Examining Macroprudential Policy through a Microprudential Lens

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Examining Macroprudential Policy through a Microprudential Lens*

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Abstract

In this paper, we examine the financial and real effects of macroprudential policies with a new identifying strategy that exploits borrower-specific provisioning levels for each bank. Locally, we compare similar firms just below and above regulatory thresholds established in Colombia during 2008–2018 for the corporate credit portfolio. Our results indicate that the scheme induces banks to increase the provisioning cost of downgraded loans. This implies that, for loans with similar risk but with a discontinuously lower rating, banks offer a lower amount of credit, demand higher quality guarantees, and impose a higher level of provision coverage through the loan-loss given default. To illustrate, a 1 percentage point (pp) increase in the provision-to-credit ratio leads to a reduction in credit growth of up to 15pp and lowers the probability of receiving new credit by up to 11pp. When mapping our results to the real sector, we find that downgraded firms are constrained in their investment decisions and experience a contraction in liabilities, equity, and total assets.

Keywords: Loan provisions; Macroprudential policies; Credit registry; Corporate registry; Panel regression discontinuity

JEL Codes: G18; G21; G28

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Examinando la política macroprudencial por medio de una visión microprudencial

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Resumen

Este documento estudia los efectos financieros y reales de la política macroprudencial asociada con el nivel de gasto en provisiones por deudor que debe realizar el sector bancario en Colombia. Por consiguiente, se estudia la cartera de créditos comerciales durante el periodo 2008-2018 y se compara localmente aquellas empresas que se aproximan por arriba y por debajo a los umbrales regulatorios relacionados con el cálculo del gasto en provisiones. Los resultados indican que el esquema de provisiones induce a los bancos a incrementar su gasto en provisiones dirigido a las firmas que presentan una reducción en su calificación. Lo anterior implica que, para créditos con un perfil de riesgo similar, pero con un cambio discontinuo en su calificación, los bancos otorgan un menor monto de crédito, demandan garantías de mejor calidad e imponen una mayor cobertura por provisiones por medio de la medida de pérdida ante incumplimiento. En particular, un aumento de un punto porcentual (pp) en la razón de provisiones a crédito conlleva a una reducción de hasta 15 pp en la tasa de crecimiento del crédito y reduce la probabilidad de acceder a un nuevo crédito en hasta 11 pp. Por su parte, el análisis que vincula estos resultados con las principales variables de desempeño de las firmas sugiere que las firmas que exhiben una reducción en su calificación son posteriormente restringidas en sus decisiones de inversión y experimentan una contracción en sus pasivos, patrimonio y activos.

Palabras clave: Provisiones; Políticas macroprudenciales; Registros crediticios; Registros corporativos; Regresión discontinua de panel

Códigos JEL: G18; G21; G28

1 Introduction

Since the financial crisis of 2007–2009, there has been an increasing interest in the effects of financial regulations —recently seen through an integrated policy framework— and which can guide central banks in promoting and sustaining financial stability (Boz et al., 2020; Erceg et al., 2020). In response to the crisis, and under the Basel III Accord, a set of new macroprudential measures were enacted to avoid the build-up of systemic vulnerabilities and allow the banking system to support the real economy through the economic cycle. One of these measures, consisting of countercyclical buffers (procyclical requirements), lies at the heart of our investigation.

Paradoxically, there is still scant evidence on the financial and real effects of these policies, in part due to the various confounding factors coupled with limited data availability. In addition, macroprudential policies usually apply to all banking institutions, complicating the identification of exogenous cross-section variation. Ultimately, separating out demand-driven factors is challenging since borrowers can simultaneously respond to economic conditions and financial regulations.

Using Colombia as case study during 2008Q1–2018Q4, we exploit a discontinuous loan provisioning scheme enacted by the Financial Regulator (*Superintendencia Financiera de Colombia*), based on the number of days past due (i.e., days in arrears), to measure the financial and real effects of this macroprudential policy on firms. Specifically, loans that exceeded certain thresholds: 30, 60, and 90 past-due days, were subject to higher (and thus costlier) provisioning levels. Within this setup, we argue that firms just below and above these thresholds are ex-ante similar (and comparable) across various types of financial variables.

For identification, we conduct a fuzzy Regression Discontinuity Design (RDD) to evaluate the impact on credit conditions. To our knowledge, we are the first to evaluate the effects of provision buffers using this approach. We argue that locally, within the vicinity of the triggering thresholds, the requirements dictated by the provisioning scheme become uncoupled from demand factors. In our estimations, we rely on the covariate-adjusted, biased-corrected, local linear estimators developed in Calonico et al. (2014) and Calonico et al. (2019). Additionally, we include firm size, firm sector, and bank fixed effects, and cluster standard errors at the bank-firm level.

In a second step, we study how the RDD results of the provisioning scheme carry over to the real economy. Using a difference in difference (DiD) approach and collapsing the data into an annual firm panel, we assess if the change in credit conditions induces a change in the performance of firms. Specifically, we follow Callaway and Sant'Anna (2021) to exploit the heterogeneous effects of each cohort of treated units at each period, relying on "group-time average treatment effects".

Our study exploits highly granular data. We use the entire Colombian credit registry of banks to corporates (15.6 million observations) with information on each loan that includes: amount, collateral, provision, delinquency rate, and credit rating. To map our analysis to the real economy, we merge these data with yearly firm-level balance sheet information (172,718 observations) from the Colombian Business Registry (*Superintendencia de Sociedades de Colombia*). Our resulting data contain 27 banks, 954,066 firms, 1.3 million existing loans, and 530,142 new loans.

We find that the Administrative Credit Risk System (*Sistema de administración de riesgo de crédito*, SARC) scheme induces banks to increase the provisioning cost of downgraded loans. This implies that, for loans with similar risk but with a discontinuously lower rating, banks offer a lower amount. Using a back-of-the-envelope calculation, a 1 percentage point (pp) increase in the provision-to-credit ratio leads to a reduction in credit growth of 15pp and 4pp at the 30- and 60-day cutoffs, respectively. Banks also demand higher quality guarantees (30-day cutoff) and impose a higher level of provision coverage through the loan-loss given default (60-day cutoff).

Regarding the extensive margin, our results indicate that, at the 30- and 60-day cutoffs, the higher provisioning cost induces banks to reduce the disbursement of new credit lines to firms of similar risk. Specifically, at the 30- and 60-day cutoffs, a 1pp increase in the provision-to-credit ratio leads to a reduction of 11pp and 2pp, respectively, in the probability of receiving disbursements net-of-amortizations, eight quarters ahead of treatment.

In addition, our DiD exercises used to evaluate the real effects of the policy around the 30-day cutoff indicate that, after the downgrade shock, the loan amount granted to firms is reduced, in line with our previous RDD results. More generally, this result confirms the inability of firms to smooth out their debt across different sources of financing. Consistent with this, firms also experience a contraction in liabilities but of a lower magnitude. Further, equity diminishes together with total assets.

Regarding the revenue performance of the treated firms, we do not find significant changes in the operating ROA, and we observe a full recovery in total profits one year after the downgrade. The above suggests a recovery of non-operating earnings. In terms of the firm balance structure, results point out that, given the lower amount of credit, firms' dependency on equity funding increases. At the same time, we find that, on average, firms present a higher cash-to-assets ratio after a downgrade. Finally, we find that just after the event, investment decisions of downgraded firms are constrained in terms of the share of net property, plant, and equipment (PP&E) expenses to assets.

In the related literature, macroprudential policies (e.g., reserve and capital requirements, limits on foreign holdings, loan-to-value, funding and liquidity ratios, among others) have generally been employed to explore the effects on financial variables at the aggregate level.[1] However, few papers have discerned the financial effects across firm borrowing, and even fewer have fully mapped their effects to the real sector. Perhaps the paper closest to ours is Jiménez et al. (2017), who evaluate the impact on credit supply cycles and real effects by exploiting changes in Spain's provisioning scheme. The authors use a DiD approach to compare banks differently affected by the shock (lender-specific); in contrast, our shock is

[1]A survey on macroprudential supervision can be found in Galati and Moessner (2018). Also, Bruno et al. (2017) provide a comparative assessment of macroprudential policies in 12 Asia-Pacific economies. In turn, Cerutti et al. (2017) document the use of macroprudential policies for 119 countries.

borrower-specific.

To our advantage, while the Colombian regulation is officially categorized as a macroprudential policy (it extends to the entire financial system), it nonetheless contains microprudential features (oversight is dictated at the firm level). Hence, the fact that we observe an across-the-financial-sector policy that is also borrower-specific allows us to exploit useful heterogeneity, i.e., each bank, depending on the quality of their borrowers, faces different provisioning costs.

In the Latin American region, several other countries have adopted similar provisioning schemes: Bolivia, Uruguay, and Peru have enacted dynamic provisioning, while Argentina, Chile, Ecuador, and Mexico have enacted constant provisioning. In Colombia, we highlight the work of Morais et al. (2021), which evaluates the introduction of the SARC scheme (2006–2008) with an event study analysis. The authors argue that provisions are concentrated on smaller borrowers and use a DiD estimation based on firm size to assess whether credit supply was weaker for riskier firms. Gómez et al. (2020) also evaluate the scheme's introduction (2006–2009) by regressing a bank-level ratio of provisions-to-loans on credit growth. In the same vein, López et al. (2014) study the introduction of the policy (2007) by using a Propensity Score Matching method to evaluate the effect on loan amount.

Our contribution to the aforementioned literature is mainly twofold. First, our RDD methodology allows us to fully disentangle demand shocks by comparing credits to similar firms. Second, our sample extends for over a decade and is not confined to a specific time period, such as when the policy was introduced, which could coincide with other specific, potentially confounding events (e.g., changing macroeconomic and financial conditions, fiscal and monetary policies). In sum, our localized approach and broad study period allow us to rule out borrower-specific responses to changing economic conditions and financial regulations.

