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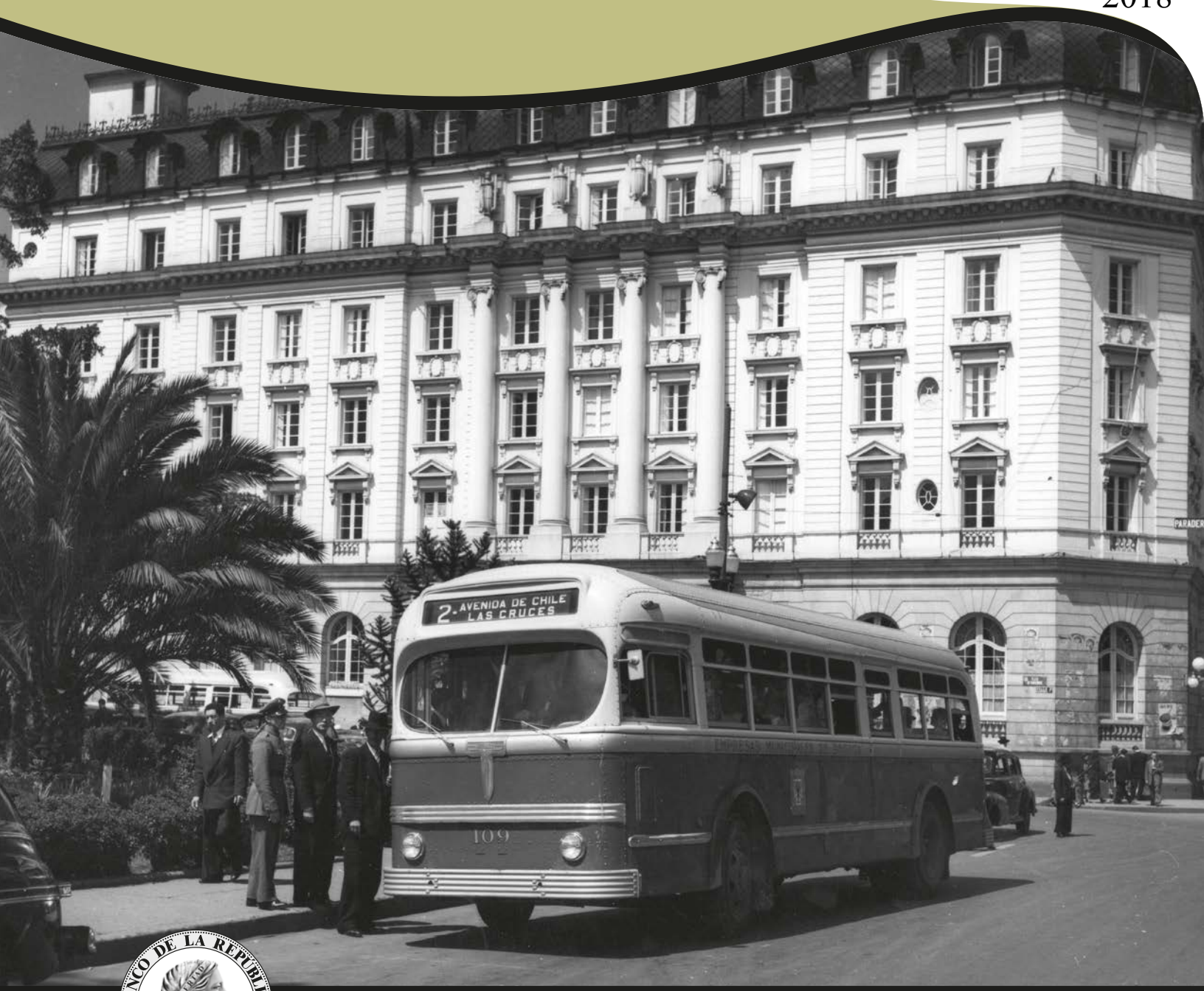

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Effects of Interest Rate Caps on Financial Inclusion*

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Abstract

In this paper we study the liberalization of the microcredit usury rate in Colombia and its effects on loan expansion. Namely, in February 2007 the interest rate ceiling for microcredit loans was lifted and fixed to 33%, while the ceiling of all other loans remained unchanged and close to 20%. We perform a Difference-in-Difference analysis by comparing the expansion of microcredit loans (treatment group) with that of corporate loans (control group). Additionally, we narrow in on similar levels of both loan size and debtor's risk in order to make microcredit and corporate portfolios more comparable. Our results indicate that this policy encouraged and facilitated financial access to entrepreneurs. Specifically, we find that, on average, the amount lent by credit establishments increased between 21.5% and 42.4%, and the number of new loans increased between 25.1% and 47.8%.

JEL Classification: G18; G28; G38

Keywords: Usury rate, interest rate caps, financial inclusion, microcredit loans, corporate loans

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Efectos de la tasa de usura sobre la inclusión financiera

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Resumen

En este trabajo estudiamos la liberalización de la tasa de usura de la cartera de microcrédito en Colombia y sus efectos sobre la expansión del crédito. Concretamente, en febrero de 2007 la tasa de usura para la cartera de microcrédito se elevó y se fijó en 33%, mientras que la tasa de usura de las otras carteras se mantuvo cerca al 20%. Realizamos un análisis de Diferencias en Diferencias al comparar la expansión de los créditos de microcrédito (grupo de tratamiento) con la de los créditos comerciales (grupo de control). Adicionalmente, analizamos niveles similares tanto del tamaño del crédito como del riesgo del deudor, con el objetivo de que ambas carteras sean más comparables. Nuestros resultados indican que esta política estimuló y facilitó el acceso financiero a las pequeñas y medianas empresas. Específicamente, encontramos que, en promedio, el monto prestado por los establecimientos de crédito aumentó entre 21.5% y 42.4%, y el número de nuevos créditos aumentó entre 25.1% y 47.8%.

JEL Classification: G18; G28; G38

Keywords: Tasa de usura, inclusión financiera, cartera microcrédito, cartera comercial.

1 Introduction

Financial inclusion has emerged as an essential public policy objective for governments and organizations worldwide. Its importance stems from the premise that access to formal financial markets has great potential to improve livelihoods, especially those in conditions of greater poverty (Bruhn and Love, 2009; Burguess and Pande, 2005; Deaton, 2010; Zhan and Sherraden, 2011). From a macroeconomic perspective, financial inclusion affects economic growth, inequality, and financial stability (Sahay et al., 2015a; Sahay et al., 2015b). Notwithstanding, multiple barriers to financial services still persist to date; most of which are based on informational costs (i.e. the inability of financial institutions to differentiate between risky and safe clients) as well as operational costs. Ultimately, these costs are translated into a set of requirements that impede access to the financial sector, such as the case of loan collateral, high interest rates, and long processes of loan documentation and verification.

One of the most controversial barriers to financial inclusion is the enactment of interest rate ceilings. While market-determined interest rates became predominant in emerging markets after the liberalization of financial systems in the late 80s (especially Latin American countries), some have maintained or enacted interest rate caps. This paper aims to shed new light on this debate, with the use of a clear-cut identification strategy brought forth by the way in which financial regulations in Colombia operated. Namely, as part of a financial inclusion government plan, in February 2007 the ceiling of microcredit loans was lifted and fixed to 33%, while leaving the ceiling of all other loans unchanged. Previously, there was only one usury rate in Colombia, of approximately 20%, which covered all segments of corporate, household, and microcredit loans.

The lift in the microcredit interest rate cap provides an ideal and unique quasi-experiment; one which allows us to estimate the causal effect on credit expansion. Specifically, we perform a Difference-in-Difference (DID) analysis by comparing the expansion of microcredit loans (treatment group) with that of corporate loans (control group), right after the microcredit ceiling was lifted. Our choice of corporate loans as a control group is based on the fact that both microcredit and corporate loans target the same type of clients, namely entrepreneurs. That is, loan submissions are tied to an economic activity or business model, and are disbursed mainly to pre-existing firms, ranging from large to micro-enterprises.

