Selective Outreach and Targeted Interest Rate Caps*

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Abstract

This paper studies how interest rate caps that target one sector affect lending and welfare when lenders selectively contact potential borrowers. Using a reform that introduced an interest rate cap on agricultural loans in Ecuador, I document that after the reform, cooperatives' lending shifted toward the non-agricultural sector while banks' agricultural lending expanded. I estimate a model of selective outreach in which lending reflects lenders' offer and borrowers' acceptance decisions. By lowering prices, a cap can increase acceptance while reducing lenders' incentive to extend offers, and targeting can lead to cross-sector offer reallocation. I find that banks and cooperatives reduced offers to agricultural borrowers, outweighing the increase in acceptance and reducing aggregate agricultural lending and welfare. The regulation shifted acceptance toward banks, whose prices fell more due to the cap, allowing their lending to increase despite fewer offers. Targeting magnified the offer contraction, accounting for most of the lending reduction and generating welfare losses in the agricultural sector. A less stringent cap would have raised agricultural borrowers' consumer surplus. The results show that, by influencing the relative strength of the offer and acceptance responses, policy design shapes the cap's ultimate effects.

Keywords: interest rate caps, regulation, credit supply, agricultural credit
JEL Codes: G21, G28, L13, L51, Q14

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1 Introduction

In many credit markets, lenders choose which potential borrowers to contact to originate loans, a practice I refer to as selective outreach. This practice is common in microfinance markets (Banerjee et al., 2013; Varangis et al., 2014; Angelucci et al., 2015) and the credit card market in the United States (Ausubel, 1999; Agarwal et al., 2010; Stango and Zinman, 2016; Han et al., 2018). In these markets, loan origination requires both an offer from the lender and acceptance by the borrower, and interest rates shape both decisions. A common motivation for interest rate caps is to make credit more accessible for borrowers in a specific sector (Maimbo and Henriquez Gallegos, 2014; Ferrari et al., 2018).[1] Such a policy makes credit cheaper for the targeted group but can also make lending to that group less attractive to lenders.[2]

This paper studies how targeted interest rate caps affect lending and welfare in markets with selective outreach. Understanding how caps influence offers and acceptance, and accounting for lenders' opportunities outside the targeted sector, are central to policy design and evaluation because these forces determine whether a cap expands or reduces lending and welfare.

I examine a reform that introduced a binding interest rate cap targeting agricultural loans in Ecuador in 2019 to expand credit access. Using original survey data, I document that lenders in this setting initiate contact with borrowers and that their outreach decisions are strategic. These facts motivate a stylized theoretical framework in which lenders choose the borrowers to whom they extend offers, subject to a capacity constraint. An offer's expected profitability is the product of its acceptance probability and the margin between the price and borrower-specific costs.[3] A lender extends an offer when its expected profitability exceeds a threshold that depends on the expected profitability of offers to other potential borrowers. Offers completely determine borrowers' choice sets, and borrowers choose the utility-maximizing alternative among their offers and an outside option.

Lending depends on offers and acceptance, and a cap can move both components in opposite directions. Since these forces modify the conversion rate of offers into loans, lend-

[1] Targeted interest rate caps are common. In the United States, the Military Lending Act caps interest rates on loans to service members, and several other countries cap rates in priority sectors (Ferrari et al., 2018).

[2] The same trade-off is induced by caps that vary by location, loan size, term, or purpose. Examples include state-level caps on personal loans in the United States (Cherry, 2026) and caps that apply to microcredit loans in Colombia (Cubillos-Rocha et al., 2021) and Cambodia (Heng et al., 2021).

[3] Throughout the paper, I use "price" to refer to the interest rate.

ing can rise even when offers fall if acceptance rises sufficiently. The cap also shifts acceptance across lenders. Lenders with higher pre-regulation interest rates cut prices by more and gain acceptance at the expense of rivals that cut prices by less. When the cap targets one sector, it changes the expected profitability of offers in that sector, and thresholds adjust so that lenders continue to use their full capacity. These changes reallocate offers across sectors. Since the cap affects acceptance, the number of offers, and their allocation, its overall effect on lending is an empirical question.

The agricultural cap in Ecuador was set between 2.5 and 9.5 percentage points (p.p.) below pre-existing caps that made no distinction between economic activities. Almost 70% of agricultural loans originated in the preceding year carried interest rates above the new cap. The market is served by cooperatives and banks that differ in governance, scale, portfolio composition, and cost structures. I find contrasting price and quantity responses across the two lender types. A regression that controls for persistent local market conditions and lender-specific shocks shows that, relative to their non-agricultural counterparts, agricultural interest rates decreased by 2.76 p.p. (10.8%) for banks and 0.67 p.p. (3.1%) for cooperatives. The larger decline for banks is consistent with their higher baseline prices. In terms of quantities, cooperatives' agricultural lending fell while their non-agricultural lending rose. In contrast, banks' agricultural credit increased substantially while their non-agricultural lending expanded modestly. Aggregating both lender types, total agricultural lending decreased 4.2% while non-agricultural lending increased 3.3%. Relative to non-agricultural lending, agricultural credit volume decreased by 30% for cooperatives while it increased by 24% for banks. In both cases, the primary margin of adjustment is the number of loans rather than loan size.

These findings show the net changes in lending associated with the targeted cap, which combine unobserved offer and acceptance decisions and reflect both the cap's level and scope (i.e., targeting to agricultural loans). To quantify the importance of each of these elements and their welfare implications, I estimate an empirical model of the targeted sector that captures the main features of the theoretical framework. Because lenders' capacity is unobserved in the data, instead of deriving the threshold from capacity, I parametrize it to reflect the cross-sector price difference induced by the cap. Because non-agricultural borrowers without a loan are also unobserved in the data, the untargeted sector enters the empirical model only through the threshold, which is interpreted as a reservation profitability that an offer must clear for a lender to extend it in the agricultural sector.

I estimate the model in two steps. The first step deals with the challenge that offers are not

observed and borrowers' choice sets are endogenous and heterogeneous. I follow He et al. (2024) and Agarwal and Somaini (2025) by using a Gibbs sampler that treats offers as a latent variable and jointly estimates borrower preferences, lender costs, and pre-reform thresholds using data on loans originated prior to the reform. Identification relies on exclusion restrictions that pin down how the borrower utility and offer equations separately affect the origination probability. The second step addresses the fact that the changes in thresholds induced by the reform are also unobserved. I infer the adjustments of thresholds that rationalize post-reform outcomes conditional on the parameters estimated in the first step and project them onto the cross-sector price difference induced by the cap.

The structural estimates imply that borrower choice sets are restricted and that outreach and demand are heterogeneous across banks and cooperatives. On the supply side, while costs are similar for both lender types, offer probabilities are larger for banks, reflecting their higher profit margins and lower pre-reform thresholds. Banks' thresholds are more sensitive to the cross-sector price difference created by the cap. On the demand side, I find that the price elasticity of demand is larger for banks than for cooperatives.

I use the estimated model to conduct three counterfactual simulations that quantify the offer and acceptance responses and the role of targeting. Simulating the implemented targeted cap on the pre-reform borrower pool reproduces the main patterns documented in the descriptive and regression analyses. I then separate the acceptance and offer responses. I find that aggregate lending would have increased if offers had not responded to the cap. However, the offer response is negative and more than four times as large as the acceptance response. The net effect is a 13.5% contraction in aggregate lending and a 7.9% loss in consumer surplus. Thus, although the acceptance response moved in the intended direction, the offer response more than offset it, so the cap reduced lending and consumer surplus, the opposite of its intended effect.

Focusing on lender types, I find that, while both banks and cooperatives reduced the number of offers, their contrasting lending adjustments are explained by the acceptance response. Since banks' baseline prices were higher, the cap reduced them more, which shifted acceptance from cooperatives to banks. Cooperatives' agricultural lending fell because they made fewer offers and lost acceptance to banks. In contrast, banks' agricultural lending rose because higher acceptance more than offset their offer contraction, illustrating that observed changes in lending need not reveal the underlying changes in supply.

To evaluate the contribution of the level and scope of the cap, I compare the targeted cap with a counterfactual in which lenders make offer decisions independently across sectors. I find that targeting accounts for 77% of the lost offers and 67% of the lending contraction under the targeted cap. A cap set at the same level but that does not target the agricultural sector would still reduce the number of offers and lending, but by much less. Such a cap would increase consumer surplus because the effect of lower prices outweighs that of fewer offers. Consequently, the targeted design of the regulation magnifies the negative offer response and is the source of the consumer surplus losses among targeted borrowers.

I then evaluate the effects of alternative caps by varying their level and scope. Less stringent caps, which mainly constrain lenders with higher baseline prices, raise consumer surplus. Without targeting, the number of offers, lending, and consumer surplus are higher than under targeting at every cap level. These results show that a well-chosen cap can benefit agricultural borrowers on average.

This paper relates to several strands of the literature. Most of the work studying the effects of interest rate caps (Madeira, 2019; Fekrazad, 2020; Cubillos-Rocha et al., 2021; Cuesta and Sepúlveda, 2021; Heng et al., 2021; Kuroishi et al., 2025; Burga et al., 2026; Cherry, 2026) examines market segments in isolation, abstracting from the fact that interest rate caps do not always cover all loans. I document cross-sector shifts in lending that followed the introduction of a targeted cap and rationalize them with a framework in which changes in relative profitability lead lenders to reallocate offers to untargeted sectors. The results highlight that targeted interest rate caps can amplify the incentives for lenders to stop serving targeted borrowers and that targeting accounts for most of the lending contraction and generates a consumer surplus loss.

More broadly, this paper contributes to the literature using structural models of competition in financial markets (Crawford et al., 2018; Allen et al., 2019; Benetton, 2021; Agarwal et al., 2024; Buchak et al., 2024; Allen et al., 2025; Benetton et al., 2025; Nelson, 2025). I document that selective outreach is the main lending channel in my setting, providing an alternative microfoundation for pricing and matching patterns that have traditionally been explained by search and other frictions. To the best of my knowledge, this is the first paper modeling lender-initiated outreach as both a supply allocation mechanism and a microfoundation for choice set formation. Even though the literal interpretation features lenders extending offers before borrowers choose, the framework can be extended to other settings where lender selectivity determines who obtains credit.

Finally, this paper is related to the literature that incorporates constraints on consumer choices (Goeree, 2008; Abaluck and Adams-Prassl, 2021; Agarwal and Somaini, 2025; He et al., 2024). Unlike models where choice set restrictions arise from consumer search or limited attention, here they are determined by the supply side. My model is closest to Gandhi (2023), who investigates selective admissions in the nursing home industry, but differs in that each lender's offer decision depends on a belief about the borrower's acceptance probability, which itself depends on rivals' offer decisions and borrower preferences, generating an equilibrium feedback loop between supply and demand.

2 Lenders' selective outreach

In many credit markets, lenders partly overcome search and matching frictions by reaching out to borrowers rather than waiting for applications. Outreach can take different forms. Credit card issuers in the United States extend offers based on credit history and other borrower characteristics (Ausubel, 1999). In other markets, loan officers are tasked with actively prospecting for new loans (Agarwal and Ben-David, 2018). In microfinance and agricultural lending, where borrowers are geographically dispersed and typically lack formal financial histories, lenders often deploy mobile loan officers to reach smallholder farmers (Varangis et al., 2014).

To understand the matching process between borrowers and lenders in Ecuador, I collected survey data from financial institutions that serve small firms.[4] The survey was designed to gather information on lenders' customer acquisition channels, the decision-making process behind outreach, and the perceived competitive environment.

The survey yields four main findings, shown in Table 1. First, all responding institutions report that they initiate contact with potential borrowers to originate loans. Second, this is the predominant origination channel. Third, fewer than one in three contacts is converted into actual loans. Fourth, outreach decisions are made strategically as respondents report considering the presence of competing institutions and prioritizing which borrowers to contact based on historical data, loan officer recommendations, and headquarters guidelines. Overall, the survey provides evidence on the relevance and the selective nature of

[4] 30 out of 60 contacted financial institutions responded to the survey. 10 of the surveyed lenders, including banks and cooperatives, are part of the sample used to estimate the empirical model. Table A.1 presents a more comprehensive list of survey responses.

lender outreach.

Table 1: Survey evidence on selective outreach

	Response
Institutions that initiate contact with potential borrowers	100%
Share of loans originated through lender-initiated contact (mean)	58.4%
Conversion rate of lender-initiated contacts [a]	27.6%
Institutions that consider competitor presence in outreach decisions	96.7%

Notes: Survey of 30 Ecuadorian financial institutions serving small firms in the microcredit segment, as defined by the regulation, conducted in 2026. The survey was administered in Spanish. Translated questions corresponding to each row: (1) "Does your institution initiate contact to attract clients for microcredit origination?" (2) "What percentage of microcredits are originated from a first contact made by the institution rather than by the client?" (3) Ratio of "How many contacts with potential clients/members initiated by the institution result in an originated loan?" to "How many contacts with potential clients/members does the institution initiate?" (4) "Is the presence of other banks/cooperatives taken into account when deciding which clients to contact?"
[a] Reports the average conversion rate across 29 respondents.

3 Theoretical framework

Motivated by the survey findings, this section presents a stylized theoretical framework showing how targeted interest rate caps affect lending in markets with selective outreach.

Model. Consider a set of borrowers, indexed by i, and lenders, indexed by j. Lenders charge a single price (interest rate) and extend offers to borrowers. Let $\sigma_{ij} \in \{0, 1\}$ indicate that lender j extends an offer to borrower i, so that borrower i's choice set is $O_i = \{j : \sigma_{ij} = 1\}$.[5]

Borrower i obtains indirect utility $u_{ij} = u(p_j, x_{ij})$ from a loan originated by lender j. Utility is decreasing in the price p_j and depends on borrower and borrower-lender characteristics denoted by x_{ij}. There is an outside option that delivers utility u_{i0}. Borrowers accept the best offer they receive as long as its utility is greater than the utility of the outside

[5]The survey shows that not all loans are originated through lender-initiated contact. In the model, the offer decision captures whether the lender is willing to serve a borrower at a given price, regardless of who initiated the contact. The implicit assumption is that extending an offer and approving a borrower-initiated application follow the same decision rule.

option:

$$a_{ij} = \mathbb{1}\{u_{ij} \geq u_{ij'} \ \ \forall j' \in O_i\} \cdot \mathbb{1}\{u_{ij} \geq u_{i0}\} \tag{1}$$

where a_{ij} is an indicator for acceptance, and $a_{ij} = 0$ if $j \notin O_i$.

Lender j faces a borrower-specific cost of lending c_{ij}, which is private information. In addition, a capacity constraint allows the lender to extend at most K_j offers. K_j is exogenous and captures operational capacity (for instance, the number of staff assigned to initiate contacts with potential borrowers).[6] Lender j chooses which borrowers to contact according to expected profitability:

$$\max_{\sigma_{ij}} \sum_i \sigma_{ij} \Pr(a_{ij} = 1)(p_j - c_{ij}) \qquad \text{s.t.} \qquad \sum_i \sigma_{ij} \leq K_j \tag{2}$$

Acceptance enters as a probability because rivals' costs are private information, which implies that lender j cannot infer its rivals' offers with certainty and therefore does not know O_i. The probability is taken over the choice sets the borrower may face.

The solution to the lender's problem is characterized by an offer rule:

$$\sigma_{ij}^* = \mathbb{1}\{\Pr(a_{ij} = 1)(p_j - c_{ij}) > \lambda_j\} \tag{3}$$

An offer is extended if its expected profitability is greater than a threshold λ_j, which is the multiplier on the capacity constraint and represents the opportunity cost of the marginal offer. Intuitively, the lender ranks borrowers by expected profitability and extends offers to the top K_j, that is, those above the threshold.

Lending depends on offers, from the supply side, and acceptance, from the demand side:

$$L_j = \underbrace{N_j}_{\text{offers}} \times \underbrace{\bar{a}_j}_{\text{acceptance rate}} \tag{4}$$

where $N_j = \sum_i \sigma_{ij}$ is the number of offers extended and $\bar{a}_j = \sum_i a_{ij} / N_j$ is the share of offers that are accepted.

[6] This assumption is consistent with Fuster et al. (2024), who present evidence suggesting that mortgage intermediation capacity does not adjust in the short run.

The targeted cap. Now suppose borrowers belong to one of two sectors and lenders charge a single price in each sector. When a binding interest rate cap $\bar{p}$ is imposed only on one of the sectors, prices drop in the targeted sector. Assume that prices in the untargeted sector are unchanged.

In the targeted sector, the cap generates two opposing forces on acceptance. As lender j's own price falls, its offers become more attractive relative to both the outside option and rivals' offers, so lender j's acceptance rises. As rivals' prices fall, lender j's acceptance falls because rivals' offers become more attractive. The net effect depends on which force dominates. The magnitude of the lender's price adjustment relative to its rivals is critical. Lenders with higher pre-regulation interest rates cut prices by more than their rivals and gain acceptance at the expense of lenders that cut prices by less.

The offer rule (3) depends on expected profitability and the threshold λ_j. For now, consider the threshold fixed. The cap affects both terms that determine expected profitability. The lower price compresses the profit margin $(p_j - c_{ij})$, so fewer borrowers clear the threshold and outreach contracts. At the same time, the change in acceptance probability is ambiguous as described above. Therefore, the overall effect on offers is undetermined.

If acceptance falls, outreach and lending decrease unambiguously. If acceptance rises and outreach expands because the gain in acceptance probability dominates the margin compression, lending increases. If acceptance rises but outreach falls, the change in lending is ambiguous. In particular, lending can rise even when the number of offers decreases.

Supply reallocation. Changes in the expected profitability of targeted borrowers modify their position in the ranking and induce an adjustment in the threshold λ_j so the lender uses its full capacity. To see this, note that if targeted borrowers become less profitable relative to untargeted borrowers, fewer of them clear the original threshold, which leaves capacity unused. The threshold then falls and untargeted borrowers who were just below the original threshold now receive offers. The opposite occurs if targeted borrowers become more profitable. Therefore, since all borrowers are compared against a common threshold that clears capacity, the targeted cap generates supply reallocation across sectors.

This stylized framework produces two takeaways. First, under selective outreach, lending depends on both offers and acceptance. The effect of a cap on offers cannot be inferred from changes in lending, since acceptance also changes. Second, when lenders allocate

limited resources across sectors, a threshold rule accounts for the profitability of opportunities in all sectors. The number of offers in the targeted sector is determined both by changes in the expected profitability of targeted borrowers and the corresponding threshold adjustment. Typically, only lending is observed in administrative data, while offers and the acceptance rate are not. The main goal of the empirical analysis is to disentangle them in order to quantify how a targeted interest rate cap affects the offer and acceptance responses, the role of targeting, and the cap's welfare effects.

4 Setting

The empirical setting is the microcredit market in Ecuador, which is characterized by low levels of credit access and high interest rates. A sizable cooperative sector coexists with banks, and lenders actively seek out borrowers to originate loans. These features are common in other low- and middle-income countries.

4.1 The credit market in Ecuador

Ecuador's financial sector is relatively underdeveloped. The International Monetary Fund's Financial Development Index ranked the country 128th out of 183 in 2021. The share of adults who borrow from a formal financial institution is comparable to the developing-country average (Development Research Group, 2022). As few financial products target excluded segments (World Bank, 2024), it is unsurprising that only about 5% of farms received credit from a formal lender in 2019 (INEC, 2022).

The regulatory framework defines microcredits as loans with productive purposes granted to borrowers whose economic activity generates up to $100,000 in annual sales. In practice, this constitutes lending to small firms. Over the period of analysis (2017–2020), more than 150 cooperatives and banks granted microcredit to both agricultural and non-agricultural borrowers.[7] While cooperatives and banks compete for the same microcredit

[7] A sizable public bank also operates in this market; however, it does not appear to play a first-order role in the context of interest rate caps, as its interest rates fall well below any regulatory cap and competition with private lenders is limited. Appendix B describes the public bank's characteristics, documents changes in its interest rates and lending around the reform, and explores substitution patterns with private agricultural credit.

borrowers, they differ in a number of dimensions, which motivates the investigation of heterogeneous responses to the reform.[8]

Cooperatives are the dominant source of formal credit for small-scale farmers, providing 55% of funds directed to the agricultural microcredit market in 2018. They are member-owned and operate on principles of self-help, self-responsibility, and self-governance (Jácome and Cordovez, 2004). They are supervised by the Superintendencia de Economía Popular y Solidaria, and concentrate their lending on microcredit and consumer segments. The average cooperative serves 8 provinces.

Banks primarily serve larger firms and account for only 12% of the agricultural microcredit market in 2018. Banks are shareholder-owned, supervised by the Superintendencia de Bancos, and maintain diversified portfolios spanning consumer, commercial, mortgage, and microcredit segments. They generally cover broad geographic areas, with the average bank serving 12 of the 23 continental provinces.