More generally, our contribution lies on precisely quantifying macroprudential effects (countercyclical provisioning scheme) in a context where the mainstream has been the implementation of countercyclical capital buffers and new methods of expected credit losses in the measurement of provisions (as recommended by Basel III and IFRS 9 standards). Regarding the latter, in the aftermath of the 2020 market turmoil brought forth by the Covid-19 pandemic, Borio and Restroy (2020) and Zamil (2020) document that regulatory agencies allow for a delayed introduction of this regulation —in light of its procyclicality— especially after an abrupt change in economic conditions. In this sense, our results support the relevance of a countercyclical design of provision expenses, given the effect on credit when provisioning costs are raised. Moreover, our real-sector findings highlight plenty of unintended consequences pertaining to macroprudential policies. Therefore, we believe policymakers can profit from this investigation by considering these findings when designing macroprudential tools.

The rest of this paper proceeds as follows. Section 2 describes the Colombian SARC scheme and presents the data. Section 3 elaborates on the fuzzy RDD and DiD empirical strategies. Section 4 presents the financial and real effects of the provisioning scheme. Finally, Section 5 briefly concludes.

2 Regulatory background and data

In 2007, the Colombian Financial Regulator (*Superintendencia Financiera de Colombia*, SFC) modified the Administrative Credit Risk System (*Sistema de administración de riesgo de crédito*, SARC) by introducing a benchmark model to calculate provision expenses in corporate and consumer credit portfolios. This framework defines provisions for credit i as expected credit losses, EL_i, as follows:[2]

$$\mathrm{EL}_i = \mathrm{EXP}_i \cdot \mathrm{PD}_i \cdot \mathrm{LGD}_i,$$

[2]Before the expected-credit loss model, provisions were defined based on ex-post default, i.e., incurred credit losses.

where EXP_i is the exposure at default (credit stock and interest payments), PD_i is the probability of default, and LGD_i is the loss given default. In addition, for the corporate portfolio (which is the focus of our investigation), regulation differentiates PD according to firm size, credit quality, and credit cycle at the entity level.[3]

As shown in Table 1, the regulation defines two phases: accumulative and contraction. These features align with a countercyclical framework that requires higher provisions in normal times (designed to cool credit booms) and lower provisions in stress periods (to mitigate credit crunches). In Colombia, these stress periods are defined according to specific variables at the entity level. Notwithstanding, in our evaluation period of 2008Q1–2018Q4, only the accumulative phase was active.

Table 1: Probability of default (PD) in the benchmark model

Debtor Size	AA	A	BB	B	CC	Default
A. *Contraction phase*						
Other debtors	5.27%	6.39%	18.72%	22.00%	32.21%	100%
Small firm	4.18%	5.30%	18.56%	22.73%	32.50%	100%
Medium firm	1.51%	2.40%	11.65%	14.64%	23.09%	100%
Large firm	1.53%	2.24%	9.55%	12.24%	19.77%	100%
B. *Accumulative phase*						
Other debtors	8.22%	9.41%	22.36%	25.81%	37.01%	100%
Small firm	7.52%	8.64%	20.26%	24.15%	33.57%	100%
Medium firm	4.19%	6.32%	18.49%	21.45%	26.70%	100%
Large firm	2.19%	3.54%	14.13%	15.22%	23.35%	100%
Delinquency days	[0,29)	[30,59)	[60,89)	[90,119)	[120,149)	[150,∞)

Source: Authors' elaboration based on SFC's data.

Regarding credit quality, the norm also establishes the maximum delinquency days that a loan can exhibit before migrating to a worse category. This is the primary source of exogenous variation that we use to uncover the causal effect of the SARC scheme. Specifically, the benchmark model establishes a minimum PD that changes discontinuously at each cutoff. Hence, the rule dictates sufficient conditions for higher provisioning levels at and above each

[3]Regarding firm size, debtors are categorized mainly in the following three groups based on their assets: small (assets $\leq$ 5,000 minimum monthly legal wages —MLW), medium (5,000 MLW $<$ assets $\leq$ 15,000 MLW) and large (assets $>$ 15,000 MLW).

cutoff. Notwithstanding, as will be elaborated in Section 3, the *fuzziness* in our design comes from the fact that banks can assign higher precautionary provisions according to their internal risk assessment.

Under Decree 2817 of 2000, the SFC supervises the SARC's implementation and is entitled to powers guaranteeing its enforcement. While the table makes explicit the discontinuous changes in provisions expenses around the cutoff points: 30, 60, 90, 120, and 150 days in arrears, we restrict our analysis to the first three thresholds (30, 60, 90) given the few observations found in the 120- and 150-day thresholds and the scarce information related with the performance of firms locating close to these cutoffs.[4]

Regarding the data, we employ the entire bank-credit registry to corporates during 2007Q1–2018Q4 (15.6 million observations). This dataset, collected by the SFC, contains quarterly administrative information at the loan level (bank-firm) and comprehends attributes such as the loan amount, economic sector, credit quality, and delinquency days, among others. This information is complemented with yearly firm-level balance sheets from the Colombian Business Registry (*Superintendencia de Sociedades de Colombia*) to study the real effects of provisions (172,718 observations during 2007–2018).

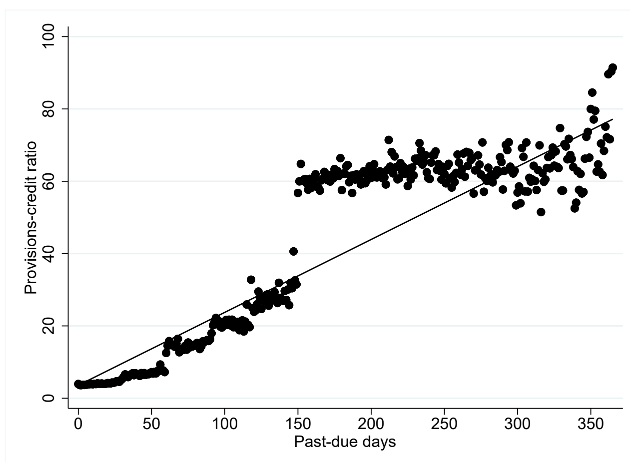
In total, our data contain 27 banks, 954,066 firms, and 1.3 million loans (bank-firm relationships). As described in Section 3, we focus on new loans (379,115 firms and 530,142 credits) to evaluate the impact on credit conditions and complement the analysis by studying the probability of receiving new credit with the whole sample. Unlike the credit registry, the Colombian Business Registry does not require all corporates to present their balance sheets. Therefore, this yearly dataset, used to study the real effects of the provisioning scheme, contains 30,263 firms.[5] As mentioned, we center our analysis on 2008–2018, a decade-long sample since the enactment of the SARC scheme in 2007.

[4]LGD depends on the quality of the collateral and delinquency days, but the first cutoff point associated with delinquency that generates a change in LGD is at 210 days after default. Hence, this discontinuous change is not part of our RDD design.

[5]The Colombian Business Registry mainly collects information from large- and medium-size corporations measured in assets or income (above 30,000 MLW; see Decree 1074 of 2015) or from firms involved in insolvency processes.

Figure 1 shows the positive relationship between risk (measured as past-due days) and the provisions-to-credit ratio for the entire sample of new loans. In line with the SARC guideline, the banking sector assigns a higher provision to risky debtors. In addition, the discontinuities in this measure are visually apparent, with the most significant jump at 150 days (the value that defines a complete loan default). Recall that our analysis does not consider the 120- and 150-day thresholds given the few observations that exhibit such magnitudes in delinquency days and the scarce information related to the performance of those firms. Henceforth, we focus our analysis on the 30-, 60-, and 90-day cutoffs (as depicted in Figure 2 below).

Figure 1: Provision-credit ratio (%) and past-due days



Notes: Equally-spaced-mean scatter plot. Source: Authors' elaboration based on SFC's data.

Table 2 presents descriptive statistics for the main loan variables analyzed. In columns 1–3, we report sample means across the different past-due day brackets: $[0, 29)$, $[30, 59)$, $[60, 89)$, similar to the last row of Table 1. The only variables that discernibly differ across brackets are provisions and rating, namely, our enforcement variables. The other columns (4–9) simply set the stage for our localized approach of the next section, which compares values within the vicinity of the triggering threshold.

Regarding the main firm's performance variables employed in the DiD exercise, Table 3 presents the descriptive statistics for firms that appear in the Colombian Business Registry with delinquency days around the 30-day cutoff point. This table highlights that differences

Table 2: Bank-debtor descriptive statistics (sample means)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Past-due day intervals								
Variable	[0, 29)	[30, 59)	[60, 89)	[25, 29)	[55, 59)	[85, 89)	[30, 35)	[60, 65)	[90, 95)
A. *Enforcement*									
Provision-credit ratio	3.94	6.29	14.25	4.69	7.89	15.80	6.15	14.78	19.85
	(4.18)	(8.31)	(11.94)	(6.04)	(10.37)	(13.85)	(8.65)	(12.99)	(15.93)
Rating	1.07	2.15	3.25	1.19	2.34	3.36	2.17	3.28	4.23
	(0.40)	(0.69)	(0.75)	(0.69)	(0.84)	(0.88)	(0.67)	(0.78)	(0.62)
B. *Baseline covariates*									
Lagged Log of credit	10.81	10.34	10.54	10.58	10.28	10.44	10.75	10.70	10.67
	(2.11)	(1.91)	(1.79)	(1.98)	(1.82)	(1.75)	(1.89)	(1.87)	(1.93)
Annual credit growth	0.09	0.10	0.14	0.02	0.16	0.19	0.04	0.12	0.13
	(1.19)	(1.02)	(0.85)	(1.07)	(0.92)	(0.76)	(0.98)	(0.73)	(0.74)
Lagged quality of guarantee	5.29	5.29	5.03	5.27	5.33	5.09	4.95	4.95	4.85
	(2.17)	(2.12)	(2.08)	(2.16)	(2.09)	(2.10)	(2.15)	(2.03)	(2.05)
Lagged LGD	47.01	47.45	47.12	47.27	48.46	47.93	46.21	46.87	47.05
	(13.31)	(12.45)	(13.70)	(13.01)	(12.60)	(14.03)	(12.71)	(14.15)	(14.35)

Notes: Provision-credit ratio and LGD are in %. Lagged covariates are one-quarter lag. Rating is a numerical scale such that worse ratings correspond to higher values. This is a integer scale where a rating decrease corresponds to a one step in the numerical scale. To calculate quality of guarantee, guarantees were assigned to a numerical scale such that a higher number implies a worse quality. Annual credit growth is the logarithmic difference between the stock of credit at quarters $t-4$ and t, in this order. Standard deviations are reported in parenthesis. Source: Authors' elaboration based on SFC's data.

