Impact of the Interest Rate Cap Law in a Context of Bank Concentration and Informality: Evidence for Peru

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Abstract

Background: The interest rate cap law took effect in the Peruvian economy in May 2021, with the cap set by the Central Bank at twice the average consumption loan rate over the previous six months. **Objectives:** The purpose of this paper is to determine the impact of the interest rate cap law in the context of the Peruvian economy, which has high levels of bank concentration and informality. **Methods/Approach**: We use monthly panel data from the Superintendence of Banking, Insurance and AFP (SBS) covering Jan 2018 to Mar 2024. To identify the policy's effect, we implement a difference-in-differences (DiD) design comparing the expansion of microcredit (treatment) with corporate loans (control). In addition, models are estimated separately for each financial subsystem, including control variables such as non-performing loans (NPLs) and the share of each credit segment in the institution's portfolio. **Results**: The reported estimates for total credit, average credit per borrower, and average lending rates were statistically insignificant across subsystems. Estimates for the number of borrowers were positive for municipal savings institutions and negative for financial companies. The parallel-trends test rejected the null in both cases. **Conclusions:** The reported estimates provide limited evidence on the effects of the interest rate cap. Bank concentration and informality provide context for interpreting the results.

Keywords: Interest rate cap; bank concentration; informality.

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Introduction

One of the most controversial policies in credit markets is the imposition of interest rate caps (Cubillos-Rocha et al., 2021; Toro, 1987). Although market-based interest rate setting became common in emerging economies after the liberalization of the financial system in the 1990s (Fry, 1982; Nellor, 1985; Maimbo & Henríquez Gallegos, 2014), policies to restrict interest rates are again being adopted. A 2018 World Bank study documented restrictions on lending rates in at least 76 countries (Ferrari et al., 2018). Calice et al. (2020) analyze the presence of interest rate controls in 108 countries and report that 58% of them impose some form of restriction on lending rates. Among the Latin American countries for which data are available, 14 maintain an active policy of limiting interest rates (see Table A1). The high interest rates charged by financial institutions have drawn regulatory authorities' attention worldwide (Christen et al., 2012). These high rates are potentially perceived as obstacles to increased investment, economic growth, and especially financial inclusion (Demetriades & Luintel, 2001; Auerbach & Siddiki, 2004). Theoretically, when maximum or minimum caps are set, market outcomes tend to be adverse, often leading to black markets and imbalances in supply and demand (Sowell & El-Hage, 2013; Nicholson, 2005).

Interest rate caps established by regulations can have detrimental effects on financial inclusion and the efficiency of the financial system (Cubillos-Rocha et al., 2021; Delvasto, 2008). According to Alper et al. (2020), these caps are price controls that encourage informal credit and exclude a large number of individuals and businesses from the formal financial system. Furthermore, they could hinder the inclusion of various low-income population sectors into the financial system. These sectors, due to their lack of credit history or collateral (Maimbo & Henríquez Gallegos, 2014), often start with higher interest rates, which significantly decrease as they demonstrate their repayment capacity and build a credit history.

International experience indicates that imposing interest rate limits has negatively affected financial inclusion by restricting access to credit, especially for low-income individuals and micro and small enterprises (MYPEs). In Chile, Madeira (2019) analyzed the gradual reduction of the maximum interest rate for consumer loans and found an 8.7% decrease in credit access. Similarly, the Superintendencia de Bancos e Instituciones Financieras de Chile (2017) estimated that by the third year of implementation of this measure, between 151,000 and 227,000 clients would have lost access to formal credit. Likewise, in Bolivia, imposing interest rate caps reduced the microcredit and SME loan portfolio by 26.1% between 2011 and 2018 (Roa et al., 2020).

However, the effects of interest rate caps on credit are not yet clear a priori, as they can decrease the supply of credit by excluding high-risk borrowers from bank credit markets, or they can increase the supply of credit by limiting the market power of banks (Burga et al., 2022). Developing economies are characterized by highly concentrated banking sectors and significant informational frictions; therefore, both the market power of banks and the risk of firms play a substantial role in credit markets. This situation can be further aggravated by labor market structure, as some countries have higher informality rates than others. With this, informational frictions become substantially more significant.

In December 2020, Peru's Congress initially approved Law No. 31143 ("Consumer Protection Law against Usury by Financial Intermediaries"). The law was published on March 18, 2021. The central bank's implementing circular set the ceilings at twice the six-month average of the relevant consumer lending rates and phased them in by institution type (Banco Central de Reserva del Perú [BCRP], 2021). Peru has one of the most concentrated banking systems in the region. As of March 2024, the four largest

banks accounted for 73.31% of assets, 71.90% of loans, and 73.28% of deposits among the 49 multiple-operation financial institutions, excluding Banco de la Nación and Agrobanco (Superintendencia de Banca, Seguros y AFP [SBS], 2024; see Table 1). In addition, the country has high levels of informality, which reached 73.9% in 2023, based on our calculation using ENAHO, module 500. Together, these factors could represent one of the greatest challenges to financial inclusion, beyond what could become the cap on interest rates.

This research aims to quantify the effects caused by interest rate caps on credit usage, to assess whether interest rate caps are indeed responsible for financial exclusion. The importance of our study lies in determining whether the adverse effects of interest rate caps on consumer, micro, and small business loans hold in the Peruvian context, given Peru's high banking concentration and high level of informality. This research hypothesizes that, in Peru, interest rate caps would have little or no significant effect on financial exclusion due to structural factors such as high bank concentration and informality. To this end, this study uses monthly panel data from the SBS for Peru covering January 2018 to March 2024 and applies a difference-in-differences (DiD) estimator, using May 2021 as the intervention month in the reported estimates.

The study extends prior evidence on Peru's 2021 reform (Burga et al., 2022) by comparing microenterprise and corporate lending across five financial subsystems and examining four credit outcomes.

The remainder of this paper is organized as follows. Section 2 presents the study framework. Section 3 describes the data. Section 4 sets out the empirical strategy. Section 5 reports and discusses the results. Section 6 concludes.

Study framework

The expansion of financial inclusion depends on how banks balance the pursuit of profitability with the probability of borrower default. In imperfect information models, an increase in the interest rate initially raises financial revenues but also increases default risk, as the portfolio shifts toward applicants with riskier profiles (Stiglitz & Weiss, 1981; Freixas & Rochet, 2008). Consequently, the bank's expected return first rises and then declines, resulting in an optimal interest rate that maximizes profitability (Stiglitz & Weiss, 1981).

Beyond this point, banks have no incentive to raise rates further, as doing so reduces their expected profits. Accordingly, they adjust on the quantity margin through credit rationing—limiting the number of loans granted instead of raising the rate (Stiglitz & Weiss, 1981; Freixas & Rochet, 2008; Demetriades & Luintel, 2001). In this context, a regulatory ceiling set above the optimal rate does not alter the equilibrium, since profit-maximizing banks choose not to exceed that threshold. The rationing that constrains the expansion of financial inclusion does not arise from the cap itself, but from the rational decision not to lend to borrowers whose high probability of default would reduce portfolio profitability (Chauvet & Jacolin, 2017). During episodes of financial stress, policy shocks and restructurings may exacerbate quantity-based contractions (Crotty & Lee, 2002).

From this perspective, an interest rate cap does not change the financial system's capacity to expand inclusion when the regulatory limit is above the rate that maximizes profitability. Banks, aiming to maximize profits, avoid setting higher rates, as including borrowers with high default risk reduces their expected return. In this case, rationing stems not from the cap but from the market equilibrium itself, where the goals of profitability and risk control do not align with expanding credit toward borrowers

with weaker credit profiles (Stiglitz & Weiss, 1981; Freixas & Rochet, 2008; Demetriades & Luintel, 2001).

Empirical evidence falls into three categories. The first includes cases where the ceiling was effectively restrictive, leading to declines in access and loan amounts. In Chile, the reduction of the maximum rate for consumer loans was applied within a relatively competitive financial system and a lower-than-average level of labor informality, which facilitated price transmission and visible adjustment among vulnerable segments, with stronger effects among low-income households (Madeira, 2019; Cuesta & Sepúlveda, 2021). Regional studies on inclusion and rate caps discuss similar mechanisms of household sensitivity and segmentation (Samreth et al., 2021). In Bolivia, by contrast, regulation operated in an environment of limited effective competition and high informality, where the main adjustment margin was quantity, leading to a contraction in microcredit and financing for small and medium-sized enterprises (Roa et al., 2020).

The second group covers contexts where the ceiling was set above effective lending rates, and the direct effects on access were small or null. In these cases, adjustments occurred outside the nominal interest rate, through higher commissions or other charges, consistent with a non-restrictive ceiling relative to the profitability-maximizing rate (Christen et al., 2012; Alper et al., 2020). Moreover, recent evidence highlights heterogeneity in transmission and non-price responses, including risk reallocation and margin changes depending on competition and regulation (Liu et al., 2023; Qadan et al., 2023; Samreth et al., 2021).

The third group examines liberalization processes or broader regulatory margins. In such cases, studies report increases in new loans or greater financial deepening, though with high heterogeneity depending on market structure and the degree of informality (Cubillos-Rocha et al., 2021; Casu et al., 2024; Nguyen & Ho, 2024). Banking industrial organization helps interpret these patterns: in concentrated systems, wider spreads and a lower willingness to compete for marginal segments are observed. The Monti–Klein framework formalizes price-setting under market power, while the Demsetz efficiency hypothesis suggests that part of concentration may reflect productivity (Bain, 1951; Demsetz, 1973; Freixas & Rochet, 2008; Matthews & Thompson, 2014). Concentration and competitive inertia interact with regulation and the business cycle, affecting screening and monitoring costs as well as credit stability (Calice & Leonida, 2018; Arestis et al., 2001).