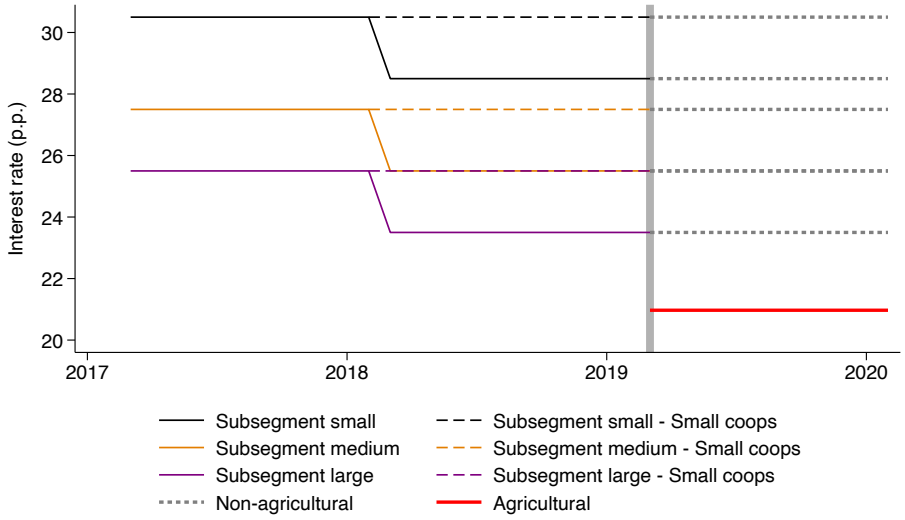
4.2 Interest rate regulation

Effective interest rate caps on new loans have been imposed since 2007.[9] While policymakers claim that the credit market is insufficiently competitive and fails to achieve a socially optimal equilibrium, critics argue that the regulation is discretionary, does not reflect market conditions, distorts capital allocation between consumer and productive lending, and, when binding, reduces the number of new borrowers (Banco Central del Ecuador, 2021; World Bank, 2024).

The regulatory framework defines several market segments and assigns different absolute caps to each.[10] The regulation further divides the microcredit segment into three subsegments based on borrower indebtedness: small (*minorista* for borrowers with debt of up to $1,000), medium (*acumulación simple* for borrowers with debt of more than $1,000 and up to $10,000), and large (*acumulación ampliada* for borrowers with debt of more than $10,000). Each subsegment carries its own interest rate cap. Since 2018, these caps also varied by lender type, with banks and large cooperatives facing lower caps than small cooperatives.

[8] 29 out of 30 surveyed lenders perceive meaningful competition between cooperatives and banks.
[9] Effective interest rates include nominal interest rates, fees, and charges.
[10] Segments include commercial loans, consumer loans, microcredits, and loans for real estate.

Figure 1: Evolution of interest rate caps in the microcredit segment



Notes: This figure displays the evolution of interest rate caps in the microcredit segment. Before 2018, three caps were defined based on borrowers' outstanding debt: (i) Subsegment small (*minorista* for borrowers with debt of up to $1,000); (ii) Subsegment medium (*acumulación simple* for borrowers with debt of more than $1,000 and up to $10,000); and (iii) Subsegment large (*acumulación ampliada* for borrowers with debt of more than $10,000). In 2018, the caps for banks and large cooperatives were reduced (solid lines), while they remained unchanged for smaller cooperatives (dashed lines). The vertical line indicates the introduction of the cap specific to agricultural loans in 2019. The red line denotes the agricultural cap and gray dotted lines represent the caps for non-agricultural activities.

The reform I study took place in March 2019, when the regulator, the Junta de Política y Regulación Monetaria y Financiera, introduced an interest rate cap specifically for agricultural and livestock activities (for simplicity, referred to here as agricultural activities).[11] The stated purpose was to increase credit access in the agricultural sector. In practice, this reform created a new subsegment within the microcredit segment that encompassed all loans linked to agricultural activities, with a cap set at 20.97%. Prior to this regulatory change, interest rate caps applied to all loans, regardless of the economic activity they financed. As shown in Figure 1, the reform lowered interest rate caps between 2.5 and 9.5 p.p.

[11] In September 2020, the caps for some segments, including the agricultural microcredit subsegment, were removed from the interest rate regulation system. The regulator acknowledged that the caps may have led to incorrect risk pricing, inefficient credit allocation, and increased financial exclusion (Junta de Política y Regulación Monetaria y Financiera, 2020). I do not study the reform reversal, as it occurred during the COVID-19 pandemic.

4.3 Data

I use administrative data on loan originations and performance provided by Ecuador's two financial supervisory agencies: the Superintendencia de Bancos and the Superintendencia de Economía Popular y Solidaria. The period of analysis runs from January 2017 to March 2020. I complement the data with information on branch locations, agricultural survey data, and land use.

Loan data. I construct a dataset covering all new microcredit loans originated between January 2017 and March 2020 by combining administrative records from both agencies. Both datasets include loan size, effective interest rate, parish, and economic activity classified at the class level of the International Standard Industrial Classification of All Economic Activities (ISIC).[12] I define agricultural loans as those associated with economic activities classified under ISIC Division A01.

Coverage is comprehensive for banks but limited to the highest asset tiers for cooperatives.[13] Cooperatives' records are richer, including borrower-level income, expenditures, wealth, education, housing status, and loan term. Bank records are aggregated at the lender–parish–subsegment–activity–month level.[14]

The analysis focuses on microcredit loans up to $10,000, which account for 89% of all loans and 92% of agricultural loans in the pre-reform period, as shown in Figure A.1.[15] I exclude larger loans because these borrowers are more likely to have greater access to credit and are therefore not the primary targets of the regulation.[16] All descriptive and regression results are robust to including these larger loans.

Default probability. I estimate a lender-specific default probability model using monthly performance snapshots of cooperatives' outstanding loans. Predicted values are used as lenders' assessment of borrower default probability. This aligns with the institutional setting, where an important share of the population does not have a credit history and

[12] A parish is the smallest administrative division in Ecuador.

[13] Cooperatives are classified into five tiers by asset size. The dataset covers tiers 1 through 3, excluding tiers 4 and 5, which account for a small share of the market and report less frequently and less reliably.

[14] For example, if a bank granted multiple loans in a given month and parish for maize production to borrowers within the same regulatory subsegment, I observe the aggregate loan amount, the average interest rate (weighted by loan size), and the total number of loans. About two-thirds of observations in the bank dataset correspond to a single loan.

[15] I exclude a small number of outliers with interest rates above 30.5% (the maximum allowed during the analysis period) or below 10% as well as loans of less than $100.

[16] This implies excluding the subsegment for large loans (*acumulación ampliada*).

lenders predict risk based on borrowers' observable characteristics. Appendix C provides details on the procedure used to predict default probabilities.

Agricultural survey data. I use data from the Continuous Agricultural and Livestock Area and Production Survey (ESPAC), conducted by Ecuador's national statistics institute (INEC). ESPAC collects information on the most important agricultural and livestock activities during the last quarter of each year, covering all 23 continental provinces with province-level representativeness. Its finance module records whether a farmer obtained a loan from a cooperative, a private bank, or a public bank.[17] I use this survey to identify agricultural producers who did not obtain credit from a formal financial institution and construct the pool of potential borrowers who do not obtain a loan in the empirical model.[18] Producers with farms exceeding 20 hectares are excluded, as smallholders are the primary borrower type in the microcredit segment.

Borrower-lender distance. I combine the locations of lenders' branches, obtained from the supervisory agencies, with land use data from MAG (2020) to approximate the distance between borrowers and lenders. I construct a parish-level measure of distance, which I assign to each borrower based on parish of residence.[19]

Descriptive statistics. Table 2 presents summary statistics for agricultural loans, borrower and market characteristics. The loan dataset covers approximately 390,000 agricultural microcredit originations. All statistics are weighted by the number of loans to account for aggregation in the bank data. The mean interest rate is 22.75% and the average loan amount is roughly $3,000, which is insufficient to cover the average production cost of one hectare of perennial crops (INEC, 2022). The mean loan term is approximately 19 months, and the mean predicted default probability is 5%.

Borrower characteristics vary substantially. Among cooperatives' customers, average monthly income is around $1,800, and only 37% have attended secondary education or more. Roughly 44% of borrowers finance annual crop production, followed by livestock

[17] The 2020 round includes information useful for understanding farmers' demand for credit: while only about 9% of farmers report having applied for a loan, 46% report that they would apply if they had access. This highlights the extent of unmet demand for formal credit.

[18] There is no equivalent data for non-agricultural producers that did not obtain a loan.

[19] The loan data do not report borrowers' exact locations but do indicate their parish of residence. From the land use data, I identify plots used for agricultural production within each parish and calculate the linear distance from the centroid of each plot to the nearest branch of every lender. I then average these distances across all plots within a parish to obtain a parish-level borrower-lender distance measure.

(32%) and perennial crops (15%). The mean distance to the originating lender's nearest branch is close to 15 kilometers. At the market level, a typical parish is served by 8 lenders, while the average lender operates only 2 branches in the provinces where it is present.

Table 2: Summary statistics

	Mean	SD	P10	P90	N
Panel A: Loan characteristics					
Interest rate (p.p.)	22.75	3.67	18.39	27.41	388,207
Loan size (USD)	2,949.25	2,339.49	700.00	6,000.00	388,207
Maturity (months)[a]	19.04	13.18	6.00	36.00	285,796
Default probability[a]	0.05	0.07	0.01	0.12	285,796
Panel B: Borrower and market characteristics					
Monthly income (USD)[a]	1,784.23	5,205.19	400.00	3,800.00	285,796
Secondary education or more[a]	0.37	0.48	0.00	1.00	285,796
Annual crops	0.44	0.50	0.00	1.00	388,207
Perennial crops	0.15	0.36	0.00	1.00	388,207
Livestock	0.32	0.47	0.00	1.00	388,207
Distance to nearest branch (km)	14.36	13.79	4.02	30.27	388,207
Number of lenders in parish[b]	8.17	6.86	2.00	17.00	994
Number of lender branches in province[c]	2.23	5.10	0.00	5.00	791

Notes: This table displays summary statistics for loan, borrower, and market characteristics.
[a] Available for cooperatives only.
[b] Observation is a parish.
[c] Observation is a lender-province.

Appendix D documents evidence motivating modeling assumptions. Specifically, it shows that lenders price almost uniformly within a regulatory subsegment, and that banks and cooperatives differ markedly in scale, funding costs, and branch density, while serving the same borrower pool.

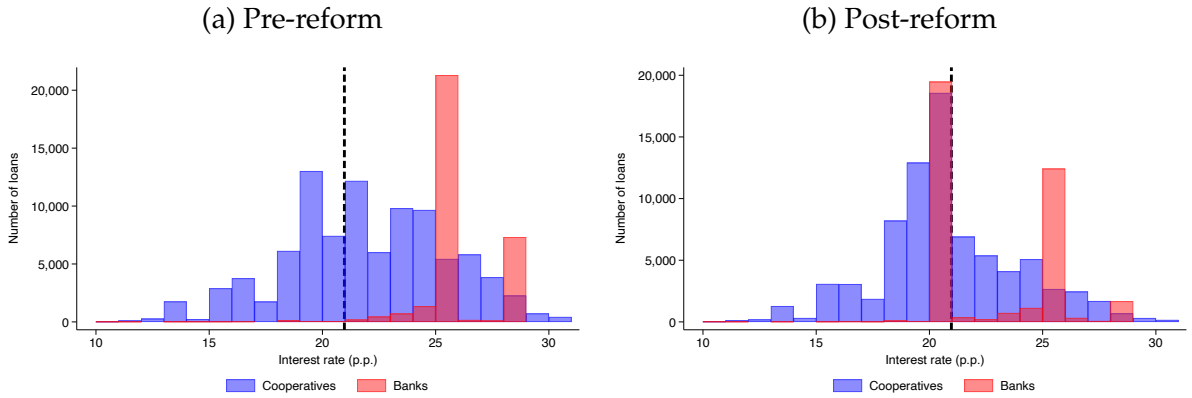
5 Price and lending adjustments following the reform

In this section, I document that the agricultural interest rate cap was binding and that it led to contrasting price and quantity responses across cooperatives and banks.

5.1 Descriptive evidence

Figure 2 plots the distribution of interest rates on agricultural loans, where panels (a) and (b) correspond to the year preceding and following the reform, respectively. Panel (a) shows that the interest rate cap was binding, as 70% of the loans (almost all loans granted by banks and 60% of those issued by cooperatives) carried interest rates above the agricultural cap. While cooperatives' interest rates are more dispersed, mostly falling within the 19%–26% range, banks' rates cluster at the caps for pre-existing regulatory subsegments. Panel (b) shows that, after the reform, interest rates bunch at the new cap. However, 40% of agricultural loans still exceeded the cap. Lenders classified these loans under other regulatory subsegments despite financing agricultural economic activities.[20]

Figure 2: Distribution of interest rates by lender type



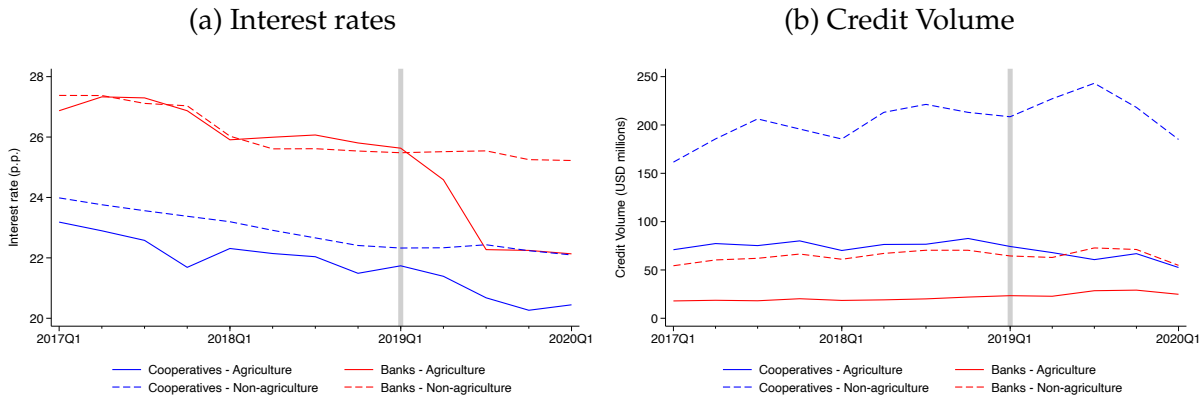
Notes: These figures show the distribution of interest rates on agricultural loans issued in the year preceding and the year after the reform. In both panels, blue and red bars represent loans granted by cooperatives and banks, respectively. The black vertical line represents the agricultural interest rate cap.

Figure 3 presents the evolution of mean interest rates and aggregate credit volume in the agricultural and non-agricultural sectors, separately for cooperatives and banks. Panel (a) shows an immediate decline in agricultural interest rates following the reform. For banks, the mean interest rate falls from around 25.8% to 22.1% within two quarters of the reform. The decline among cooperatives is smaller, with the average agricultural rate decreasing from 21.8% to 20.3% by the last quarter of 2019. In contrast, interest rates on non-agricultural loans remain largely stable for both lender types.

Panel (b) suggests that cooperatives' agricultural credit volume decreases following the

[20]Noncompliance rates are similar across lender types. Regressing an indicator for noncompliance on a bank indicator with parish and month fixed effects, and clustering standard errors at the lender level, yields a coefficient of 0.017 (p-value = 0.90).

Figure 3: Evolution of interest rates and credit volume by sector and lender type



Notes: These figures plot the evolution of mean interest rates and aggregate credit volume across sectors and lender types. In both panels, solid lines denote agricultural lending, while dashed lines correspond to non-agricultural lending. The vertical line indicates the introduction of the cap specific to agricultural loans.

reform, while their non-agricultural credit volume increases. For banks, changes in sectoral credit volumes are difficult to discern from the figure. To make these changes clearer, Table 3 reports sector-level annual credit volume. Following the reform, cooperatives' agricultural lending decreases by 13.8% while their non-agricultural credit expands by 3.9%. In contrast, banks' lending increases in both sectors, with a markedly larger expansion in agriculture (32.2% vs. 1.5%). Aggregating across lender types, agricultural lending decreases by 4.2% while non-agricultural lending increases by 3.3%.

Table 3: Agricultural and non-agricultural annual credit volume (USD millions)

	Pre-reform		Post-reform		% Change	
	Ag.	Non-ag.	Ag.	Non-ag.	Ag.	Non-ag.
Cooperatives	296.8	812.1	255.7	843.6	-13.8	3.9
Banks	78.8	254.8	104.1	258.6	32.2	1.5
Total	375.6	1,066.9	359.8	1,102.2	-4.2	3.3

Notes: This table reports annual credit volume originated by cooperatives and banks in the agricultural and non-agricultural sectors. Change reports the percent change for each cell. Each period corresponds to the twelve months between March of year t and February of year $t+1$. Pre-reform: March 2018–February 2019. Post-reform: March 2019–February 2020.

5.2 Differential changes across sectors

The preceding descriptive analysis is based on national-level statistics that can mask important heterogeneity across lenders and locations. Thus, I adopt a regression framework to examine these patterns more rigorously. I aggregate the data at the lender–sector–parish–quarter level, distinguishing between agricultural and non-agricultural sectors. The rationale for defining the unit of analysis at this narrow geographic level is that supply-side responses likely depend on the local competitive environment. The sample is restricted to lender–sector–parish combinations that received credit in every quarter prior to the reform.

Evolution of differential changes across sectors. I use an event-study design comparing outcomes across sectors to document their evolution around the time of the reform. The estimating equation is:

$$y_{jlst} = \sum_{\substack{e=-8 \\ e\neq -1}}^{4} \rho_e \left[Agriculture \cdot \mathbb{1}\{t - t_0 = e\} \right] + \iota_{jls} + \iota_{jt} + \eta_{jlst} \tag{5}$$

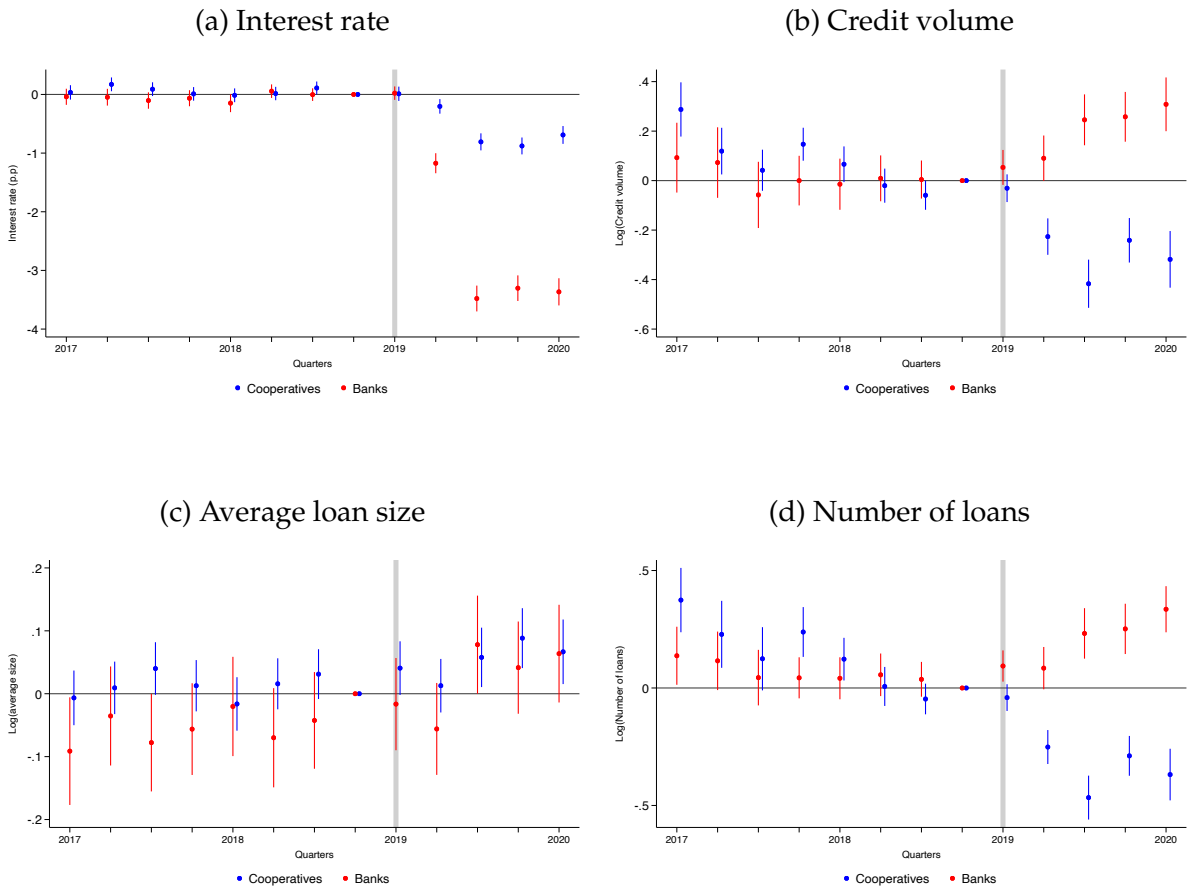
where y_{jlst} is an outcome for lender j, parish l and sector s in quarter t. *Agriculture* is a binary variable that takes the value of 1 for the agricultural sector and zero otherwise. The event-time indicator $\mathbb{1}\{t - t_0 = e\}$ equals one in quarter e relative to the reform quarter t_0. ι_{jls} is a lender–parish–sector fixed effect that absorbs time-invariant heterogeneity in outcome levels as well as unobserved factors that do not change over time such as the local market structure. ι_{jt} is a lender–quarter fixed effect that absorbs seasonality and time-varying unobserved lender-specific shocks. The coefficients ρ_e measure the differential change in agricultural relative to non-agricultural outcomes within a lender, relative to the reference period ($e = -1$; the last quarter in 2018).

