) - (\sigma - 1) \log w + (\sigma - 1) \log \omega_i. \quad (15)$$

Maintaining the normalization $w = 1$, the treatment effect on revenue is then

$$\Delta \log P_i Q_i = +(\sigma - 1) \Delta \log \omega_i + (\sigma - \theta) \Delta \log P_m. \quad (16)$$

Here the first term is simply $\Delta \log \omega_i = \gamma T_i$. Using (12), the constant markup price, and the fact that the treatment is random, we can write

$$\begin{aligned} \Delta \log P_m &= \frac{1}{1 - \sigma} \Delta \log \int \omega_i^{\sigma - 1} di \\ &= \frac{1}{1 - \sigma} \log \left((1 - S_m + S_m e^{\gamma(\sigma - 1)}) \int \omega_i^{\sigma - 1} di \right) - \frac{1}{1 - \sigma} \log \int \omega_i^{\sigma - 1} \\ &= \frac{1}{1 - \sigma} \log (1 - S_m + S_m e^{\gamma(\sigma - 1)}). \quad (17) \end{aligned}$$

A first-order Taylor approximation of the last expression around $\gamma = 0$ yields $-S_m \gamma$.

Proof of Proposition 2. The producer surplus is just the increase in profits which increases the budget of the representative consumer. For the consumer surplus, note that by the envelope theorem, the welfare effect of a marginal price change is equivalent to its budget impact at current prices. Assuming γ is small, the cost-saving from the treatment is approximately γR_m.

Specification check. Table A1 shows that when including both the share of competitors and the share of non-competitors treated, the former has a consistently larger (more negative) coefficient, suggesting that our definition of the set of the firm's competitors is meaningful.

Table A1: Specification Check: Competitors

Dep. var.:	log sales	profit	log employment	log wage bill	fixed assets	log materials	shutdown
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
post	0.04**	4.79***	0.041***	0.063***	2.393	0.026	0.089***
	(0.017)	(1.68)	(0.014)	(0.014)	(1.519)	(0.022)	(0.014)
post*treatment	0.092***	12.74***	0.069***	0.082***	5.020	0.077*	-0.035***
	(0.028)	(2.213)	(0.025)	(0.026)	(4.502)	(0.044)	(0.01)
post*share competitors treated	-0.07	-11.84	-0.073*	-0.075*	-6.064	-0.053	0.017
	(0.07)	(18.09)	(0.037)	(0.042)	(5.919)	(0.097)	(0.021)
post*share non-competitors treated	-0.007	0.016	0.01	0.012	3.809	0.003	-0.022
	(0.071)	(19.21)	(0.047)	(0.051)	(6.853)	(0.111)	(0.024)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,220	8,220	8,220	8,164	8,220	8,213	8,343
Number of firms	2,781	2,781	2,781	2,779	2,781	2,781	2,781