Informality explains why adjustment tends to occur through quantities. The lack of traceable income and assets, limited collateral availability, and high monitoring costs increase the required risk premium. If the rate necessary to cover this risk exceeds the rate that maximizes profitability, the efficient response is to ration credit rather than continue raising rates. This mechanism appears in developing economies and is associated with collateral constraints, enforcement problems, and high selection costs (Evans & Jovanovic, 1989; Williamson, 1987; Banerjee & Duflo, 2005; Gatti & Honorati, 2008; Loayza, 2018; Ayyagari et al., 2018; Didier & Schmukler, 2014). In Sub-Saharan Africa, where informality is widespread, formal banks concentrate their portfolios among traceable clients, reproducing the exclusion of riskier segments even without strict interest rate controls (Chauvet & Jacolin, 2017). A useful distinction is between "exit informality" and "exclusion informality," depending on their causes and policy responses (Oviedo et al., 2009). At the margin, public debt shocks may compete for bank resources and increase the cost of private financing ("crowding out"), reinforcing rationing when the banking space is limited (Liu et al., 2023). From a firm-level perspective, persistent informality reflects low productivity and limited transition

toward formality; its reduction is slow and depends on development and entrepreneurial human capital (La Porta & Shleifer, 2014).

The Colombian case provides a useful contrast, combining a historical experience of control and liberalization with a relatively more competitive banking sector. Evidence points to limited impacts on aggregate credit supply and a transition toward more flexible ceiling schemes, reinforcing the idea that the competitive context determines whether policy transmission operates through price or quantity (Toro, 1987). Overall, the magnitude of the effects in the region appears to depend on the degree of banking competition and structural informality, which shape the dominant adjustment margin among intermediaries (see also Liu et al., 2023; Qadan et al., 2023).

This framework helps interpret the Peruvian case. Congress approved the law in December 2020, and Law No. 31143 was published in March 2021 (Diario Oficial El Peruano, 2021). The cap applied to eligible new disbursements from May 10 for banks, June 1 for municipal savings institutions, and July 1 for other financial institutions (BCRP, 2021). The measure was justified as a consumer protection mechanism intended to increase credit access. However, the Instituto Peruano de Economía (IPE, 2023) warned that interest rate caps could restrict access to formal credit and make informal lending more attractive. The Peruvian financial system is highly concentrated: the four main banks (BCP, BBVA, Scotiabank, and Interbank) accounted for 73.31% of assets, 71.90% of loans, and 73.28% of deposits among the 49 multiple-operation financial institutions as of March 2024 (SBS, 2024; see Table 1), granting them the capacity to set conditions with limited rival pressure and favoring strict selection criteria for formal clients with verifiable records.

Additionally, labor informality reached 73.9% in 2023, reducing the likelihood of financial inclusion and heightening perceived risk for unregistered activities. This figure far exceeds the regional average of 48% in the same year (Organización Internacional del Trabajo [OIT], 2023). Household data show that informal employment reduces the probability of financial inclusion and increases exclusion, consistent with informational frictions and adverse selection (Aurazo & Gasmi, 2023). At the macro level, productive bank concentration in Latin America has been linked to credit deceleration and greater banking selectivity, reinforcing the rationing channel (Barriga, 2020). Under these conditions, it is plausible that effective lending rates remained below the regulatory limit and that risk filters continued to dominate allocation decisions, meaning that a ceiling would not alter the price equilibrium and that potential adjustments would manifest in quantities or unregulated charges (Alper et al., 2020).

Table 1
Structure of Multiple-Operation Financial Institutions, March 2024

	Number of Institutions	Assets Million Soles	%	Loans Million Soles	%	Deposits Million Soles	%
Multiple-operation financial institutions	49	590,592	100.00	400,754	100.00	388,555	100.00
Banks	17	525,578	88.99	345,786	86.28	346,892	89.28

Big four banks	4	432,961	73.31	288,155	71.90	284,722	73.28
Other banks	9	82,452	13.96	49,983	12.47	55,228	14.21
Specialized in consumption	4	10,165	1.72	7,648	1.91	6,942	1.79
Non-Banks	32	65,014	11.01	54,968	13.72	41,663	10.72
Financial companies	9	17,252	2.92	14,398	3.59	8,633	2.22
Municipal savings and loans	12	42,793	7.25	36,415	9.09	31,854	8.20
Rural savings and loans	5	1,736	0.29	1,404	0.35	1,176	0.30
Loan companies	6	3,234	0.55	2,752	0.69	-	-

Note: Percentage shares are calculated relative to the 49 multiple-operation financial institutions, excluding Banco de la Nación and Agrobanco. The "Big four banks" (BCP, BBVA, Scotiabank, and Interbank) are identified by total assets, while banks "Specialized in consumption" are identified using the SBS grouping criterion of having more than 50% of their total direct loan portfolios concentrated in consumer credit. The remaining nine banks are grouped under "Other banks". Banking figures include BCP's foreign branches.
Source: Authors' calculations based on financial statements published by the Superintendencia de Banca, Seguros y AFP (SBS, 2024), March 2024 (files B-2201, B-3101, C-1101, C-2101, and C-4103).

This framework provides the necessary elements to empirically test the policy's implications. If the BCRP ceiling were set above the profitability-maximizing rate, the cap should not affect access or average loan amounts. In a context of high bank concentration and informality, the dominant margin of adjustment would be quantity; therefore, no generalized declines in total credit or number of debtors are expected after the reform. If micro-level marginal reactions existed, they would likely appear as portfolio reallocations or adjustments in non-interest charges, rather than aggregate credit reductions (see also Liu et al., 2023; Qadan et al., 2023; Samreth et al., 2021).

Data

The data used in this study come from the Superintendencia de Banca, Seguros y AFP (SBS) and are disaggregated by subsystem, type of credit, and financial institution. The dataset covers the period from January 2018 to March 2024 and is structured as a monthly panel at the level of institution and credit type. It includes all institutions supervised by the SBS across the five subsystems, namely Commercial Banks, Municipal Savings and Loans, Rural Savings and Loans, Financial Companies, and Loan Companies. This ensures representativeness of the Peruvian financial system. The analysis focuses on key outcomes such as Log Credit Amount, Log Average Credit Amount, Log Number of Borrowers, and the Active Lending Interest Rate. The three credit outcomes measured as amounts or borrower counts are log-transformed. The lending rate is reported in levels.

The empirical strategy examines the impact of interest rate caps on credit usage, with microenterprise loans defined as the treatment group and corporate loans as the control group. This choice is valid because corporate lending, like loans to large and

medium-sized enterprises, was not subject to the legislation imposing interest rate caps. Table 2 defines the variables. Table A2 summarizes the credit classifications.

Table 2
Summary of Variables Used

Variable Name	Description
Log Credit Amount	Natural logarithm of the outstanding loan balance (in thousands of PEN) for each institution and credit segment. This is the main outcome variable used to measure the effect of the interest rate cap.
Log Average Credit Amount	Natural logarithm of the average loan amount, computed as the ratio of total credit to the number of borrowers. Used to assess changes in the average credit supplied per client.
Log Debtors	Natural logarithm of the number of borrowers in each institution and credit segment. Reflects the level of access to credit
Average Active Interest Rate	Average active interest rate applied to loans.
Non-Performing Loans (NPLs)	Share of direct loans that are overdue or under judicial collection (% of gross loans). Indicates credit portfolio quality.
Participation	Share of each credit segment in the institution's total loan portfolio. Controls for changes in credit composition.
Charge-offs	Share of loans written off in the period, expressed as a percentage of gross loans. Captures losses recognized by institutions and controls for changes in portfolio risk.
Post-cap Period	Dummy equal to 1 for months after May 2021, when the interest rate cap was implemented.
Treatment × Post-cap	Interaction between the treatment and post-cap dummies. Captures the average treatment effect of the interest rate cap (DiD estimation).
Treatment Group	Dummy equal to 1 for microenterprise loans (affected by the interest rate cap) and 0 for corporate loans (unaffected)
Credit Type	Classification of loans into Microenterprises, Small Enterprises, Medium Enterprises, Large Enterprises and Consumer Loans (see Table A2 for detailed definitions)

Source: Authors' Work

Empirical Strategy

To measure the impact of loan rate caps on financial exclusion, a difference-in-differences (DiD) estimator is applied as the identification strategy. This method is appropriate for identifying the policy effects due to the targeted characteristics of the diverse financial institutions and the relative exogeneity of the policy itself. Following the methodology of Cubillos-Rocha et al. (2021) and Roa et al. (2020), we use corporate loans as the control group and microenterprise loans as the treatment group. Identification requires parallel trends in the untreated potential outcomes. Pre-intervention trends help diagnose this assumption (Angrist & Pischke, 2009). We also incorporate covariates such as the proportion of the loan portfolio, NPLs, and charge-offs by type of loan. NPLs (SBS criteria) / Direct Loans (%) refers to

the percentage that represents the share of direct loans that are either overdue or in judicial collection. According to SBS criteria, the classification of a loan as overdue depends on the number of days in arrears, which varies by loan type: for corporate loans and loans to large and medium-sized enterprises, the threshold is more than 15 days overdue; for loans to small and micro-enterprises, it is 30 days; for mortgage and consumer loans, a payment is considered overdue after 30 days, while the entire loan balance is classified as overdue after 90 days of NPL. Charge-offs are retained because they capture realized portfolio losses that can directly affect lending capacity and risk appetite, making them a relevant control for time-varying portfolio stress not absorbed by fixed effects. The empirical model is formulated as follows:

$$Dependent\ Variable_{it} = \alpha_0 + \alpha_1 treat_i * post_t + \alpha_2 Participation_{it} + \alpha_3 NPL_{it} + \alpha_4 ChargeOffs_{it} + \lambda_i + \mu_i + \varepsilon_{it} \quad (1)$$

Four models were estimated using different dependent variables, including: the logarithm of the total loan amount, the logarithm of the average loan amount, the logarithm of the number of borrowers, and the average lending rate. The subindices i and t indicate the type of credit by financial institution and month, respectively. The interaction term $treat_i$ x $post_t$, which includes the treatment variable $treat_i$ and the treatment period variable $post_t$, is the central explanatory variable in this study. The coefficient a_1 of this term measures the actual effect of the policy on each dependent variable. NPLs and participation by type of credit are included as control variables. In addition, fixed effects by type of credit per financial institution λ_i, and temporal fixed effects μ_t, control for unobservable characteristics that remain constant over time. This design allows us to isolate the effect of the intervention by comparing the differences in outcomes between the groups before and after the interest rate cap was applied.