Although specification (5) resembles a standard difference-in-differences design, I do not interpret the coefficients as causal estimates of the reform's effect on agricultural lending. As predicted by the theoretical framework in Section 3, the interest rate cap may have induced lenders to redirect offers from the agricultural to the non-agricultural sector, where interest rates are not compressed by the reform. Such reallocation would violate the Stable Unit Treatment Value Assumption, as non-agricultural outcomes are also affected by the reform. This in turn invalidates the parallel trends assumption in the post-reform pe-

riod, since the control group's change is endogenous to the treatment. The coefficients are still informative about how lenders adjusted their pricing and portfolio composition following the cap, after accounting for seasonality, persistent local market conditions, and time-varying lender-specific shocks.

Figure 4 presents the estimated coefficients from equation (5). Results for cooperatives and banks are obtained from separate regressions. The vertical gray bar indicates the quarter in which the agricultural interest rate cap was introduced.

Figure 4: Relative changes in agricultural lending



Notes: These figures show results from equation (5). Dots represent quarter-specific regression coefficients and vertical lines indicate 95% confidence intervals. Standard errors are clustered at the lender-parish-sector level. The vertical gray bar represents the quarter in which the agricultural interest rate cap was introduced. The reference period is the last quarter in 2018. Results for cooperatives and banks are estimated separately. Results for credit volume and number of loans are from Poisson pseudo maximum likelihood regressions.

Panel 4a focuses on the mean interest rate weighted by loan size. There is no systematic

pre-reform difference between the agricultural and non-agricultural sectors, suggesting that the comparison is informative. Interest rates on agricultural loans begin to decline immediately after the reform and reach a stable level within two quarters. Six months after the reform, interest rates charged by cooperatives had decreased by approximately 0.8 p.p., while banks' rates had declined by more than 3.0 p.p.

Having established that agricultural interest rates decreased following the reform, I turn to quantity outcomes. I use a Poisson pseudo-maximum-likelihood estimator, as some units have zero lending in the post-reform period. Panel 4b shows that changes in lending following the cap differ sharply across lender types. While agricultural credit volume decreased relative to the non-agricultural sector for cooperatives, it increased for banks. For banks, there are no strong systematic differences across sectors in the pre-reform period. For cooperatives, there is a negative pre-trend, but the post-reform decline is substantially larger than the pre-trend extrapolation.

I investigate whether these responses are driven by adjustments along the intensive or extensive margins. Panel 4c displays estimated coefficients for average loan size, indicating a relative increase for both lender types. By contrast, Panel 4d shows that the number of loans was the primary channel of adjustment.

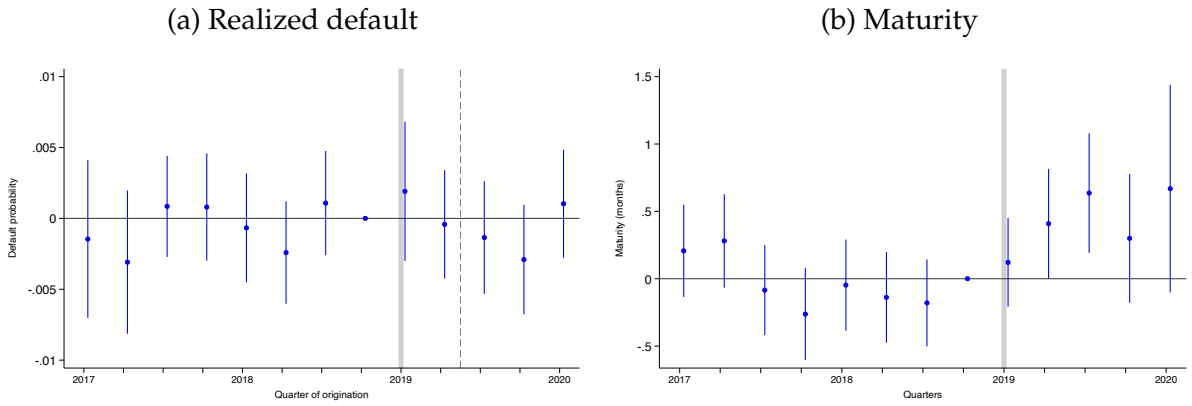
The regulation may also have affected loan performance and maturity. To examine these dimensions, I estimate a loan-level version of (5) with lender–month, lender–sector, and canton fixed effects. Because of data availability, the analysis is restricted to cooperative loan originations. Figure 5a examines the differential change in realized default, with the horizontal axis indicating the quarter of origination. A loan is in default if it is at least 90 days past due at any point in the six months after origination.[21] There is no differential change in default across sectors. Figure 5b plots the differential change in maturity, measured in months. Maturity increases slightly after the reform.[22]

Overall differential changes across sectors. I use the following specification to summarize the overall differential change in agricultural relative to non-agricultural lending outcomes:

[21] I use a six-month window because loan performance after February 2020 reflects COVID-19 related payment deferrals. Results are robust to a twelve-month window definition.

[22] Pooling post-reform quarters, average maturity rises by 0.4 months (2% of the pre-reform mean).

Figure 5: Relative changes in agricultural loans' default and maturity



Notes: These figures show results from a loan-level version of (5) with lender–month, lender–sector, and canton fixed effects. Dots represent quarter-specific regression coefficients and vertical lines indicate 95% confidence intervals. Standard errors are clustered at the lender-parish-sector level. The vertical gray bar represents the quarter in which the agricultural interest rate cap was introduced. The dashed line in Figure 5a marks the first origination cohort whose default rate is measured after March 2020. The reference period is the last quarter in 2018.

$$y_{jlst} = \rho \, Agriculture \cdot Post_t + \iota_{jls} + \iota_{jt} + \eta_{jlst} \tag{6}$$

where $Post_t$ is an indicator equal to one for quarters after the reform and the remaining variables are defined as above. I omit the first quarter of 2019, as it is partially treated.

Table 4 presents estimated coefficients from equation (6), showing that the reform reduced average interest rates on agricultural loans. For cooperatives, the estimated decline is 0.67 p.p. (3.1% relative to the baseline) while for banks it is 2.76 p.p. (10.8% relative to the baseline). Regarding quantities, agricultural credit volume decreased by approximately 30.7% for cooperatives, while it increased by 23.9% for banks. This divergence is largely explained by the number of loans, which declined 37.6% for cooperatives and expanded 18.2% for banks. The size of the average agricultural loan increased by roughly 4.4% for cooperatives and 8.1% for banks.

In markets with selective outreach, lending combines whether a lender reaches out to a borrower and whether the borrower accepts. Since neither of these decisions is separately observed in the data, the descriptive and regression evidence alone cannot determine the channels through which the cap affects lending, the role of targeting, or the cap's welfare implications. In the next section, I estimate an empirical model to address these questions.

Table 4: Differential changes in agricultural relative to non-agricultural lending outcomes

	(1) Interest rate	(2) Log(Credit volume)	(3) Log(Number of loans)	(4) Log(Average size)
Cooperatives				
$Agriculture \cdot Post_t$	-0.668***	-0.367***	-0.472***	0.043***
	(0.041)	(0.042)	(0.051)	(0.012)
Observations	30,062	31,179	31,179	30,062
R-squared	0.925	0.921	0.860	0.535
Lender-sector-parish FE	Yes	Yes	Yes	Yes
Lender-quarter FE	Yes	Yes	Yes	Yes
Baseline	21.444	56,379.588	17.875	8.035
Banks				
$Agriculture \cdot Post_t$	-2.764***	0.214***	0.167***	0.078***
	(0.087)	(0.055)	(0.053)	(0.019)
Observations	8,533	8,716	8,716	8,533
R-squared	0.828	0.970	0.928	0.671
Lender-sector-parish FE	Yes	Yes	Yes	Yes
Lender-quarter FE	Yes	Yes	Yes	Yes
Baseline	25.506	53,179.249	20.820	7.780

Notes: This table reports results from equation (6). Results for cooperatives and banks are estimated separately. Results for Log(Credit volume) and Log(Number of loans) come from Poisson pseudo maximum likelihood regressions (pseudo r-squared reported). Standard errors in parentheses are clustered at the lender-parish level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

6 Empirical model of the targeted sector

Data limitations prevent me from directly estimating the theoretical framework described in Section 3. In particular, data on non-agricultural producers who do not obtain a loan are not available, which precludes including the untargeted sector since estimation requires the full pool of potential borrowers. Capacity is also unobserved. Therefore, I estimate a partial-equilibrium model of the targeted sector, in which the untargeted sector enters only through the offer threshold. Instead of deriving the threshold from the capacity constraint, I parametrize it to reflect the cross-sector price difference induced by the cap.

The empirical model captures the main features of the theoretical framework. Lending depends on offers and acceptance, and the targeted cap affects both in an ambiguous way. On the demand side, lower prices make borrowers more likely to accept an offer. How-

ever, because prices drop by different magnitudes across lenders, a lender with a small price adjustment can end up with a lower acceptance probability as lenders with larger price adjustments become relatively more attractive. On the supply side, the cap reduces the profitability of an originated loan but also changes the offer's acceptance probability. The offer threshold adjusts according to the relative profitability across sectors.

6.1 Model

Borrowers, indexed by i, belong to a regulatory subsegment h. Lenders, denoted by j, originate loans by simultaneously extending offers to borrowers. Loan offers represent supply as they constitute the total quantity that lenders are willing to offer at a given price. Borrowers choose from the offers they receive and do not actively search for loans. I abstract from the decision that determines loan size, focusing on the extensive margin of borrowing.

Borrower acceptance. The indirect utility that borrower i in regulatory subsegment h obtains from borrowing from lender j is:

$$u_{ij} = \alpha_i p_{hj} + \beta_y y_{ij} + \beta_z z_{ij} + \beta_{zb} \text{large}_i \times \text{bank}_j + \xi_j + \varepsilon_{ij} \tag{7}$$

$$\alpha_i = \alpha + \alpha_z \text{large}_i$$

where p_{hj} is the interest rate that lender j charges in regulatory subsegment h, y_{ij} is the logarithm of the number of branches of lender j in borrower i's province, z_{ij} are borrower and borrower-lender characteristics: distance between i's location and j's nearest branch, an indicator for secondary education, indicators for annual and perennial crops (with livestock as the omitted category), and large_i, an indicator for loans above \$1,000. ξ_j captures unobserved attributes through lender fixed effects, and ε_{ij} is an idiosyncratic taste shock distributed standard normal.

Unlike other settings, prices are not tailored at the borrower level and lenders charge a single interest rate for each regulatory subsegment. I allow the price sensitivity to vary between borrowers with loan sizes above and below \$1,000. This threshold coincides with the debt level used to define the pre-existing regulatory subsegments. Similarly, I allow preferences over lender type to differ for borrowers above this threshold, through the interaction $\text{large}_i \times \text{bank}_j$.

Each borrower accepts the offer that yields the highest indirect utility from among those received. Importantly, borrower i can only obtain a loan from lender j if j extended an offer. That is, lender j is included in i's choice set, O_i, if and only if an offer was made. All borrowers have access to an outside option with utility $u_{i0} = \varepsilon_{i0}$. If no offer provides higher utility than the outside option, the borrower declines all offers and does not borrow.

Offer decision. Lender j decides whether to extend an offer to each borrower. If lender j extends an offer to borrower i, its expected profitability is:

$$\pi_{ij} = \omega_{ij} \cdot \underbrace{(p_{hj}(1 - d_{ij}) - c_j)}_{\text{Origination payoff}} - C_{ij} - \nu_{ij}, \quad \nu_{ij} \overset{\text{i.i.d.}}{\sim} N(0, \sigma_\nu^2) \tag{8}$$

Expected profitability depends on an idiosyncratic cost shock ν_{ij} that is private information of lender j. Since rivals' offers are not observed, the lender forms a belief about the probability that borrower i accepts its offer, denoted by ω_{ij}. The equilibrium condition that makes this belief consistent with borrower behavior and the offer process is given below.

If the borrower accepts the offer, the lender obtains an origination payoff equal to the price adjusted by the lender's assessment of the borrower's default probability, d_{ij}, net of origination costs, c_j. Origination costs represent expenses that are incurred only if the loan is actually originated, such as funding costs, provisioning, and other operational costs. I assume that they are common to all lenders of the same type and do not vary across borrowers. That is, $c_j = \kappa_{\tau(j)}$, where $\kappa_{\tau(j)}$ is a lender-type-specific parameter with $\tau(j)$ denoting lender j's type. This specification allows cost structures to differ between cooperatives and banks. To extend an offer, the lender incurs a borrower-specific offer cost $C_{ij} = \gamma_z z_{ij}$ that depends on observable characteristics z_{ij} as defined above.

Lender j extends an offer to borrower i if its expected profitability is greater than a threshold λ_j:

$$\sigma_{ij} = 1\{\pi_{ij} > \lambda_j\} \tag{9}$$

where σ_{ij} is a binary variable equal to one if the offer is extended. The threshold λ_j is a reservation profitability that an offer needs to clear to be extended. I parametrize the threshold as:

$$\lambda_j = \delta_{\tau(j)} + \psi_{\tau(j)} p_j^{gap} \tag{10}$$

where $\delta_{\tau(j)}$ and $\psi_{\tau(j)}$ are lender-type-specific parameters. The price gap variable p_j^{gap} approximates the stringency of the regulation:

$$p_j^{gap} = \max\{p_j^{un} - \bar{p}, 0\} \tag{11}$$

where p_j^{un} is lender j's average interest rate in the untargeted sector and $\bar{p}$ is the targeted interest rate cap. I set $p_j^{gap} = 0$ in the absence of the targeted interest rate cap, so $\delta_{\tau(j)}$ is the pre-reform threshold. The price gap is also zero when the cap is not binding relative to the lender's untargeted-sector pricing. The key assumption is that the threshold responds to the sectoral price distortion induced by the targeted cap. A positive $\psi_{\tau(j)}$ means that lenders facing a larger cross-sector price gap raise the threshold for agricultural offers.[23]

The offer equation that is estimated is:

$$\sigma_{ij} = \mathbb{1}\Big\{\omega_{ij}\big[(1 - d_{ij})\, p_{hj} - \kappa_{\tau(j)}\big] - \gamma_z z_{ij} - \nu_{ij} \;>\; \delta_{\tau(j)} + \psi_{\tau(j)}\, p_j^{gap}\Big\} \tag{12}$$

Consistent beliefs. For all borrower-lender pairs, beliefs are consistent with borrower preferences and the offer process:

$$\omega_{ij} = \sum_{O_{i-j}} \Pr(O_{i-j} \mid \omega) \cdot \Pr\Big(u_{ij} \geq u_{ij'} \;\; \forall j' \in O_{i-j} \cup \{j\}\Big) \tag{13}$$

where O_{i-j} is the choice set that includes the offers of rivals and the outside option. This equilibrium condition implies a fixed point as beliefs are a function of the distribution of choice sets, which in turn depend on all lenders' offers, which themselves are a function of beliefs. Because the expression accounts for all possible choice sets, beliefs represent the unconditional probability that a borrower accepts an offer.

Origination probability. The probability that borrower i obtains a loan from lender j is the product of the probability that lender j extends an offer and the probability that borrower i accepts it, $\Pr(\sigma_{ij} = 1) \times \omega_{ij}$.

Regulation. The regulation directly affects prices. Although the targeted interest rate cap

[23] In the theoretical framework, the cap lowers the profitability of targeted borrowers, and the threshold falls so that the lender continues to use its full capacity. However, it falls by less than the profitability of targeted borrowers, so the difference between the two widens. In the empirical model, where the threshold is identified relative to agricultural profitability, a larger price gap raises the threshold. In both cases, the difference between the threshold and the profitability of targeted borrowers increases, and fewer offers are extended to targeted borrowers.

was intended to apply to all loans in the agricultural sector, the data reveal substantial noncompliance. To accommodate this, loans in the agricultural sector are allowed to be classified either in the agricultural subsegment or in one of the pre-existing subsegments where the agricultural cap does not apply. For simplicity, I take the assignment of borrowers to regulatory subsegments as exogenous.[24]

For loans in the agricultural subsegment, lenders are forced to price at the cap if the pre-reform interest rate p^*_{hj} is higher than the targeted cap $\bar{p}$. Thus, lender j's price under the cap is $p_{hj} = \min\{p^*_{hj}, \bar{p}\}$. This simple price adjustment rule, which abstracts away from lenders' strategic price setting, is supported by the data. Figure A.2 shows the change in prices predicted by the rule and those observed in the data for post-reform loans in the agricultural subsegment, showing that the rule approximates the data remarkably well. I further assume that when the agricultural cap is introduced, interest rates do not change for loans in the non-agricultural subsegments. Therefore, prices for agricultural loans classified under pre-existing regulatory subsegments do not change under the cap. Figure A.3 supports this assumption by showing that post-reform interest rates on non-agricultural loans and on agricultural loans classified under non-agricultural subsegments are not different. In addition, Appendix E shows that the price difference between pre-existing subsegments did not change in response to the reform.

6.2 Estimation and identification

Estimation. The main challenge to estimation is that offers are not observed in the data. I estimate the model primitives in two steps. In the first step, I use pre-reform data to jointly estimate parameters corresponding to borrower preferences, lender costs, and pre-reform thresholds through a Gibbs sampler that treats offers as a latent variable.

The Gibbs sampler includes a data augmentation step that imposes the restrictions implied by the model (He et al., 2024; Agarwal and Somaini, 2025). Lenders that originate a loan must deliver the highest utility among all lenders in the borrower's choice set, and lenders extend offers only to borrowers whose expected profitability exceeds the threshold. The sampler allows me to condition on choice sets when drawing borrower preferences and to condition on borrower utilities when updating lender cost parameters, effectively transforming the problem into two one-sided choice problems. The method

[24] Footnote 35 explains the procedure used to assign borrowers to regulatory subsegments under the targeted cap.

iteratively draws from the corresponding conditional distributions, constructing an ergodic Markov chain of parameter draws that converges to the joint distribution.

This approach is convenient because it deals with the dimensionality of the problem without having to integrate over the large number of potential choice sets. The Gibbs sampler is also well suited to computing beliefs about acceptance probabilities, a key equilibrium object. Instead of solving for a fixed point at every iteration, beliefs co-evolve with parameters within the chain and are iteratively updated by drawing a small number of simulated choice sets.[25] Appendix F provides further details on the Gibbs sampler.

In the second step, I use post-reform data along with the first-step estimates to infer how the thresholds adjusted in response to the reform. In practice, I simulate the model using the estimated parameters on the post-reform borrower pool, and find the threshold values that make the model-predicted market shares match their observed counterparts. This results in a set of lender-specific threshold changes $\widehat{\Delta\lambda_j}$, which I project onto the price gap:

$$\widehat{\Delta\lambda_j} = \psi_{\tau(j)} p_j^{gap} + \epsilon_j \tag{14}$$

where ϵ_j captures threshold adjustments not explained by the price gap. I assume $E[\epsilon_j \mid p_j^{gap}, \tau(j)] = 0$.

Estimation sample. The estimation sample is restricted to agricultural potential borrowers in the central Andes region, where most loans are originated.[26] Lenders with negligible market shares, which do not drive the empirical pattern documented in Section 5, are aggregated into a synthetic lender which is treated as a third lender type. Appendix G provides further details on the construction of the estimation sample.

Identification. Separately identifying borrower preferences and lender costs is challenging because choice sets are endogenous, heterogeneous, and unobserved to the researcher. Following He et al. (2024) and Agarwal and Somaini (2025), I use exclusion restrictions that trace how the parameters on each side influence the origination probability. Specifically, demand shifters enter only borrower preferences, while choice-set shifters affect

[25] I solve for the belief fixed point at the posterior mean of the parameters, computed from the draws after the chains converge. To do so, I rescale the parameters in the borrower utility equation according to the variance of the Extreme Value Type 1 distribution and use the corresponding closed-form expression for choice probabilities and its derivatives (Train, 2009). This procedure greatly reduces the computational burden and simplifies the assessment of convergence.

[26] As mentioned above, data limitations preclude extending the sample to non-agricultural borrowers, which would allow the quantification of effects in the untargeted sector.

only lender costs.

I use the logarithm of the number of lender j's branches in i's province as a demand shifter. Because distance between the borrower and the lender's nearest branch also enters the demand equation, the density of the local branch network is not intended to capture convenience or access, but lender visibility and borrower awareness.[27] This is consistent with anecdotal evidence suggesting that agricultural producers are more likely to accept an offer from a lender if someone they know has had a positive experience with it. The branch network does not enter the offer equation because the relevant cost determinant for serving a borrower is the distance to the nearest branch.

Lenders' assessment of borrowers' default probability is used as the choice-set shifter. It is well known that lenders consider risk when deciding whom to serve. However, borrowers are unlikely to have preferences over how lenders assess their default probability. A borrower's true default risk is known to the borrower and does not vary across lenders, so it should not drive lender choice. How a given lender perceives that risk, on the other hand, is not observed by the borrower and cannot influence her decision.