Table 3: Firm's performance descriptive statistics (sample means)

	(1)	(2)	(3)	(4)
	Past-due day intervals			
Variable	[0, 29)	[30, 59)	[25, 29)	[30, 35)
A. *Balance sheet dynamic*				
Lagged log of credit	12.60	13.03	13.21	13.26
	(2.59)	(2.15)	(2.21)	(2.08)
Lagged log of liabilities	15.12	14.99	15.13	15.06
	(1.54)	(1.46)	(1.46)	(1.45)
Lagged log of equity	15.04	14.68	14.84	14.72
	(1.54)	(1.44)	(1.46)	(1.46)
Lagged log of assets	15.92	15.65	15.81	15.70
	(1.45)	(1.37)	(1.38)	(1.38)
B. *Profitability*				
Lagged ROA	4.64	3.64	3.81	3.45
	(6.08)	(5.67)	(5.56)	(5.42)
Lagged ROA-operating profits	8.60	8.55	8.53	8.28
	(9.48)	(9.05)	(8.81)	(9.07)
C. *Balance sheet composition*				
Lagged equity-to-assets ratio	48.11	43.45	43.50	42.43
	(21.71)	(19.69)	(20.22)	(19.79)
Lagged cash-to-assets ratio	6.31	4.57	4.70	4.30
	(8.10)	(6.82)	(6.92)	(6.42)
Lagged net PP&E expenses-to-assets ratio	3.22	3.25	3.36	3.16
	(5.65)	(5.84)	(5.80)	(5.79)

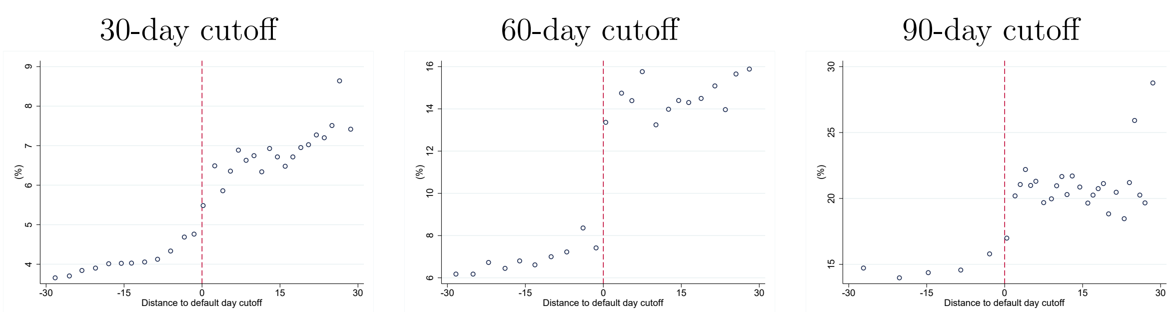
Notes: All variables correspond to a one-year lag with respect to the period when the firm registers delinquency days inside the buckets presented. Variables in Panels (B) and (C) are in %. Standard deviations are reported in parenthesis. Source: Authors' elaboration based on the Colombian Business Registry's data.

between debtors around the 30-day threshold tend to disappear in a statistical sense when observations are closer to the cutoff. The above evidence motivates the more structured approach elaborated in section 3, founded on the similarities of firms one-year before they were at either side of the cutoff.

3 Empirical strategy

To study the financial and real effects of provision buffers, we exploit multiple discontinuities used in the enforcement of the Colombian provisioning scheme, SARC. Specifically, loans that exceeded certain thresholds: 30, 60, and 90 past-due days, were subject to higher (and thus costlier) provisioning levels (Figure 2). Intuitively, the discontinuities embedded in the triggering rule create a quasi-experiment in which we compare loans with similar days in arrears but with different provisioning costs. In the simplest design, observations just above and below each threshold are considered as *treated* and *control*, respectively. Thus, under the continuity-based approach of potential outcomes, causal identification in RDD stems from the discontinuous change in the probability of receiving treatment and the estimation of the outcome variable at either side of the cutoff point.

Figure 2: Provision-credit ratio



Notes: Equally-spaced RD plot based on Calonico et al. (2015). Source: Authors' elaboration based on SFC's data.

We use these discontinuous changes in provision levels to evaluate two sets of empirical exercises at the loan and firm level. First, at the loan level, we employ a fuzzy RDD at each cutoff point to evaluate how loan conditions change due to costlier provisions. Second, we study how the results of the previous exercise impact the real economy. Using a DiD approach, we assess if the change in credit conditions alters the performance of firms.

3.1 Impact on credit conditions

For identification, we employ a fuzzy RDD at each cutoff point, where the treatment assignment differs from perfect compliance as stipulated by the rule (i.e., the rule strongly predicts treatment but not in 100% of cases). Namely, in this setting the probability of being treated jumps discontinuously at each cutoff, but not from 0 to 1, since banks may prudentially assign a worse credit rating (together with a higher provision level) to their debtors before being mandated by regulation.

Formally, we define the assignment of treatment D_{bft} as a dummy variable equal to 1 if bank b assigns a worse rating to firm f in quarter t. We conduct separate exercises for each threshold of delinquency days, $c = 30, 60, 90$.[6] In our fuzzy RDD,

$$\lim_{x \downarrow c} \Pr(D_{bft} = 1 | X_{bft} = x) \neq \lim_{x \uparrow c} \Pr(D_{bft} = 1 | X_{bft} = x), \tag{1}$$

where the running variable X_{bft} denotes days in arrears of each loan. The discontinuities in equation (1) arise from the SARC scheme that dictates banks to worsen debtors' credit rating once $X_{bft} \geq c$.

It follows that, given an outcome variable Y_{bft}, $\mathbb{E}\left[Y_{bft+j} | X_{bft} = x\right] = \alpha^j + \theta^j \mathbf{1}\left\{x \geq c\right\} + \mathbb{E}\left[\varepsilon_{bft+j} | X_{bft} = x\right]$ which implies that:

$$\begin{aligned} &\lim_{x \downarrow c} \mathbb{E}\left[Y_{bft+j} | X_{bft} = x\right] - \lim_{x \uparrow c} \mathbb{E}\left[Y_{bft+j} | X_{bft} = x\right] \\ &= \left(\alpha^j + \theta^j + \lim_{x \downarrow c} \mathbb{E}\left[\varepsilon_{bft+j} | X_{bft} = x\right]\right) - \left(\alpha^j + \lim_{x \uparrow c} \mathbb{E}\left[\varepsilon_{bft+j} | X_{bft} = x\right]\right) = \theta^j, \end{aligned} \tag{2}$$

[6] For 30-, 60-, and 90-day cutoffs, the rating is lowered to A or below, BB or below, and B or below, respectively. Once a credit is treated, it remains in the treatment group. Control observations are those units that have not yet been treated at each period.

where the second equality holds because, by continuity of $\mathbb{E}\left[\varepsilon_{bft+j}|X_{bft}=x\right]$, the right and left limits for the conditional means of the error terms are the same and cancel out.

The fuzzy RDD uses the above discontinuous change in the outcome variable and the probability of being treated to estimate the average causal effect of a higher provision (lower rating). This effect, in period j after treatment, is given by

$$\tau_c^j = \frac{\lim_{x\downarrow c}\mathbb{E}[Y_{bft+j}|X_{bft}=x] - \lim_{x\uparrow c}\mathbb{E}[Y_{bft+j}|X_{bft}=x]}{\lim_{x\downarrow c}\mathbb{E}[D_{bft}|X_{bft}=x] - \lim_{x\uparrow c}\mathbb{E}[D_{bft}|X_{bft}=x]}. \tag{3}$$

Intuitively, equation (3) shows the ratio between the jump in the outcome variable and the share of compliant observations (those that are triggered by the rule and receive treatment). Consequently, τ_c^j captures the local effect of provisions on credit conditions.[7]

For estimation, we rely on the covariate-adjusted, biased-corrected, robust, local linear estimators developed by Calonico et al. (2014) and Calonico et al. (2019). Based on these estimators, we construct Impulse Response Functions (IRFs) in the same vein as Jorda's (2005) method of local projections in up to 8 quarters ahead of treatment, holding the assignment to treatment and control as in the initial period.