However, in spite of having the same interest rate cap prior to treatment, these types of loans differ in two key aspects: loan size and debtor's riskiness. For instance, a microcredit loan cannot exceed the sum of 25 monthly minimum wages. Also, larger and older firms, which borrow in larger quantities, tend to have a higher probability of repayment. Consequently, and as part of our identification strategy, we narrow in on similar levels of both loan size and debtor's risk. That is, we argue that for a given set of firm-level covariates, a small-to-medium firm seeking credit is indifferent between submitting a loan request through either a microcredit or corporate portfolio. Hence, conditional on loan size and the ex-ante probability of repayment, loans are *as good as randomly allocated* among both treatment and control groups.

Our unit of measurement compares each type of loan within each financial institution in the country, at a weekly frequency, during the period of 2006-2008. To our knowledge, there is no other study that uses this data granularity (covering the entire banking sector) to evaluate usury rates. Moreover, comparing different loan portfolios under the same bank or institution reinforces the idea that they respond equally to any common shock in the economy. In other words, the difference between treatment and control groups is more likely to be constant over time, thus meeting the parallel trends assumption. Nonetheless, we additionally control for various loan-type characteristics to avoid for potential time-varying factors, i.e. those that can affect microcredit and corporate loans in different ways. Further, similar to Bruhn and Love (2014), we control for the possibility that linear time trends in outcome variables may be different between bank-specific portfolios.

Our results indicate that the usury rate hike in microcredit loans encouraged and facilitated access to the financial system. On average, we find that the number of new loans increased between 41.8% and 57.7% and the loan amount increased between 27.5% and 75.7%. These results are robust across all specifications and time windows considered. Additionally, we conduct panel regressions to evaluate the validity of the parallel trends assumption and find strong evidence to support it. Namely, in periods prior to intervention, we do not reject the null that all interaction coefficients (associated with pre-treatment time dummies) are equal to zero. Finally, our results hold once we control for different bank-specific time trends in the regressions.

Our paper is mainly related to two strands of literature. First, it contributes to the interest rate caps (and more specifically usury rates) literature, in which there is a general lack of consensus regarding its effects. This is mostly due to the inconclusive body of empirical evidence and the absence of more rigorous empirical exercises. Advocates of usury rates state that they serve as a consumer protection policy against financial institutions that use information asymmetries to justify high and excessive lending rates. Along those lines, interest rate caps are most often used in low-income groups, where microfinance institutions impose the highest rates on a larger volume of low-value loans. Besides, in remote or rural areas, prices charged by these institutions are generally non-competitive, being higher than the real cost of lending. In this context, interest rate caps protect vulnerable segments by ensuring a maximum rate on loans (Dewatripont et al., 1994). Other reasons among advocates include: (1) providing short-term credit in strategic industries where a market failure exists, and (2) supporting vulnerable sectors until they become sustainable (Miller, 2013). However, to our knowledge there is only a handful of empirical exercises that support usury rates, most of them focused on the South Korean case (Arestis et al., 2001; Crotty and Lee, 2002).

On the other hand, critics of usury rates sustain that their use is an inefficient and distorted tool for lowering interest rates, especially in the long run. Some studies show that interest rate caps reduce transparency and limit access to credit to the most vulnerable populations (Helms and Reille, 2004; Maimbo and Gallegoz, 2014). In fact, they claim that the imposition of caps magnify the problem of asymmetric information, since credit institutions cannot charge a high-enough-rate to a large pool of borrowers with unidentifiable creditworthiness. Institutions therefore end up lending to people with higher collateral, and excluding those who have little or no access to credit. In addition, interest rate ceilings can increase the cost of loan screening, which is harmful to financial outreach. Using panel data for different countries and financial institutions, empirical studies primarily suggest that the existence of interest rate ceilings is associated with lower levels of financial deepening and financial inclusion. These results also apply to the Colombian case (Agudelo and Steiner, 2012; Capera et al., 2011). In sum, studies in this strand of literature conclude that the most effective policies to reducing lending rates and to improving access to credit are those which directly affect the initial market failure (e.g measures that enhance competition and product innovation, financial consumer protection laws, and financial literacy).

We note that there are not many studies on the effects of interest rate ceilings on financial access, possibly due to the lack of data. Of note is that most of the aforementioned studies use simple descriptive statistics (Maimbo and Gallegoz, 2014), or simple linear regressions analysis (Campion et al., 2010, Capera et al., 2011). In contrast, we focus on a natural experimental framework to identify causal effects of usury rates on lending.

Second, our work relates to a more ample and established literature based on the effects of reducing physical barriers on financial inclusion. For example, Burguess and Pande (2005) find, for the Indian case, that the expansion of bank branches to rural areas significantly increase bank credit and savings. In addition, Aportela (1999) finds that the expansion of the public institution *Patronato del Ahorro Nacional* increases savings by 3-5% and finds that effects are even more prominent for the poorest households. Bruhn and Love (2009) and Ruiz et al. (2010) study the expansion of *Banco Azteca* through one of the largest retailers in Mexico, *Grupo Elektra*, in which more than 800 Banco Azteca branches simultaneously opened. Ruiz (2010) finds that households in municipalities with Banco Azteca are more likely to borrow from banks and less likely to borrow from pawnshops. Finally, Carabarín et al. (2018) show a positive effect of the entrance of banking correspondents on both the volume and number of savings accounts.

Except for the work of Burguess and Pande (2005), these studies use diverse DID strategies to evaluate the impact of exogenous policies on financial inclusion. We proceed with a similar empirical strategy in our investigation. Nonetheless, these studies focus primarily on physical access barriers. A significant gap still exists in the literature regarding the impact of other supply-side barriers, such as pecuniary barriers. This matter is particularly relevant for emerging market economies (Roa and Carvallo, 2018). To the extent of our knowledge, this is the first paper to evaluate the effect of a pecuniary barrier on credit access –the lifting of interest rate ceilings– in a quasi-experimental framework.

Our paper proceeds as follows: The next section describes the data, and the contextual and regulatory characteristics of the Colombian case. Section 3 presents the methodology that revolves around a quasi-experimental framework. In this section, a panel data analysis is also carried out, where we control for *time* and *bank* fixed effects as well as macroprudential and government policies. Section 4 presents results and section 5 concludes.

2 Data and Contextual Characteristics

2.1 Usury Rates and Financial Inclusion in Colombia

The Colombian penal code defines the usury rate as the maximum interest rate associated with any credit operation, and establishes criminal charges for anyone who exceeds it. From its enactment in 1990, the usury rate was construed as a consumer protection policy, intended to avoid excess charges. The usury rate was calculated as 1.5 times the current bank interest rate (CBIR), which in turn was a weighted average of corporate, household, and microcredit loans. Since the year 2000, the CBIR has been overseen and certified by the Financial Superintendency (*Superintendencia Financiera de Colombia*).[1] The different loan portfolios (excluding housing loans) are defined as follows:

- Microcredit: loans that do not exceed the amount of 25 monthly minimum wages. Loan submissions must be tied to an economic activity or business model. Loans are generally disbursed to micro-enterprises (i.e. firms with less than 10 workers and with assets of less than 500 monthly minimum wages).

- Corporate: loans in which submissions are tied to an economic activity. Loans are targeted to firms (ranging from large to small) that meet collateral and other set of requirements.