The role of these shifters is to identify how changes in the acceptance probability and the offer probability separately affect the origination probability. This is the basis for disentangling the effect of a variable that enters both equations. Sufficient variation in one of the shifters generates a system of linearly independent equations whose solution yields the parameters of interest (He et al., 2024).[28]

The imposition of exclusion restrictions raises concerns of misspecification. For the branch network, the exclusion restriction would fail if a denser branch network reduces screening or monitoring costs through channels other than proximity. For default probability, the restriction would fail if risk assessments are correlated with non-price contract terms,

[27] Using branch networks as an exclusion restriction is common in quantitative models of credit markets. For instance, Benetton (2021), Benetton et al. (2025), and Brugués and De Simone (2026) use it as an instrument that shifts the extensive but not the intensive margin of borrowing. Cuesta and Sepúlveda (2021) assume that the local branch network shapes consideration sets but does not directly affect borrower choice conditional on consideration.

[28] For intuition, suppose there is a single lender and the goal is to identify the demand and supply parameters associated with distance to the nearest branch. Consider two potential borrowers located at the same distance from the branch but with different assessments of default probabilities. Because the assessment of default probability enters only the offer equation, variation in the assessment of default probability across these two borrowers generates variation in the origination probability that is driven entirely by the supply side. As there are two equations (one per borrower) and two unknowns (the demand and supply coefficients on distance), once the parameters on the excluded variables are pinned down, the solution recovers the separate effects of distance on acceptance and on offers.

such as collateral or cosigner requirements that vary with perceived risk and that borrowers value independently of price. A violation of either restriction could induce bias in the entire set of estimates.

Beyond separating demand from supply parameters, disentangling the two cost components is also necessary. Variation in beliefs about acceptance probabilities, coming from the demand and choice-set shifters, allows origination costs c_j to be separately identified from offer costs C_{ij}. In the offer equation (12), origination costs are scaled by ω_{ij} as these costs are incurred only upon origination. Offer costs, in contrast, are incurred regardless of whether the borrower accepts the offer. Because, for any given lender, ω_{ij} varies across borrowers, a change in origination costs has a larger effect on the offer probability for borrowers with higher acceptance probabilities, while a change in offer costs affects all borrowers similarly. This variation identifies the two cost components separately.

In addition, the offer equation requires a location normalization, as only the difference between profitability and the threshold is identified. I normalize the intercept of the expected profitability to zero. I further assume that offer costs do not have a lender-type component, so that the parameters $\delta_{\tau(j)}$ can be interpreted as pre-reform thresholds. Threshold changes induced by the reform are identified by the variation in post-reform market shares that is unexplained by the model's prediction. The projection of threshold changes on the price gap variable is identified as long as the cross-sectoral price gaps induced by the cap vary sufficiently across lenders to explain the inferred threshold adjustments.

Finally, while I do not address price endogeneity in the borrower utility equation (7), I expect the resulting bias to be limited. Because the specification includes lender fixed effects, endogeneity would arise only if lenders observe borrower- or market-specific demand shocks and adjust prices accordingly. However, as documented in Appendix D, lenders use highly uniform pricing within each regulatory subsegment, regardless of geography.

7 Estimation results

Preferences, costs, and pre-reform thresholds. Table 5 presents means and standard deviations of the posterior distribution obtained from the Gibbs sampler.[29] To calculate the statistics, I run the sampler for 250,000 iterations and discard the first 125,000 as burn-in. Convergence is assessed by visual inspection of the chains and by verifying that the Potential Scale Reduction Factor (PSRF) for each coefficient is close to 1.[30]

The signs of the borrower utility estimates are as expected. Higher prices and greater distance to a lender's nearest branch are associated with lower demand. Demand from borrowers with larger loans is more price-sensitive. In contrast, lenders with larger branch networks are more likely to be chosen. Agricultural producers who are more educated, grow crops, or have larger loans are more likely to accept an offer.

In the offer equation, greater distance between a lender's branch and a borrower increases the cost of extending an offer, as expected. Lender-type parameters κ_τ and δ_τ corresponding to the intercept capture the baseline applicable to the synthetic lender, while the bank and cooperative parameters are deviations from this baseline.

Table 6 reports model-implied economic objects. Origination and offer costs are similar for banks and cooperatives. The profit margin conditional on acceptance, defined as the difference between the price adjusted by the assessed default probability and origination costs, is higher on average for banks, reflecting their higher price levels. There is substantial heterogeneity in profit margins for cooperatives. Beliefs about acceptance probabilities are slightly higher for cooperatives, while offer probabilities are markedly larger for banks. This reflects banks' higher profit margins and lower pre-reform thresholds. Finally, the average borrower receives approximately 4 expected offers out of an average of 20 feasible lenders.

Table 7 reports demand and supply own-price elasticities computed from an ex-ante perspective. On the demand side, I compute the derivative of beliefs with respect to prices holding offer behavior fixed.[31] I find that a 1% increase in prices reduces demand by

[29] He et al. (2024) present Monte Carlo simulations showing that the posterior standard deviation from the Gibbs sampler approximates standard errors well.

[30] The PSRF measures the factor by which the scale of the posterior distribution might shrink with additional iterations (Gelman et al., 2013). A value near 1 indicates that allowing the sampler to run for longer is unlikely to change the results. I compute it by splitting each chain in half after discarding the burn-in. All factors are below 1.1.

[31] As in the computation of the belief fixed point, I rescale the borrower utility parameters to match the

Table 5: Parameter estimates

	Borrower utility		Offer equation	
	Mean	SD	Mean	SD
Price	-0.135	(0.015)	1.000	–
Price × Large loan (> \$1,000)	-0.115	(0.020)		
Log(branches)	0.756	(0.046)		
Log(distance to branch)	-0.017	(0.041)	0.689	(0.068)
1(Education)	0.665	(0.083)	0.177	(0.062)
1(Perennial)	0.305	(0.126)	-0.121	(0.088)
1(Annual)	0.434	(0.072)	0.220	(0.061)
Large loan (> \$1,000)	3.037	(0.490)	-0.024	(0.079)
Bank × Large loan (> \$1,000)	-0.029	(0.099)		
Default probability			1.000	–
κ_τ Intercept			8.484	(0.743)
κ_τ Bank			6.141	(1.033)
κ_τ Cooperative			6.238	(0.840)
δ_τ Intercept			0.041	(0.130)
δ_τ Bank			0.458	(0.145)
δ_τ Cooperative			0.913	(0.132)
Standard deviation of ν_{ij}			1.122	(0.076)

Notes: This table contains posterior mean and standard deviation in parentheses. I run the Gibbs sampler 250 thousand times and use the last 125 thousand to calculate the statistics. The borrower utility equation includes lender fixed effects. The coefficients on price and default probability are normalized to 1 in the offer equation. The sample is composed of loans originated in the year preceding the reform.

6.0% for banks and 5.0% for cooperatives. These estimates are at the higher end of values reported in the structural credit market literature.[32] This finding is in line with the consideration-set literature, where accounting for constraints on consumer choice sets produces larger price elasticities than assuming full choice sets.[33]

On the supply side, I assess how price changes affect the probability of extending an offer, which reflects a trade-off between higher profitability of originated loans and a lower probability of acceptance. On average, a 1% increase in prices reduces the offer

variance of the Extreme Value Type 1 distribution, which yields a closed-form expression for the choice probability derivatives. Monte Carlo exercises confirm that this approximation performs well.

[32] The magnitudes are consistent with elasticities reported by Brugués and De Simone (2026) for commercial lending in Ecuador, Benetton (2021) and Buchak et al. (2024) for mortgage lending in the U.K. and the U.S., and Cherry (2026) for the U.S. personal loan market.

[33] For instance, Goeree (2008) shows that price coefficients are larger in magnitude when consumer choice sets are restricted. Gandhi (2023) finds a similar pattern in a setting with supply-side selection.

Table 6: Model-implied economic objects by lender type

	Banks	Cooperatives
Origination cost	14.624	14.721
Offer cost	1.963	2.118
	(0.572)	(0.646)
Threshold	0.499	0.954
Profit margin	9.560	5.768
	(1.992)	(3.552)
Beliefs	0.198	0.235
	(0.093)	(0.155)
Offer probability	0.345	0.121
	(0.262)	(0.156)
Expected offers per borrower	3.694	
	(1.552)	

Notes: This table presents economic objects implied by the parameter estimates. Values are averages across borrower-lender pairs in the pre-reform period, with standard deviations across pairs in parentheses. Origination cost and thresholds do not vary at the borrower level. Origination cost, offer cost, thresholds, and profit margin are in percentage points. Profit margin is the difference between the price adjusted by the assessed default probability and origination cost. Offer probability is the ex-ante probability that a lender extends an offer to a given borrower. Expected offers per borrower account for all lenders. Beliefs are computed from a fixed point in beliefs evaluated at the posterior means of the parameters. Offer probability and expected offers per borrower are averages across model simulations.

Table 7: Demand and supply own-price elasticities

	Mean	Median	P10	P90
Demand elasticity				
Banks	-5.953	-6.563	-7.315	-3.717
Cooperatives	-4.982	-4.977	-7.380	-2.739
Supply elasticity				
Banks	-3.312	-2.902	-7.786	0.161
Cooperatives	0.315	0.243	-5.870	6.146

Notes: Price elasticities at the borrower-lender level. Demand elasticity is the percent change in acceptance probability. Supply elasticity is the percent change in offer probability. Pairs with offer or acceptance probability below 0.01 are excluded.

probability by about 3.3% for banks, suggesting that the demand-side response outweighs the direct profitability gain. For cooperatives, the offer probability increases 0.3% for a 1% increase in prices. There is substantial heterogeneity across borrowers, particularly for cooperatives. The tenth percentile of the supply elasticity is -5.9% while the ninetieth percentile is 6.1%. As shown in Figure A.4, demand and supply price elasticities are positively correlated. When demand is inelastic, the profitability effect dominates and lenders increase their outreach. Conversely, a higher price reduces the incentive to extend offers to borrowers whose demand is elastic, for they are more likely to reject them. This trade-off highlights the distributional consequences of interest rate caps.

Post-reform thresholds. Figure 6 presents the estimates from equation (14). Coefficients are positive, indicating that as the cross-sector price differential induced by the targeted regulation widens, the threshold for agricultural offers rises and the number of offers in the targeted sector drops, all else equal. The coefficient for banks is larger than that for cooperatives.[34]

Model fit. Figure A.5 compares market shares observed in the data with those predicted by the model. The pre-reform predictions rely entirely on the estimated preference, cost, and pre-reform threshold parameters, suggesting that the model captures the data well. The post-reform predictions fit the data almost exactly, given that the market shares are targeted when inferring the offer threshold adjustments.

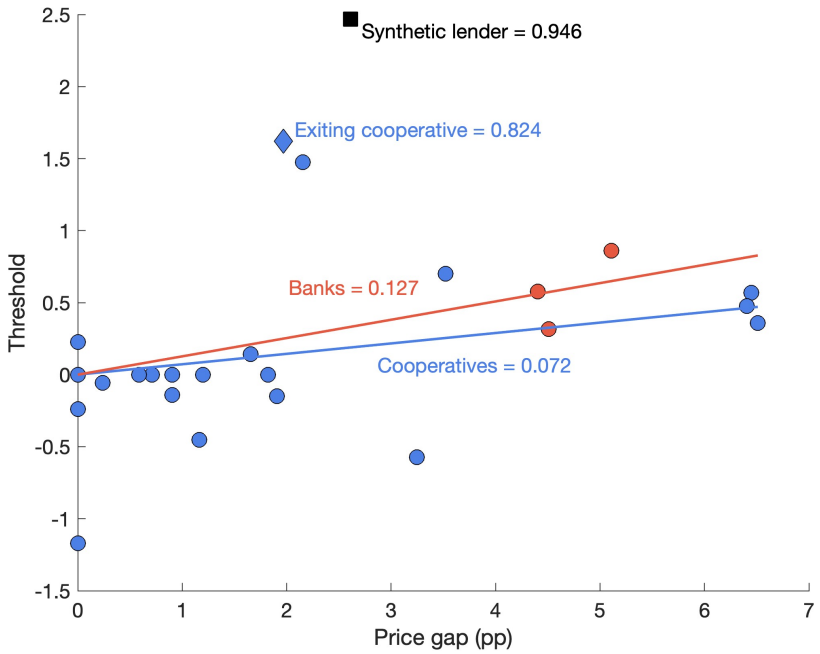
Table A.2 shows that the model reproduces moments from the lender survey, which is not used in estimation. The average conversion rate of offers into originated loans predicted by the model is 27%, compared with 28% in the survey. Similarly, the distribution of the number of offers implied by the model approximates the distribution of the perceived number of rivals contacting a borrower reported in the survey.

8 Counterfactuals

I conduct counterfactual simulations to quantify the offer and acceptance responses, the role of targeting, and the welfare effects of the targeted interest rate cap. In addition, I examine how these effects vary with the cap's level and scope. All counterfactuals are

[34]The synthetic lender and one cooperative with atypical behavior have independent coefficients. This cooperative held a considerable market share in the pre-reform period, continued lending for a few months after the reform, and then exited the agricultural lending market entirely.

Figure 6: Inferred changes in thresholds and price gaps



Notes: This figure displays inferred changes in offer thresholds on the vertical axis and the price gap between the average interest rate in the untargeted sector and the agricultural cap on the horizontal axis. Each point represents a lender. Fitted lines correspond to estimates from equation (14). The synthetic lender has an independent coefficient (black marker) as it aggregates both small cooperatives and banks and its characteristics do not reflect any single institution. One cooperative with an atypical behavior (diamond marker) has a separate coefficient. This institution held a considerable market share in the pre-reform period, continued lending for a few months after the reform, and then exited the agricultural lending market entirely.

applied to the pre-reform borrower pool.[35] Each scenario sets prices and thresholds as described below. Then, I compute a fixed point in beliefs, simulate offers and acceptance, and compute welfare measures.

Offers, acceptance, and targeting. I implement three counterfactual simulations that involve changes in prices and offer thresholds as detailed in Table 8. The baseline is the scenario without the targeted cap. The first counterfactual scenario (CF1) imposes the targeted cap; that is, prices are constrained by the cap and thresholds adjust according

[35] Noncompliance with the agricultural cap is taken as exogenous by assigning each borrower to a regulatory subsegment based on predicted compliance probabilities. To that end, I estimate a logistic regression on the post-reform sample to predict whether a loan is classified under the agricultural subsegment and use the fitted probabilities to classify pre-reform borrowers. The explanatory variables in the logistic regression include borrower and borrower-lender characteristics and lender fixed effects. Borrowers with the highest predicted probabilities are assigned to the agricultural subsegment, with the cutoff chosen so that the share of compliant loans matches the post-reform share.

to the cross-sector price gap. Comparing this scenario against the baseline quantifies the effect of the reform, which combines the responses of both offers and acceptance. The second counterfactual simulation (CF2) applies the same prices as in CF1 but holds the number of offers fixed. The comparison of CF2 against the baseline quantifies the role of acceptance, while comparing CF2 against CF1 isolates the offer response. The targeted cap also combines two policy design features: the cap level (set at 20.97%) and scope (i.e., targeting to agricultural loans). The third counterfactual simulation (CF3) applies the same prices as in CF1 but holds thresholds at their pre-reform levels. This scenario implicitly assumes that lenders make offer decisions independently across sectors. Under equation (10), it also approximates a cap whose scope is not restricted to the agricultural sector and does not induce a price gap across sectors. The comparison of CF3 against the baseline measures the contribution of the level of the cap, while comparing CF3 against CF1 quantifies the role of targeting.

Table 8: Counterfactual simulations

	Baseline	**CF1 Targeted cap**	**CF2 Offers fixed**	**CF3 Independent sectors**
Prices	Pre-reform	Post-reform	Post-reform	Post-reform
Threshold	Pre-reform	Post-reform	–	Pre-reform
Offer response		Yes	No	Yes
The comparison quantifies				
vs. Baseline		Acceptance + offers	Acceptance	Cap level
vs. CF1		Cap level + targeting	Offers	Targeting

Table 9 summarizes the changes in prices, market shares, and outreach under the counterfactual scenarios. Column (1) reports baseline levels without the cap, and Column (2) reports the effects of the targeted cap. The model predictions align with the empirical pattern described in Section 5. The average transacted price falls by 1.66 p.p. (7.00% relative to the baseline). Total lending, measured by market shares, decreases 13.5% relative to the baseline. Cooperatives' market share falls by 10.24 p.p., while banks' share rises by 1.49 p.p. Both lender types reduce their outreach, and the expected number of offers per borrower falls by about 50%.

Column (3) shows that total lending would have increased if offers had not responded to the cap, suggesting that lower prices encourage borrowers to accept offers they would otherwise reject. The acceptance response increases banks' and decreases cooperatives'

Table 9: Changes in market outcomes under counterfactual scenarios

			Channel		Policy design	
	Baseline	Targeted cap	Acceptance	Offers	Cap level	Targeting
		(CF1 − Baseline)	(CF2 − Baseline)	(CF1 − CF2)	(CF3 − Baseline)	(CF1 − CF3)
	(1)	(2)	(3)	(4)	(5)	(6)
Price						
Posted	23.24	-1.43	-1.43	0	-1.43	0
Transacted	23.67	-1.66	-1.48	-0.18	-1.44	-0.22
Market shares						
Banks	18.50	1.49	5.92	-4.43	5.71	-4.22
Cooperatives	46.35	-10.24	-3.47	-6.77	-8.60	-1.64
Total (banks and cooperatives)	64.84	-8.74	2.45	-11.19	-2.89	-5.85
Outreach						
Banks' offer probability	0.35	-0.16	0	-0.16	-0.04	-0.13
Coops' offer probability	0.12	-0.05	0	-0.05	-0.02	-0.03
Expected offers per borrower	3.69	-1.76	0	-1.76	-0.39	-1.36

Notes: Column (1) reports pre-reform levels. Columns (2)–(6) report differences between the indicated scenarios. CF1 is the targeted cap as implemented (post-reform prices and offer thresholds). CF2 holds offers at their baseline levels under post-reform prices. CF3 applies the same cap level, holding offer thresholds at baseline. All simulations use the pre-reform borrower pool. Results for the synthetic lender are not reported. The posted price is the average rate across feasible borrower-lender pairs, while the transacted price is weighted by origination probability. Market shares are calculated over the full pre-reform borrower pool. Interest rates and market shares are in percentage points.

market shares. Since banks' baseline prices are higher, the cap compresses them more, narrowing the price difference that made borrowers take cooperatives' loans. Therefore, similar prices shift market share from cooperatives to banks. Column (4) shows that the offer response is negative and reduces total lending because it is more than four times as large as the acceptance response. Both lender types reduce their number of offers. Overall, cooperatives' agricultural lending decreases because they make fewer offers, and acceptance shifts toward banks. In contrast, banks' agricultural lending increases because higher acceptance more than offsets their offer contraction. The results for banks show that lending can increase despite a decline in the number of offers.

Column (5) quantifies the role of the cap level. Both lender types still reduce outreach relative to the baseline, but the expected offers per borrower decrease by only 10.6%. The only offers that are no longer extended are those that fail to clear the pre-reform reservation profitability. Total lending falls by 2.89 p.p. Column (6) isolates targeting, showing that it accounts for 77.3% of the lost offers and 66.9% of the lending contraction under the targeted cap. This finding highlights how policy design shapes the overall impact of the cap by influencing the relative strength of the offer and acceptance responses.

Welfare. Table 10 presents changes in welfare measures. Consumer surplus is the maximum utility over the borrower's realized offer set, divided by the price coefficient and averaged across simulations. The targeted cap reduces consumer surplus by 7.88% of the

baseline. Column (3) shows that consumer surplus increases due to the acceptance channel. Some borrowers who declined their offers at baseline accept them under the prices induced by the cap, while borrowers who accept their offers in both scenarios benefit from lower prices. However, the effect of the offer contraction in column (4) outweighs the gain from lower prices. Column (5) indicates that consumer surplus rises by 15.50% of the baseline if lenders make offer decisions independently across sectors, even when total lending falls (see Table 9). Column (6) shows that consumer surplus losses for targeted borrowers come from the targeted design of the regulation and not from the cap level.

Table 10: Changes in welfare under counterfactual scenarios

			Channel		Policy design	
	Baseline	Targeted cap	Acceptance	Offers	Cap level	Targeting
		(CF1 − Baseline)	(CF2 − Baseline)	(CF1 − CF2)	(CF3 − Baseline)	(CF1 − CF3)
	(1)	(2)	(3)	(4)	(5)	(6)
Consumer surplus						
Level	6.56	-0.52	1.47	-1.99	1.02	-1.53
% of baseline		-7.88	22.42	-30.30	15.50	-23.37
Expected profit per borrower						
Level	5.22	-1.39	-0.63	-0.76	-0.33	-1.06
% of baseline		-26.64	-12.06	-14.58	-6.28	-20.36

Notes: Column (1) reports pre-reform levels. Columns (2)–(6) report differences between the indicated scenarios. CF1 is the targeted cap as implemented (post-reform prices and offer thresholds). CF2 holds offers at their baseline levels under post-reform prices. CF3 applies the same cap level, holding offer thresholds at baseline. All simulations use the pre-reform borrower pool. Consumer surplus is the maximum utility over the borrower's realized offer set, divided by the price coefficient and averaged across simulations. Expected profit per borrower is the profit margin minus the offer cost, weighted by the origination probability. Both measures are expressed in interest-rate points. The second row of each block expresses the change as a percentage of the baseline level in column (1).