Parallel trend test

To ensure internal validity, we performed several robustness checks to assess the parallel-trends hypothesis. This hypothesis requires that, in the absence of treatment and conditional on a relevant history, the difference between treated and control groups would have remained constant during the pre-treatment period:

$$Dependent\ Variable_{it} = \beta_o + \beta_1 \sum_{t=2018m1}^{2021m4} treat_i * time_t + \beta_2 Participation_{it} + \beta_3 NPL_{it} + \beta_4 ChargeOffs_{it} + \mu_t + \varepsilon_{it} \quad (2)$$

where the interaction term, $treat_i \times time_t$, was introduced as a new explanatory variable, replacing the baseline regression model's time and policy interaction term t$treat_i \times post_t$. Here, timei represents the time variable before and after policy implementation, and β1 denotes the DiD coefficient used to scrutinize parallel trends. This coefficient serves to measure the dynamic effects of the interest rate cap. In addition, charge-offs are included as a control variable to capture contemporaneous portfolio losses that may affect lending behavior beyond what is absorbed by fixed effects

Endogeneity problem

However, the model may face an endogeneity problem due to the potential bidirectional relationship between the loan amount and NPLs. On the one hand, as larger loans are granted, the probability of a portion of these loans becoming NPLs increases. On the other hand, NPLs can also influence the loan amount, as financial institutions adjust the volume of loans they issue based on perceived risk. This simultaneous relationship can lead to biased estimates if not properly addressed.

To correct this endogeneity problem, a two-stage instrumental variables (IV) model is estimated, where the variable NPL is instrumented using its own lags. The lags of the NPLs are considered valid instruments under two conditions: relevance and exogeneity. Relevance is satisfied because NPLs tend to be persistent over time, implying that the lags of the variable are correlated with the current level of the NPLs. Exogeneity is fulfilled by assuming that the lags of the NPL are not correlated with the contemporaneous error term, thus eliminating the possibility that these instruments directly affect the dependent variable (loan amount) through channels other than NPLs.

First stage equation

$$NPL_{it} = \gamma_o + \gamma_1 NPL_{it,t-1} + \gamma_2 NPL_{it,t-2} + \gamma_3 NPL_{it,t-3} + \gamma_1 treat_i * post_t + \lambda_i + \mu_t + \varepsilon_{it} \quad (3)$$

Second stage equation

$$Dependent\ Variable_{it} = \theta_0 + \theta_1 treat_i * post_t + \theta_2 Participation_{it} + \theta_3 NPL_{it} + \theta_4 ChargeOffs_{it} + \lambda_i + \mu_i + v_{it} \quad (4)$$

Equation (3) represents the first stage of an instrumental variables (IV) model, where NPL_{it} is estimated as a function of its lags and an interaction between a treatment variable and the post-treatment period. In this model, the lags ($NPL_{i,t-1}$, $NPL_{i,t-2}$, $NPL_{i,t-3}$) capture persistence over time, allowing control for inertia in NPL, while the interaction term ($treat_i$ x $post_t$) captures the impact of the treatment after its implementation. Additionally, entity-fixed effects (λ_i) and time-fixed effects (μ_t) are included to account for unobserved heterogeneity specific to each entity and general time trends. In the second stage, the instrumented value of NPL obtained from the first stage is included as a regressor, and the coefficient θ_3 measures its effect on the loan amount.

Results and Discussion

We estimate four difference-in-differences specifications separately for each financial subsystem using administrative SBS data from January 2018 to March 2024. The policy effect is identified by the interaction $Treat_i \times Post_t$, where treated segments are those subject to the ceiling and the post period begins in May 2021, the common cutoff used in the reported specifications. Before reporting the coefficients of interest, we verify internal validity with event-study diagnostics and parallel-trends tests (Table 3). In most specifications the pre-treatment coefficients are jointly insignificant, which is consistent with parallel trends. A placebo event study centered in May 2020 shows no

spurious dynamics around the pseudo-shock. Complete event-study figures and placebo graphs are reported in the Appendix.

Table 3
Parallel Trend Test

Model 1				
Y: Log credit amount	Diff-Diff		Parallel Trend Test	
Subsystem	d.ftep	F	P>F	Result
Commercial Banks	–0.263	0.460	0.504	✓
Municipal saving and loans	0.187	0.040	0.841	✓
Rural saving and loans	–0.146	1.760	0.212	✓
Financial companies	–0.108	0.070	0.800	✓
Loan companies	0.841	1.450	0.251	✓
Model 2				
Y: Log average credit amount	Diff-Diff		Parallel Trend Test	
Subsystem	d.ftep	F	P>F	Result
Commercial Banks	–0.369	1.650	0.210	✓
Municipal saving and loans	–0.092	0.010	0.920	✓
Rural saving and loans	0.102	0.530	0.480	✓
Financial companies	0.250	0.400	0.537	✓
Loan companies	0.657	1.020	0.331	✓
Model 3				
Y: Log debtors	Diff-Diff		Parallel Trend Test	
Subsystem	d.ftep	F	P>F	Result
Commercial Banks	0.105	0.280	0.603	✓
Municipal saving and loans	0.279	9.070	0.006	X
Rural saving and loans	–0.248	1.310	0.276	✓
Financial companies	–0.358	4.440	0.049	X
Loan companies	0.185	1.500	0.243	✓
Model 4				
Y: Average active interest rate	Diff-Diff		Parallel Trend Test	
Subsystem	d.ftep	F	P>F	Result
Commercial Banks	–0.002	0.020	0.877	✓
Municipal saving and loans	–0.010	1.730	0.201	✓
Rural saving and loans	–0.047	1.420	0.258	✓
Financial companies	–0.041	1.830	0.192	✓
Loan companies	–0.007	0.760	0.399	✓

Note: Ho → Linear trends are parallel.
Source: Authors' Work

The reported interaction estimates are statistically insignificant across all five subsystems for the log of the total credit amount (Table 4), the log of average credit

per borrower (Table 5), and the average lending rate (Table 7). Table 6 reports two statistically significant estimates for the log of the number of borrowers.$\text{Treat}_i \times \text{Post}_t$

For the log of the number of borrowers, the reported interaction coefficients are positive for municipal savings institutions (0.279) and negative for financial companies (−0.358). Table 3 rejects parallel trends in both cases ($p = 0.006$ and $p = 0.049$, respectively), limiting causal interpretation of these estimates.

Covariates behave in line with expectations. NPLs are negatively and significantly associated with credit amounts in several subsystems. To address potential endogeneity in NPLs, we re-estimate the models with instrumental variables, using lags of NPLs as instruments. The first stage is relevant given the persistence of delinquency, and the second stage preserves the qualitative conclusions: the ceiling has no average effect on Log Credit Amount. We also include Write-offs (Castigos) as a parsimonious loss-control variable; its coefficient varies by outcome and subsystem.

Placed against prior evidence, these findings align with settings in which ceilings are not strongly restrictive, and lenders adjust on non-price margins. In Peru, Burga et al. (2022) study the 2021 ceiling and show that lending rates decline while aggregate lending remains broadly unchanged because banks reallocate toward safer borrowers in concentrated markets and the share of NPLs falls. By contrast, Roa et al. (2020) find that a tighter cap in Bolivia reduced the outstanding microcredit and SME loan portfolios of MFIs by 26.1 percent relative to the corporate loan portfolio of banks, which is consistent with rationing when the ceiling imposes an effective restriction on high-cost intermediaries. Symmetrically, Cubillos-Rocha et al. (2021) report that loosening the microcredit usury limit in Colombia expanded microcredit relative to corporate credit. Taken together, these studies indicate that the sign and magnitude of effects depend on how restrictive the ceiling is, rather than on its mere existence.

The studies differ in the credit segments, outcomes, and institutions analyzed. The Peruvian estimates reported here use subsystem-level credit stocks and averages, which may respond differently from new lending to a cap that applies to new disbursements.

Two structural features of the Peruvian market help rationalize the small average effects. First, the ceiling is formula-based and recalculated semiannually at twice the six-month average interest rate on new consumer loans with a short lag. Second, high bank concentration and informational frictions shape transmission. Lenders adjust loan size, maturities, collateral, and screening rather than posted rates or aggregate volumes. Moreover, informality provides outside options that weaken the pass-through from posted rates to aggregate lending. When a ceiling is not highly restrictive, formal lenders can preserve portfolio risk by tightening screening and shifting toward safer, smaller-ticket clients, while higher-risk applicants self-select into informal credit or remain unserved. Evidence for Peru documents a non-linear competition–risk link consistent with these margins of adjustment. Using bank–region–time data, Pozo and Rojas (2023) find an inverted U-shaped relationship between competition and risk-taking. In addition, using cross-country panel evidence and an occupational-choice framework, Sirisankanan (2017) shows that the effect of financial development on informality is ambiguous ex ante and state-dependent, with the empirical association turning less negative as income rises. In a market like Peru's, with high informality and concentration, a moving-average ceiling is therefore more likely to reshape who is served and on what terms than to induce large changes in aggregate volumes.

Regional evidence on market structure and inclusion further supports this interpretation. For Latin America, Barriga (2020) links productive concentration with slower expansion of credit supply, which helps explain muted average effects when

ceilings are not highly restrictive. In Peru, a department-level panel based on Sarma's index shows that concentration, measured by HHI, is the main determinant constraining inclusion, while technology raises inclusion but with diminishing marginal effects where deployment and user capabilities are limited (Boitano & Abanto, 2020). At a broader policy level, international synthesis work characterizes interest-rate caps as a blunt instrument with heterogeneous effects across contexts and stresses that competition policy, consumer protection, disclosure, and credit-information systems are central to sustained reductions in borrowing costs and broader inclusion (Maimbo & Henríquez Gallegos, 2014). Cross-country evidence also suggests a non-monotonic relationship between concentration and stability that operates through profitability, pricing, diversification, and monitoring. At low concentration, more concentration can improve stability, whereas at high concentration it can raise fragility, with offsetting channels at intermediate levels (Calice & Leonida, 2018). Evidence from China documents crowding out of private borrowing by government debt (Bai et al., 2024). Complementary work on financial structure and inclusion underscores that competition gains materialize more strongly when inclusion is already high, while concentrated systems tend to channel credit toward better-connected firms (Chauvet & Jacolin, 2017).