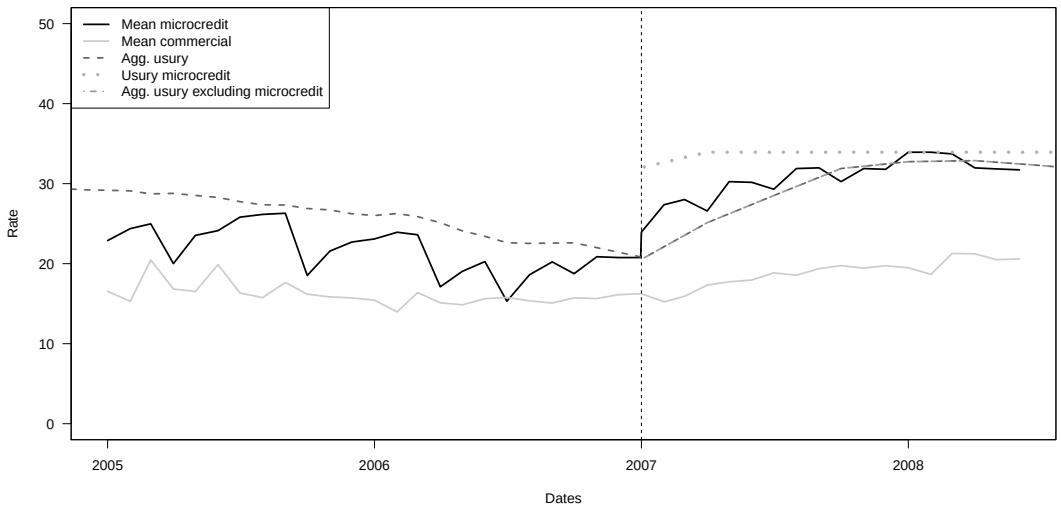
- Household: loans destined for the acquisition of goods and services, and other non-commercial purposes (e.g. payroll, automobiles, credit cards, etc.). Loan portfolio includes (but reported separately) consumption loans of low amount (i.e. loans of up to 2 monthly minimum wages and with a payment term of up to 36 months).

As seen in Figure 1, before 2007, there was only one usury rate in Colombia (dotted line), close to 20%. In February 2007 the Financial Superintendency lifted the interest rate ceiling for microcredit loans to 33%. This treatment event, which is the main focus of our investigation, was part of a comprehensive government plan that started at the onset of 2006, with the creation of the "Opportunities Bank Program" (*Banca de las Oportunidades*). Figure 1 also shows that the sudden increase in the microcredit usury rate (relative to corporate loans) lasts for approximately 1 year, before the two usury rates converge in 2008. That is, given the way the CBIR is computed, the initial microcredit

[1] See Superintendencia Financiera de Colombia (2017) for a detailed regulation on the current bank interest rate.

usury hike, coupled with a moderate increase in the average rate of all other loans during 2007, gradually increased the usury rate for corporate loans.

Figure 1: Usury rate and Mean rate for Microcredit and Corporate Loans



The figure shows the mean and usury rate for corporate and microcredit loans. In February 2007, the Financial Superintendency lifted the interest rate ceiling for microcredit loans to 33%.

While the mean loan rates for microcredit and corporate loans provide an informative history during our sample period, we also note that the different usury rates were in all cases binding. Specifically, for the period prior to 2007, there were 130,000 new microcredit and 55,000 new corporate loans within close vicinity of the usury rate (within 90-100% of the usury rate). This amounts to 69% and 23% of all microcredit and corporate loans, respectively, during 2005-2007. More notably is that after the microcredit usury rate hike in 2007, there were 144,000 new microcredit loans above the aggregated usury rate, which no longer applied to microcredit but still applied to all other loan portfolios (i.e. corporate and household). Put differently, 144,000 new loans, equivalent to 76% of all microcredit loans during 2007-2008, would have been restricted to a lower rate (we believe most would not have existed) had the usury rate hike not taken place.

Apart from the usury rate lift on microcredit loans, the main channel through which the Opportunities Bank Program operated was the state-owned bank, *Banco Agrario*. However, the way that *Banco Agrario* determines interest rates, sources of funding, and portfolio decisions, is very different from the rest of the banking system. For example,

government subsidies are applied to credit acquisition through lower rates (see Figure 3 in Appendix A). Also, *Banco Agrario* provides loan incentives (e.g. *Incentivo a la Capitalizacion Rural* –ICR), and subsidized insurances to substitute out the use of collateral (e.g. *Fondo Agropecurio de Garantias*).[2] As such, we exclude *Banco Agrario* from our empirical exercises, since the nature of this bank can potentially distort our results, and contaminate the effects of usury rates with those of government subsidies.

Nonetheless, we do control for the number of Non-Banking Correspondents (NBCs) pertaining to each financial institution. These NBCs, launched in July 2006, intended to increase coverage of financial services nation-wide, though initially, almost all NBCs belonged to *Banco Agrario* and only provided limited services such as payment of services and transfer of funds. In this sense, by including NBCs in our empirical exercise, we also evaluate the effects of physical barriers. Nevertheless, it was only after 2008 that NBCs began to offer a more complete set of services, especially those related to loan submission (e.g. cash deposits and withdrawals, balance inquiries, and disbursements and payments for active credit operations).[3]

Finally, we control for financial regulations and macroprudential policies during the period of study. Namely, in the two years preceding the 2008 world financial crisis, Colombia experienced an episode of strong credit growth following a comprehensive restructuring of commercial bank assets. As such, the Central Bank of Colombia imposed limits on foreign holdings (net assets denominated in foreign currency relative to equity) so as to limit speculation on the currency and avoid a large substitution of domestic debt (see Mora-Arbeláez et al., 2015). We thus control for this measure, by including bank-specific net foreign holdings (*Posicion Propia de Contado*) in the exercises that follow. To control for other policies such as capital controls, we use time and bank fixed effects in the panel data analysis.[4]

[2]Most of the subsidies of *Banco Agrario* are channeled through FINAGRO. A detailed description of the regulatory framework can be seen in Superintendencia Financiera de Colombia (2017).

[3]See Superintendencia Financiera de Colombia (2013) and Ministerio de Hacienda y Crédito Público (2016).

[4]From 2007 through 2008 the central bank introduced controls on capital inflows, i.e. unremunerated reserve requirements, requiring foreign investors to freeze 40% of portfolio investments during 6 months without interest payments.

2.2 Data

Our data, which come from the Colombian Financial Superintendency, comprise the entire credit registry of the banking system. In fact, we also observe other credit establishments, such as companies (trust, insurance, and capitalization) and cooperatives. Specifically, we use the 341-Form which reports, at a daily frequency (although in quarterly reports), every loan along with its counterpart across the microcredit, corporate, and households portfolios. Our database is remarkably rich, since it contains loan-specific information on the issuance date, maturity, interest rate, type of collateral, risk rating (ex-ante probability of default), and number of non-performing days. In total, from January 2005 until June 2008 we observe 389, 419 new loans extended to corporates and 376, 156 new loans disbursed to micro-enterprises by 20 financial institutions (including 11 commercial banks).

In the exercises that follow, we limit our attention to new loans (credit flow) since our main interest is to analyze financial inclusion.[5] Namely, we focus on the expansion of microcredit (treatment group) and corporate loans (control group), right before and after the microcredit ceiling was lifted. Our choice of corporate loans as a control group is based on the fact that both microcredit and corporate loans target the same type of clients, namely entrepreneurs. We do however, recognize (and thus control for) key differences in these types of loans, mainly average loan size and debtor's riskiness. Hence, conditional on loan size and the ex-ante probability of repayment, we argue that loans are *as good as randomly allocated* among both treatment and control groups.

In our empirical exercises, we aggregate weekly over the number and size of new loans, per financial institution. These two variables (in logarithms) constitute our outcome variables. In order to capture possible heterogeneity among institutions due to their business scope, size, and portfolio management strategies, among others, we also include bank-specific data, such as: liabilities, total loans, equity, securities, number NBCs, and amount of foreign holdings. Additionally, we control for loan-specific characteristics such as: provisions, expected dividends, re-discounted rates, loan maturity, and value of collateral (see Table 8 of Appendix C for a more detailed data description).

[5]New loans are identified based on their issuance date.

Table 1 presents the outcome variables and loan-specific covariates of our empirical exercise. The table shows the difference of means (between treated and control groups) prior to treatment, from January 2005 up until December 2006. As shown, values are statistically significant in all cases, which confirm pre-existing differences (in levels) between the two groups. In particular, we observe a difference of 0.56 in the number of new loans, and -3.56 in the amount of new loans (both expressed in logarithms). Recall that the parallel trends assumption is valid as long as, in the absence of treatment, the average difference between outcome variables across loan portfolios pre and post-2007 would have been the same. Intuitively, outcome variables can differ in levels but not in changes. The latter is explored more in-depth in the next section.