Expected profit per borrower is the profit margin minus the offer cost, weighted by the origination probability. Expected profit per borrower falls by 26.64% due to the targeted cap. Columns (3) and (5) mainly reflect the compression of margins between prices and origination costs. Columns (4) and (6) account for the equilibrium effect of lower origination probabilities generated by changes in the number of offers.

Figure A.6 reveals substantial heterogeneity in welfare effects across borrowers under the targeted cap. While most borrowers are negatively affected, around a quarter of them experience a welfare gain (Panel A). As expected, welfare losses increase with borrower risk (Panel B). The relationship between changes in welfare and distance to the closest lender branch is non-linear, but borrowers who are most geographically isolated experience a markedly larger loss in consumer surplus (Panel C). Borrowers with a higher number of offers received in the absence of the cap experience larger welfare losses (Panel D). This

pattern reflects that, without the targeted cap, borrowers with more offers also pay lower interest rates. As the number of offers decreases due to the cap, they lose the benefits of lender competition.

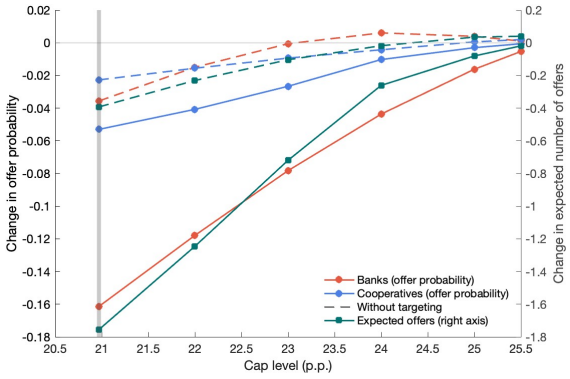
Policy design: cap level and scope. The preceding results evaluate the cap at its implemented level and scope. To assess how these features of the regulation shape lending and welfare, I simulate a range of cap levels both holding thresholds at pre-reform levels and letting thresholds adjust according to equation (10). The levels range from the lowest pre-existing cap among the subsegments in the sample (25.5%) to the implemented level (20.97%). When thresholds are held at pre-reform levels, the number of offers does not adjust according to the cross-sector price differential induced by the cap. I refer to this as a cap without targeting.

Figure 7 presents changes relative to the pre-reform baseline. Panel (a) shows that the number of offers decreases as the cap becomes more stringent. When the cap is targeted, the number of offers falls at a much higher rate. For banks, untargeted caps above 23% do not lead to a reduction in the number of offers extended. Panel (b) shows that every cap in the simulated range would lead banks to increase lending and cooperatives to reduce it. Targeting reduces lending. Caps without targeting above 23% do not change overall lending meaningfully. Panel (c) indicates that the mean change in consumer surplus is positive for caps of 23% or above, with and without targeting. Moreover, caps without targeting generate mean consumer surplus gains across the entire range of simulated levels. While a cap always produces winners and losers, its effects become more heterogeneous as it becomes more binding. Overall, this analysis suggests that the choice of the level and scope of the cap is critical. A looser cap, whether it is targeted or not, mainly constrains lenders with high baseline prices and can raise consumer surplus without large distributional consequences. Targeting lowers the number of offers, lending, and consumer surplus.

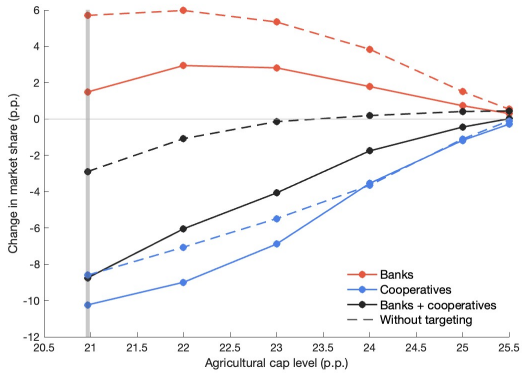
Credit access. Expanding credit access is a common motivation for targeted interest rate caps. Credit access is given by the set of credit contracts available to a borrower, encompassing both their number and terms. Cheaper contracts imply more access. In markets with selective outreach, interest rate caps create a trade-off between the number of offers extended and the price of these offers. I use consumer surplus to measure the value of credit access, as it aggregates the number and terms of offers into a single statistic

Figure 7: Changes in market outcomes and consumer surplus varying the cap's level and scope

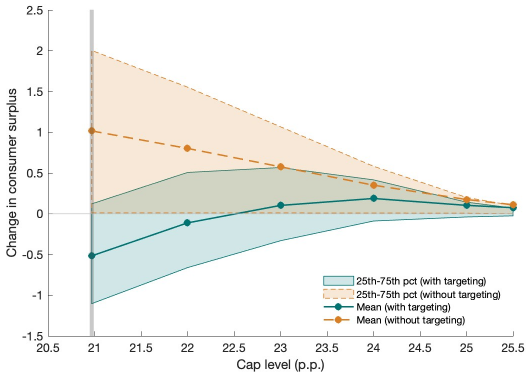
(a) Offer probability and offers per borrower



(b) Market shares



(c) Consumer surplus



Notes: These figures report changes in outcomes relative to the pre-reform baseline across a grid of cap levels. The vertical gray bar marks the cap that was implemented. Solid lines correspond to the scenario in which lenders' offer thresholds respond to the cap. Dashed lines, labeled "without targeting", hold thresholds at their pre-reform level. Panel (a) plots changes in offer probabilities (left axis) and in the expected number of offers per borrower (right axis). Panel (b) plots changes in market shares. Panel (c) plots the mean change in consumer surplus, with shaded areas representing the 25th–75th percentile range of borrower-level changes in each scenario. Consumer surplus is measured in interest-rate points.

that is increasing in both.

I quantify the contribution of lower prices and fewer offers to the change in the value of credit access under the targeted cap.[36] On average, lower prices raise credit access by 22.4% relative to the baseline, while the offer contraction lowers it by 30.3%. Figure

[36]The offer component is computed by valuing offers at baseline prices. The price component values the set of baseline offers at counterfactual prices. Because offers and prices are not independent, there is a residual that captures their interaction.

A.7 groups borrowers in bins of equal size according to the change in credit access value they experience under the targeted cap, showing that the benefits of lower prices outweigh the loss from fewer offers for some borrowers. Figure A.8 compares the measure based on consumer surplus with changes in observed lending, a measure traditionally used as a proxy for access. The two coincide in direction when a borrower gains or loses a loan. However, when loan status does not change, observed lending does not respond to changes in the number or terms of available contracts. In contrast, consumer surplus captures these changes, providing a more complete description of the heterogeneous effects of the regulation on credit access.

The role of selectivity. Appendix H examines the broader implications of lender selectivity. I remove selectivity by holding the number of offers fixed while preventing lenders from choosing which borrowers to approach. At baseline, selectivity shifts the composition of served borrowers toward those who are less costly to serve, and it raises consumer surplus by increasing the number of realized loans and improving match quality. Under interest rate caps, the selective model matches the change in observed characteristics of borrowers that obtain a loan more closely than the nonselective model.

Robustness. Appendix I adds a Nash-Bertrand pricing stage to the model to check the robustness of the main results. The origination costs implied by the pricing first-order condition are close to those estimated with the Gibbs sampler. Counterfactual results with strategic pricing differ little from the main results, because most prices are pinned at the regulatory caps.

Appendix J relaxes the assumption that the targeted and untargeted sectors are linked only through price differences. I let thresholds also respond to cross-sector differences in other profitability fundamentals such as demand elasticities, default probabilities, and the distance between lender branches and borrowers. The finding that targeting explains most of the offer response is robust to the definition of the threshold adjustment rule. Under the alternative specification, targeting accounts for 73.9% of the lost offers, compared with 77.3% in the main specification.

9 Conclusion

Interest rate caps often target a subset of loans to make credit more accessible. In markets where lenders initiate contact with borrowers, targeted caps influence the offer and acceptance decisions by lowering prices and altering the relative profitability across sectors, shaping the overall effects on lending and consumer surplus. This paper studies these forces in the context of a reform that introduced a lower interest rate cap for agricultural loans in Ecuador.

The results highlight that designing interest rate caps and evaluating their equilibrium effects require separating the offer and acceptance responses and accounting for lenders' opportunities outside the targeted sector. I find that the contraction of offers outweighed the increase in acceptance, reducing agricultural lending and consumer surplus, the opposite of the cap's intended effect. Lenders forced by the cap to cut prices more aggressively gain acceptance at the expense of their rivals, so their lending can rise even as their offers fall. This finding illustrates that changes in lending can be a misleading measure of credit supply. Moreover, targeting magnifies the offer contraction in the agricultural sector, turning the consumer surplus gain from the cap level into a loss. The cap's level and scope determine whether the policy benefits or harms the borrowers it targets.

While this paper focuses on how interest rate caps affect the targeted sector, measuring the spillovers to untargeted sectors is a necessary next step. Beyond the role of regulation, understanding the broader implications of lender outreach as opposed to borrower search is an important avenue for future work.

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Appendix

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A Additional tables and figures

Table A.1: Ecuadorian Financial Institutions Survey

	Response
Panel A: Outreach patterns	
Institutions that initiate contact with potential borrowers	30/30 (100%)
Share of loans originated through lender-initiated contact [a]	58.4%
Conversion rate of lender-initiated contacts [b]	27.6%
Institutions with mobile loan officers	30/30 (100%)
Panel B: Outreach decisions	
Primarily made by headquarters	17/30 (57%)
Primarily made by branch or officer	7/30 (23%)
Accounts for competitor presence	29/30 (97%)
Perceived number of rivals contacting a borrower	
0	0/30 (0%)
1-2	6/30 (20%)
3-4	11/30 (37%)
5 or more	12/30 (40%)
Panel C: Pricing decisions	
Primarily headquarters	29/30 (97%)
Panel D: Competition	
Banks and cooperatives compete	29/30 (97%)
Public and private lenders compete [c]	6/30 (20%)

Notes: Survey of 30 Ecuadorian financial institutions serving the microcredit segment, conducted in 2026. The survey was administered in Spanish.

[a] Reports the average across respondents.

[b] Reports the average conversion ratio implied by the number of contacts and acceptance across 29 respondents.

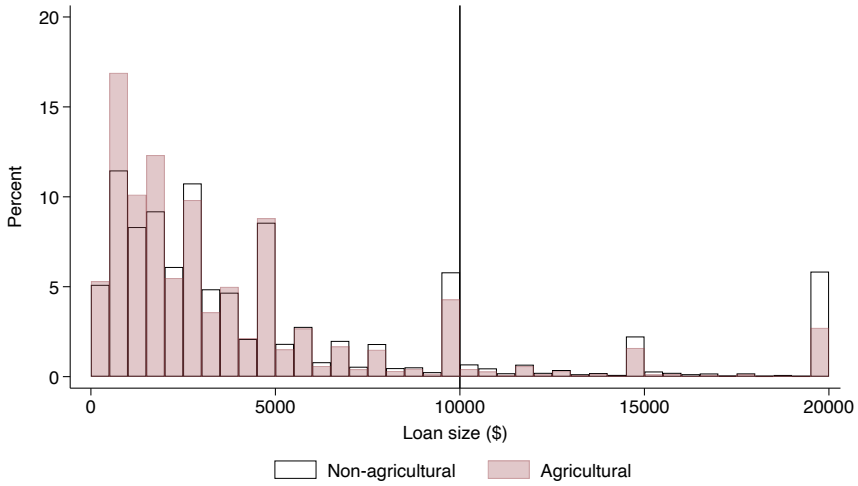
[c] Refers to competition with the retail public bank.

Table A.2: Model fit: moments not used in estimation

	Survey	Model
Offer conversion rate	28%	27%
Number of offers received per borrower		
0	0%	0%
1–2	20%	30%
3–4	37%	38%
5 or more	40%	33%

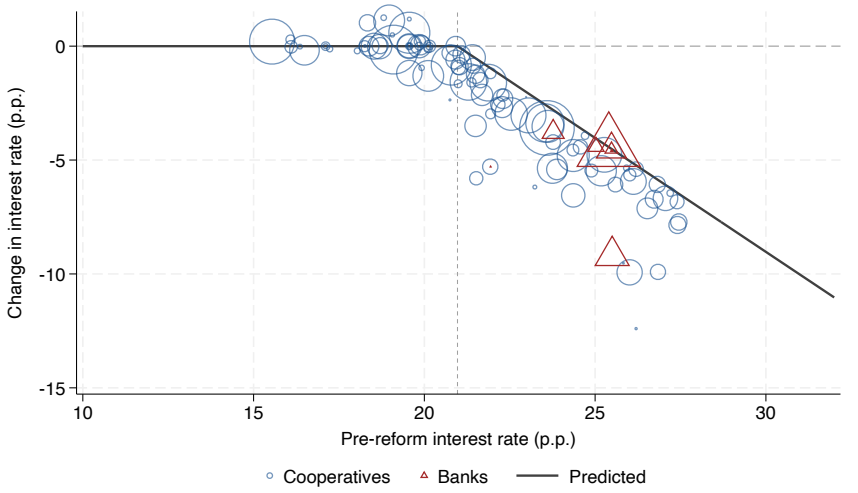
Notes: This table reports moments not targeted in estimation. The survey column reports the implied offer conversion rate and the distribution of perceived number of rivals contacting a borrower from the lender survey described in Table A.1. The model column reports the conversion rate and the distribution of the number of offers per borrower implied by the fixed point in beliefs evaluated at posterior mean parameters in the pre-reform period. The conversion rate is computed as the offer-weighted average acceptance probability across feasible borrower-lender pairs.

Figure A.1: Distribution of loan size by sector



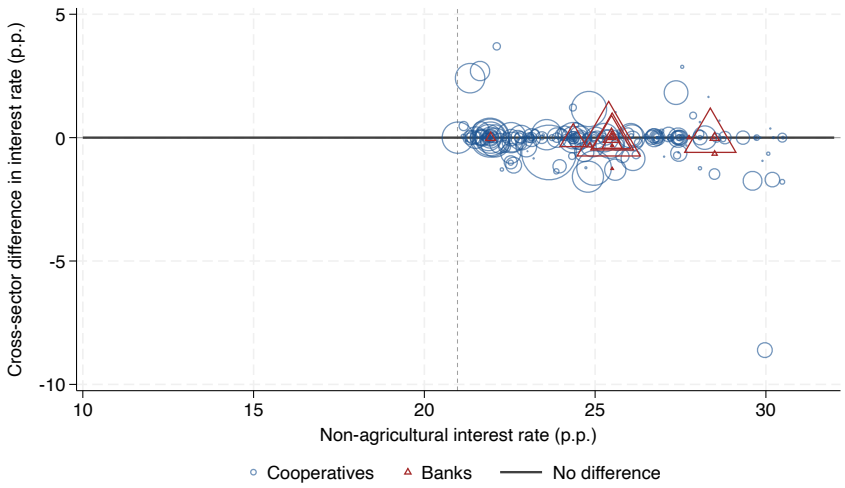
Notes: This figure displays the distribution of loan size across agricultural and non-agricultural sectors. Loan size is winsorized at $20,000. The vertical line depicts the $10,000 threshold. The sample includes loans originated in the year preceding the reform.

Figure A.2: Changes in prices for post-reform loans in the agricultural subsegment



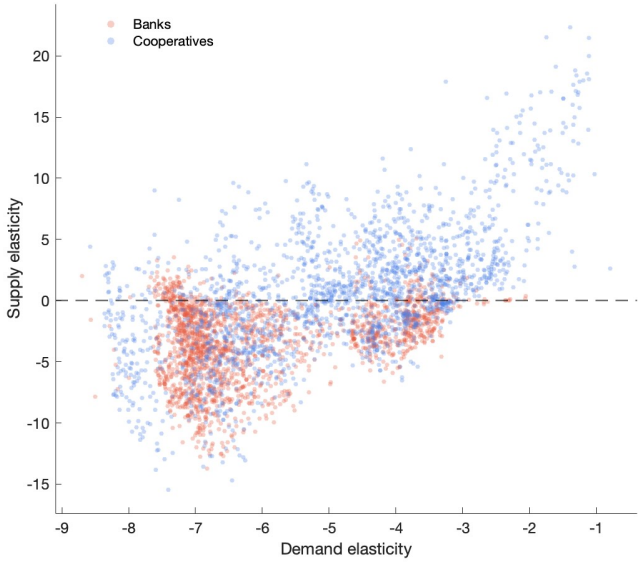
Notes: This figure displays changes in interest rates under the targeted cap at the regulatory subsegment–lender level for post-reform loans classified under the agricultural subsegment. The horizontal axis shows the pre-reform interest rate, while the vertical axis shows the change in the interest rate between the pre- and post-reform periods. Circles denote cooperatives and triangles denote banks; marker size is proportional to pre-reform credit volume. The black line shows the predicted change given by $p_{hj} = \min\{p^*_{hj}, \bar{p}\}$, where p^*_{hj} is the pre-reform interest rate and $\bar{p} = 20.97$ is the agricultural cap.

Figure A.3: Cross-sector differences in prices for post-reform loans in pre-existing subsegments



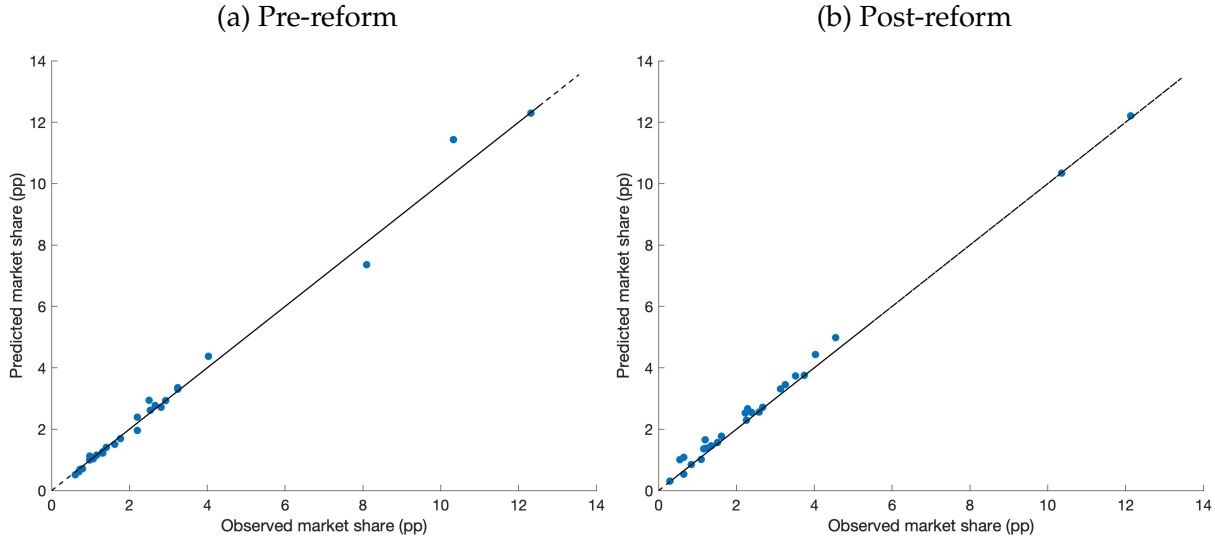
Notes: This figure displays the difference in post-reform interest rates between non-agricultural loans and agricultural loans classified under pre-existing subsegments at the regulatory subsegment–lender level. The horizontal axis shows the non-agricultural interest rate, while the vertical axis shows the cross-sector difference in interest rates. Circles denote cooperatives and triangles denote banks; marker size is proportional to pre-reform credit volume. The black line shows the no-difference benchmark.

Figure A.4: Demand and supply own-price elasticities



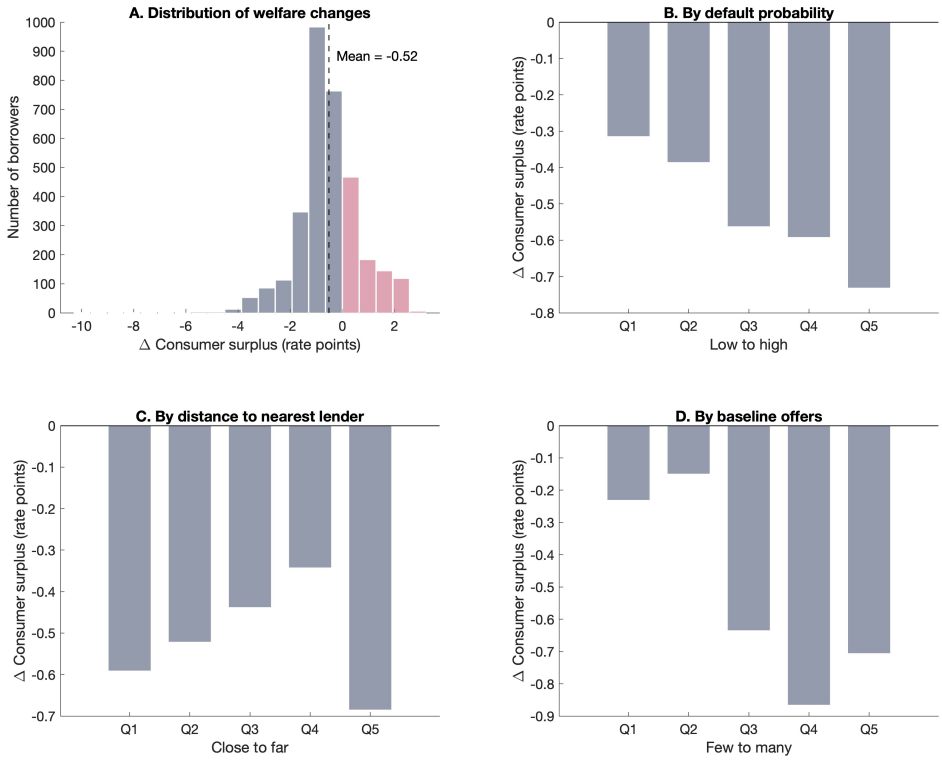
Notes: This figure displays estimated demand and supply own-price elasticities for a random sample of borrower-lender pairs.