Table 4

Difference-in-Differences Estimates for Total Credit Amount in Logarithms

Main Variables	Commercial Banks	Municipal saving and loans	Rural saving and loans	Financial Companies	Loan Companies
NPL	-0.040*** (0.009)	-0.018 (0.013)	-0.000*** (0.000)	-0.005 (0.036)	-0.157*** (0.016)
Participation	0.134** (0.054)	0.212*** (0.053)	0.068*** (0.005)	0.076*** (0.021)	-0.060 (0.036)
Charge-offs	-0.000 (0.000)	0.000 (0.000)	0.000*** (0.000)	0.000 (0.000)	0.000 (0.000)
Treat Ptreat	-0.263 (0.389)	0.187 (0.923)	-0.146 (0.110)	-0.108 (0.421)	**0.841 (0.700)**
Number of obs	2160	1728	820	1406	878
Control groups	15	12	6	10	7
Treatment groups	15	12	6	10	7
Fixed Effects	✓	✓	✓	✓	✓

*Note. Standard errors are in parentheses. *p < .10, **p < .05, ***p < .01; Source: Authors' Work*

Table 5

Difference-in-Differences Estimates for Average Credit per Borrower in Logarithms

Main Variables	Commercial Banks	Municipal saving and loans	Rural saving and loans	Financial Companies	Loan Companies
NPL	-0.049*** (0.005)	-0.023* (0.013)	-0.000*** (0.000)	0.026* (0.015)	-0.024 (0.014)
Participation	0.046** (0.021)	0.186*** (0.051)	0.065*** (0.005)	0.071*** (0.023)	**0.0**35 (0.030)
Charge-offs	-0.000 (0.000)	0.000 (0.000)	0.000*** (0.000)	-0.000** (0.000)	-0.000 (0.000)

Treat Ptreat	-0.369 (0.287)	-0.092 (0.902)	0.102 (0.139)	0.250 (0.397)	0.657 (0.653)
Number of obs	2160	1728	820	1406	878
Control groups	15	12	6	10	7
Treatment groups	15	12	6	10	7
Fixed Effects	✓	✓	✓	✓	✓

*Note. Standard errors are in parentheses. *p < .10, **p < .05, ***p < .01; Source: Authors' Work*

Table 6

Difference-in-Differences Estimates for Number of Borrowers in Logarithms

Main Variables	Commercial Banks	Municipal saving and loans	Rural saving and loans	Financial Companies	Loan Companies
NPL	0.009* (0.005)	0.006 (0.004)	0.000 (0.000)	-0.032 (0.026)	-0.133*** (0.016)
Participation	0.088** (0.044)	0.026*** (0.003)	0.003*** (0.001)	0.005* (0.002)	-0.095*** (0.021)
Charge-offs	-0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)	-0.000** (0.000)
Treat Ptreat	0.105 (0.200)	0.279*** (0.093)	-0.248 (0.216)	-0.358** (0.170)	0.185 (0.151)
Number of obs	2160	1728	820	1406	878
Control groups	15	12	6	10	7
Treatment groups	15	12	6	10	7
Fixed Effects	✓	✓	✓	✓	✓

*Note. Standard errors are in parentheses. *p < .10, **p < .05, ***p < .01; Source: Authors' Work*

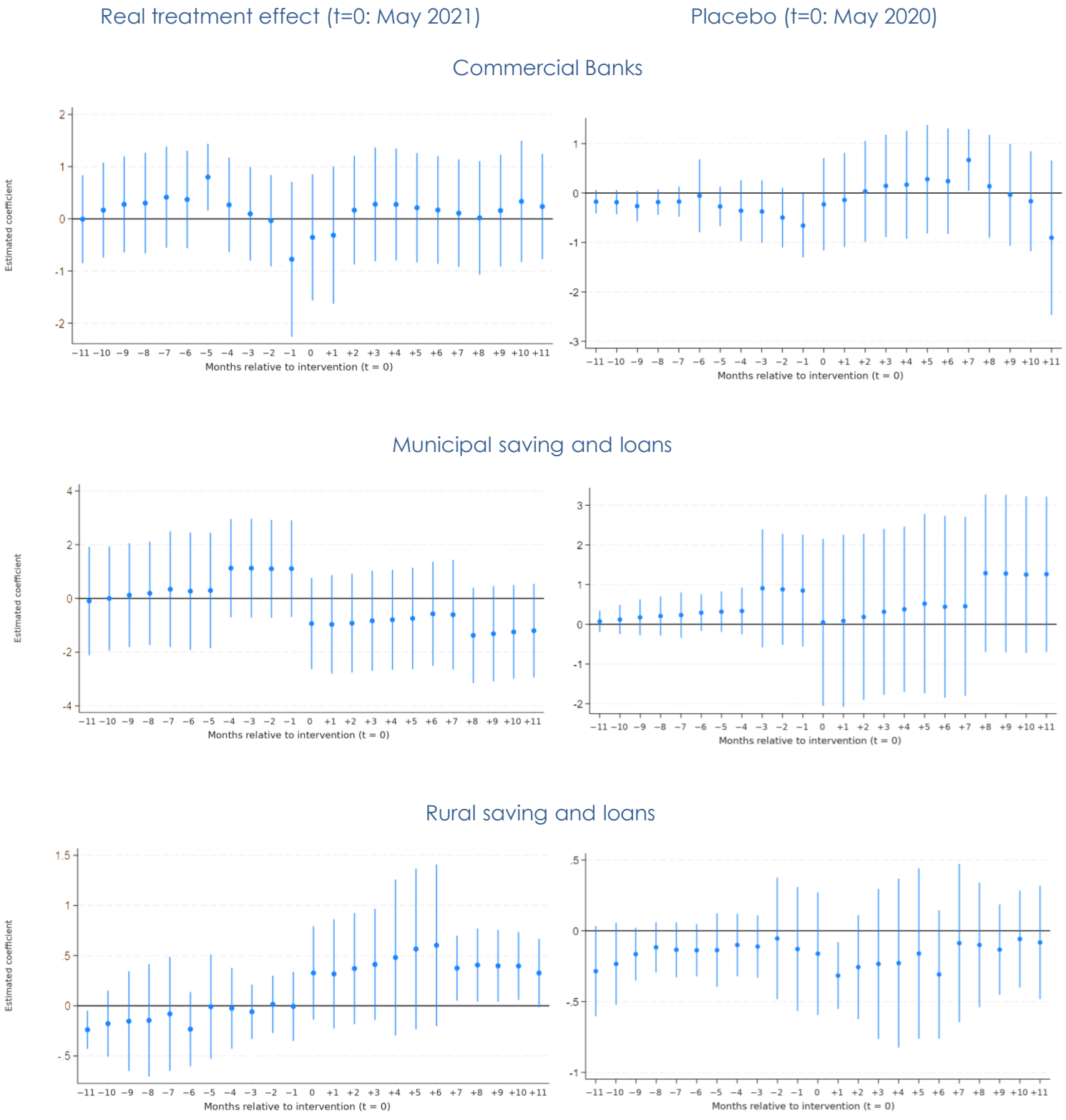
Table 7

Difference-in-Differences Estimates for Average Lending Rate

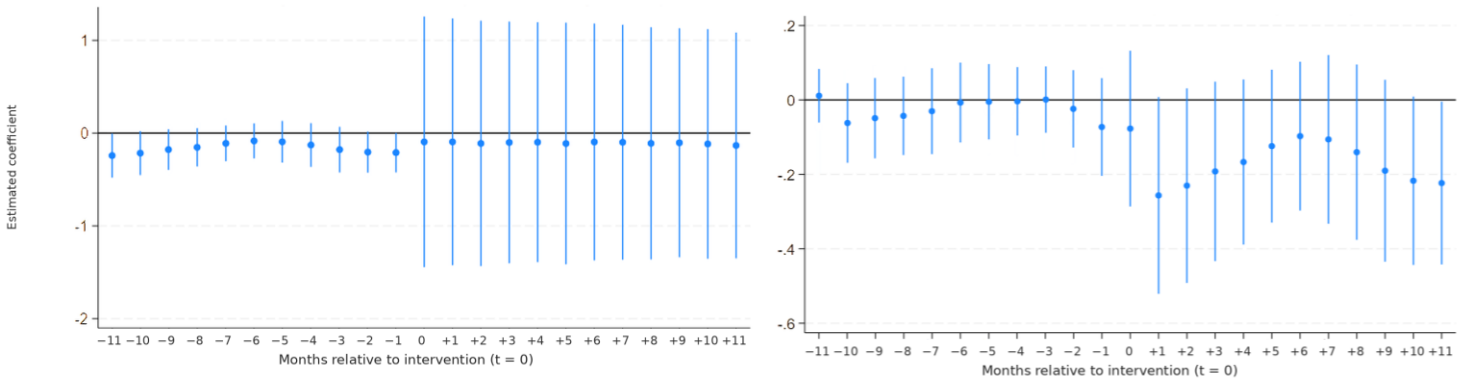
Main Variables	Commercial Banks	Municipal saving and loans	Rural saving and loans	Financial Companies	Loan Companies
NPL	-0.000* (0.000)	0.002** (0.001)	0.000*** (0.000)	-0.000 (0.000)	- 0.006*** (0.001)
Participation	0.004 (0.004)	0.000*** (0.000)	0.000*** (0.000)	0.000 (0.000)	-0.002 (0.002)
Charge-offs	0.000* (0.000)	-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)	0.000 (0.000)
Treat Ptreat	-0.002 (0.012)	-0.010 (0.008)	-0.047 (0.039)	-0.041 (0.030)	-0.007 (0.008)
Number of obs	2160	1728	820	1406	878
Control groups	15	12	6	10	7
Treatment groups	15	12	6	10	7
Fixed Effects	✓	✓	✓	✓	✓

*Note. Standard errors are in parentheses. *p < .10, **p < .05, ***p < .01; Source: Authors' Work*