Table 1: Differences in Means: Control versus Treatment

	Mean Control	Mean Treated	Diff	t	$Pr(\|T\| > \|t\|)$
Outcome Variables					
Ln number of new credits	3.07	3.64	0.56	7.8	0.000***
Ln amount of new credits	8.35	4.79	-3.56	37.8	0.000***
Loan-Specific Variables					
Value of collateral[a]	12.00	21.00	8.79	5.84	0.0000***
Loan maturity	24.45	27.62	3.17	2.69	0.0073***
Re-discounted rate	26.15	45.01	18.85	8.46	0.0000***
Expected dividends[a]	8.72	2.09	1.22	3.09	0.0020***
Provisions[a]	35.00	57.00	23.00	6.21	0.0000***

Authors' calculations. ***, **, and * denotes statistical significance at the 1, 5, and 10 percent level respectively. Sample period covers January 2005 to December 2006. The control group corresponds to corporate loans while the treatment group corresponds to the microcredit portfolio.[a] For easier readability, variables are expressed in 10^9 COP.

3 Empirical Methodology

We choose a Difference-in-Difference (DID) estimator to identify the effects of the microcredit usury hike. Namely, the trend in the control group (corporate loans) allows us to approximate what would have occurred in the treatment group (microcredit loans), in the absence of treatment.[6] More formally, the average treatment effect on the treated, in a DID setting, is exemplified as follows:

$$\begin{aligned} DID &= E(\Delta_{treated} - \Delta_{control} \mid X_{it}, D_i = 1) \\ &= E[(y_{1,t+1} - y_{1,t}) - (y_{0,t+1} - y_{0,t}) \mid X_{it}, D_i = 1] \end{aligned} \tag{1}$$

where y_1 and y_0 denote potential outcomes with and without exposure to treatment (D), and the matrix X_t contains all relevant information (loan-specific characteristics) that explain for any potential time-varying differences between treatment and control groups, i.e. those that can affect microcredit and corporate loans in different ways. As such, we estimate the following equation:

$$Y_{ijt} = \alpha + \gamma D_j + \lambda T_t + \beta(D_j \times T_t) + \delta' X_{ijt} + \epsilon_{ijt}, \tag{2}$$

where, i represents each financial institution, j the type of credit portfolio, and t is the time period. Our outcome variable, Y_{ijt}, denotes either the number of new loans or the total amount of new loans. D_j is the treatment variable, $D_j = 1$ for microcredit, and $D_j = 0$ for corporate. Hence, γ is associated with the constant differences between loan portfolios. Similarly, $T_t = 1$ if the period corresponds to after treatment ($\geq$ 2007), and 0 otherwise. It follows that λ is related to the conditions that change over time for both portfolios. Our coefficient of interest is β.

Additionally, we conduct different robustness exercises to evaluate the validity of the parallel trends assumption; essential to ensure internal validity of our estimates. It requires that in the absence of treatment, and conditional on a relevant history X_{ijt}, the difference between treatment and control groups is constant over time (visual inspection is useful when time series analysis is used). The violation of the parallel trend assumption can lead to biased estimation of the causal effect. Consequently, to corroborate for the

[6] This is actually a weaker version of the *Conditional Independence Assumption* –CIA–, as stated in Angrist and Pischke (2008).

parallel trends assumption we follow the test suggested by Angrist and Pischke (2008) that is based on the following model:

$$Y_{ijt} = \alpha_i + \gamma D_j + \lambda_t + \sum_{\tau=0}^{m} \beta_{-\tau}(D_j \times T_{t-\tau}) + \sum_{\tau=1}^{q} \beta_{\tau}(D_j \times T_{t+\tau}) + \delta^{'} X_{ijt} + \epsilon_{ijt}, \qquad (3)$$

where, α_i, γ and λ_t are the bank, portfolio, and time (week) fixed effects, respectively. Under the parallel trends assumption, the coefficients (β_τ) should not be statistically significant. We examine this in two ways: first we conduct a graphical analysis of these coefficients. Second, under this assumption we test whether the coefficients of the lead interacted terms are jointly zero, $H_o : \beta_1 = \beta_2 = ... = \beta_q = 0$, with the usual F-test. Alternatively, coefficients of the lag interacted terms, i.e. $\beta_{-1}, \beta_{-2}, ..., \beta_{-m}$ capture the impact of treatment throughout each period in time.

Finally, as an additional robustness test and following Bruhn and Love (2014), we control for the possibility that linear time trends in outcome variables may be different between loan-specific portfolios. Formally, we test whether our results still hold once we control for different bank-specific time trends in the regressions. As such, we estimate the following regression:

$$Y_{ijt} = \alpha + \gamma D_j + \lambda T_t + \theta_i(Z_i \times T_t) + \beta\,(D_{ij} \times T_t) + \delta^{'} X_{ijt} + \epsilon_{ijt}, \qquad (4)$$

where again, α, γ and λ are the bank, portfolio, and time fixed effects, respectively and Z_i contain all bank-specific dummies. thus, the additional interaction, θ_i accounts for the different bank-specific linear time trends.

4 Results

4.1 DID Exercises

This section presents results for the Differences-in-Differences (DID) exercises. Given a delay in the implementation of the legislation in 2007QI, we exclude this quarter (2007QI) from our analysis.

We consider four different definitions of the control group – i.e. corporate credit: (i) the first refers to the full sample; (ii) the second restricts the sample to loans that

do not exceed 25 monthly minimum wages (MMW); (iii) the third narrows in on loans with a larger ex-ante probability of default, and which comprises only Small and Medium Enterprises (SMEs), as reported by the Financial Superinentency; and (iv) the fourth constrains the control group by using the last two definitions (ii) + (iii). In brief, these specifications intend to make both microcredit and corporate portfolios more comparable. The reason why we trim the sample to 25 MMW in specification (ii) and (iv), is that microcredit loans, by definition, cannot exceed this amount. We also consider the probability of default in specification (iii) and (iv), since this variable ensures that loan recipients in both groups are SMEs. Additionally, we consider three different time windows: 2005QI-2008QII (reported in the main text), and 2005QI-2008QI, 2005QI-2007QIV (reported in Table 6 of Appendix B).

Tables 2 and 3 display the results of the DID regressions in which our coefficient of interest is labeled as "Interaction" and corresponds to $\hat{\beta}$ in equation (2). Table 2 shows results for the number of new loans (in logarithms) and indicates that, on average, the usury hike in microcredit increases new loans in between 21.5% and 42.4% after treatment. These results hold for all the specifications we consider.[7] Similarly, Table 3 shows that the value of new loans (in logs) rises between 25.1% and 47.8%. Tables 2 and 3 also show that the pre and post-treatment difference for the loan portfolios are negatively significant for most specifications ($\hat{\lambda}$ in equation 2). Therefore, the number and size of new corporate and microcredit loans decrease during the period of study, due to the evolution of time invariant characteristics of both portfolios. This effect, in absolute value, is bigger for the most restrictive definition of corporate loans.

On the other hand, the initial difference between the two groups, captured by the treatment dummy ($\hat{\gamma}$ in equation 2) and conditional on the control variables, is significant in most specifications. So, as noted in Section 2, there were significant initial differences between the two portfolios. The sign and magnitude of these differences seem to depend on the specification. Notably, when we narrow in on corporate loans with SMEs and with amounts less than 25 MMW (most restrictive definition), the effect of the intervention is larger for the number of new loans.

[7]Similar results for different time windows are shown in Table 6 of Appendix B.