Figure A.5: Model fit: market shares



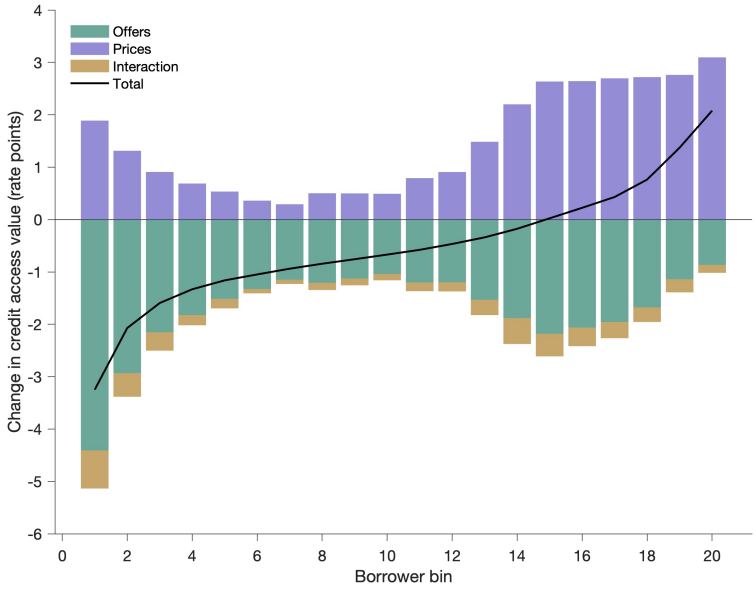
Notes: These figures show market shares observed in the data and predicted by the model in the pre- and post-reform periods. Each dot is a lender. The 45-degree black line represents a perfect fit.

Figure A.6: Heterogeneity in consumer surplus changes



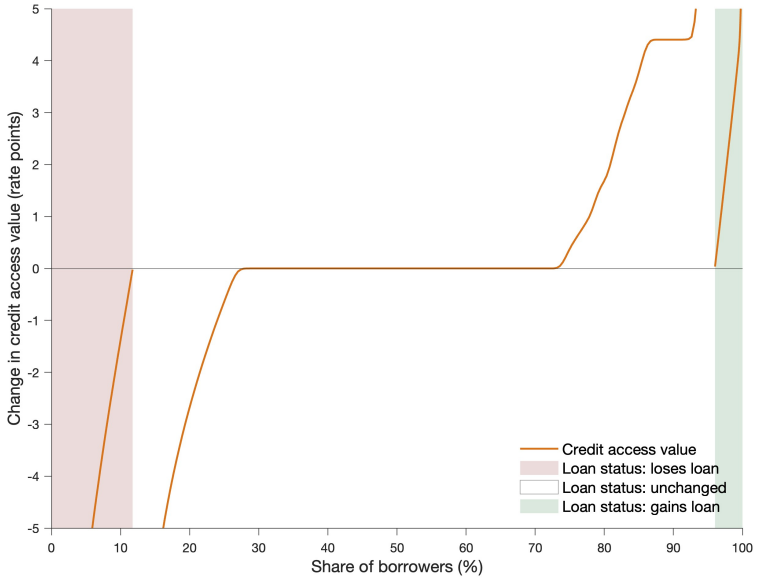
Notes: This figure presents heterogeneity in changes in consumer surplus. Panel A displays the distribution of changes. Panels B, C, and D classify borrowers into quintiles by default probability, distance to the closest lender branch, and baseline number of expected offers, respectively. Each bar represents the mean change in consumer surplus within each quintile.

Figure A.7: Heterogeneity in the contributions of prices and offers to changes in credit access



Notes: This figure presents heterogeneity in the contributions of offers and prices to the change in the value of credit access under the targeted cap. Borrowers are sorted by their total change and grouped into bins of equal size. Bars show the average of each component within a bin and the solid line shows the total. The value of credit access is expressed in interest-rate points. The interaction component is computed as a residual and accounts for the dependence between offers and prices.

Figure A.8: Comparison between two measures of credit access



Notes: This figure compares two measures of the change in credit access induced by the targeted cap. The solid line is the change in the value of credit access, expressed in interest-rate points. Borrowers are grouped by the change in their loan status and sorted within group. Shading indicates the change in loan status, the measure implied by observed lending. Both measures are computed within each simulation draw and averaged across draws.

B Public bank

A large retail public bank offers subsidized loans and accounted for roughly one third of agricultural microcredit in the year preceding the reform.[37] The public bank differs from private lenders in several dimensions. It charges substantially lower interest rates, its geographic reach is broader (with operations in all continental provinces), it has historically experienced high default rates that have required government bailouts, and anecdotal evidence suggests that its disbursement process is considerably longer. Furthermore, unlike private lenders, the retail public bank does not engage in proactive borrower outreach, as borrowers must initiate the process themselves by visiting a branch.

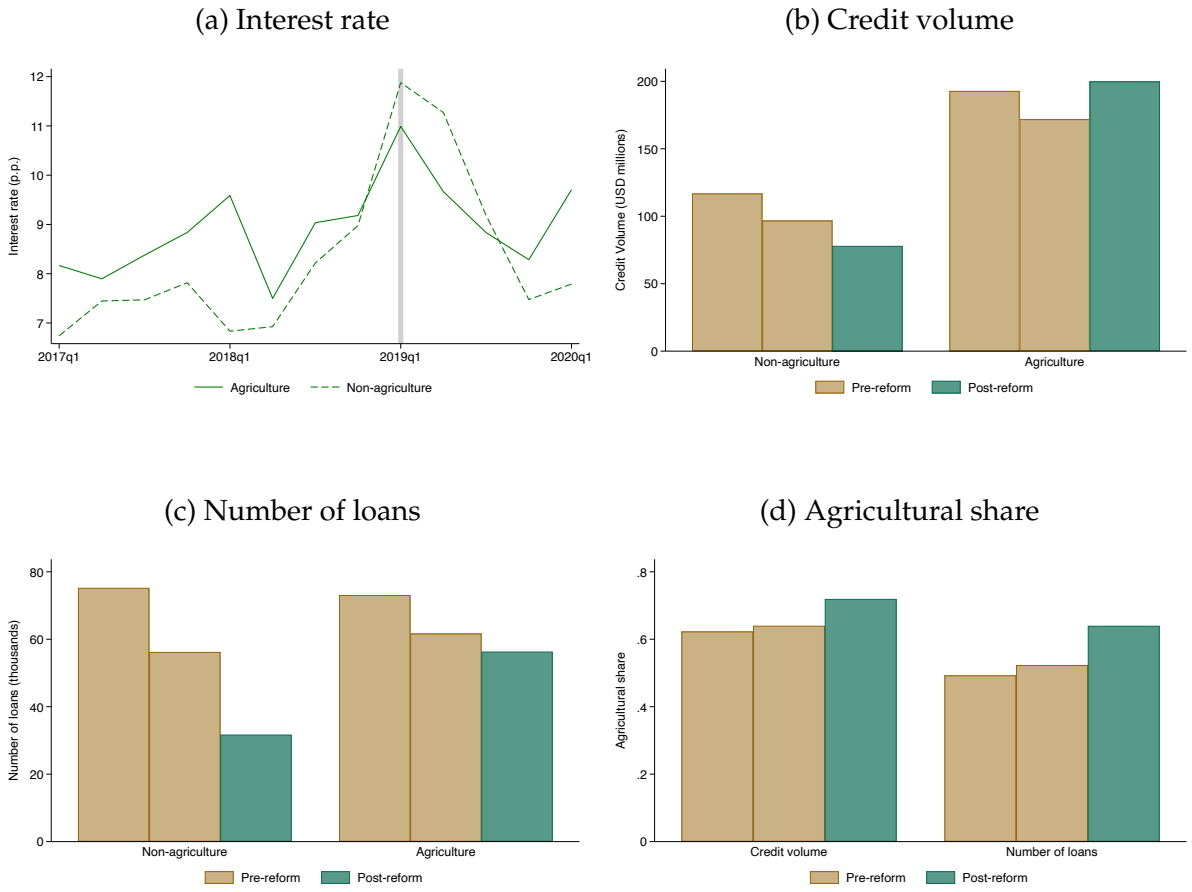
I exclude the public bank from the main sample for two reasons. First, agricultural lending by the public bank is unlikely to have been directly affected by the regulatory change, given that the cap (20.97%) was set far above its average pre-reform agricultural interest rate (9.2%). Figure B.1 plots the evolution of the mean interest rate and credit volume for agricultural and non-agricultural loans issued by the public bank. Interest rates in both sectors follow a similar pattern after the regulatory change. After the reform, agricultural credit volume returns to levels observed two years earlier, while non-agricultural lending continues on a downward trend. Notably, the number of loans decreases in both sectors in the post-reform period. The decline in non-agricultural lending causes the agricultural share of the public bank's portfolio to rise after the reform. However, this shift does not appear to be driven by the reform.

Second, substitution between the public bank and private lenders appears to be limited. Theoretically, even though the public bank is not directly affected by the interest rate cap, it could still be indirectly affected through general equilibrium forces. The direction of these effects is ambiguous. On the one hand, if private lenders lower their prices, some borrowers could switch from the public bank to cooperatives or banks, reducing public lending. On the other hand, if the reform induces rationing by private lenders, the public bank could expand its lending to fill part of the gap.

To assess the degree of co-movement between the public bank and private lenders, I regress the change in private lenders' credit volume in a parish–quarter combination on the corresponding change in public bank lending. The specification includes parish and quarter fixed effects, and standard errors are clustered at the parish level. The coefficient

[37] There is a second public bank that operates mainly as a second-tier institution that finances other retail lenders. This second-tier bank issued only 22 microcredits over the study period.

Figure B.1: Lending by the public bank



Notes: These figures show interest rates and lending in the agricultural and non-agricultural sectors for the public bank. Panel (a) plots the evolution of mean interest rates across sectors with solid lines denoting interest rates on agricultural loans, and dashed lines corresponding to non-agricultural loans. Panels (b) and (c) show annual credit volume and number of loans. Panel (d) shows the annual agricultural lending share, defined as the ratio of agricultural lending to total microcredit lending. Each bar in panels (b), (c), and (d) corresponds to the twelve months between March of year t and February of year $t+1$.

of interest captures the extent to which private lending and public lending comove within a parish over time. Results reported in Table B.1 show that the estimated coefficient is close to zero and statistically insignificant. This remains true both in the full sample and when the sample is split at the date of the reform.

As a benchmark, I estimate an analogous regression in which the dependent variable is the change in cooperatives' lending and the explanatory variable is the change in banks' lending. In contrast to the public–private comparison, the two types of private lenders exhibit substantial co-movement. Over the full sample, for each additional dollar of lending

Table B.1: Correlation between changes in public and private lending

	Δ Private lenders			Δ Cooperatives		
	(1)	(2)	(3)	(4)	(5)	(6)
Δ Public bank	0.069	0.070	0.066			
	(0.043)	(0.045)	(0.042)			
Δ Banks				0.333**	0.663**	0.110
				(0.141)	(0.275)	(0.109)
Observations	11,519	6,717	4,775	11,519	6,717	4,775
R-squared	0.046	0.063	0.166	0.052	0.083	0.170
Time period	Full	Pre-reform	Post-reform	Full	Pre-reform	Post-reform

Notes: This table reports coefficients from regressions of quarterly changes in credit volume across lender types. Private lenders include cooperatives and banks. All regressions include parish and quarter fixed effects. Standard errors, clustered at the parish level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

by banks, cooperatives increase their lending by 33 cents, a relationship that is statistically significant. This co-movement is driven entirely by the pre-reform period, during which the coefficient is 0.66, indicating strong coordinated movements. After the policy change, however, the relationship weakens considerably and becomes statistically insignificant, which is consistent with cooperatives and banks adjusting their lending in different ways following the reform.

Overall, this analysis suggests that the public bank operates independently of the affected segment where banks and cooperatives interact. Responses from the lender survey corroborate this conclusion. Only 20% of the respondents report that private lenders compete with the public bank. Given the minimal substitution, excluding the public bank from the empirical model is unlikely to alter the main conclusions.

C Predicted default probabilities

Risk is a key dimension along which lenders screen borrowers. The empirical model includes predicted default probabilities in the offer equation to proxy for lenders' assessment of borrower risk. I predict default using monthly snapshots of the performance of cooperatives' outstanding loans from a logistic regression of an indicator for 90 or more days past due on borrower and loan observed characteristics and province fixed effects.[38]

[38]The explanatory variables are income, wealth, loan size, loan term, an indicator for elementary education or less, indicators for having a mortgage and being a homeowner, indicators for cosigners and guarantors,

The estimation sample is restricted to loans originated between July 2016 and December 2018 whose repayment periods are fully observed by December 2019.[39]

Table C.1 presents the coefficients and marginal effects of a logistic regression pooling all cooperatives.[40] Figure C.1 presents a binned scatterplot of realized versus predicted default probabilities for loans included in the default model estimation, showing that predictions approximate the distribution of default risk well. To further validate the model, Table C.2 regresses realized default on the predicted default probability for loans excluded from the default model estimation (those not scheduled to be fully repaid by December 2019). The coefficient on predicted default is positive, highly significant, and stable across specifications, suggesting that the default model captures the relevant variation in repayment risk.

In the empirical model, I use lender-specific predicted default probabilities to capture heterogeneity in screening across lenders. These are constructed by estimating the logistic regression separately for each cooperative. Table C.3 presents summary statistics on the number of observations, goodness of fit, and mean predicted default across lender-specific regressions. Predicted default probabilities for banks come from the pooled cooperative model.

Although this is not the actual risk assessment performed by financial institutions, I expect it to provide a reasonable approximation. Lenders typically base their assessments on information collected during borrower visits, complemented by risk scores from credit bureaus. When borrowers lack a credit history, both lenders and credit bureaus rely on socioeconomic characteristics to determine a risk level using the type of variables included in my default probability model.

an indicator for joint loans, an indicator for repayment frequency lower than monthly.

[39] This restriction excludes loans originated in the post-reform period and loans with due dates extending into the COVID-19 pandemic. Approximately 75% of loans originated between July 2016 and December 2018 are included in the sample.

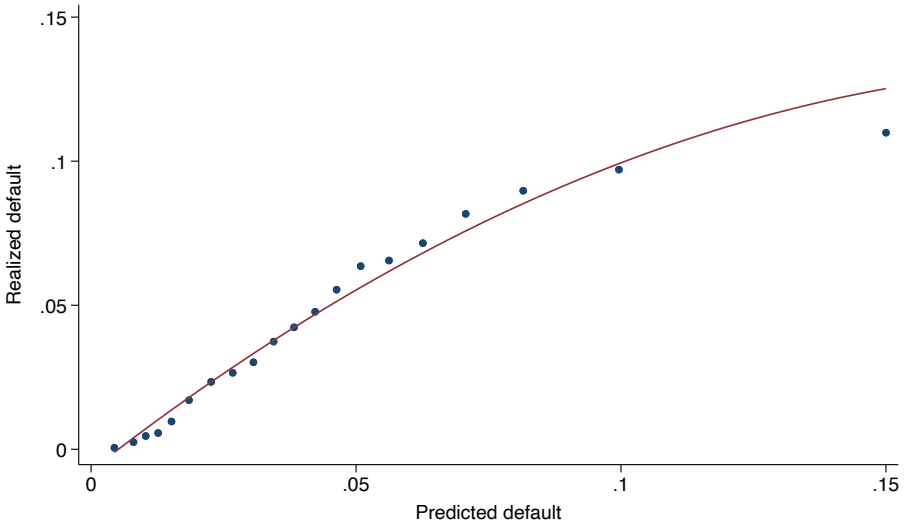
[40] Because I do not observe default for loans granted by banks, the regression only includes loans originated by cooperatives.

Table C.1: Default probability model

	1(Default)	
	(1) Coefficients	(2) Marginal Effects
Log(income)	0.009*	0.000*
	(0.006)	(0.000)
Log(wealth)	-0.069***	-0.002***
	(0.003)	(0.000)
Log(loan size)	-0.114***	-0.004***
	(0.008)	(0.000)
Elementary education	0.087***	0.003***
	(0.014)	(0.000)
Loan term (months)	0.065***	0.002***
	(0.001)	(0.000)
Mortgage	-0.248***	-0.008***
	(0.031)	(0.001)
Home owner	-0.305***	-0.009***
	(0.015)	(0.000)
Cosigner	-0.487***	-0.015***
	(0.018)	(0.001)
Guarantor	-0.199***	-0.006***
	(0.018)	(0.001)
Joint loan	0.160**	0.005**
	(0.066)	(0.002)
Low repayment frequency	-1.063***	-0.033***
	(0.032)	(0.001)
Constant	-3.420***	
	(0.073)	
Observations	540,897	540,897
Pseudo R^2	0.075	
Province FE	Yes	

Notes: Logit regression of an indicator for 90+ days past due. Robust standard errors in parentheses. Sample restricted to loans originated by cooperatives between July 2016 and December 2018 with fully observed repayment periods. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure C.1: In-sample predicted and realized default



Notes: This figure displays a binned scatterplot of realized default and predicted default probability according to the pooled regression. The sample is composed of loans included in the default model estimation. The red line is a quadratic fit of the relationship between both variables.

Table C.2: Out-of-sample predicted and realized default

	1(Default)		
	(1)	(2)	(3)
Pr(Default)	0.041***	0.042***	0.042***
	(0.002)	(0.002)	(0.002)
Interest rate		0.002***	0.002***
		(0.000)	(0.000)
Agriculture			-0.001
			(0.001)
Observations	731,550	731,550	731,550
R-squared	0.040	0.041	0.041
Dep. var. mean	0.026	0.026	0.026
Lender FE	Yes	Yes	Yes
Origination date FE	Yes	Yes	Yes
Parish FE	Yes	Yes	Yes
Subsegment FE	Yes	Yes	Yes

Notes: Results from regressions of loans originated by cooperatives between January 2017 and December 2019 that were not scheduled to be fully repaid by December 2019. These loans were excluded from the default model estimation. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.3: Summary statistics predicted default by lender

	Mean	SD	p10	p90	N
Pseudo R^2	0.152	0.156	0.052	0.268	142
Mean predicted default	0.107	0.114	0.029	0.191	142
Observations	3,674.1	5,401.7	160	8,455	142

Notes: This table presents summary statistics on lender-specific default probability models. Each observation corresponds to a cooperative.

D Additional evidence on pricing, lender, and borrower heterogeneity

Price dispersion. Table D.1 reports measures of interest rate dispersion computed at the parish level, using agricultural loans originated in the 12 months preceding the reform. Panel A presents statistics across lenders within each regulatory subsegment. Interest rates exhibit moderate dispersion, with a coefficient of variation of 0.11, an interquartile ratio of 1.15, and a 90–10 percentile ratio of 1.29 for the medium subsegment. Dispersion levels are similar for the small subsegment. Panel B shows that within-lender-subsegment variation is substantially lower. Cooperatives display a coefficient of variation of 0.05 and a 90–10 percentile ratio of 1.09, while banks price almost uniformly, with a coefficient of variation of 0.01 and a 90–10 percentile ratio of 1.02.

Table D.1: Measures of price dispersion

	Mean	SD	CV	75/25	90/10	N
Panel A: Across lenders, within subsegment						
Small subsegment (*minorista*)	23.08	2.65	0.12	1.16	1.30	780
Medium subsegment (*acumulación simple*)	21.52	2.28	0.11	1.15	1.29	935
Panel B: Within lender-subsegment						
Cooperatives	22.07	1.16	0.05	1.05	1.09	6,796
Banks	25.62	0.32	0.01	1.01	1.02	1,433

Notes: Each statistic is first computed at the parish level and then averaged across parishes. Panel A computes statistics across lenders operating within the same parish and subsegment. Small subsegment covers loans for borrowers with debt of up to $1,000. Medium subsegment refers to loans for borrowers with debt of more than $1,000 and up to $10,000. Panel B computes statistics across loans from the same lender, subsegment, and parish. N is the number of parish-level observations in Panel A and parish-lender-subsegment observations in Panel B. The 75/25 and 90/10 columns report the ratio of the corresponding percentiles. Sample: agricultural loans originated up to 12 months prior to the reform.

These patterns, suggesting that most variation in interest rates stems from differences across lenders, are reinforced by regression evidence. Regressing interest rates on borrower and loan characteristics, and controlling for month and parish fixed effects, yields an R-squared of 0.50.[41] Adding lender fixed effects raises the R-squared to 0.70, indicating that lender identity alone accounts for a substantial share of the remaining variance. This evidence indicates that lenders use highly uniform pricing within each subsegment,

[41] The covariates included in the regression are: borrower income, education, housing status, economic activity, and predicted default probability, loan size, term, payment frequency, collateral, and regulatory subsegment. The sample includes loans originated by cooperatives (due to data availability) in the 12 months prior to the regulatory change.

with only minimal idiosyncratic variation across borrowers, and motivates the structural assumption that lenders set a single price per subsegment.