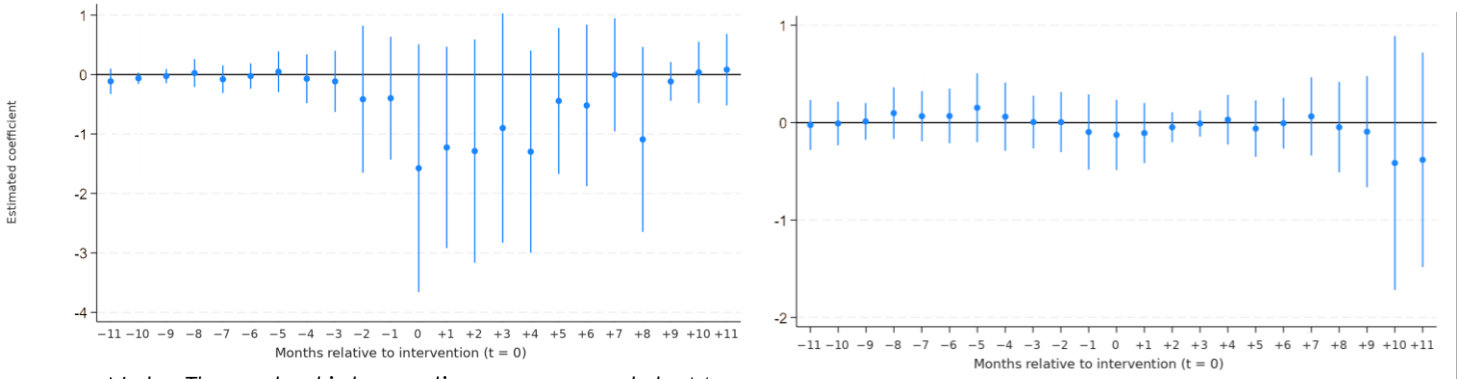
Figure 1
Event-Study Estimates of Log Total Credit Amount: Real and Placebo Shocks by Subsystem



Financial Companies



Loan Companies



Note: The actual intervention corresponds to May 2021, while the placebo intervention corresponds to May 2020 (t = 0). The horizontal axis shows months relative to each intervention, and the vertical axis reports estimated coefficients. The reference category comprises observations outside the displayed event window ($t \leq -12$ and $t \geq 12$), conditional on the aggregate post-treatment indicator included in the original specification. The period immediately preceding the intervention ($t = -1$) is estimated rather than omitted. Dots represent estimated coefficients, and vertical lines indicate 95% confidence intervals. Standard errors are clustered at the panel-unit level.
Source: Authors' Work

Conclusions

This paper evaluates the impact of Peru's 2021 interest rate cap on credit outcomes in a market with high bank concentration and widespread informality. Using administrative SBS data from January 2018 to March 2024, we estimate difference-in-differences models separately for each financial subsystem and assess pre-intervention trends. The reported estimates for total credit, average credit per borrower, and lending rates are statistically insignificant across subsystems. The two statistically significant estimates for the number of borrowers occur in subsystems where the parallel-trends test rejects the null.

The aggregate outcomes can reflect offsetting changes in lending across borrowers. Concentration and informality are plausible mechanisms that warrant direct analysis.

The policy message is straightforward. If the objective is to lower borrowing costs and expand meaningful access, measures that target structural frictions such as bank concentration, information asymmetries, and the persistence of informality are likely

to be more effective than interest rate caps. Complementary actions in competition policy, consumer protection, credit information, and digital capabilities can improve risk assessment and reduce non-price barriers to inclusion.

Two limitations point to next steps. First, our main outcomes are subsystem-level stocks and averages, so borrower-level microdata and loan-flow measures would allow sharper tests of reallocation channels. Second, aggregate estimates may mask regional and segment heterogeneity. Future work could exploit department-level variation, measure concentration with HHI, and include a financial inclusion index such as the index developed by Sarma (2008) to study how interest rate caps interact with local market structure and informality.

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Appendix

Table A1
Interest Rate Caps and Financial Inclusion in Latin America

Country	Interest Rate Caps	Comment	Financial Inclusion (15)
Brasil	Yes	From January 2024, total interest and financial charges on revolving or installment credit-card balances cannot exceed 100% of the original unpaid amount. This is a limit on cumulative charges, rather than an annual interest-rate ceiling. (1)	41.3%
Chile	Yes	The Financial Market Commission (CMF) publishes the maximum conventional interest rates applicable to lending operations. The statutory limits are based on average market rates and vary according to the type, amount, currency and term of the loan. (2)	24.2%
Argentina	Yes	Under the Credit Card Law (Law No. 25,065), the interest rate charged by bank issuers may not exceed their rate on personal loans by more than 25%. The earlier ceilings introduced through BCRA Communication A 5590 are not an appropriate description of the 2024 framework. (3)	32.1%
Panama	No		10.4%
Uruguay	Yes	In credit operations where the loaned capital or nominal value of the discounted document, excluding interest or charges, is less than the equivalent of 2,000,000 UI (indexed units), interest rates will be considered usurious when they exceed the average interest rates by more than 55%. If the loaned capital is greater than or equal to 2,000,000 UI, interest rates will be considered usurious when they exceed the average interest rates by more than 90%. (4)	42.0%
Colombia	Yes	The statutory usury ceiling is generally 1.5 times the current banking interest rate certified by the financial supervisor for the corresponding credit category. Rates and ceilings are updated periodically. (5)	19.0%
Costa Rica	Yes	The Central Bank publishes maximum annual lending rates for ordinary loans and microcredit, in colones and foreign currency, under the applicable consumer-protection framework. The ceilings are recalculated for each reference period. (6)	17.5%
Peru	Yes	It is equivalent to 2 times the average of the end-of-month observations of the average interest rates of the financial system for consumer loans in national currency published by the Superintendence of Banking, Insurance, and AFP. (7)	22.1%
Mexico	No		n.a.
Dominican Republic	No		21.8%
Bolivia	Yes	With the aim of promoting productive credit and limiting usury practices, Law No. 393 ("The Law"), enacted in August 2013 and implemented in mid-2014, imposed maximum limits on interest rates for productive credits, especially microcredits. Specifically, it defined maximum interest rate limits of 6% for productive credits to companies, 6-7% for productive credits to SMEs, and 11.5% for productive microcredits. Additionally, the Law also established mandatory portfolio quota compliance	20.0%

		targets for productive credits. (8)	
Paraguay	Yes	Interest rates will be considered usurious if the effective compensatory and punitive rates exceed by thirty percent (30%) the average effective annual rates applied by banks and financial entities for consumer credits, according to the terms and currencies in which such credits are granted. (9)	13.8%
Ecuador	Yes	Maximum effective lending rates are established by credit segment and published by the Central Bank of Ecuador. The applicable limits in 2024 differ across commercial, consumer and microcredit categories. (10)	23.2%
Venezuela	Yes	In 2024, the Central Bank of Venezuela regulated credit-card financing rates, including a maximum annual rate of 60%, a minimum of 17%, and an additional maximum 3% annual rate for overdue obligations. (11)	11.1%
Honduras	No	Credit-card interest rates are subject to a statutory maximum. Under the reforms to the Credit Card Law, the financial supervisor identifies a maximum annual financing rate of 54%. (16)	11.1%
Nicaragua	Yes	The maximum interest rate between individuals will be up to double the weighted average rate charged by commercial banks authorized in the country, published by the Central Bank of Nicaragua. No additional charges that directly or indirectly alter the previously agreed interest rate will be allowed. (12)	12.4%
El Salvador	Yes	Under the Usury Law, the Central Reserve Bank establishes and publishes maximum annual effective rates for different credit categories. The ceilings vary by the type and amount of credit and follow the applicable statutory calculation rules. (13)	10.7%
Guatemala	Yes	The 2024 Credit Card Law allows card issuers and borrowers to agree freely on financing interest rates. Previously reported credit-card rates were market observations, not statutory interest-rate ceilings. (14)	n.a.

Note:
(1) https://www.bcb.gov.br/estatisticas/juros-cartao-de-credito?anoMes=202407&anoMesNull=true
(2) https://www.cmfchile.cl/educa/621/w3-propertyvalue-1188.html
(3) https://www.argentina.gob.ar/normativa/nacional/ley-25065-55556/actualizacion
(4) https://www.impo.com.uy/bases/leyes/18212-2007/11
(5) https://www.superfinanciera.gov.co/preguntas-frecuentes/12/12-tasas-de-interes/
(6) https://gee.bccr.fi.cr/indicadoreseconomicos/cuadros/frmvercatcuadro.aspx?codcuadro=+5508&idioma=1
(7) https://www.bcrp.gob.pe/docs/Transparencia/Normas-Legales/Circulares/2021/circular-0008-2021-bcrp.pdf
(8) https://www.asfi.gob.bo/sites/default/files/2025-08/Evaluaci%C3%B3n%20del%20Sistema%20Financiero%20a%20Junio%20de%202024.pdf
(9) https://www.bcp.gov.py/es/web/institucional/limites-para-tasas-usurarias
(10) https://contenido.bce.fin.ec/documentos/Estadisticas/SectorMonFin/TasasInteres/TasasVigentes062024.htm
(11) https://upload.wikimedia.org/wikipedia/commons/1/1c/2024-10-29_Gaceta_Oficial_de_Venezuela.pdf
(12) https://bcn.gob.ni/tasas-activas-promedio-ponderadas-para-prestamos-entre-particulares
(13) https://www.bcr.gob.sv/regulaciones/upload/NTALCU.pdf?v=1681656036
(14) https://www.congreso.gob.gt/detalle_pdf/decretos/13631
(15) https://openknowledge.worldbank.org/server/api/core/bitstreams/a57b273f-12e1-5b10-89e2-d546f2ea7125/content
(16) https://www.cnbs.gob.hn/la-cnbs-responde/
Reference (11) corresponds to the Central Bank of Venezuela (BCV), Official Gazette No. 42,995 (October 29, 2024).
Financial inclusion (15) measures formal borrowing, including credit cards, in 2021, as a percentage of adults aged 15 and older (World Bank Global Findex 2021). Data for Mexico and Guatemala are unavailable because these countries were not surveyed in 2021.
Regulatory reference year: 2024. "Yes" indicates the existence of an interest rate cap applicable to at least one credit category.
Source: Authors' compilation based on regulatory sources (1)–(14) and (16), and the World Bank Global Findex 2021 (15).