Concretely, for the case of no covariates and centering the running variable around the respective cutoff points ($\tilde{X}_{bft}=X_{bft}-c$),[8] our first- and second-stage estimations are based on local non-parametric linear regressions summarized as follows:

$$\arg\min_{\boldsymbol{\gamma}} \sum_{b=1}^{B}\sum_{f=1}^{F_b}\sum_{t=1}^{T-8}(D_{bft}-\gamma_0^j-\gamma_1^j\tilde{D}_{bft}-\gamma_2^j\tilde{X}_{bft}-\gamma_3^j\tilde{X}_{bft}\tilde{D}_{bft})^2 K\left(\frac{\tilde{X}_{bft}}{h_j}\right), \tag{4}$$

$$\arg\min_{\boldsymbol{\beta}} \sum_{b=1}^{B}\sum_{f=1}^{F_b}\sum_{t=1}^{T-8}(Y_{bft+j}-\beta_0^j-\beta_1^j\hat{D}_{bft}-\beta_2^j\tilde{X}_{bft}-\beta_3^j\tilde{X}_{bft}\hat{D}_{bft})^2 K\left(\frac{\tilde{X}_{bft}}{h_j}\right), \tag{5}$$

where $\tilde{D}_{bft}=1\{\tilde{X}_{bft}\geq 0\}$ and $\hat{D}_{bft}$ is the predicted probability of treatment based on the

[7] Notice that, in contrast to an OLS estimator, our design allows for the correlation between the treatment status and the error term. The fundamental identifying assumption is that this error term is not distributed differently in the neighborhood of the cutoff. For a thorough review on the necessary RDD assumptions and applications, we refer readers to Villamizar-Villegas et al. (2022).

[8] To avoid cumbersome notation and facilitate reading, we delete c superscripts henceforth.

running variable and which is used to instrument compliant observations (i.e., equation 5 uses the predicted treatment from equation 4). Also, j denotes the 8 quarters ahead response ($j = 1, ..., 8$), $K(\cdot)$ a triangular kernel function, and h_j the bandwidth used in regressions. The terms $\tilde{X}_{bft}\tilde{D}_{bft}$ and $\tilde{X}_{bft}\hat{D}_{bft}$ allow for different slopes of the running variable at either side of each cutoff.

In our estimations, we follow the robust, biased-corrected estimator of Calonico et al. (2014), who propose a data-driven estimator of the bandwidth and take into account the bias introduced in the point estimator in the inference approach. Based on Calonico et al. (2019), we also control for bank, debtor' sector and size fixed effects, and we cluster standard errors at the bank-firm level.

We focus on new loans (flow variable), as opposed to credit stocks, since it allows for a cleaner identification by filtering out pre-existing loans that would not be expected to react.[9] We evaluate the following "intensive margin" outcome variables: provisions-to-credit ratio (enforcement variable), the log change in credit amount, credit rating, LGD, and collateral quality.

To complement this analysis, we evaluate the extensive margin (i.e., the probability of receiving additional credit). For this, we define a dummy outcome variable switched on if there is an increase in credit from quarter t to $t + j$. The variable stays at one henceforth to evaluate the cumulative effect.

3.2 Impact on firms' performance

To evaluate the effect on firms' performance, we employ yearly balance sheet information from the Colombian Business Registry. Given the low frequency of the data, we can no longer isolate the effects from different past-due day cutoffs, so we focus only on the 30-day threshold. Additionally, we collapse the information of the credit registry into an annual

[9]To evaluate the effects on new loans, at each horizon $t + j$, we restrict the sample to bank-firm relationships with non-negative credit value changes relative to period t. Notice that the change in credit amount can be written as disbursements minus amortizations. Hence, we measure the effects over firms that receive disbursements net-of amortizations.

panel data structure where each observation corresponds to a given firm and year. Therefore, the main caveat is that we are unable to observe the past-due days of each firm with each bank but rather to identify whether a firm was downgraded in its credit rating (i.e., from rating AA to rating A or lower). In the control group, we consider firms that were not yet treated in the analysis period.[10] To further increase comparability, we restrict the sample to firms within a 10-day vicinity radius of the cutoff (20–40 days in arrears) during the whole study period 2008–2018 (for robustness, we present different choices of the comparison group). Following this approach, we move from an RDD to a DiD identification strategy.

In contrast with the RDD exercise, there are reasons to believe that, in the yearly panel, the control and treatment groups differ from each other. For instance, treated firms might participate in riskier activities or perform poorly compared to untreated firms, which can potentially induce a different propensity to be treated. In addition, firms are treated at different periods, and the effect of a downgrade can be dynamic. To this end, we use the methodology presented in Callaway and Sant'Anna (2021), which is suitable to address these challenges. This methodology avoids the problems identified in Goodman-Bacon (2021) and Sun and Abraham (2021) with the traditional two-way fixed effects approach in settings with variation in treatment timing such as this one. It overcomes these issues by exploiting the heterogeneous effects of each cohort of treated units at each period, relying on "group-time average treatment effects". Next, different aggregations of these estimators can be computed to analyze the dynamic effect of treatment.

More formally, a group-time average treatment effect is the expected difference between the outcome at year t for firms that were treated in year g, $Y_{ft}(g)$, and the counterfactual outcome, $Y_{ft}(0)$:

$$ATT(g,t) = \mathbb{E}\left[Y_{ft}(g) - Y_{ft}(0) \mid G_{fg} = 1\right], \tag{6}$$

[10] Callaway and Sant'Anna (2021) propose tow comparison groups: never-treated units and not-yet-treated units. We opt for not-yet-treated firms as our benchmark control group since firms that eventually get downgraded are more comparable with the treatment group (more so than firms that have always remained in the top rating).

where G_{fg} is a binary variable that takes the value of one if a unit is treated at period g, indicating that it belongs to cohort g (we denote $g = 0$ as never treated firms). Notice that we define treatment as being downgraded for the first time; therefore, the treatment status is an absorbing state.

The key identification assumption to infer the unobservable potential outcome of the treated $\mathbb{E}\left[Y_{ft}(0) \mid G_{fg} = 1\right]$ is a conditional parallel trends assumption which can be stated as:

$$\mathbb{E}\left[Y_{ft}(0) - Y_{ft-1}(0) \mid X_f, G_{fg} = 1\right] = \mathbb{E}\left[Y_{ft}(0) - Y_{ft-1}(0) \mid X_f, D_{ft} = 0, G_{f0} = 0\right] \tag{7}$$

for each $g = 1, ..., T-1$, where D_{ft} is a binary variable that indicates if a firm has already been treated at year t. Intuitively, this assumption implies that, in the absence of treatment and conditional on a set of observable variables X_f, treated firms would have behaved similarly to not-yet treated firms between period t and $t-1$. This assumption allows us to control for the individual heterogeneity of firms, and to control for firm sector and firm size fixed effects. In estimations, we cluster standard errors at the firm level.

In addition, we assume that there is no treatment anticipation. That is, some anticipation could occur to some degree, but only for shorter periods of time. Given the annual frequency of our data, we argue that it is unlikely that either firms or banks could have foreseen if a firm was going to surpass (or not) the 30-day cutoff a year in advance.

Based on this set of assumptions, the group-time average treatment effects are non-parametrically identified using the doubly robust estimator proposed by Sant'Anna and Zhao (2020). This method estimates the propensity score of being treated and the outcome evolution for the comparison group, and is robust to misspecification in any of these two.

Lastly, Callaway and Sant'Anna (2021) discuss different ways to aggregate the group-time average treatment effects. In our exercises, since we expect that the downgrade exhibits dynamic effects on firms' behavior, we focus on the aggregation by time to treatment (i.e., the event study aggregation). We study the dynamics of firms' loans, total liabilities, equity, assets, and profits as outcome variables. We also study firms' funding structure, liquidity, and investment.

4 Results

This section presents the main results concerning the impact of the SARC provisioning scheme. First, we present the scheme's impact on loan conditions using a fuzzy RDD. Next, using a DiD method, we turn to the yearly firm-level balance sheet data to evaluate provisions' impact on firm performance.

4.1 Loan conditions

Loan provisioning (enforcement)

Table 4 presents the effects of the SARC scheme on the provisions-to-credit ratio in the period of the downgrade event ($j = 0$) and, for completeness, the RDD estimates four periods before the downgrade ($j = -4, ..., -1$) and eight periods after ($j = 1, ..., 8$). Essentially, this tests for the *enforcement* of the policy. In this table (and throughout this section), robust standard errors are clustered at the bank-firm level, and estimations include separately bank-, firm sector-, and firm size- fixed effects. Each column in Table 4 presents the results for a specific past-due day threshold.

Results indicate that loans that barely pass the 30- and 60-day cutoffs (and thus receive a worse rating) present a contemporaneous 0.6pp and 6.5pp higher provisions-to-credit ratio. At the 90-day cutoff, the estimate is also positive but not statistically different from zero, which might be related to a prudential behavior of banks: at these high past-due days, banks assign high provisions to their credits regardless of their rating. Estimates also show that the initial shock is persistent and amplifies over the 1–8 quarter window after the downgrade event. The provision-to-credit ratio increases by more than 4pp (in all columns). Regarding periods before treatment (and in the same vein as testing for the parallel trends assumption) we find no differences in most cases. Moreover, when statistical differences are found, they are low relative to the initial shock.