Differences between banks and cooperatives. Banks and cooperatives differ along multiple dimensions. Table D.2 turns to balance sheet indicators.[42] Banks are substantially larger than cooperatives as measured by total assets. They allocate a smaller share of their assets to the loan portfolio, which is also less concentrated in microcredits. Their deposits-to-liabilities ratio is lower, indicating more diversified funding sources and substantially lower funding costs.

Table D.2: Balance sheet indicators

	Cooperatives		Banks		Bank - Coop		
	Mean	SD	Mean	SD	Diff	p-value	N
Assets (USD million)	92.259	210.823	1,771.16	2,822.144	1678.902	0.01	147
Loan-to-assets ratio	0.759	0.089	0.648	0.151	-0.111	< 0.01	147
Microcredit share	0.566	0.274	0.332	0.408	-0.234	0.01	147
Liabilities-to-assets ratio	0.833	0.077	0.824	0.102	-0.009	0.70	147
Deposits-to-liabilities ratio	0.882	0.097	0.763	0.250	-0.119	0.04	147
Non-performing loans	0.087	0.068	0.135	0.205	0.048	0.30	147
Funding cost	5.961	1.330	3.803	1.881	-2.158	< 0.01	147
Operating cost	8.196	2.988	10.425	7.015	2.229	0.17	147
Return on assets	0.007	0.012	-0.004	0.069	-0.012	0.45	147

Notes: This table reports differences in balance-sheet indicators measured in 2018 between cooperatives and banks. Microcredit share is microcredit-to-loan portfolio ratio. Non-performing loans are specific to microcredit. Funding cost is the ratio of interest expenses and commissions payable to deposits. Operating cost is the ratio of total operating expenses to deposits. Return on assets is profits-to-assets ratio.

Table D.3 presents loan and operational characteristics measured before the reform. Banks charge interest rates that are 3.66 p.p. higher than cooperatives and issue loans that are, on average, 18.9% ($635) smaller. The two lender types allocate similar shares of their microcredit portfolios across sectors and within the agricultural sector. However, they differ significantly in their branch network. Banks are located approximately 5.7 km farther from their borrowers but operate around seven times more branches.

Table D.4 uses data from the agricultural survey to compare borrowers who obtained credit from each lender type in 2019. Bank borrowers operate farms that are 30% larger

[42] I obtain lenders' annual balance sheets from the supervisory agencies and use them to construct proxies for costs. Funding cost is approximated as the ratio of interest expenses and commissions payable to total deposits (interbank and non-bank), capturing the effective rate lenders pay to finance loan originations. Operating cost is calculated as the ratio of total operating expenses to total deposits. Together, these components account for nearly the entire expense structure reported in lenders' balance sheets.

Table D.3: Loan and operational characteristics

	Cooperatives		Banks		Bank - Coop		
	Mean	SD	Mean	SD	Diff	p-value	N
Interest rate	22.806	3.794	26.348	1.814	3.662	< 0.01	992,887
Loan size ($)	3,367.351	2,725.583	2,924.507	1,914.65	-635.074	0.01	992,887
1(Agriculture)	0.302	0.459	0.270	0.444	-0.009	0.94	992,887
1(Perennial)	0.047	0.211	0.042	0.201	-0.001	0.93	992,887
1(Annual)	0.145	0.352	0.098	0.297	-0.036	0.43	992,887
1(Livestock)	0.075	0.263	0.128	0.334	0.061	0.26	992,887
Distance (km)	11.250	13.267	17.140	13.978	5.736	< 0.01	992,887
Number of branches	8.706	9.032	57.111	65.71	48.405	< 0.01	154
Number of parishes	66.522	77.87	92.056	128.39	25.533	0.40	154

Notes: This table reports differences in characteristics obtained from the loan data set between cooperatives and banks. The sample is composed of loans issued prior to the reform. Results from regressions for variables other than number of branches and number of parishes are at the loan level, include subsegment fixed effects and cluster standard errors at the lender level.

and are 64% more likely to have an irrigation pump. Across all other observable characteristics, there are no significant differences between customers of the two lender types at the 5% level.

Table D.4: Agricultural borrower characteristics

	Mean		Bank - Coop		
	Cooperatives	Banks	Diff	p-value	N
1(Male)	0.800	0.849	0.049	0.163	544
Age	49.02	50.079	1.059	0.391	544
Elementary education	0.717	0.682	-0.035	0.436	544
1(Seguro social campesino)	0.281	0.366	0.085	0.057	544
Farm area	10.523	13.71	3.187	0.046	544
1(Farm owner)	0.829	0.839	0.009	0.787	544
1(Annual)	0.287	0.243	-0.044	0.286	544
1(Perennial)	0.408	0.404	-0.004	0.941	544
Cattle	6.939	9.461	2.522	0.139	544
Porcine	1.693	2.946	1.253	0.140	544
Labor	2.760	2.590	-0.170	0.383	544
Tractor	0.294	0.281	-0.013	0.762	539
Irrigation pump	0.116	0.19	0.074	0.040	539
Drain	0.055	0.04	-0.015	0.434	539

Notes: This table reports results from separate regressions of the outcome variable on an indicator for farmers who obtained a loan from a bank. Regressions are weighted by survey weights. The sample consists of farmers who obtained a loan of up to $10,000 in 2019.

The differences in balance sheet structure and geographic footprint suggest that cooper-

atives and banks may respond differently to the interest rate cap, while the similarity in borrower composition indicates that they serve observationally comparable customers.

E Changes in prices on pre-existing regulatory subsegments

Prior to the reform, the interest rate regulation defined subsegments with different price caps that continued to exist in the post-reform period to cover non-agricultural loans. However, most agricultural loans that did not comply with the agricultural cap were classified under the pre-existing subsegments. This feature allows me to test whether the interest rates on loans classified under these subsegments changed around the time of the reform in the targeted and untargeted sectors. I estimate the following specification:

$$p_{ijlt} = \iota_j + \iota_l + \rho_1 \cdot Subsegment_i + \rho_2 \cdot Post_t + \rho_3 \cdot Post_t \times Subsegment_i + \epsilon_{ijlt} \qquad \text{(E1)}$$

where $Subsegment_i$ is an indicator for *microcrédito minorista*, ι_j and ι_l are lender and parish fixed effects. The coefficient of interest is ρ_3, which captures whether the difference in interest rates across pre-existing subsegments changed after the reform. The sample covers loans originated between March 2018 and February 2020, that is, one year before and one year after the reform, and excludes loans classified under the agricultural subsegment.

Table E.1 presents the results from separate regressions for cooperatives and banks across sectors. There are three key findings. First, lenders charge systematically different prices across pre-existing subsegments (between 1.0 and 2.7 p.p.). Second, the average interest rate in the pre-existing regulatory subsegments did not change significantly after the reform, as all coefficients associated with *Post* are statistically insignificant. Third, the *Post* × *Subsegment* coefficients are economically small (less than one-fifth of the baseline difference) and statistically insignificant in all specifications, except for cooperatives in the agricultural sector (column 1). Because some compliant agricultural loans were also classified under the pre-existing subsegments, I augment equation (E1) with an indicator for loans with interest rates below the agricultural cap (20.97%) and its interaction with the *Post* indicator. Once these potentially misclassified agricultural loans are controlled for, the *Post* × *Subsegment* coefficient for cooperatives in the agricultural sector shrinks and becomes statistically insignificant (column 3). Taken together, these results indicate that the price difference between pre-existing subsegments did not change in response to

the reform.

Table E.1: Changes in interest rates on pre-existing regulatory subsegments

	Interest rate							
	Cooperatives				Banks			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Agriculture	Non-agriculture	Agriculture	Non-agriculture	Agriculture	Non-agriculture	Agriculture	Non-agriculture
Subsegment	1.412***	1.785***	1.008***	1.285***	2.616***	2.575***	2.723***	2.695***
	(0.321)	(0.442)	(0.185)	(0.306)	(0.298)	(0.199)	(0.223)	(0.126)
Post	-0.152	-0.208	-0.181	-0.165	-0.256	-0.0654	0.0351	-0.0675
	(0.190)	(0.150)	(0.144)	(0.165)	(0.295)	(0.0522)	(0.0696)	(0.0495)
Post × Subsegment	-0.493*	-0.377	-0.203	-0.0803	-0.0687	0.0930	-0.359	0.0722
	(0.269)	(0.348)	(0.143)	(0.169)	(0.454)	(0.0534)	(0.426)	(0.0475)
$1(p < 20.97)$			-5.117***	-5.115***			-6.543***	-11.88***
			(0.411)	(0.311)			(0.699)	(0.550)
Post $\times 1(p < 20.97)$			0.280	0.159			1.847**	1.259
			(0.223)	(0.211)			(0.728)	(1.244)
Lender FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Parish FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	144,044	472,679	144,044	472,679	47,657	164,487	47,657	164,487
R-squared	0.613	0.653	0.838	0.842	0.693	0.534	0.881	0.810

Notes: This table reports coefficients from equation (E1) estimated separately for cooperatives and banks, agricultural and non-agricultural sectors. The sample includes loans originated between March 2018 and February 2020, and excludes loans classified under the agricultural regulatory subsegment. The dependent variable is the loan interest rate. Post is an indicator for months after the regulatory change. Subsegment is an indicator for loans classified under the *microcrédito minorista* subsegment. $1(p < 20.97)$ is an indicator for loans that carry an interest rate lower than agricultural cap and is meant to control for misclassification of compliant agricultural loans. Standard errors clustered at the lender level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

F Gibbs Sampler

The Gibbs sampler jointly estimates borrower preference parameters β, lender cost and thresholds parametersγ, and the scale of the profit shock σ_ν by treating borrower utilities, expected profitability, and offers as latent variables. The algorithm iteratively draws from the conditional distributions implied by the model's restrictions: lenders that originate a loan must have found it profitable to extend an offer, and borrowers who obtain a loan must prefer the lender over all other lenders in their choice set and the outside option.

Let $u_{ij} = \bar{u}_{ij} + \varepsilon_{ij}$ denote borrower i's latent utility from lender j, where $\bar{u}_{ij} = X^u_{ij}\beta$ is the systematic component and $\varepsilon_{ij} \sim N(0,1)$. Let $u_{i0} \sim N(0,1)$ denote the latent utility from the outside option. Let $\pi_{ij} = \bar{\pi}_{ij} + \nu_{ij}$ denote lender j's latent expected profitability from extending an offer to borrower i, net of its reservation profitability, where

$$\bar{\pi}_{ij} = \omega_{ij}(1 - d_{ij})p_j - \omega_{ij}c_j - C_{ij} - \delta_{\tau(j)}$$

is the systematic component and $v_{ij} \sim N(0, \sigma_v^2)$ is a cost shock. Lender j extends an offer to borrower i if and only if $\pi_{ij} > 0$, that is, if the difference between revenue and costs exceeds the reservation profitability $\delta_{\tau(j)}$. In estimation, $\bar{\pi}_{ij}$ includes one intercept per lender type. I assume baseline offer costs are common across lender types and normalize them to zero, so the lender-type intercepts are interpreted as thresholds, $\lambda_j = \delta_{\tau(j)}$, since the cross-sector price gap is zero in the pre-reform estimation sample. I collect the parameters corresponding to cost and the thresholds in γ, so that $\pi_{ij} = X_{ij}^v \gamma + \nu_{ij}$, where X_{ij}^v includes lender-type indicators with coefficients $-\delta_\tau$. I use $m(i)$ to denote the lender that originated borrower i's loan, with $m(i) = 0$ (the outside option) for borrowers without a loan, and O_i to denote her choice set.

The priors are $\beta \sim N(\mu_\beta^0, \Sigma_\beta^0)$, $\gamma \sim N(\mu_\gamma^0, \Sigma_\gamma^0)$, and $\sigma_v^2 \sim IG(n_0/2,\, s_0/2)$. The hyperparameters μ_β^0, Σ_β^0, μ_γ^0, and Σ_γ^0 are set to be diffuse, and $n_0 = 3$ and $s_0 = 2$ give a weakly informative prior on σ_v^2. I describe each block of the sampler below.

Block 1: Draw lender latent profitability. Given current borrower latent utilities u and parameters (γ, σ_ν), the model imposes the following restrictions on π_{ij}. Draws are from truncated normal distributions centered at $\bar{\pi}_{ij}$ with variance σ_v^2.

(a) *Borrower-lender pairs with a loan* ($j = m(i) \neq 0$). The lender made an offer, so $\pi_{ij} > 0$. In addition, since costs and the threshold enter with a negative sign in $\bar{\pi}_{ij}$, the latent variable must satisfy $\pi_{ij} < \omega_{ij}(1 - d_{ij})p_j$:

$$\pi_{ij} \sim TN\Big(\bar{\pi}_{ij},\ \sigma_v^2,\ 0,\ \omega_{ij}(1 - d_{ij})p_j\Big)$$

(b) *Borrower-lender pairs without a loan where borrower i prefers lender j to the lender that originated her loan* ($j \neq m(i)$, $u_{ij} > u_{im(i)}$). The borrower would have chosen lender j had an offer been made, so the lender must not have made an offer: $\pi_{ij} < 0$:

$$\pi_{ij} \sim TN\Big(\bar{\pi}_{ij},\ \sigma_v^2,\ -\infty,\ 0\Big)$$

(c) *Borrower-lender pairs without a loan where borrower i does not prefer lender j* ($j \neq m(i)$, $u_{ij} \leq u_{im(i)}$). Whether or not the lender made an offer is irrelevant, as the borrower would not have chosen lender j regardless. The only restriction is the upper bound:

$$\pi_{ij} \sim TN\Big(\bar{\pi}_{ij},\ \sigma_v^2,\ -\infty,\ \omega_{ij}(1 - d_{ij})p_j\Big)$$

Block 2: Draw borrower latent utilities. Given the current lender latent profitability π, the implied choice set for borrower i is $O_i = \{j : \pi_{ij} > 0\}$. The restrictions on borrower latent utilities follow from the observed borrower-lender pair with a loan.

(a) *Lenders not in the choice set* ($j \notin O_i$). No restriction applies:

$$u_{ij} \sim N(\bar{u}_{ij},\ 1)$$

(b) *Lender that originates the loan* ($j = m(i) \neq 0$). The borrower prefers this lender to all other lenders in her choice set and to the outside option:

$$u_{ij} \sim TN\left(\bar{u}_{ij},\ 1,\ \max\left\{\max_{j' \in O_{i,-j}} u_{ij'},\ u_{i0}\right\},\ +\infty\right)$$

(c) *Lenders in the choice set that do not originate the loan* ($j \in O_i$, $j \neq m(i)$). The borrower does not prefer this lender:

$$u_{ij} \sim TN\Big(\bar{u}_{ij},\ 1,\ -\infty,\ u_{im(i)}\Big)$$

(d) *Outside option*. For borrowers with loans ($m(i) \neq 0$), u_{i0} is bounded above by the utility from the lender that originated the loan: $u_{i0} \sim TN(0,\ 1,\ -\infty,\ u_{im(i)})$. For borrowers without a loan ($m(i) = 0$), u_{i0} exceeds the utility from all lenders in the choice set: $u_{i0} \sim TN(0,\ 1,\ \max_{j \in O_i} u_{ij},\ +\infty)$.

Block 3: Draw parameters. Given the augmented latent utilities u and latent profitability π, the model reduces to two one-sided problems. Since $u_{ij} = X^u_{ij}\beta + \varepsilon_{ij}$ and $\pi_{ij} = X^v_{ij}\gamma + \nu_{ij}$ with normal errors, the conditional posteriors of β and γ follow from normal-normal conjugacy:

$$\beta \mid u, X^u \sim N(\bar{\mu}_\beta,\ \bar{\Sigma}_\beta) \qquad \gamma \mid \pi, X^v, \sigma_\nu \sim N(\bar{\mu}_\gamma,\ \bar{\Sigma}_\gamma)$$

where $\bar{\Sigma}_\beta = (X^{u\prime}X^u + \Sigma_\beta^{0\,-1})^{-1}$, $\bar{\mu}_\beta = \bar{\Sigma}_\beta(X^{u\prime}u + \Sigma_\beta^{0\,-1}\mu_\beta^0)$, $\bar{\Sigma}_\gamma = (X^{v\prime}X^v/\sigma_\nu^2 + \Sigma_\gamma^{0\,-1})^{-1}$, and $\bar{\mu}_\gamma = \bar{\Sigma}_\gamma(X^{v\prime}\pi/\sigma_\nu^2 + \Sigma_\gamma^{0\,-1}\mu_\gamma^0)$.

Given the new draw of γ, the scale of the cost shock is drawn from its inverse-gamma

conditional posterior:

$$\sigma_v^2 \mid \pi, X^v, \gamma \sim IG\left(\frac{n_0 + N}{2},\ \frac{s_0 + \sum_{(i,j)} (\pi_{ij} - X_{ij}^v \gamma)^2}{2}\right)$$

where the sum runs over the N feasible borrower-lender pairs. Borrower-lender pairs that are assumed to be infeasible are excluded from both regressions and from the residual sum of squares.[43]

Block 4: Update beliefs. At each iteration of the Gibbs sampler, beliefs ω_{ij} are updated given the current parameter draws. Rather than solving for the fixed point at each iteration, beliefs are updated with a small number of simulation draws, allowing them to co-evolve with parameters within the chain.

Specifically, given the current $(\beta, \gamma, \sigma_v, \omega)$, the belief update proceeds as follows. For each simulation draw: (i) draw taste shocks ε_{ij} and outside option utilities u_{i0}; (ii) draw offers from lenders other than j according to the current offer probabilities $\Phi(\bar{\pi}_{ij}/\sigma_v)$; (iii) for each lender j, include its offer into the choice set and determine whether borrower i would choose j given the resulting choice set $O_{i,-j} \cup \{j\}$. The updated belief is the fraction of simulations in which borrower i accepts lender j's offer:

$$\omega_{ij}^{new} = \frac{1}{N_{sim}} \sum_{n_{sim}=1}^{N_{sim}} \mathbf{1}\{i \text{ chooses } j \mid O_{i,-j}^{n_{sim}} \cup \{j\}\}$$

After Blocks 1–4, systematic utilities $\bar{u}$ and systematic profitability $\bar{\pi}$ are recomputed with the updated parameters and beliefs, and the sampler proceeds to the next iteration.

G Construction of the empirical model estimation sample

The estimation sample covers loans originated in the twelve months preceding and following the reform. To reduce the computational burden, the sample is restricted to provinces in the central Andes region: Bolívar, Chimborazo, Cotopaxi, Pastaza, and Tungurahua. While there are 72 active lenders, only 25 have a market share above 1%, collectively accounting for roughly 90% of originations. I aggregate the remaining lenders into

[43] Only lenders with observed loans in i's province are assumed to be feasible.

a single synthetic lender and include it in the estimation sample.[44] An alternative would be to absorb these lenders into the outside option. I instead retain them as a synthetic lender so that the outside option maintains the interpretation of potential borrowers who do not obtain credit from any formal institution. Because the sampler treats this synthetic lender as a single institution when it is in fact an aggregation of different lenders, I allow it to have its own offer threshold.

As identification relies on variation in origination probabilities across individuals with and without a loan, I complement the administrative origination data with survey data on agricultural producers, ESPAC. Specifically, using the survey sampling weights, I draw agricultural producers who report not having borrowed but express willingness to obtain a loan, and add them to the pre-reform borrower pool. Borrowers without a loan represent 25% of the sample.[45] Unfortunately, information on non-agricultural producers without a loan is not available in the survey data. Including non-agricultural borrowers would allow me to identify cross-sector differences in price elasticities and quantify the effects of the cap on the untargeted sector.

For the post-reform period, I construct a comparable borrower pool by resampling from the pre-reform period so that the distribution of observable characteristics remains stable across periods. To do so, I discretize continuous variables and define borrower types by grouping on observable characteristics. I then add pre-reform borrowers to each type in the post-reform period until the number of borrowers per type is approximately balanced across periods. These resampled borrowers are assumed to not get a loan. This procedure is designed to approximate an underlying pool of potential borrowers that is stable across years, and borrowers who do not obtain a loan after the reform may or may not have obtained a loan prior to it.

H The role of selectivity

The reform may have also prompted adjustments in the composition of the borrower pool and loan characteristics. I investigate whether the characteristics of agricultural borrowers changed after the reform using the following specification:

[44] Focusing on the largest lenders is common in the literature. For instance, Cuesta and Sepúlveda (2021) and Nelson (2025) focus on the 12 and 19 largest lenders in their respective settings.
[45] Results are robust to alternative assumptions on the size of the borrower pool without a loan.

$$y_{jlt} = \rho\, Post_t + \iota_j + \eta_{jlt}, \tag{H1}$$

where y_{jlt} is the mean of a borrower characteristic for lender j in parish l and quarter t, computed over agricultural loans and weighted by the number of loans. Unlike equation (6), time fixed effects are not included as they are collinear with the $Post_t$ indicator given that the sample is restricted to the targeted sector. Standard errors are clustered at the lender level and the first quarter of 2019 is omitted. Parameter ρ shows whether within-lender averages of borrower characteristics shifted after the reform.