Table 2: DID Results for the number of new loans: 2005QI - 2008QII

Variables	Full sample	25 MMW	SMEs	SMEs & 25 MMW
	(1)	(2)	(3)	(4)
Interaction	0.526***	0.400***	0.303**	0.276**
	(0.124)	(0.128)	(0.119)	(0.125)
Period dummy	-0.404***	-0.615***	-0.492***	-0.623***
	(0.0393)	(0.0733)	(0.0490)	(0.0807)
Treatment dummy	-0.0286	1.314***	2.108***	2.763***
	(0.0823)	(0.0837)	(0.0832)	(0.0813)
Constant	-0.381	-3.559***	-5.016***	-4.292***
	(0.609)	(0.846)	(0.682)	(1.062)
Observations	5,027	3,862	4,056	2,864
R-squared	0.475	0.319	0.468	0.476

Authors' calculations. ***, **, and * denotes statistical significance at the 1, 5, and 10 percent level respectively. Columns (1) - (4) show the impact of the usury hike in microcredit loans. Column (1) presents the results for the whole sample; (2) for corporate loans smaller than 25 minimum wages; (3) for corporate loans catered to Small and Medium Enterprises; and (4) for corporate loans banks with the aforementioned restrictions in (2) + (3). Robust standard errors in parentheses.

Table 3: DID Results for the value of new loans: 2005QI - 2008QII

Variables	Full sample	25 MMW	SMEs	SMEs & 25 MMW
	(1)	(2)	(3)	(4)
Interaction	0.480***	0.497***	0.282**	0.499***
	(0.145)	(0.139)	(0.125)	(0.135)
Period dummy	-0.0601	-0.443***	-0.249***	-0.641***
	(0.0530)	(0.0918)	(0.0567)	(0.0989)
Treatment dummy	-4.212***	1.457***	-0.524***	2.811***
	(0.0944)	(0.0890)	(0.0854)	(0.0882)
Constant	2.897***	-2.271**	-8.986***	-5.243***
	(0.947)	(0.903)	(1.374)	(1.680)
Observations	4,951	3,702	3,972	2,748
R-squared	0.636	0.291	0.342	0.479

Authors' calculations. ***, **, and * denotes statistical significance at the 1, 5, and 10 percent level respectively. Columns (1) - (4) show the impact of the usury hike in microcredit loans. Column (1) presents the results for the whole sample; (2) for corporate loans smaller than 25 minimum wages; (3) for corporate loans catered to Small and Medium Enterprises; and (4) for corporate loans banks with the aforementioned restrictions in (2) + (3). Robust standard errors in parentheses.

We present the set of bank-specific and loan-specific controls of our DID exercise in Table 7 of Appendix B. As shown, liabilities and total loans positively affect credit expansion. Bank Securities on the other hand, seem to have a crowding out effect on loans across the different specifications, as suggested in Ghosh et al. (2018). On the other hand, Non-Banking Correspondents have an almost null effect on loan expansion (most coefficients are not significant, and the size is almost negligible). The amount of net foreign holdings relative to equity, with a few exceptions, has a general negatively effect (probably due to a lesser degree of substitution between different currency-denominated assets). Most of the loan-specific controls (provisions, expected dividends, re-discounted rates, loan maturity, and value of collateral) are small and mostly non-significant. Further, our main results hold regardless of which controls are included and the order in which they are introduced.

Nonetheless, as an additional robustness test, and following Bruhn and Love (2014), we control for the possibility that linear time trends in outcome variables may be different between bank-specific portfolios. Results of equation 4 are shown in Table 4, accounting for the interaction of bank and time fixed effects. As observed, our results hold and are very similar to those presented in Tables 2 and 3, for all the specifications considered.

Table 4: DID Results allowing for different linear bank-time trends

	Logarithm of the number of new loans				Logarithm of the amount of new loans			
Variables	Full sample (1)	25 MMW (2)	SMEs (3)	SMEs & 25 MMW (4)	Full sample (5)	25 MMW (6)	SMEs (7)	SMEs & 25 MMW (8)
	2005-I 2008-II							
Interaction	0.443***	0.627***	0.445***	0.741***	0.378**	0.730***	0.398**	1.041***
	(0.108)	(0.122)	(0.112)	(0.132)	(0.170)	(0.191)	(0.163)	(0.188)
Treatment dummy	-0.263***	1.197***	1.754***	2.961***	-4.375***	1.145***	-0.946***	2.689***
	(0.0717)	(0.0786)	(0.0728)	(0.0808)	(0.0967)	(0.102)	(0.0888)	(0.0915)
Bank-specific time trends	YES	YES	YES	YES	YES	YES	YES	YES
Observations	5,026	3,862	4,056	2,864	4,950	3,702	3,972	2,748
R-squared	0.933	0.899	0.901	0.910	0.969	0.898	0.954	0.916

Authors' calculations. ***, **, and * denotes statistical significance at the 1, 5, and 10 percent level respectively. Columns (1) - (8) show the impact of the usury hike in microcredit loans. Column (1) and (5) presents the results for the whole sample; (2) and (6) for corporate loans smaller than 25 minimum wages; (3) and (7) for corporate loans catered to Small and Medium Enterprises; and (4) and (8) for corporate loans banks with the aforementioned restrictions in (2) + (3). Robust standard errors in parentheses.

4.2 Parallel trends

This section presents graphical and statistical evidence on the parallel trends assumption, crucial for identifying causal effects. As described in equation 3, we expect interaction terms previous to 2007 (i.e. β_τ) to be not significant. Figure 2 depicts the coefficients of interaction terms using Newey-West standard errors.[8] To clarify, results are based on a weekly panel data analysis but we include quarterly dummies so as to avoid over-parameterizing the model. For expositional purposes, we only consider the two most restricted definitions of the control group: the one that restricts the sample to SMEs, and the one that restricts to both SMEs and to a maximum loan size of 25 MMW. Figures 4d and 5d in Appendix B show results for these same definitions but for alternative time periods: 2005QI-2008QI and 2005QI-2008QIV.

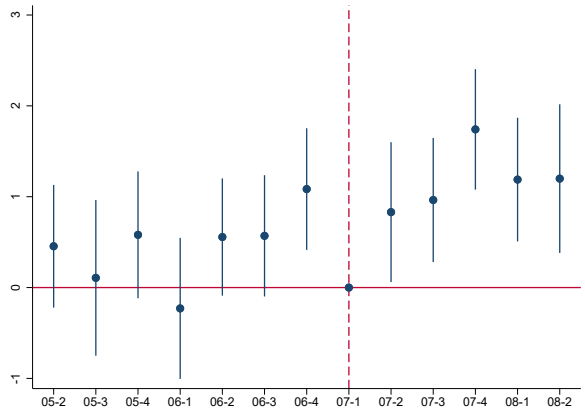
Figure 2 shows that all β_τ coefficients previous to treatment are not significant for all panels, except for the 06-04 quarter in panels (a) and (b).[9] After treatment, the figure shows significant and positive results for $\beta_{-\tau}$ that are consistent to those reported in Tables 2 and 3. The effects later subside towards the second quarter of 2008, which is reasonable, given that that the sudden increase in the microcredit usury rate (relative to corporate loans) also lasts for approximately one year, before the two usury rates converge in 2008 (see Section 2).

Finally, Table 5 shows a more formal statistical test proposed in Angrist and Pischke (2008), where the null is $H_o : \beta_1 = \beta_2 = ... = \beta_q = 0$. Results show that, at the 5% significance level, there is no evidence to reject the null, indicating that all interaction coefficients previous to 2007 are not significant. Again, this holds for different time windows and definitions of the control group. Thus, we conclude that the parallel trends assumption holds.

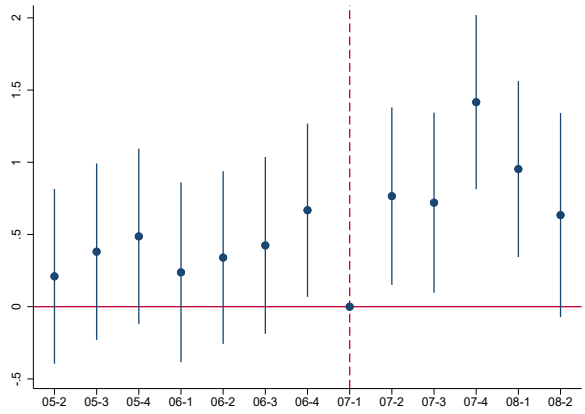
[8] Additional exercises (not reported) using the methodology in Driscoll & Kraay, and Beck & Katz yield similar results.