Table A2
Classification of Credit Types and Their Descriptions

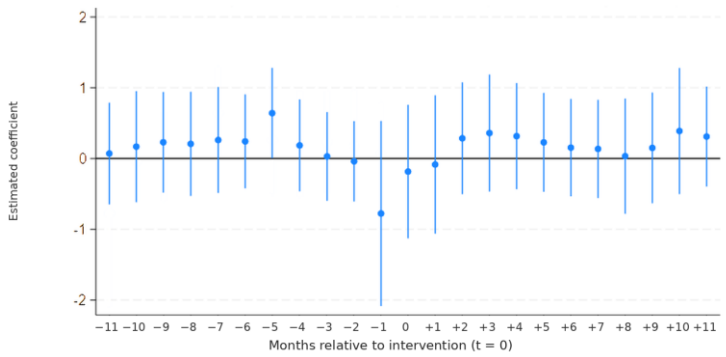
Credit Type	Description
Corporate Credits	Loans granted to legal entities with annual sales exceeding S/ 200 million in the previous two years, based on their most recent audited annual financial statements.
Large business credits	Loans granted to legal entities with annual sales exceeding S/ 20 million but not exceeding S/ 200 million in the previous two years, or with outstanding debt instruments in the capital market during the preceding year.
Medium Business credits	Loans granted to legal entities with total financial system debt exceeding S/ 300,000 in the previous six months, provided they are not classified as corporate or large enterprises. This category also includes individuals whose debt, excluding housing mortgages, exceeds S/ 300,000, provided part of that debt corresponds to small- or microenterprise loans.
Small Business credits	Loans for production, commercialization, or service activities granted to individuals or legal entities with total financial system debt exceeding S/ 20,000 but not exceeding S/ 300,000 in the previous six months, excluding housing mortgages.
Micro-Business credits	Loans for production, commercialization, or service activities granted to individuals or legal entities with total financial system debt not exceeding S/ 20,000 in the previous six months, excluding housing mortgages.
Consumer credits	Loans granted to individuals to finance goods, services, or expenses unrelated to business activities, including revolving and non-revolving credit.
Mortgage credits for housing	Loans granted to individuals to finance goods, services, or expenses unrelated to business activities, including revolving and non-revolving credit.

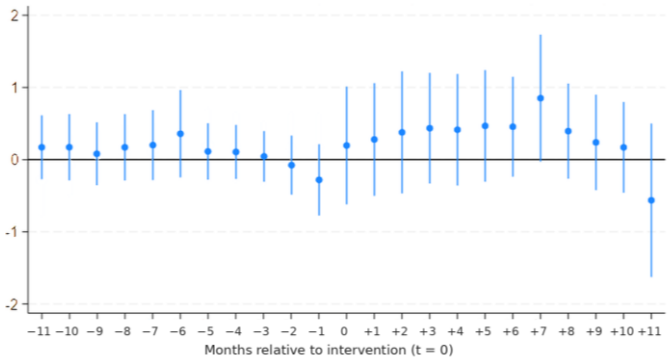
Note: Consumer credits include both revolving and non-revolving loans.
Source. Adapted from Superintendencia de Banca, Seguros y AFP (2008), Chapter I, Section 4.

Figure A1
Event-Study Estimates of Log Average Credit Amount: Real and Placebo Shocks by Subsystem

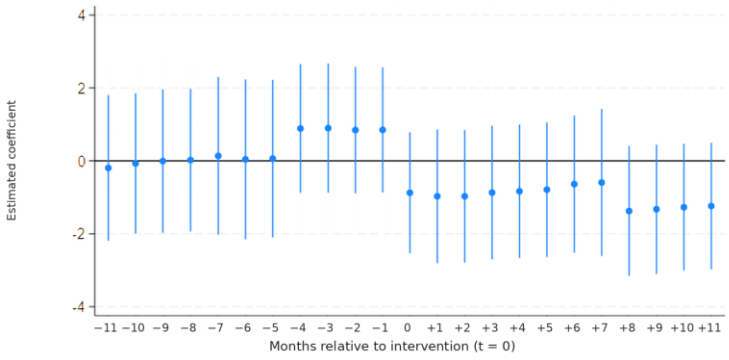
Real treatment effect (t=0: May 2021) Placebo (t=0: May 2020)