The above results indicate that the SARC scheme induces banks to increase costs, where the highest contemporaneous impulse of the policy shock is at the 60-day cutoff.[11] Afterward, and for ease of interpretation, we will take into account the magnitude of the contemporaneous shock to compare the effects of the policy at each cutoff.[12]

Table 4: Effect of the SARC on the provision-to-credit ratio (pp)

j	(1) 30-day cutoff	(2) 60-day cutoff	(3) 90-day cutoff
-4	0.164	0.0517	-1.653***
	(0.114)	(0.248)	(0.524)
-3	-0.163*	0.191	-1.898***
	(0.0902)	(0.225)	(0.610)
-2	0.0927	0.308***	-0.439
	(0.0658)	(0.143)	(0.326)
-1	-0.0625	0.0857	-0.369**
	(0.0731)	(0.129)	(0.232)
$j = 0$	**0.623*** **	**6.530*** **	**1.456**
	(0.0865)	**(0.192)**	**(0.886)**
1	1.165***	8.920***	15.63***
	(0.128)	(0.757)	(2.739)
2	2.468***	10.54***	13.53***
	(0.212)	(0.891)	(3.710)
3	3.302***	11.41***	14.20***
	(0.246)	(0.980)	(5.325)
4	3.994***	9.315***	11.47***
	(0.221)	(0.952)	(3.699)
5	4.184***	9.944***	14.60***
	(0.233)	(0.975)	(4.052)
6	4.746***	8.514***	14.06***
	(0.247)	(1.003)	(3.611)
7	4.361***	7.053***	12.41***
	(0.289)	(0.938)	(3.042)
8	4.380***	6.980***	9.448***
	(0.310)	(0.993)	(3.172)
Observations (left) in $j = 0$	17,681	5,390	2,007
Observations (right) in $j = 0$	13,170	4,524	2,344
Bandwidth in $j = 0$	4.858	8.364	6.344

Notes: Fuzzy RDD estimates for $j = -4, ..., 8$. Bandwidth choices and significance levels calculated according to the robust, bias-corrected method of Calonico et al. (2014) and Calonico et al. (2019). Estimations include bank-, firm sector-, and firm size- fixed effects. Robust standard errors clustered at the bank-firm level in parentheses. Estimations are restricted to new credits, i.e., bank-firm relationships that have increased credit in any period between t and $t + 8$. *** p<0.01, ** p<0.05, * p<0.1. Source: Authors' elaboration based on SFC's data.

[11] Although not reported for the sake of brevity, the results of the first stage regressions are statistically significant and indicate a strong discontinuity in the probability of being downgraded.

[12] We focus on the contemporaneous shock because, as will be clear later, the post-downgrade effects are also determined by the impact on other variables, such as credit flow and LGD.

Loan conditions (intensive margin)

To analyze the dynamic effect of a higher provision requirement, we estimate IRFs as described in equations (4) and (5) (see Section 3.1) for 1–8 quarters ahead of the credit downgrade event. Panels (a), (b), and (c) of Figure 3 display the effects for the 30-, 60-, and 90-day cutoffs, respectively. Bandwidth choices and confidence intervals are based on Calonico et al. (2014) and Calonico et al. (2019).

Results indicate that downgraded loans exhibit a persistent lower credit rating.[13] Additionally, we find that loan amount is gradually reduced at the 30- and 60-day cutoffs until it permanently stays 9pp and 28pp lower than the control group, respectively. Using the magnitude of the policy shock presented in Table 4, the above is consistent with a reduction in credit growth of 15pp and 4pp at the 30- and 60-day cutoffs, respectively, when the provision-to-credit ratio increases by 1pp.[14]

Concerning the quality of guarantees, we find no significant effects at the 60- and 90-day cutoffs, whereas there is a higher quality requirement at the 30-day cutoff.[15] At the same time, the loan-loss given default is persistently increased for treated loans at the 60-day cutoff, whereas we find no significant effects at the other cutoffs. The above results indicate that banks have internal policies that increase provisions (through higher loan-loss given default) and demand higher quality guarantees for credits with worse ratings, even if the ex-ante risk is similar.

In sum, we find that the SARC scheme induces banks to increase the provisioning cost of downgraded loans. The above implies that, for loans with similar risk but with a discontinuously lower rating, banks offer a lower amount (at the 30- and 60-day cutoffs), demand higher quality guarantees (30-day cutoff), and impose a higher level of provision coverage through

[13] To measure the effect, the scale of credit rating is multiplied by -1 so that negative effects are interpreted as lower ratings.

[14] For reference, the average provision-to-credit ratio is close to 5% at the 30-day cutoff, so a 1pp increase is equivalent to a 20% jump in provisioning costs.

[15] To measure the effect, the scale of quality of guarantee is multiplied by -1 so that positive effects are interpreted as higher quality.

the loan-loss given default (60-day cutoff). Interestingly, given the same policy shock, the effect on credit growth is highest at the 30-day cutoff.

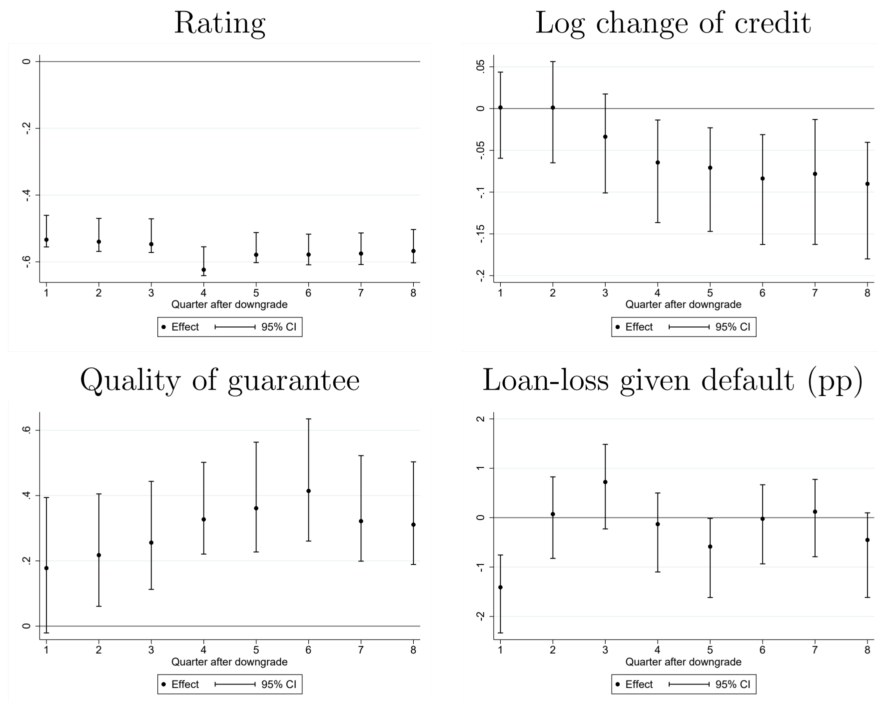
Probability of receiving new credit (extensive margin)

Based on the fuzzy RDD explained in Section 3.1, Figure 4 shows the IRFs of the effect of treatment on the probability of receiving new credit 1–8 quarters ahead of the downgrade event. Panel (a) displays the effects for the 30 default day cutoff, and Panels (b) and (c) plot the results for the 60- and 90-day cutoffs.

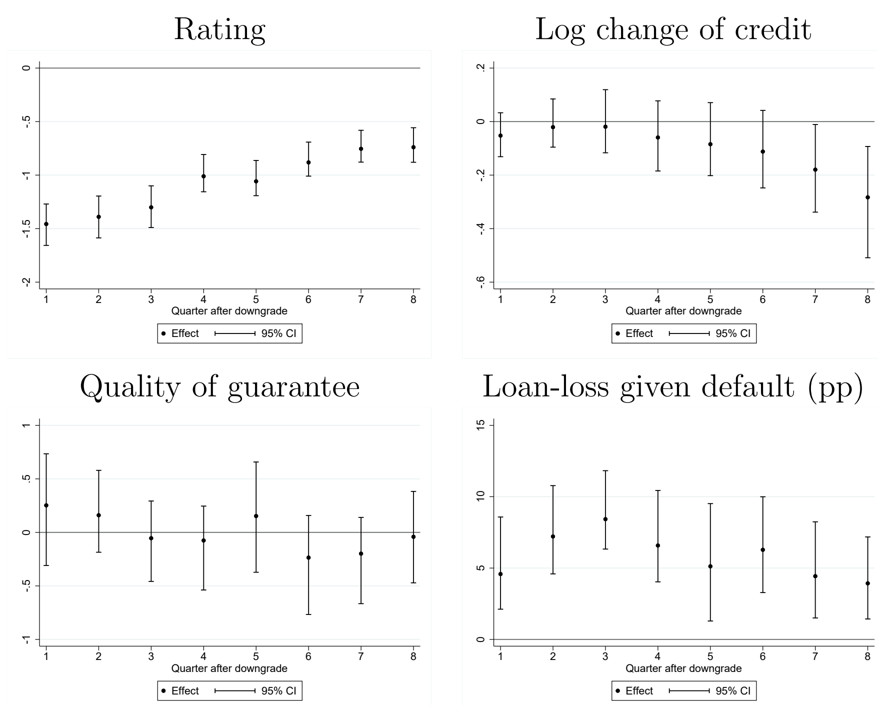
Results indicate that, at the 30- and 60-day cutoffs, the higher provision cost triggered by the SARC scheme induces banks to reduce the extension of new credit to bank-firm relationships of similar risk. At the 30-day cutoff, downgraded credits exhibit, on average, a 7pp lower probability of receiving disbursements net-of-amortizations eight quarters ahead of treatment, whereas the effect is 16pp at the 60-day cutoff. When the provision-to-credit ratio increases by 1pp, the above translates into a probability reduction of 11pp at the 30-day cutoff and of 2pp at the 60-day cutoff. By contrast, no effect is found at the 90-day cutoff.

Paradoxically, results seem to indicate that, at the 30-day cutoff, there is a higher probability of receiving new credit two quarters after the downgrade event. Nevertheless, this result can be explained by amortizations. The above is explored in Appendix A for different outcome variable definitions. As observed, the first quarter results attenuate, and the direction of the main results hold.