[9] We note that the 06-04 quarter coefficient in panel (b) is not significant at the 5% significance level.

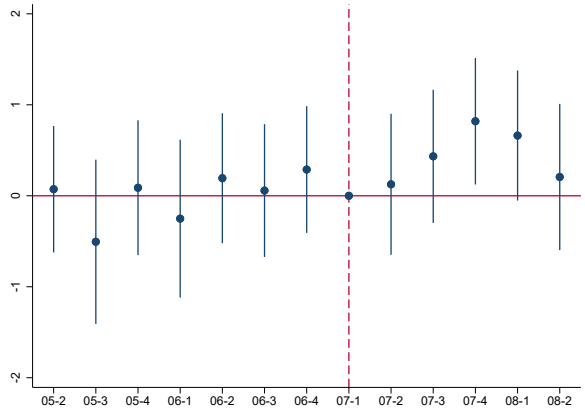
Figure 2: Interaction coefficients to test for parallel trends: 2005QI-2008QII



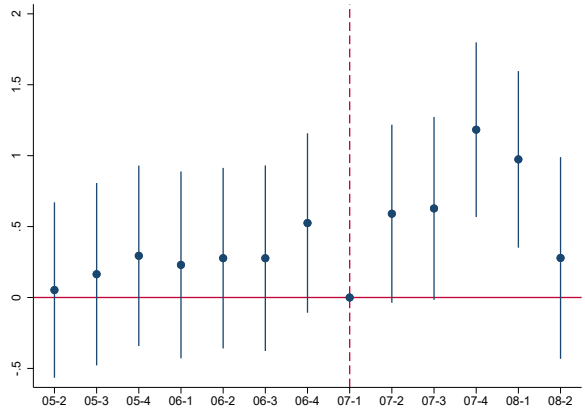
(a) Value of new loans (SMEs and 25 MMW)



(b) Number of new loans (SMEs and 25 MMW)



(c) Value of new loans (SMEs)



(d) Number of new loans (SMEs)

Authors' calculations. The Figure presents estimates for each interaction coefficient in equation (3), with lines that indicate the width of 90% confidence intervals. All specifications control for bank, and time fixed effects. In our analysis, we exclude 2007QI due to a delay in the implementation of the program, which is why the coefficient interaction for this period is, by construction, zero.

Table 5: Parallel trends test

Dependent variable	SMEs (1)		SMEs & 25 MMW (2)	
2005QI - 2008QI				
Ln of the number of new loans	F(6, 3995)	0.31	F(6, 2804)	0.39
	p-value	0.93	p-value	0.88
Log of the amount of new loans	F(6, 3911)	0.55	F(6, 2689)	1.80
	p-value	0.77	p-value	0.09
2005QI - 2008QII				
Log of the number of new loans	F(6, 3637)	0.42	F(6, 2551)	0.44
	p-value	0.87	p-value	0.85
Log of the amount of new loans	F(6, 3555)	0.66	F(6, 2439)	1.85
	p-value	0.64	p-value	0.09
2005QI - 2007QIV				
Log of the number of new loans	F(6, 3318)	0.48	F(6, 2324)	0.45
	p-value	0.83	p-value	0.84
Log of the amount of new loans	F(6, 3244)	0.72	F(6, 2219)	1.92
	p-value	0.63	p-value	0.07

Authors' calculations. The Table presents F-tests where the null hypothesis is that all coefficients previous to intervention are equal to zero, $H_O : \beta_{2005QII} = \cdots = \beta_{2006QIV} = 0$. We cannot reject the null at the 95% level of confidence, which indicates that the parallel trends assumption holds. For these specifications we control for bank, and time fixed effects.

5 Conclusions

In this paper we aim to shed light on one of the most controversial barriers to financial inclusion: the enactment of usury rates. More specifically, we study the liberalization of the microcredit usury rate in Colombia during 2007 and its effects on loan expansion. This liberalization was part of a government financial inclusion plan, due to the low levels of access to credit through financial institutions. In this sense, our paper is mainly related to two strands of literature. First, it contributes to the interest rate caps (and more specifically usury rates) literature, in which there is a general lack of consensus regarding its effects. Second, our work relates to a more ample and established literature based on the effects of reducing barriers on financial inclusion.

As part of our identification strategy, we perform a Difference-in-Difference analysis, by comparing the expansion of microcredit loans (treatment group) with that of corporate loans (control group), right after the microcredit ceiling was lifted. Our main finding is that the liberalization policy encouraged and facilitated access to the financial system. Namely, we find that, on average, the amount lent by credit establishments increased between 27.5% and 75.7%, and the number of new loans increased between 41.8% and 57.7%. Therefore, we conclude that the liberalization policy had a positive impact on credit access in the financial system .

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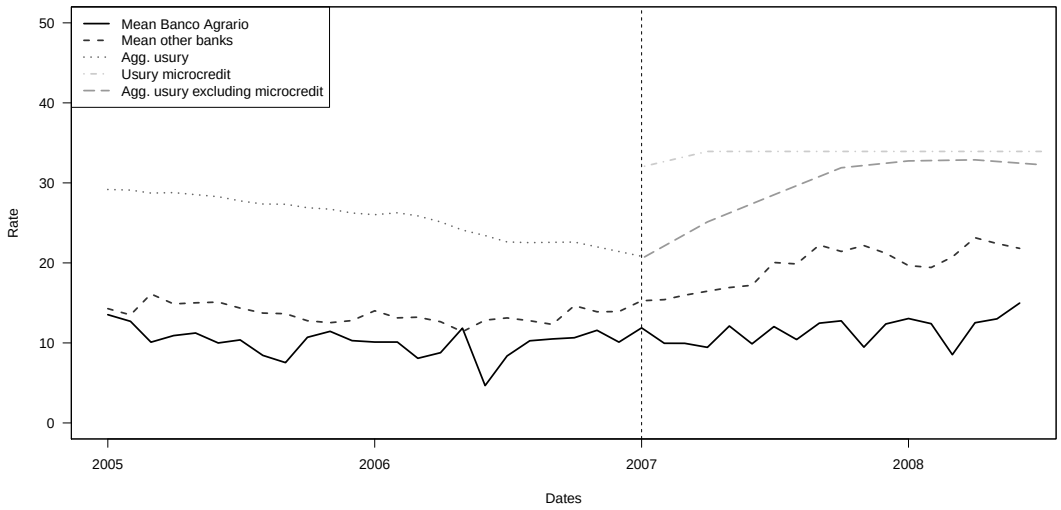
Appendix A State Owned Bank: *Banco Agrario*

Figure 3: Mean rate for loans disbursed by *Banco Agrario* vs other banks



Appendix B Robustness

Table 6: Alternative Time Windows for DID exercise: 2005QI-2008QI, 2005QI-2007QIV