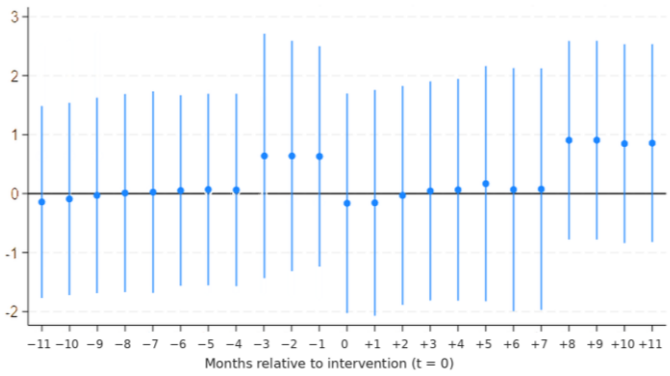
Commercial Banks



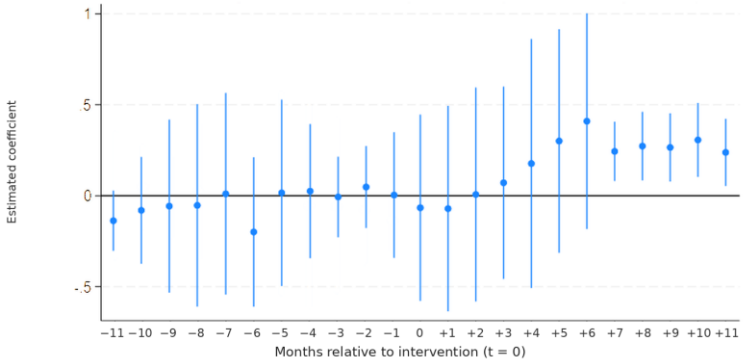


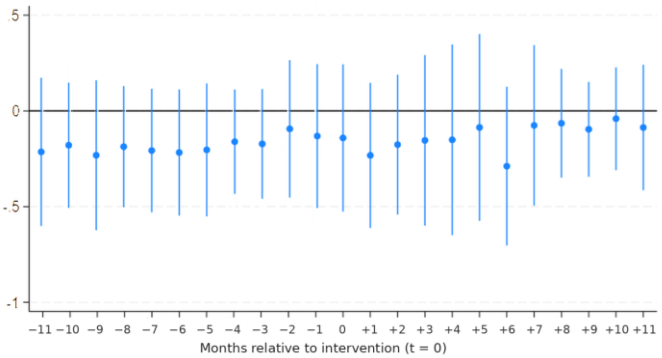
Municipal saving and loans



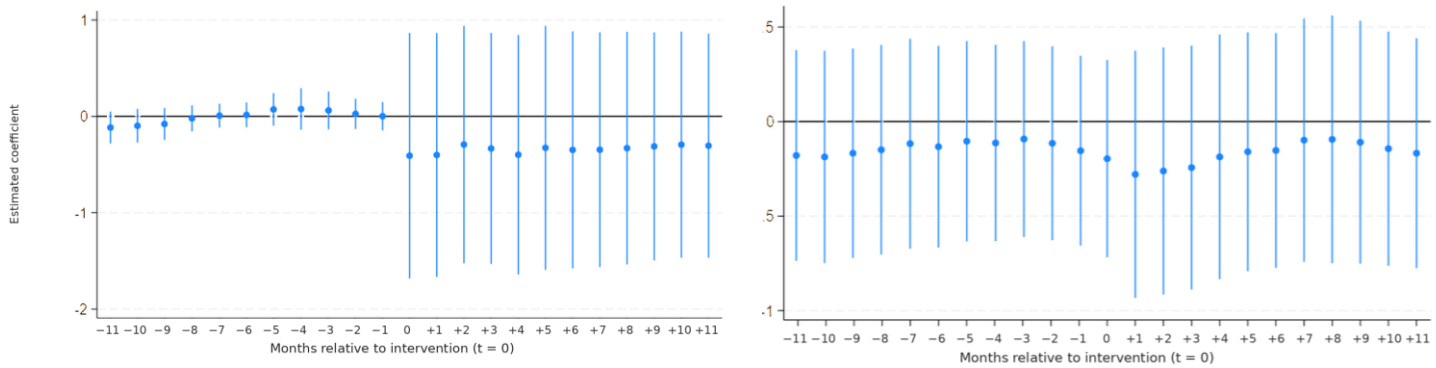


Rural saving and loans

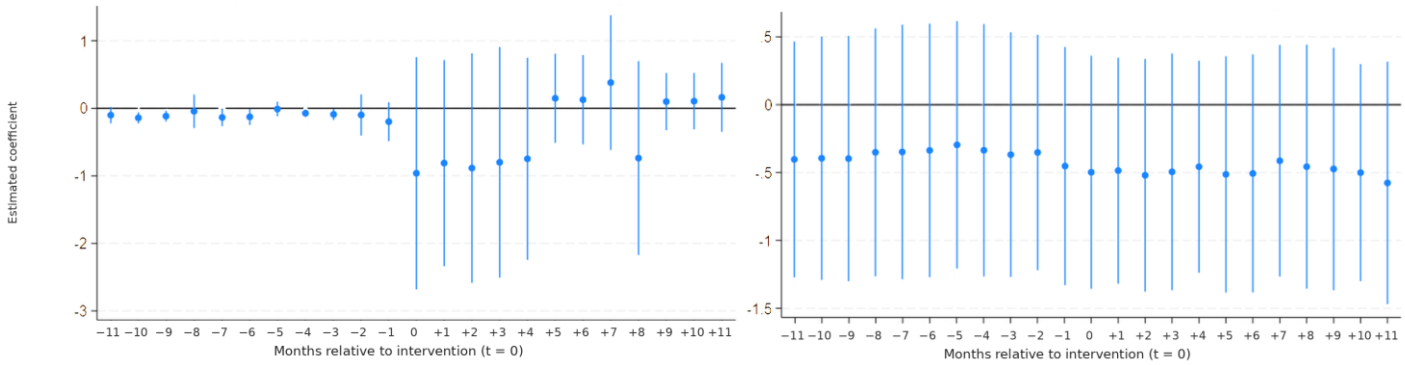




Financial Companies



Loan Companies

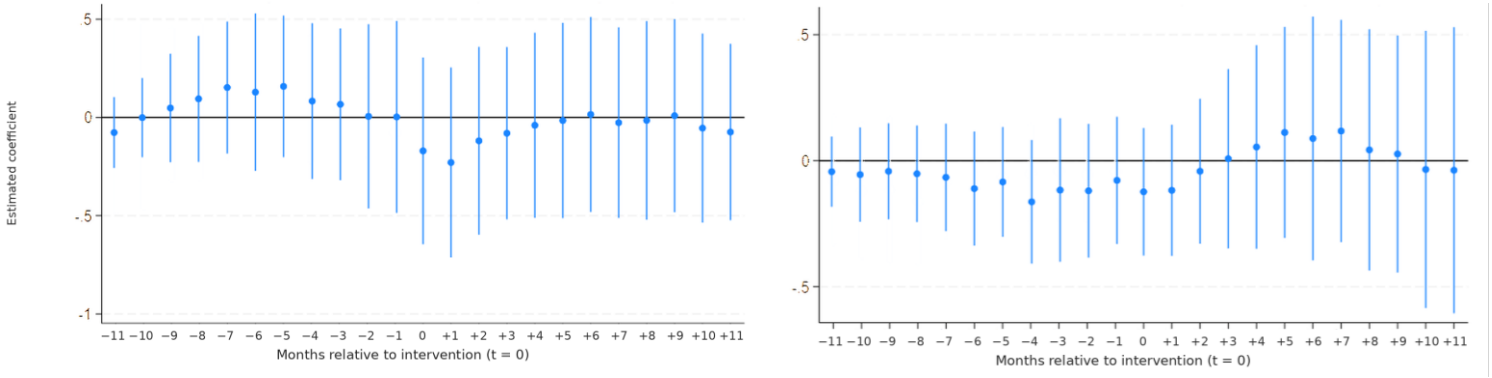


Note: The actual intervention corresponds to May 2021, while the placebo intervention corresponds to May 2020 (t = 0). The horizontal axis shows months relative to each intervention, and the vertical axis reports estimated coefficients. The reference periods are those outside the displayed event window ($t \leq -12$ and $t \geq 12$), conditional on the aggregate post-treatment effect included in the original specification. In particular, $t = -1$ is estimated rather than omitted. Dots represent estimated coefficients, and vertical lines indicate 95% confidence intervals. Standard errors are clustered at the panel-unit level.
Source: Authors' Work

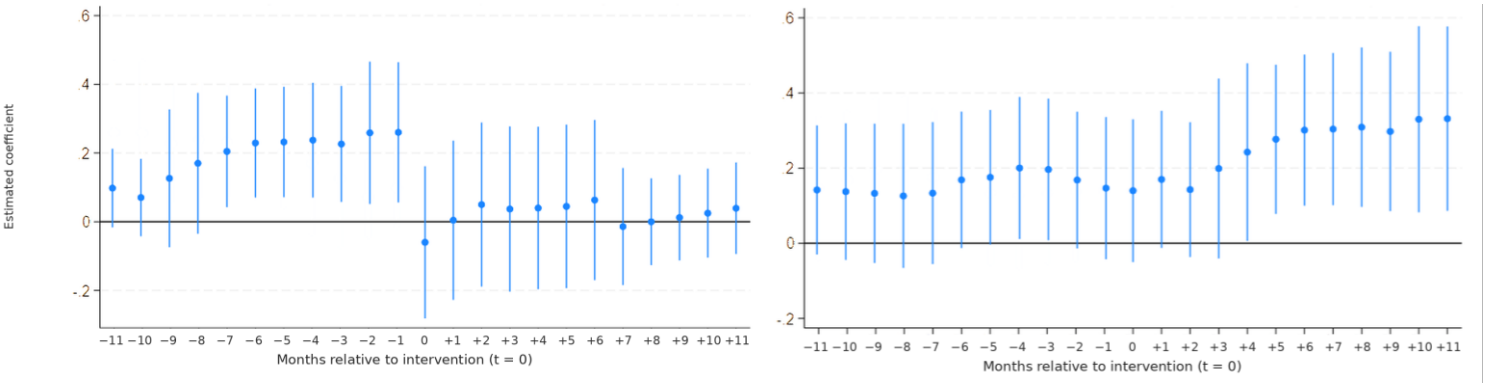
Figure A2
Event-Study Estimates of Log Number of Debtors: Real and Placebo Shocks by Subsystem

Real treatment effect (t=0: May 2021) Placebo (t=0: May 2020)