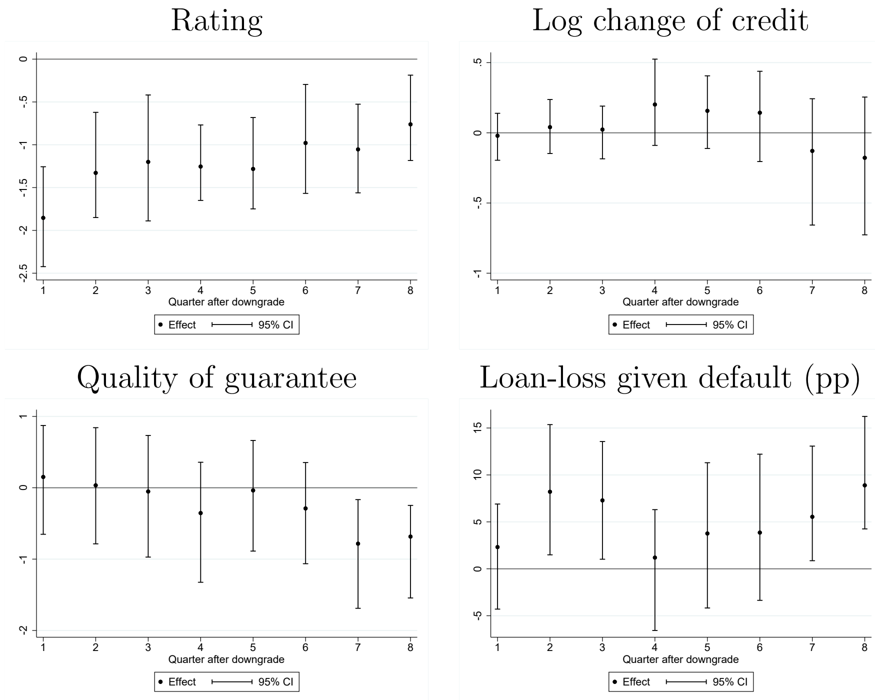
Figure 3: Effects on new credit conditions



(a) 30-day cutoff



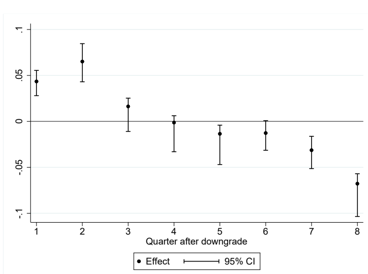
(b) 60-day cutoff



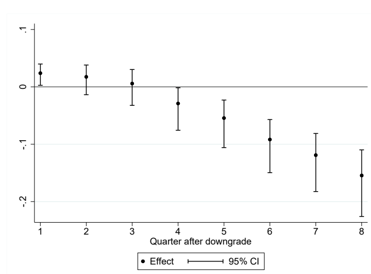
(c) 90-day cutoff

Notes: Fuzzy RDD estimates. Bandwidth choices and significance levels calculated according to the robust, bias-corrected method of Calonico et al. (2014) and Calonico et al. (2019). Estimations include bank-, firm sector-, and firm size- fixed effects. Robust standard errors are clustered at the bank-firm level. Estimations are restricted to bank-firm relationships that have increased credit in any period between t and $t+j$. Estimations for quality of guarantee and loan-loss given default are restricted to credit relationships with guarantees. Source: Authors' elaboration based on SFC's data.

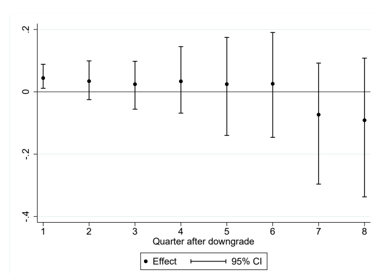
Figure 4: Effects on the probability of receiving new credit



(a) 30 days cutoff



(b) 60-day cutoff



(c) 90-day cutoff

Notes: Fuzzy RDD estimates. Bandwidth choices and significant levels calculated according to the robust, bias-corrected method of Calonico et al. (2014) and Calonico et al. (2019). Estimations include bank-, firm sector-, and firm size- fixed effects. Robust standard errors are clustered at the bank-firm level. Source: Authors' elaboration based on SFC's data.

Local identification

The main identifying assumption of our fuzzy RDD design is that, *locally*, there are no significant differences between treated observations (loans just above the arrears day cutoff) and control observations (loans that barely missed the cutoff). Even though the above assumption cannot be directly tested, it has some testable implications. Specifically, a discontinuous distribution of the running variable at the cutoffs would raise doubts about the exogeneity of the design and could suggest some degree of "manipulation" by agents. With this in mind, Appendix B presents the stacked frequency distribution of the running variable (centered at each cutoff). According to this analysis, we observe bunching at the 30-, 60-, and 90-day cutoffs (a large number of observations at these values vs. just below). Nonetheless, we believe that this pattern comes from the nature of the variable we are studying. First, in credit contracts, payment days are commonly agreed upon on the month's last day. Hence, it is more probably to find loans at 30, 60, and 90 days in default than at 29, 59, and 89 days. Second, we expect that if banks and firms were manipulating the past-due day variable, we would observe the contrary (i.e., many more credits at the left-hand side of the cutoffs) since it would benefit firms (better rating) and banks (lower provisioning cost) if borrowers do not surpass the thresholds.

In Appendix B, we provide empirical evidence that supports our statement about the absence of manipulation. In the absence of manipulation, we expect that treated and control loans are not systematically different prior to the shock. Thus, as first exercise, we present a falsification test where the dependent variables correspond to loan condition measures, 1–4 quarters before the downgrade. The results of this test show that, although there are differences in some variables before the downgrade event, they vanish as long as the sample is restricted to a smaller bandwidth (more comparable units), in line with the assumptions of the RDD strategy that we employ. In essence, focusing attention locally around the cutoff guarantees that observable and unobservable financial and macroeconomic fundamentals do not vary with the provision policy. As a second test, we report "donut hole" estimations.

This test re-estimates the results presented in Section 4.1 but excluding observations at the cutoff (where we observe bunching). The results of this test indicate that our main findings are not sensitive to the inclusion of the credits located just at the cutoffs (i.e., those suspicious of manipulation).

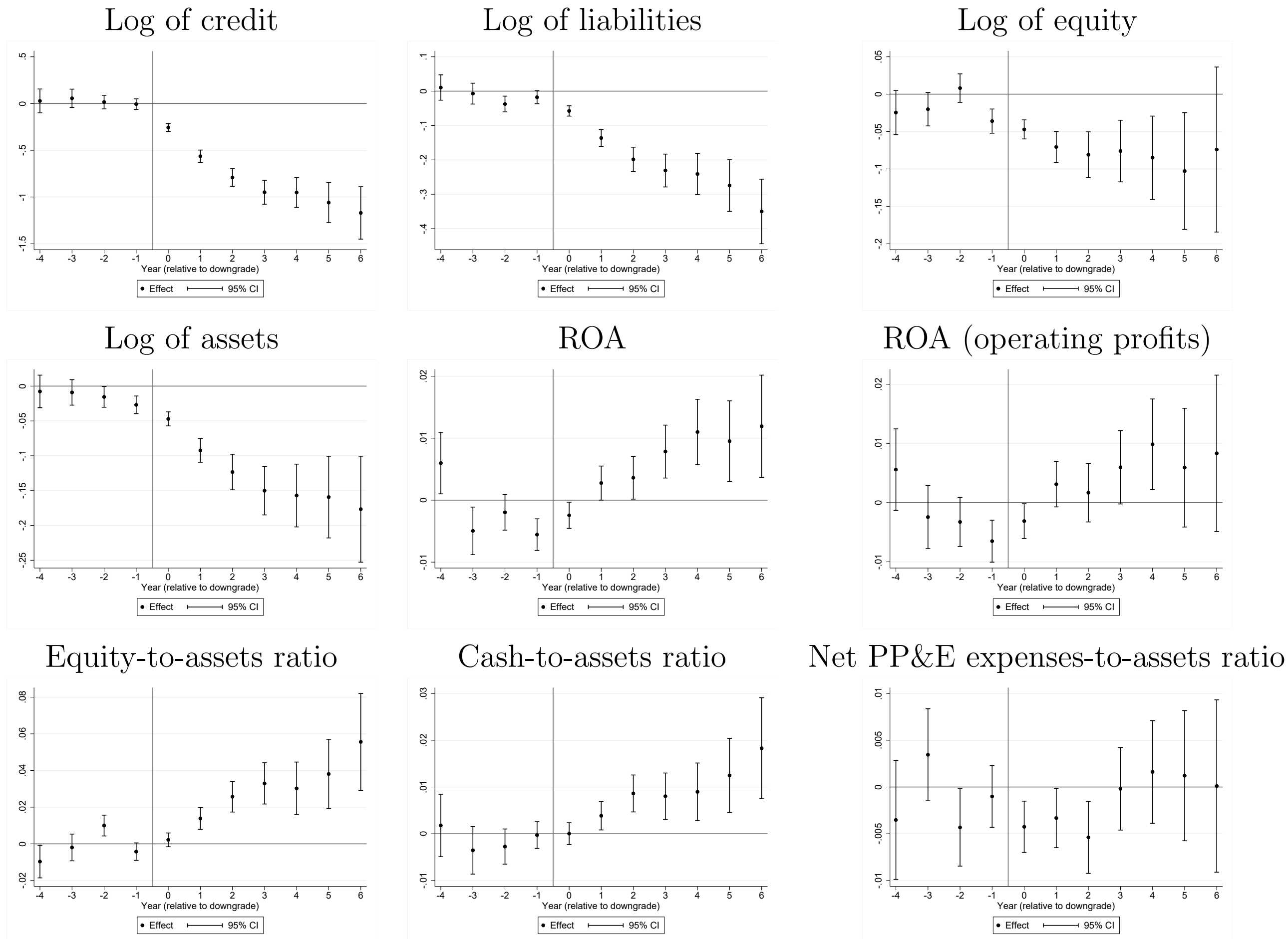
4.2 Firms' performance

The previous exercise provided evidence that at certain thresholds, the SARC scheme, by increasing provisioning costs, triggered banks to reduce the extension of credit, demand higher quality guarantees, and increase the provision coverage through the loss given default. This section studies the real effects of the SARC scheme by using firm balance sheet annual data merged with the credit registry data. We compare downgraded firms to not-yet-but-eventually treated firms. Since we observe balance sheet information yearly, we define the treatment status if any bank downgraded a firm in a given year. Given the lower frequency of data, we focus on the 30-day cutoff point, i.e., firms downgraded from rating AA to A or lower. Moreover, to improve comparability, we restrict the sample to firms in the 10-day neighborhood of the 30-day threshold during 2008–2018, in line with the RDD exercise.