Variables	Logarithm of the number of new loans				Logarithm of the value of new loans			
	Full sample	25 MMW	SMEs	SMEs & 25 MMW	Full sample	25 MMW	SMEs	SMEs & 25 MMW
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				2005-I 2008-I				
Interaction	0.608***	0.547***	0.546***	0.590***	0.441***	0.578***	0.446***	0.699***
	(0.128)	(0.130)	(0.123)	(0.126)	(0.150)	(0.144)	(0.129)	(0.138)
Period dummy	-0.428***	-0.761***	-0.619***	-0.812***	-0.0381	-0.548***	-0.350***	-0.751***
	(0.0424)	(0.0788)	(0.0506)	(0.0904)	(0.0566)	(0.101)	(0.0584)	(0.105)
Treatment dummy	-0.0271	1.364***	2.165***	2.804***	-4.212***	1.490***	-0.462***	2.848***
	(0.0820)	(0.0831)	(0.0802)	(0.0814)	(0.0943)	(0.0891)	(0.0805)	(0.0887)
Constant	-0.355	-4.062***	-5.055***	-4.404***	3.346***	-2.321**	-9.265***	-4.473***
	(0.647)	(0.884)	(0.697)	(1.086)	(1.000)	(0.939)	(1.352)	(1.624)
Observations	4,594	3,540	3,696	2,609	4,518	3,387	3,614	2,496
R-squared	0.484	0.349	0.483	0.502	0.647	0.296	0.345	0.487
				2005-I 2007-IV				
Interaction	0.553***	0.540***	0.520***	0.623***	0.364**	0.616***	0.420***	0.750***
	(0.143)	(0.142)	(0.135)	(0.142)	(0.170)	(0.165)	(0.147)	(0.158)
Period dummy	-0.425***	-0.792***	-0.619***	-0.911***	-0.0173	-0.598***	-0.330***	-0.823***
	(0.0461)	(0.0858)	(0.0542)	(0.100)	(0.0617)	(0.115)	(0.0638)	(0.111)
Treatment dummy	-0.0269	1.380***	2.168***	2.809***	-4.210***	1.499***	-0.464***	2.846***
	(0.0819)	(0.0822)	(0.0796)	(0.0807)	(0.0942)	(0.0887)	(0.0804)	(0.0884)
Constant	-0.228	-4.117***	-5.923***	-4.857***	3.574***	-2.508**	-10.32***	-4.528***
	(0.690)	(0.937)	(0.777)	(1.173)	(1.054)	(1.010)	(1.490)	(1.677)
Observations	4,187	3,219	3,375	2,379	4,118	3,078	3,301	2,274
R-squared	0.482	0.355	0.483	0.499	0.646	0.294	0.337	0.479

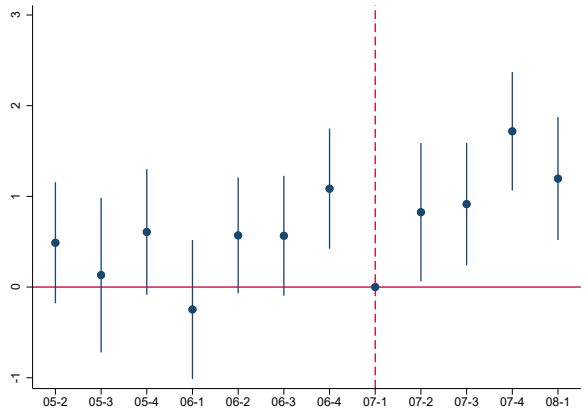
Authors' calculations. ***, **, and * denotes statistical significance at the 1, 5, and 10 percent level respectively. Columns (1) - (4) show the impact of the usury hike in microcredit loans. Column (1) and (5) presents the results for the whole sample; (2) and (6) for corporate loans smaller than 25 minimum wages; (3) and (7) for corporate loans catered to Small and Medium Enterprises; and (4) and (8) for corporate loans banks with the aforementioned restrictions in (2) + (3). Robust standard errors in parentheses.

Table 7: DID Results showing all control variables: 2005QI-2008QII

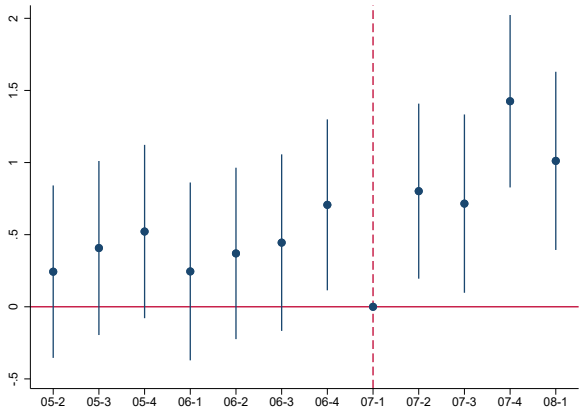
Variable	Logarithm of the number of new loans				Logarithm of the value of new loans			
	Full sample	25 MMW	SMEs	SMEs & 25 MMW	Full sample	25 MMW	SMEs	SMEs & 25 MMW
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	2005-I 2008-II							
Interaction	0.526***	0.400***	0.303**	0.276**	0.480***	0.497***	0.282**	0.499***
	(0.124)	(0.128)	(0.119)	(0.125)	(0.145)	(0.139)	(0.125)	(0.135)
Period dummy	-0.404***	-0.615***	-0.492***	-0.623***	-0.0601	-0.443***	-0.249***	-0.641***
	(0.0393)	(0.0733)	(0.0490)	(0.0807)	(0.0530)	(0.0918)	(0.0567)	(0.0989)
Treatment dummy	-0.0286	1.314***	2.108***	2.763***	-4.212***	1.457***	-0.524***	2.811***
	(0.0823)	(0.0837)	(0.0832)	(0.0813)	(0.0944)	(0.0890)	(0.0854)	(0.0882)
Bank-Specific								
Ln liabilities	1.586***	1.517***	1.084***	1.207***	1.366***	1.576***	0.423***	0.973***
	(0.103)	(0.144)	(0.117)	(0.175)	(0.129)	(0.156)	(0.148)	(0.197)
Ln total loans	0.0263	0.197***	0.296***	0.182**	0.209***	0.172**	0.895***	0.350***
	(0.0526)	(0.0707)	(0.0581)	(0.0881)	(0.0767)	(0.0773)	(0.106)	(0.131)
Ln securities	-0.262***	-0.528***	-0.370***	-0.576***	-0.0356	-0.509***	-0.274***	-0.537***
	(0.0281)	(0.0544)	(0.0314)	(0.0725)	(0.0304)	(0.0544)	(0.0307)	(0.0652)
Ln equity	-1.174***	-0.831***	-0.675***	-0.622***	-1.127***	-0.890***	-0.568***	-0.542***
	(0.0645)	(0.0763)	(0.0696)	(0.0890)	(0.0750)	(0.0841)	(0.0763)	(0.0959)
Non Banking Correspondents	6.95e-05	4.49e-05	-0.000251***	6.39e-05	0.000752***	0.000186	0.000117	-0.00109***
	(0.000103)	(0.000190)	(8.16e-05)	(0.000114)	(4.98e-05)	(0.000143)	(0.000142)	(0.000136)
Foreign Holdings	-3.082***	-3.811***	-0.0251	-0.410	1.419**	-3.620***	2.655***	0.975
	(0.469)	(0.777)	(0.648)	(0.909)	(0.691)	(1.011)	(0.743)	(1.136)
Loan-Specific								
Value of collateral	8.28e-09	-0.000195	-0.000137	-0.000730***	1.85e-06***	-9.07e-05	8.91e-05	-0.000659***
	(4.15e-07)	(0.000147)	(0.000139)	(0.000186)	(5.38e-07)	(0.000156)	(0.000167)	(0.000207)
Loan maturity	-5.83e-05*	-0.000161	-0.000106	-0.000146	4.26e-05	-0.000135	2.07e-05	-0.000119
	(3.25e-05)	(0.000221)	(0.000172)	(0.000167)	(4.33e-05)	(0.000180)	(5.74e-05)	(0.000136)
Re-discounted rate	0.00857***	0.00335***	0.00320***	0.00227***	0.00427***	0.00290***	0.00231***	0.00197***
	(0.000391)	(0.000525)	(0.000551)	(0.000442)	(0.000514)	(0.000451)	(0.000491)	(0.000357)
Expected dividends	-3.89e-06**	-0.00168	-0.00444***	-0.00670***	-6.31e-06**	-0.00164	-0.00426***	-0.00771***
	(1.92e-06)	(0.00121)	(0.00114)	(0.000898)	(2.91e-06)	(0.00120)	(0.00126)	(0.000984)
Loan provisions	2.57e-07	0.000274***	0.000437***	0.000631***	2.00e-06***	0.000275***	0.000411***	0.000716***
	(2.46e-07)	(7.69e-05)	(7.07e-05)	(6.25e-05)	(3.34e-07)	(7.42e-05)	(7.55e-05)	(6.25e-05)
Constant	-0.381	-3.559***	-5.016***	-4.292***	2.897***	-2.271**	-8.986***	-5.243***
	(0.609)	(0.846)	(0.682)	(1.062)	(0.947)	(0.903)	(1.374)	(1.680)
Observations	5,027	3,862	4,056	2,864	4,951	3,702	3,972	2,748
R-squared	0.475	0.319	0.468	0.476	0.636	0.291	0.342	0.479

Authors' calculations. ***, **, and * denotes statistical significance at the 1, 5, and 10 percent level respectively. Columns (1) - (4) show the impact of the usury hike in microcredit loans. Column (1) and (5) presents the results for the whole sample; (2) and (6) for corporate loans smaller than 25 minimum wages; (3) and (7) for corporate loans catered to Small and Medium Enterprises; and (4) and (8) for corporate loans banks with the aforementioned restrictions in (2) + (3). Robust standard errors in parentheses.