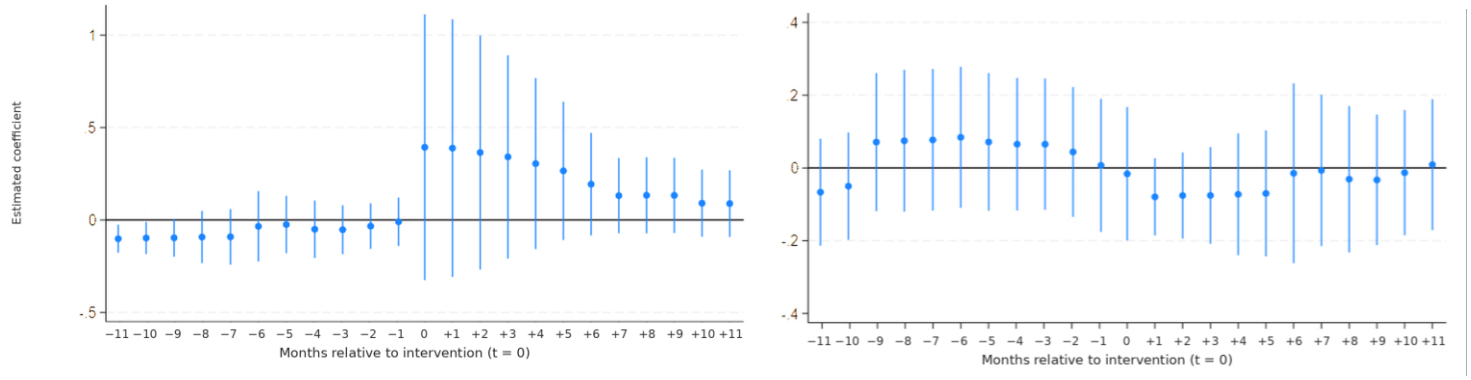
Commercial Banks



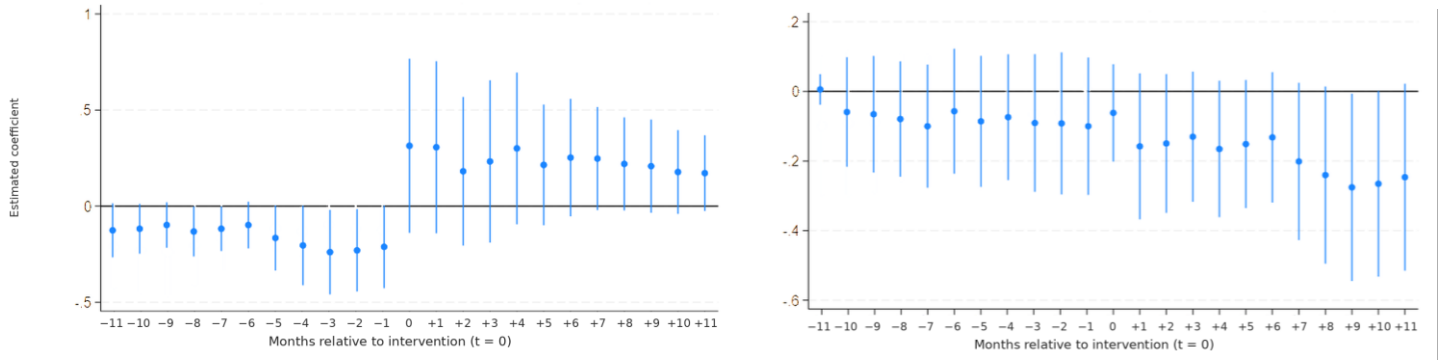
Municipal saving and loans



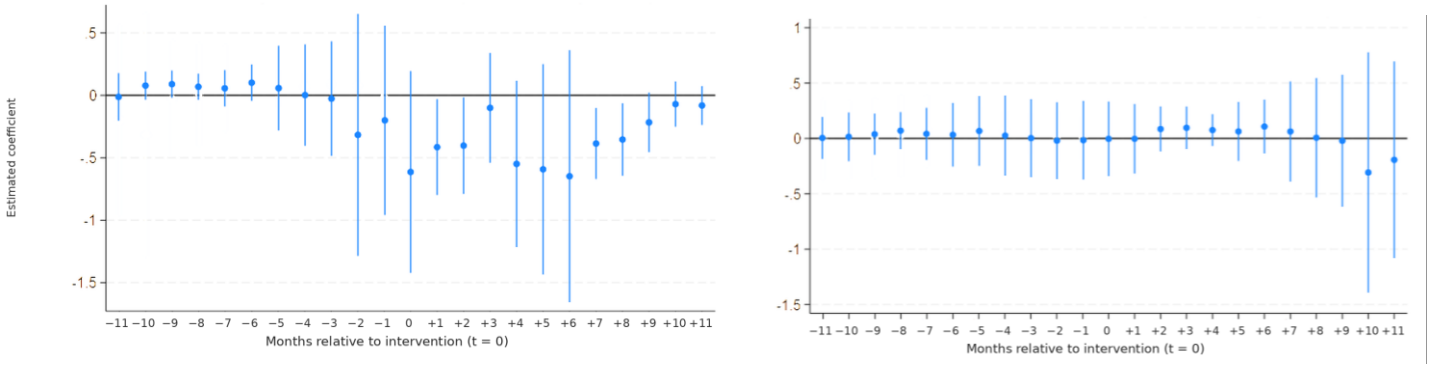
Rural saving and loans



Financial Companies



Loan Companies

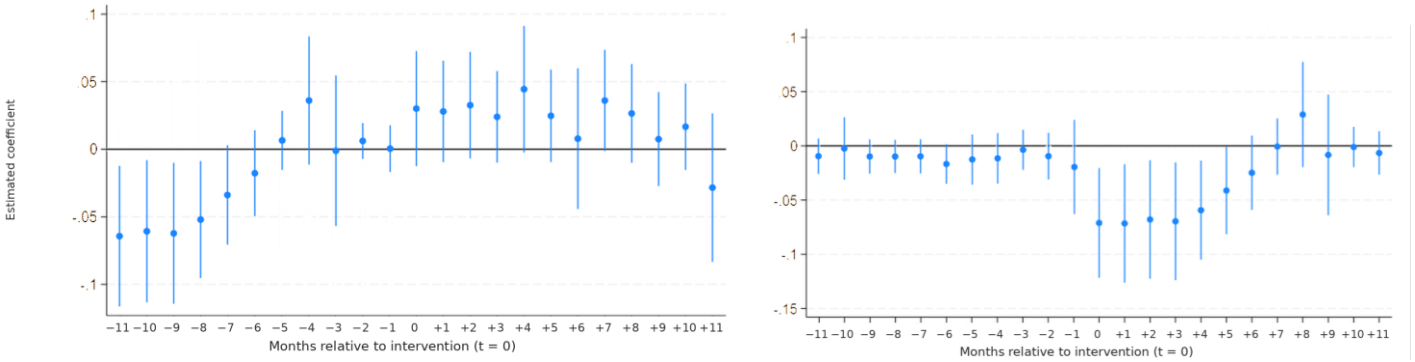


Note: The actual intervention corresponds to May 2021, while the placebo intervention corresponds to May 2020 (t = 0). The horizontal axis shows months relative to each intervention, and the vertical axis reports estimated coefficients. The reference periods are those outside the displayed event window ($t \leq -12$ and $t \geq 12$), conditional on the aggregate post-treatment effect included in the original specification. In particular, $t = -1$ is estimated rather than omitted. Dots represent estimated coefficients, and vertical lines indicate 95% confidence intervals. Standard errors are clustered at the panel-unit level.
Source: Authors' Work

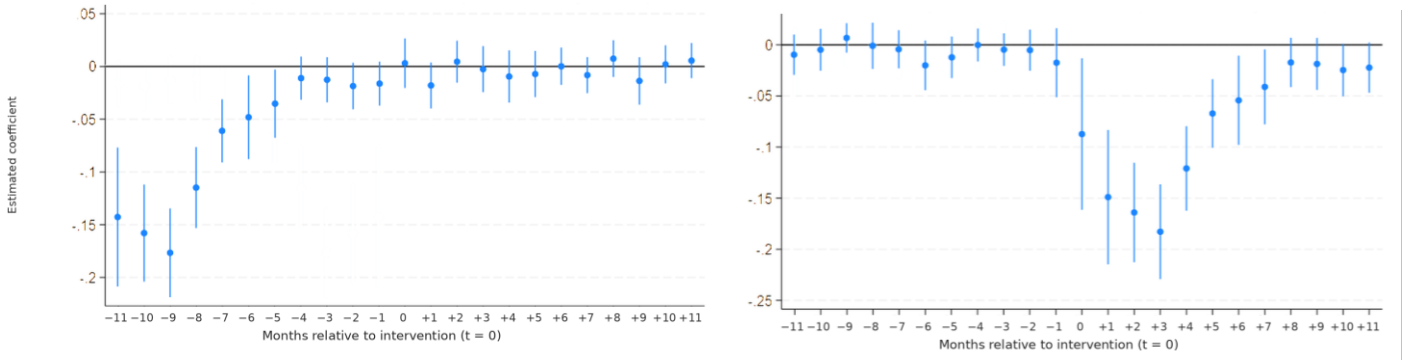
Figure A3
Event-Study Estimates of Average Active Interest Rate: Real and Placebo Shocks by Subsystem

Real treatment effect (t=0: May 2021) Placebo (t=0: May 2020)

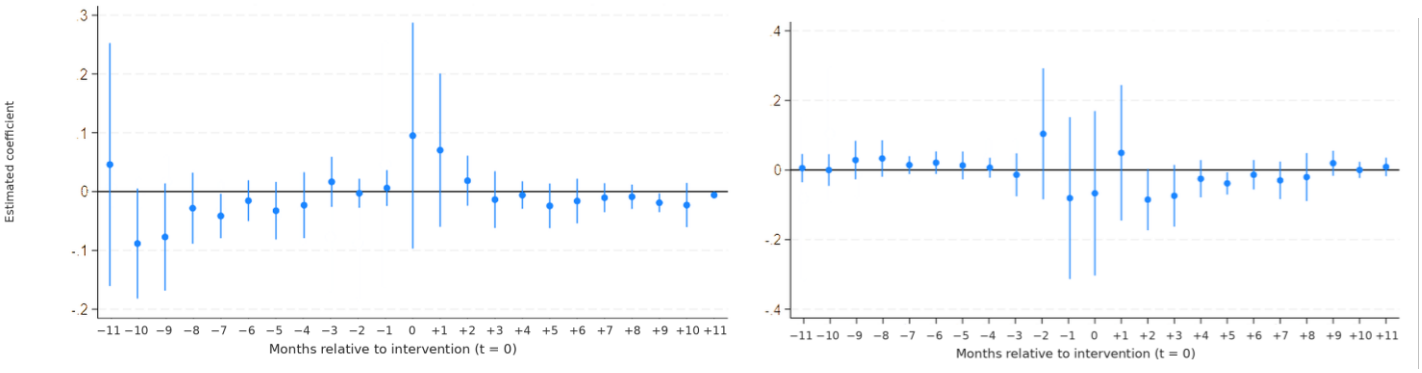
Commercial Banks



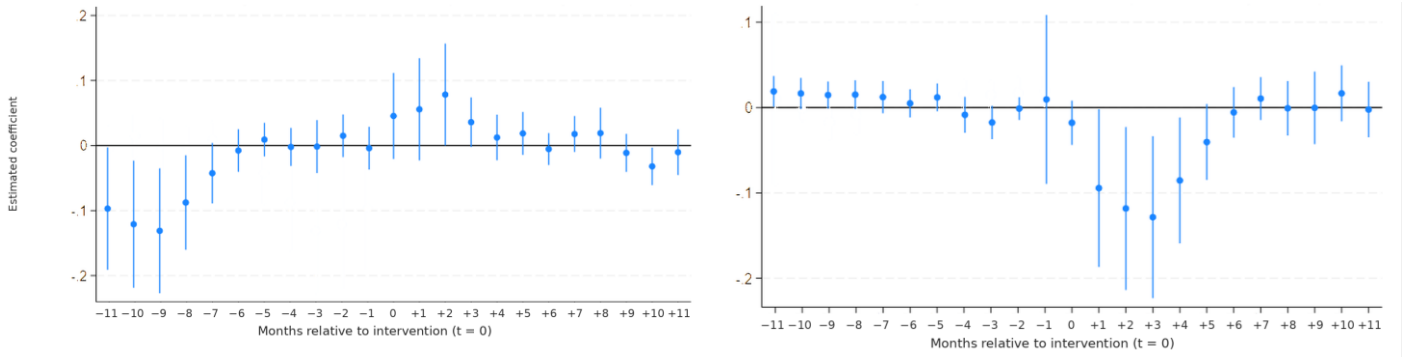
Municipal saving and loans



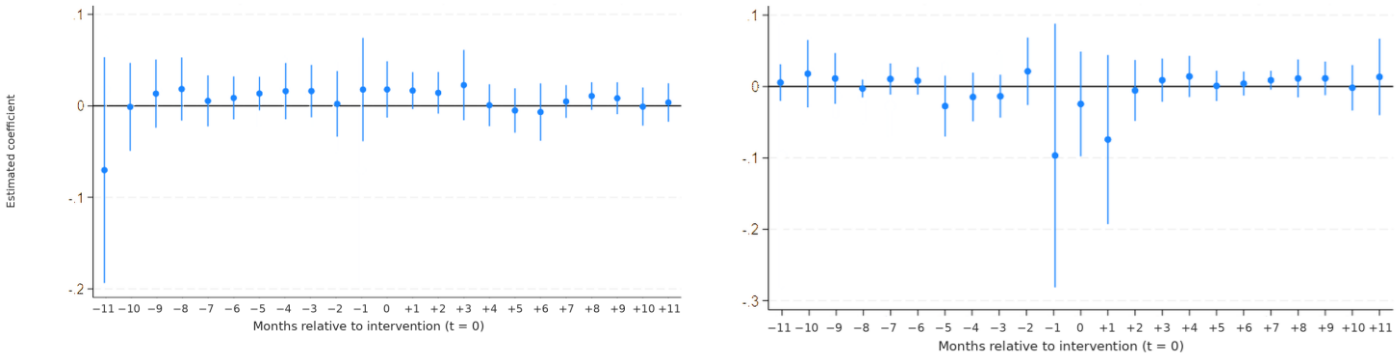
Rural saving and loans



Financial Companies



Loan Companies



Note: The actual intervention corresponds to May 2021, while the placebo intervention corresponds to May 2020 (t = 0). The horizontal axis shows months relative to each intervention, and the vertical axis reports estimated coefficients. The reference periods are those outside the displayed event window ($t \leq -12$ and $t \geq 12$), conditional on the aggregate post-treatment effect included in the original specification. In particular, $t = -1$ is estimated rather than omitted. Dots represent estimated coefficients, and vertical lines indicate 95% confidence intervals. Standard errors are clustered at the panel-unit level.
Source: Authors' Work