Figure 5 presents the results relative to the downgrade year. Estimations are conducted following Callaway and Sant'Anna (2021), as explained in Section 3.2, clustering robust standard errors at the firm level and including sector and size fixed effects. Additionally, the analysis is performed four years before the event (to show evidence of the parallel trends assumption) and six years later.

Results indicate that, after the downgrade shock, the loan amount granted to firms is reduced. Therefore, the negative effect on credit found with the RDD exercise translates into a reduction in total firms' credit. More generally, it confirms the inability of firms to smooth out their debt across different sources of financing. Consistent with this, firms also experience a contraction in liabilities but of a lower magnitude. Furthermore, the equity diminishes together with the level of assets.

Figure 5: Effects on firms' performance



Notes: DiD estimates based on Callaway and Sant'Anna (2021). For each estimation, the sample is winsorized with percentiles 1 and 99. Estimates include sector and size fixed effects. Robust standard errors are clustered at the firm level. Source: Authors' elaboration based on data from the Colombian Business Registry.

When it comes to the revenue performance of the treated firms, we do not find significant changes in the operating ROA and observe a recovery in total profits one year after the downgrade. The conjunction of the previous results might suggest a recovery of non-operating earnings. In terms of firms' balance structure, results point out that, given the lower amount of credit, firms' dependency on equity funding increases, as indicated by the dynamics of the equity-to-assets ratio. At the same time, we find that, on average, firms present a higher cash-to-assets ratio after a downgrade. Finally, the results for the ratio of net PP&E expenses to assets indicate that, just after the event, investment decisions of downgraded firms are constrained. The pre-trend results presented in Figure 5 also allow us to conclude that, in general, we can rely on the parallel trends assumption required by the DiD strategy of identification.

Appendix C investigates the robustness of the results presented in this section when different comparison groups are selected. This exercise shows that results are not affected when the past-due day restriction is not considered. It also confirms that the best comparison group is firms that eventually get downgraded. In particular, it shows that if firms that are never downgraded are included in the comparison group, results might be highly contaminated since we would be comparing healthy firms with those of a more fragile nature.

5 Conclusions

Macroprudential policies have generally been employed to explore the effects on financial variables at the aggregate level. However, only few papers have discerned the financial effects across firm borrowing, and even fewer have fully mapped their effects to the real sector. Using Colombia as case study, we exploit a discontinuous loan provisioning scheme enacted by the Financial Regulator based on the number of days past due (i.e., days in arrears). Specifically, loans that exceeded certain thresholds: 30, 60, and 90 past-due days, were subject to higher (and thus costlier) provisioning levels.

To our advantage, while the regulation is officially categorized as a macroprudential policy (it extends to the entire financial system), it nonetheless contains micro-prudential features (oversight is dictated at the firm level). Hence, the fact that we observe an across-the-financial-sector policy that is also borrower-specific allows us to exploit useful heterogeneity. For identification, we conduct a fuzzy RDD to evaluate the impact on credit conditions. In a second step, we study how the results of the provisioning scheme carry over to the real economy, using a DiD approach.

Our results indicate that the scheme, in fact, induces banks to increase the provisioning cost of downgraded loans. This implies that, for loans with similar risk but with a discontinuously lower rating, banks offer a lower amount of credit, demand higher quality guarantees, and impose a higher level of provision coverage through the loan-loss given default. More specifically, a 1 percentage point (pp) increase in the provision-to-credit ratio leads to a reduction in credit growth of up to 15pp and lowers the probability of receiving new credit by up to 11pp.

In addition, our DiD exercises used to evaluate the real effects of the policy point out that, given the lower amount of credit, firms' dependency on equity funding increases. At the same time, we find that, on average, firms present a higher cash-to-assets ratio after a downgrade. Finally, we find that just after the event, investment decisions of downgraded firms are constrained in terms of the share of net PP&E expenses to assets.

We believe that our investigation can be helpful and applied to several other contexts. For example, several other countries have adopted similar provisioning schemes in the Latin American region (such as Bolivia, Uruguay, Peru, Argentina, Chile, Ecuador, and Mexico). More generally, our contribution lies on precisely quantifying macroprudential effects in a context where the mainstream has been the implementation of countercyclical capital buffers and new methods of expected credit losses in the measurement of provisions (as recommended by Basel III and IFRS 9 standards).

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Appendix

A Sensitivity checks

The results regarding the probability of receiving new loans presented in Section 4.1 indicate that, at the 30-day cutoff, there is a higher probability of receiving new loans two quarters after the downgrade event. Nevertheless, given that we analyze disbursements net of amortizations, this result might be explained by this last variable (amortizations). Treated credits may have a different amortization dynamic after the downgrade event, which might affect our definition of the dummy of receiving new credit.

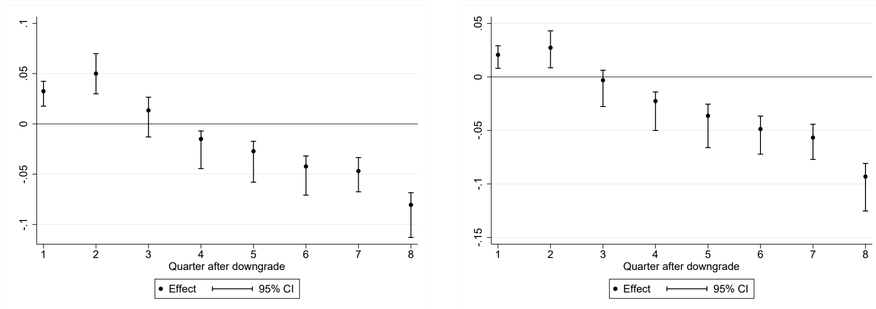
Consequently, this appendix explores different definitions for the outcome variable. In particular, we employ two additional definitions: i) bank-firm relationships that show an increase in their outstanding credit line higher than 3%, the value that corresponds to the Colombian Central Bank inflation target, and ii) bank-firm relationships that exhibit an increase higher than 10%. This value corresponds to the median annual growth of credit for bank-firm relationships that appear in our sample in periods t and $t + 4$, but not in between this interval. The logic behind this definition is that by using this restricted sample, we get a criterion to define new credit that is not contaminated by amortizations.

Figure A1 shows the results for the alternative definitions of the dummy variable. Left panels show estimations under the 3% definition, and right panels report results under the 10% definition. Panels (a), (b), and (c) display the effects for the 30-, 60-, and 90-day cutoffs, respectively. Note that, at the 30-day cutoff, the first quarter's results attenuate, and the final directions of our benchmark estimates hold. Regarding the other cutoffs, the results are not highly affected. In general, the probability of receiving new credit is robust to the different definitions of the dummy variable.

Figure A1: Effects on the probability of receiving new credit, alternative definitions

3% definition

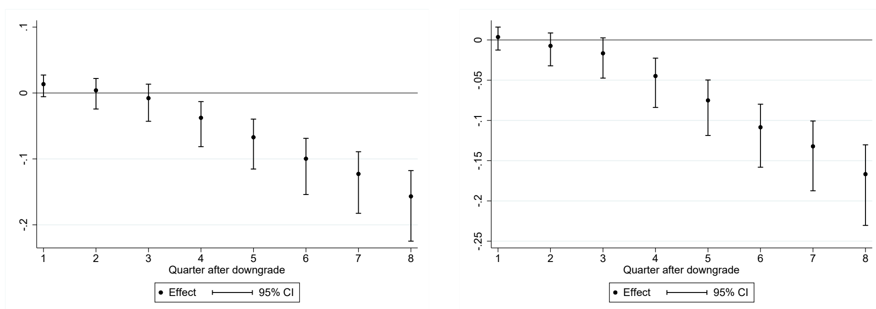
10% definition



(a) 30-day cutoff

3% definition

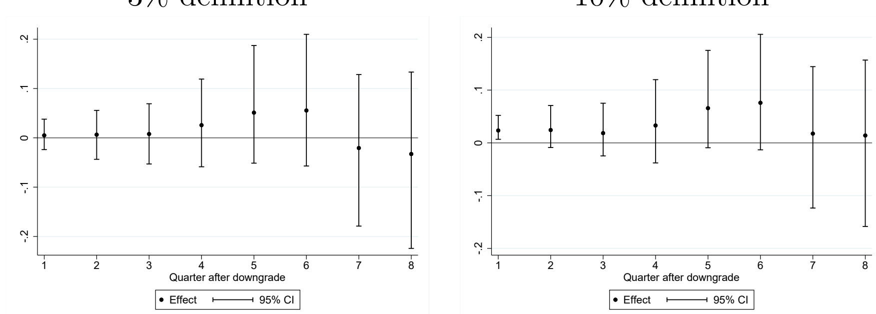
10% definition



(b) 60-day cutoff

3% definition

10% definition



(c) 90-day cutoff

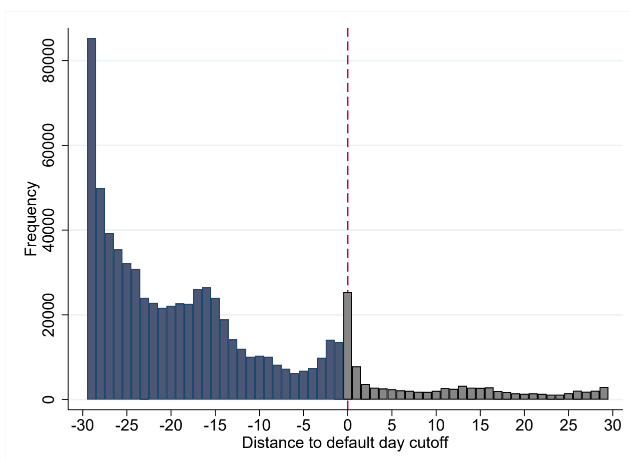
Notes: Fuzzy RDD estimates for different definitions of new credits. Bandwidth choices and significant levels calculated according to the robust, bias-corrected method of Calonico et al. (2014) and Calonico et al. (2019). Estimations include bank-, firm sector-, and firm size- fixed effects. Robust standard errors are clustered at the bank-firm level. Source: Authors' elaboration based on SFC's data.