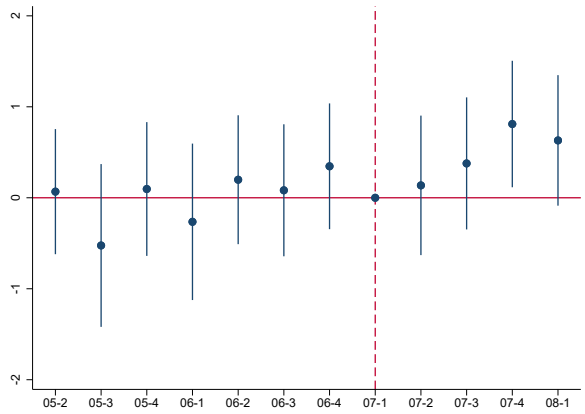
Figure 4: Interaction coefficients to test for parallel trends: 2005QI-2008QI



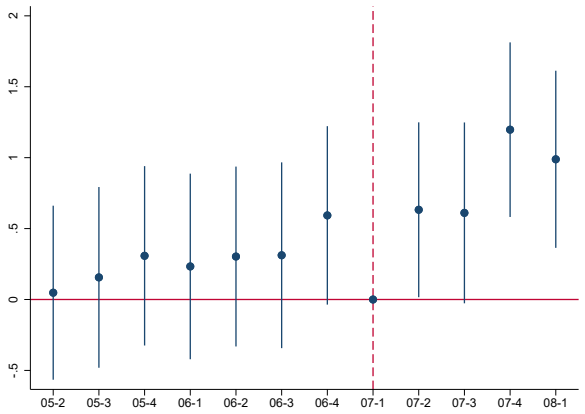
(a) Logarithm of amount - SMEs and 25 MLCW



(b) Logarithm of number - SMEs and 25 MLCW



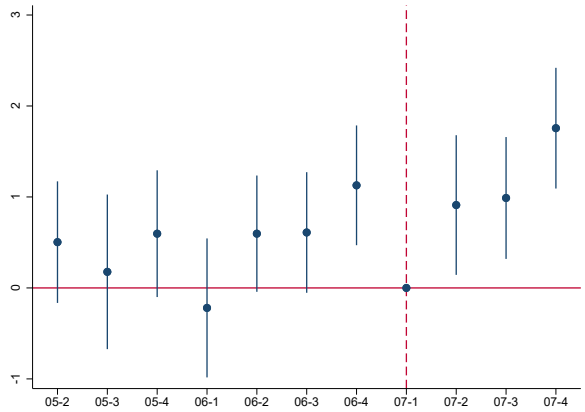
(c) Logarithm of amount - SMEs



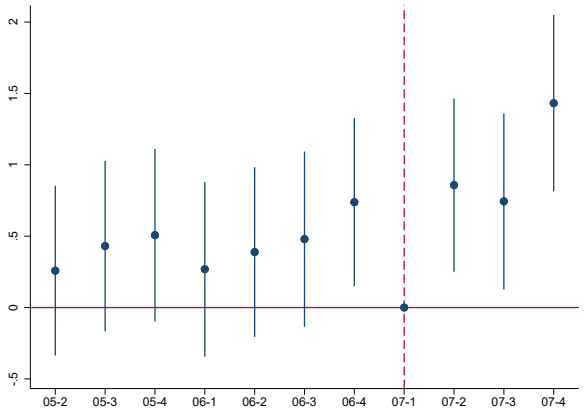
(d) Logarithm of number - SMEs

Authors' calculations. The Figure presents estimates for each interaction coefficient in equation (3), with lines that indicate the width of 90% confidence intervals. All specifications control for bank, and time fixed effects. In our analysis, we exclude 2007QI due to a delay in the implementation of the program, which is why the coefficient interaction for this period is, by construction, zero.

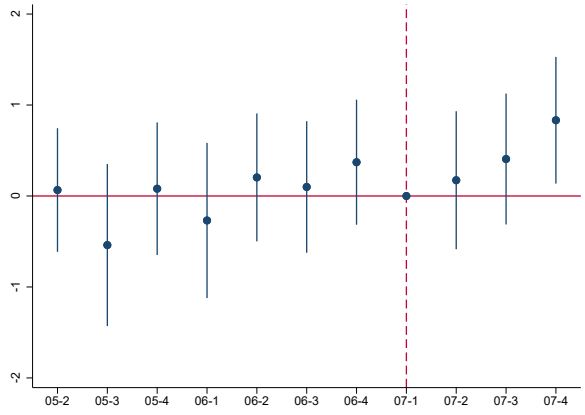
Figure 5: Interaction coefficients to test for parallel trends: 2005QI-2007QIV



(a) Logarithm of amount - SMEs & 25 MLCW

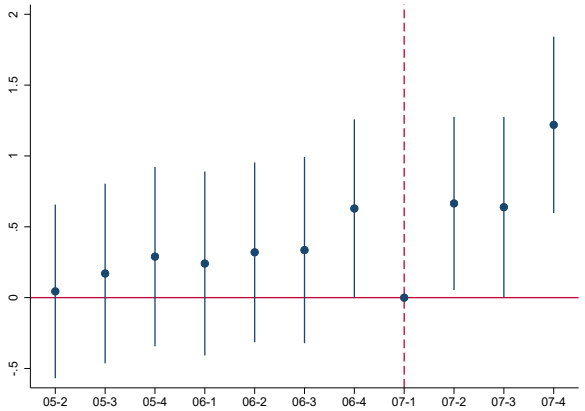


(b) Logarithm of number - SMEs & 25 MLCW



(c) Logarithm of amount - SMEs

I



(d) Logarithm of number - SMEs

Authors' calculations. The Figure presents estimates for each interaction coefficient in equation (3), with lines that indicate the width of 90% confidence intervals. All specifications control for bank, and time fixed effects. In our analysis, we exclude 2007QI due to a delay in the implementation of the program, which is why the coefficient interaction for this period is, by construction, zero.

Appendix C Data

Table 8: Data description

Variable	Units in COP	Frequency
Number of new loans	Number of loans	Weekly
Value of new loans	10^6	Weekly
Liabilities	10^9	Monthly
Total Loans	10^9	Monthly
Securities	10^9	Monthly
Equity	10^9	Monthly
Non banking correspondents	Number of NBCs	Monthly
Foreign Holdings	% share to equity	Weekly
Value of collateral	10^6	Weekly
Loan maturity	Number of days	Weekly
Re-discounted rate	%	Weekly
Expected dividends	10^6	Weekly
Provisions	10^6	Weekly

Our data come from the Colombian Financial Superintendency. We observe banks and other credit establishments, such as companies (trust, insurance, and capitalization) and cooperatives. Specifically, we use the 341-Form which reports, at a daily frequency (although in quarterly reports), every loan along with its counterpart across the microcredit, corporate, and households portfolios.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