Table H.1 shows the results from equation (H1). Predicted default probability increased 0.004 (6.0% relative to the baseline mean), the distance between borrowers and lenders decreased 0.8% and the share of high-income borrowers increased 4.3 p.p. (8.6% relative to the baseline mean).[46] Conversely, changes in the share of highly educated borrowers, and in the share of loans for perennial and annual crop production are not statistically significant.[47]

Table H.1: Changes in agricultural borrower characteristics

	(1) Pr(Default)	(2) Log(distance to branch)	(3) High income	(4) High education	(5) Perennial crop	(6) Annual crop
Post	0.004***	-0.008**	0.043***	-0.005	0.007	-0.014
	(0.001)	(0.004)	(0.010)	(0.008)	(0.010)	(0.011)
Observations	15,900	15,900	15,900	15,900	15,900	15,900
R-squared	0.323	0.258	0.340	0.109	0.291	0.325
Lender FE	Yes	Yes	Yes	Yes	Yes	Yes
Baseline	0.067	2.321	0.500	0.357	0.161	0.376

Notes: This table reports results from equation (H1). The sample aggregates data of agricultural borrowers at the lender-parish-quarter level weighting by the number of loans. Baseline reports means in the quarter preceding the reform. Standard errors in parentheses are clustered at the lender level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The increase in default probability is at odds with a natural expectation that financial institutions would prioritize lending to more creditworthy borrowers when they are constrained. To investigate this further, I compare agricultural and non-agricultural borrowers using specification (6), restricting the sample to cooperative originations (the sample used to estimate the default model).[48] The top row of Table H.2 shows no significant

[46] High-income is defined as above-median income.

[47] High-education is defined as having attended secondary education or more.

[48] Realized default outcomes are not examined, as the repayment period for post-reform loans coincides

differential change in predicted default probability across sectors after the reform. The remaining rows report results from separate regressions in which the determinants of the default model serve as outcome variables. Agricultural borrowers' income and wealth increased relative to non-agricultural borrowers, as did loan size, loan term, and the share of loans with a cosigner.

Table H.2: Differential changes in variables used in the default model

	Agriculture × Post	N	Baseline
Default probability	0.000	30,062	0.066
	(0.001)		
Log(income)	0.050**	30,062	6.883
	(0.023)		
Log(wealth)	0.141***	30,062	9.213
	(0.031)		
Log(loan size)	0.071***	30,062	7.834
	(0.013)		
Elementary education	0.005	30,062	0.649
	(0.007)		
Loan term (months)	0.715***	30,062	22.304
	(0.193)		
Mortgage	0.001	30,062	0.034
	(0.003)		
Home owner	0.009	30,062	0.544
	(0.007)		
Cosigner	0.021***	30,062	0.374
	(0.006)		
Guarantor	0.003	30,062	0.400
	(0.006)		
Joint loan	-0.001	30,062	0.010
	(0.001)		
Low repayment frequency	0.003	30,062	0.246
	(0.005)		

Notes: This table reports the coefficient on the interaction of an agriculture indicator with a post-reform indicator. The sample aggregates data at the lender–parish–sector level, weighting by the number of loans. Each row corresponds to a separate regression. All regressions include lender–parish–sector and lender–quarter fixed effects. Standard errors in parentheses are clustered at the lender–parish–sector level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Overall, the results suggest that the default probability is not the primary dimension along which lenders screen their borrowers when interest rate caps are imposed. This

with the onset of the COVID-19 pandemic, which prevents a clean analysis.

is consistent with an environment where credit history is not universal and other characteristics such as income and borrower-lender distance may be better determinants of lending profitability.

I now use the model to capture how characteristics beyond creditworthiness factor in lenders' borrower screening and highlight the role of selectivity. I first simulate a scenario under the pre-existing caps in which lenders are not selective when extending offers. Lenders' outreach capacity is held fixed, but all borrowers have the same probability of receiving an offer. This approximates a setting in which lenders choose aggregate outreach effort but do not discriminate among potential borrowers.[49]

Columns (1)–(3) in Table H.3 present a comparison between the selective and non-selective scenarios. Not surprisingly, selectivity changes the composition of the pool of borrowers toward individuals with lower default probabilities, who are located closer to their branches and who obtain larger loans. Welfare is higher when lenders are selective as it not only increases the total number of realized loans but also improves the match quality.

I also explore the interaction between selective outreach and interest rate caps. Columns (4) and (5) report the effect of the targeted cap, each measured against its own baseline. The selective model predicts that the targeted cap shifts the borrower pool toward lower default probabilities, smaller distance to branch, and higher loan sizes. The predictions on distance and loan size match the data patterns. By contrast, the non-selective model incorrectly predicts that distance to branch would increase under the cap. Changes in market shares are larger under the selective benchmark. Finally, the selective model predicts a smaller welfare loss, as selectivity leads to more realized loans and higher match quality even under the cap.

I Incorporating the pricing decision

The model abstracts from price formation because two simple assumptions capture the observed price patterns induced by the cap. First, post-reform prices of loans classified under the agricultural subsegment adjust to the cap if the pre-reform price was above it and remain unchanged otherwise. Second, post-reform prices of agricultural loans clas-

[49] This is similar to Goeree (2008)'s setting in which firms choose the aggregate level of advertising, which affects all consumers' awareness probability.

Table H.3: Market outcomes and welfare under selective and non-selective outreach

	(1)	(2)	(3)	(4)	(5)
	Baseline			Effect of the cap	
	Selective	Non-selective	Difference (2) − (1)	Selective	Non-selective
Matched borrower characteristics					
Default probability	0.050	0.070	0.020	-0.003	0.001
Log(distance to branch)	2.27	2.60	0.33	-0.07	0.01
Large loan ($>$ \$1,000)	0.76	0.75	-0.01	0.02	0.01
Price					
Posted	23.24	23.24	0	-1.43	-1.43
Transacted	23.67	23.65	-0.01	-1.66	-1.64
Market shares					
Banks	18.50	20.57	2.07	1.49	0.22
Cooperatives	46.35	41.77	-4.57	-10.24	-8.85
Outreach					
Banks' offer probability	0.35	0.35	0.00	-0.16	-0.16
Cooperatives' offer probability	0.12	0.12	0.00	-0.05	-0.05
Expected offers per borrower	3.69	3.69	0.00	-1.76	-1.75
Welfare					
Consumer surplus	6.56	5.77	-0.79	-0.52	-0.65
Expected profit per borrower	5.22	4.71	-0.51	-1.39	-1.77

Notes: Columns (1) and (2) report pre-reform levels under the estimated selective and non-selective scenarios, holding each lender's overall offer volume fixed. Column (3) is the difference. Columns (4) and (5) report the effect of the targeted cap, each measured against its own baseline. The posted price is the average rate across feasible borrower-lender pairs, while the transacted price is weighted by match probability. All simulations use the pre-reform borrower pool. Matched borrower characteristics are match-probability weighted averages. Market shares are in percentage points; welfare measures are in interest-rate points.

sified under the pre-existing regulatory subsegments equal the prices of non-agricultural loans.

To explore the consequences of relaxing these assumptions, I incorporate a Nash-Bertrand pricing stage to the model. This allows prices to respond to the regulation in equilibrium, with consequences for the offer and acceptance decisions. I do not use this as the main specification because the Nash-Bertrand first-order condition is unlikely to hold with equality even in the pre-reform period. This is because banks price at the pre-existing interest rate caps, while cooperatives may not exploit their market power because of their governance structure and social mission. In both cases, lenders charge below the profit-maximizing markup. Previous research addresses some of these challenges separately, but jointly accommodating cooperative objectives, selective outreach, and binding price controls is beyond the scope of this paper.[50]

[50] For instance, Fan and Zhang (2022) develop a procedure to accommodate binding pricing constraints that relies on observing products for which the first-order optimality condition holds. Duarte et al. (2025) pro-

Despite these caveats, I use the pricing stage to check the robustness of the main results in two ways. First, I compare the origination costs estimated with the Gibbs sampler to those implied by the pricing first-order condition. Second, I compare the counterfactual results with and without strategic price adjustment.

Pricing model. Since the data show little price dispersion within regulatory subsegment at the lender level, I assume that lenders charge a single price for each regulatory subsegment. Consistent with the institutional features of the industry, I assume that prices are set before the offer decision. At the pricing stage, lenders know the distribution of the cost shocks ν_{ihj}, but not their realization. Moreover, they take their threshold λ_j as given.[51]

Lender j chooses prices to maximize the ex-ante expected profits from the offer decision:

$$\max_{p_{hj}} \mathbb{E}_\nu[\Pi_j] = \sum_h \sum_i \mathbb{E}_\nu\left[\max\left\{\omega_{ij}(p_{hj}(1-d_{ij})-c_j) - C_{ij} + \nu_{ij} - \lambda_j,\ 0\right\}\right] \tag{I1}$$

where ex-ante profits take the form of the expected option value of extending an offer. That is, the lender expects a positive payoff when it is optimal to extend an offer and zero otherwise. The expectation is taken over the distribution of profit shocks.

The first-order condition satisfies:

$$\frac{\partial \mathbb{E}_\nu[\Pi_j]}{\partial p_{hj}} = \sum_i \Pr(\sigma_{ij}=1) \cdot \left[\omega_{ij}(1-d_{ij}) + \frac{\partial \omega_{ij}}{\partial p_{hj}}\left(p_{hj}(1-d_{ij}) - c_j\right)\right] = 0 \tag{I2}$$

Unlike standard models, where every consumer contributes equally to the firm's pricing objective, here the Bertrand first-order condition is weighted by the probability of extending an offer, $\Pr(\sigma_{ij}=1)$.[52] Intuitively, borrowers who are more likely to receive an offer

vide an empirical framework that relies on excluded instruments and cross-market variation in residual demand elasticity to test whether consumer cooperatives' pricing differs from that of profit maximizing firms.

[51] Treating the threshold as given at the pricing stage is consistent with both the empirical model and the theoretical framework. In the empirical model of the targeted sector, the threshold depends on the wedge created by the cap rather than on the lender's own price, so lenders take it as given with or without the cap. In the theoretical framework, λ_j is the multiplier on the capacity constraint, and the pricing stage maximizes the value function of the offer stage. By the envelope theorem, the derivative of this value function with respect to λ_j is zero at the optimum. Therefore, price-induced changes in λ_j drop out of the pricing first-order condition.

[52] In this first-order condition, beliefs play the role of choice probabilities and prices are discounted by

have greater influence in the pricing decision, as the lender tailors prices to the set of borrowers it expects to later select.

Derivation of the pricing first-order condition. The expected payoff in equation (I1) can be decomposed into the probability that an offer is made times the conditional expected payoff given that it is made. The lender's pricing problem becomes:

$$\max_{p_{hj}} \ \mathbb{E}_\nu[\Pi_j] = \sum_h \sum_i \Pr(\sigma_{ij} = 1) \left(\pi_{ij} + \mathbb{E}\left[\nu_{ij} \mid \sigma_{ij} = 1\right] - \lambda_j\right),$$

where $\pi_{ij} = \omega_{ij}\left(p_{hj}(1 - d_{ij}) - c_j\right) - C_{ij}$ is the deterministic component of expected profitability.

Given that $\nu_{ij} \sim N(0, \sigma_\nu^2)$, the offer probability is:

$$\Pr(\sigma_{ij} = 1) = \Pr(\pi_{ij} + \nu_{ij} - \lambda_j > 0) = \Phi(b_{ij}),$$

where $b_{ij} = (\pi_{ij} - \lambda_j)/\sigma_\nu$ and $\Phi(\cdot)$ denotes the standard normal CDF. The expected shock conditional on an offer is:

$$\mathbb{E}\left[\nu_{ij} \mid \sigma_{ij} = 1\right] = \mathbb{E}\left[\nu_{ij} \mid \nu_{ij} > \lambda_j - \pi_{ij}\right] = \frac{\int_{\lambda_j - \pi_{ij}}^{\infty} \frac{\nu}{\sigma_\nu} \phi\left(\frac{\nu}{\sigma_\nu}\right) d\nu}{\int_{\lambda_j - \pi_{ij}}^{\infty} \frac{1}{\sigma_\nu} \phi\left(\frac{\nu}{\sigma_\nu}\right) d\nu}$$

where $\phi(\cdot)$ is the standard normal PDF. Using $\partial\phi(\nu)/\partial\nu = -\nu\phi(\nu)$ and the symmetry of the normal distribution, we have:

$$\mathbb{E}\left[\nu_{ij} \mid \sigma_{ij} = 1\right] = \sigma_\nu \frac{\phi(b_{ij})}{\Phi(b_{ij})}.$$

Then, the ex-ante profit function is:

$$\mathbb{E}_\nu[\Pi_j] = \sum_h \sum_i \left\{\Phi(b_{ij})\left(\pi_{ij} - \lambda_j\right) + \sigma_\nu\, \phi(b_{ij})\right\}$$

Since $\frac{\partial b_{ij}}{\partial p_{hj}} = \frac{1}{\sigma_\nu} \cdot \frac{\partial \pi_{ij}}{\partial p_{hj}}$, the first-order condition for the price in a given regulatory subseg-

default probabilities.

ment is:

$$\begin{aligned}\frac{\partial \mathbb{E}_\nu[\Pi_j]}{\partial p_{hj}} &= \sum_i \Big[\phi(b_{ij})\, b_{ij} + \Phi(b_{ij}) - \phi(b_{ij})\, b_{ij}\Big] \cdot \frac{\partial \pi_{ij}}{\partial p_{hj}} \\ &= \sum_i \Phi(b_{ij}) \cdot \frac{\partial \pi_{ij}}{\partial p_{hj}} \\ &= \sum_i \Pr(\sigma_{ij} = 1) \cdot \left[\omega_{ij}(1 - d_{ij}) + \frac{\partial \omega_{ij}}{\partial p_{hj}} \left(p_{hj}(1 - d_{ij}) - c_j\right)\right] = 0,\end{aligned}$$

The terms involving ϕ cancel, reflecting the trade-off between the extensive margin and the conditional value of the profit shock at the margin.

Origination costs. In the main specification, origination costs are estimated with the Gibbs sampler and identified through the exclusion restrictions and the variation they generate in acceptance probabilities. I invert equation (I2) at observed pre-reform prices to recover the origination cost c_j^{FOC} that rationalizes the observed price as profit-maximizing. The two sets of estimates are close. The average difference is 0.53 p.p. (standard deviation of 0.93), or approximately 3.5% of origination costs estimated with the sampler.

Counterfactual results incorporating the pricing decision. Table I.1 reports changes in market outcomes and welfare under the main counterfactual simulations, using c_j^{FOC} to predict equilibrium prices.[53] The results are close to those in Tables 9 and 10. With Nash-Bertrand pricing, the cap reduces the average transacted price by 1.71 p.p., compared to 1.66 p.p. when prices adjust mechanically to the cap. Offers fall by 48.5% compared to 47.7%. The two approaches differ little because most prices are pinned at the regulatory caps in both. Strategic pricing affects only unconstrained prices in the agricultural and pre-existing subsegments, which respond to changes in each subsegment's borrower pool caused by the reclassification induced by the reform. Because the model covers only the agricultural sector, prices in pre-existing subsegments are set with respect to agricultural borrowers.

[53] To solve for equilibrium prices, I rescale the parameters in the demand equation according to the variance of the Extreme Value Type 1 distribution, as explained in footnote 25.

Table I.1: Changes in market outcomes and welfare with pricing model

			Channel		Policy design	
	Baseline	Targeted cap (CF1 − Baseline)	Acceptance (CF2 − Baseline)	Offers (CF1 − CF2)	Cap level (CF3 − Baseline)	Targeting (CF1 − CF3)
	(1)	(2)	(3)	(4)	(5)	(6)
Price						
Posted	23.15	-1.45	-1.45	0	-1.49	0.05
Transacted	23.71	-1.71	-1.50	-0.21	-1.45	-0.26
Market shares						
Banks	18.23	1.97	5.66	-3.69	6.21	-4.24
Cooperatives	47.20	-11.02	-2.74	-8.28	-9.68	-1.34
Total (banks and cooperatives)	65.43	-9.04	2.92	-11.97	-3.46	-5.58
Outreach						
Banks' offer probability	0.35	-0.16	0	-0.16	-0.04	-0.12
Coops' offer probability	0.13	-0.06	0	-0.06	-0.03	-0.03
Expected offers per borrower	3.77	-1.83	0	-1.83	-0.50	-1.33
Welfare						
Consumer surplus	6.60	-0.59	1.46	-2.05	0.93	-1.52
Expected profit per borrower	5.25	-1.51	-0.77	-0.74	-0.36	-1.15

Notes: Column (1) reports pre-reform levels. Columns (2)–(6) report differences between the indicated scenarios. CF1 is the targeted cap as implemented (post-reform prices and offer thresholds). CF2 holds offers at their baseline levels under post-reform prices. CF3 applies the same cap level, holding offer thresholds at baseline. All simulations use the pre-reform borrower pool. Results for the synthetic lender are not reported. The posted price is the average rate across feasible borrower-lender pairs, while the transacted price is weighted by origination probability. Market shares are calculated over the full pre-reform borrower pool. Market shares are in percentage points; welfare measures are in interest-rate points.

J Thresholds under calibrated cross-sector profitability gap

The mapping in projection (14) relies on the cross-sector price difference to predict changes in the threshold λ_j. It omits the role of other determinants of relative profitability such as demand elasticities, default, and offer costs. A cap that does not induce a price wedge across sectors could still affect relative profitability if the untargeted sector has stronger fundamentals. This would lead to threshold adjustments. To assess this possibility, I calibrate the cross-sector gap in fundamentals using data from the untargeted sector. I treat the pre-reform gap as zero, so the calibrated gap measures the change in relative profitability induced by a cap that does not impose a price wedge across sectors.

I measure the mean difference in predicted default probabilities and in distance to the nearest lender branch across sectors. Default probabilities are slightly lower in the targeted sector, while distance to the nearest branch is larger. I also estimate a logit demand specification on agricultural and non-agricultural borrowers, including an interaction of price with an indicator for agricultural borrowers. As shown in Table J.1, the coefficient on the interaction is negative and statistically insignificant. If anything, agricultural demand is more elastic, which would raise acceptance more in the targeted sector. Because this difference would make agricultural lending relatively more attractive, I exclude it

from the calibrated gap.

Table J.1: Demand Estimates by Sector

	1(Borrower-lender match)			
	(1)	(2)	(3)	(4)
Price	-0.107***	-0.117***	-0.109***	-0.103***
	(0.031)	(0.014)	(0.019)	(0.018)
Price × Agriculture				-0.017
				(0.027)
Log(branches)	2.530***	2.309***	2.370***	2.368***
	(0.266)	(0.116)	(0.156)	(0.156)
Log(distance to branch)	-1.195***	-0.927***	-1.092***	-1.091***
	(0.159)	(0.211)	(0.179)	(0.178)
Observations	721,254	1,195,533	1,916,787	1,916,787
Sample	Agriculture	Non-agriculture	Both sectors	Both sectors

Notes: This table presents results from a conditional logistic regression. The dependent variable is an indicator for matched borrower-lender pairs. The sample includes loans originated in the year preceding the reform. For consistency with the structural estimation of the model, lenders with low market shares are aggregated into a synthetic lender. Columns (1) and (2) restrict the sample to agricultural and non-agricultural loans, respectively. Columns (3) and (4) pool both sectors. Column (4) includes the interaction of price with an agricultural sector indicator. All regressions include lender fixed-effects. Standard errors in parentheses are clustered at the canton level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

I define the gap in fundamentals as:

$$f_j^{gap} = \max\{20.97 \times \bar{d}_j^{gap} + |\gamma_{dist}| \times \overline{dist}_j^{gap}, 0\} \tag{J1}$$

where $\bar{d}_j^{gap}$ is the mean difference in predicted default probabilities, $\overline{dist}_j^{gap}$ is the mean difference in distance to the closest branch and $|\gamma_{dist}|$ is the absolute value of the estimated parameter associated with distance in the offer equation. I replace p_j^{gap} with f_j^{gap} in projection (14) to predict the change in thresholds.

I simulate the effects of the cap using these predicted changes in thresholds (CF3'), and compare them with the effects of the targeted cap. The results are similar to those under the pre-reform threshold scenario, as shown in Table J.2. Targeting accounts for 73.9% and 64.9% of the contraction in expected offers per borrower and lending, respectively.

Table J.2: Changes in market outcomes under calibrated threshold changes

		Policy design	
	Targeted cap (CF1 − Baseline) (1)	Cap level (CF3′ − Baseline) (2)	Targeting (CF1 − CF3′) (3)
Price			
Posted	-1.43	-1.43	0
Transacted	-1.66	-1.44	-0.22
Market shares			
Banks	1.49	5.42	-3.93
Cooperatives	-10.24	-8.50	-1.73
Total (banks and cooperatives)	-8.74	-3.08	-5.67
Outreach			
Banks′ offer probability	-0.16	-0.04	-0.12
Coops′ offer probability	-0.05	-0.02	-0.03
Expected offers per borrower	-1.76	-0.45	-1.30

Notes: This table presents the quantification of the cap level and targeting using equation (J1) to adjust thresholds (CF3′). Column (1) shows the effect of the targeted cap for reference. All simulations use the pre-reform borrower pool. Results for the synthetic lender are not reported. Interest rates and market shares are in percentage points.