B Localized approach

This appendix conducts an exercise to evaluate some testable implications of the main RDD identifying assumptions. This empirical strategy requires that there must not be significant differences between treated loans just above and below each threshold in the neighborhood of the delinquency day cutoffs. Although this assumption cannot be directly tested, it has some testable implications. In particular, even if the RDD does not require continuity of the density of the running variable at each cutoff, a discontinuous distribution would raise doubts about the randomness of the treatment assignment at the cutoff and could suggest some "manipulation" by agents.

Figure B1 plots the frequency distribution of the stacked distance of the running variable with respect to each cutoff for the whole sample. It is evident that there is an increase in observations at the 30-, 60-, and 90-day cutoffs.[16] However, we believe this arises from the nature of the variable we are studying and not from agents manipulating it. First, in credit contracts, payment days are commonly arranged on the last day of the month. Therefore, it is more likely to find loans at 30, 60, and 90 days in default than in 29, 59, and 89 days. Second, we believe that if banks and firms were manipulating the past-due day variable, we would observe the contrary (i.e., more credits at the left of the cutoffs) because it would benefit firms (better rating) and banks (lower provision cost) if borrowers do not surpass the thresholds. The rest of this appendix offers empirical evidence for our reasoning.

Figure B1: Stacked distribution of centered running variables



Notes: For visualization, distance equal to -30 and 30 is excluded. Source: Authors' elaboration based on SFC's data.

[16]The discontinuity test of McCrary (2008) rejects the null that the distribution is continuous at the cutoff.

Falsification test

As a first exercise, we test manipulation of the running variable by investigating if credits were actually different before the downgrade. In the absence of manipulation, we expect treated and control bank-firm relationships that receive new credit not to be systematically different at the cutoff. If this were not the case, the results of the RDD would be contaminated by pre-existent differences in credit conditions.

Based on this discussion, Figure B2 plots a falsification test where the dependent variables are credit condition measures 1–4 quarters before the downgrade. It includes the following variables: provision-credit ratio (our enforcement variable), the log change of real credit (relative to period 0), quality of guarantees, and loan-loss given default. Note that robust standard errors are clustered at the bank-firm level and estimations include separately: bank-, firm sector-, and firm size- fixed effects. Panels (a), (b), and (c) display the falsification tests for the 30-, 60-, and 90-day cutoffs, respectively. Each column in Figure B2 shows the results for different bandwidths of arrears days (BW=15, 10, 5 days in arrears).

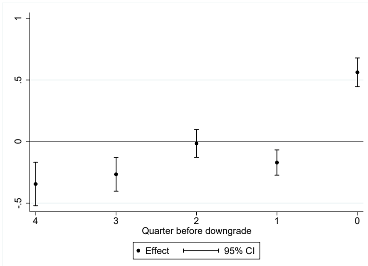
We first report that, in line with Table 4, the sub-figures presented in the 1^{st} row (all panels) show that the "enforcement" variable (provision-to-credit ratio) is statistically significant at the time of the event ($j = 0$), as expected, given the SARC scheme.

Second, all other rows (2–4) illustrate the local nature of the fuzzy RDD design. For all variables, the results of the falsification test show that differences between treated and control credits are reduced as long as the sample is restricted to more comparable units (lower bandwidth). For instance, at the 30-day cutoff, treated credits have higher quality collateral and lower loss given default before the downgrade event. However, the differences vanish when restricting the sample to a bandwidth of 5 days. We observe a similar pattern for the log change of credit at the 90-day threshold.

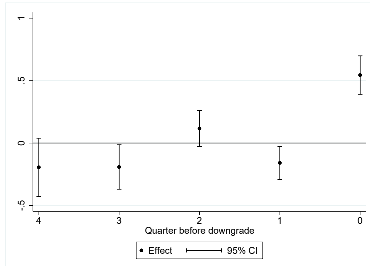
Figure B2: Falsification test

(i) Provision-credit ratio (pp)

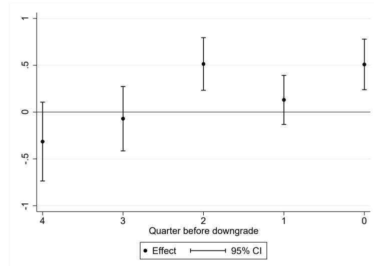
BW = 15 days



BW = 10 days

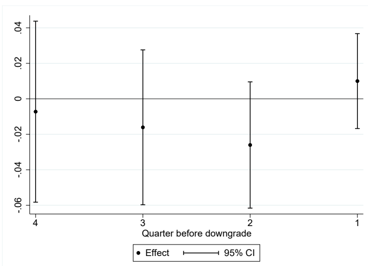


BW = 5 days

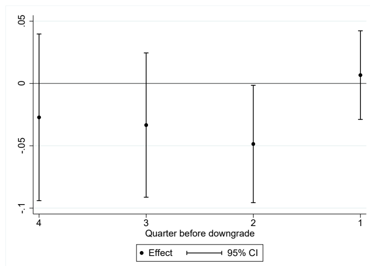


(ii) Log change of credit

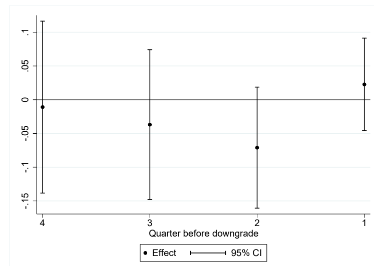
BW = 15 days



BW = 10 days

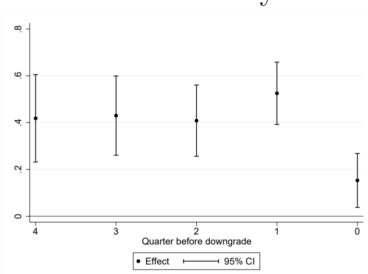


BW = 5 days

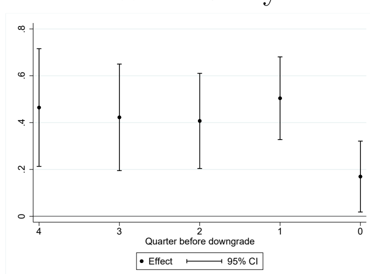


(iii) Quality of guarantee

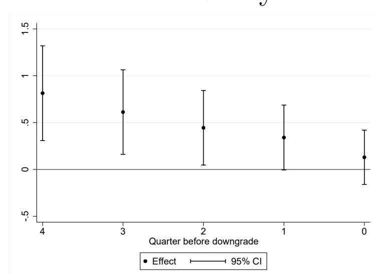
BW = 15 days



BW = 10 days

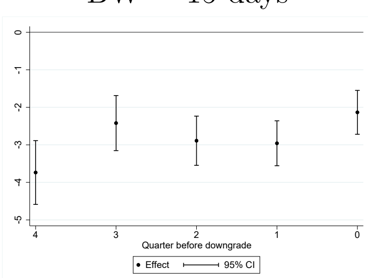


BW = 5 days

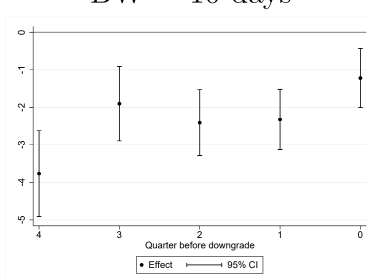


(iv) Loss Given Default (pp)

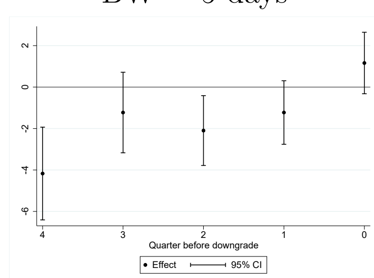
BW = 15 days



BW = 10 days



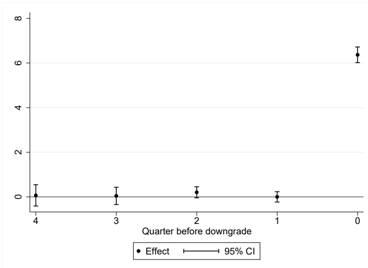
BW = 5 days



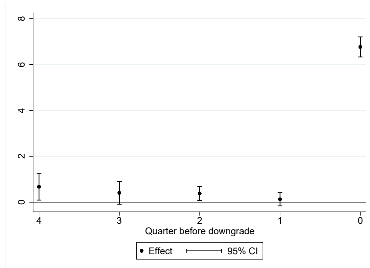
(a) 30-day cutoff

(i) Provision-credit ratio (pp)

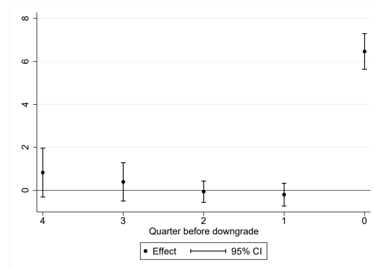
BW = 15 days



BW = 10 days

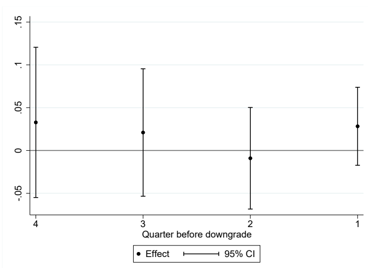


BW = 5 days

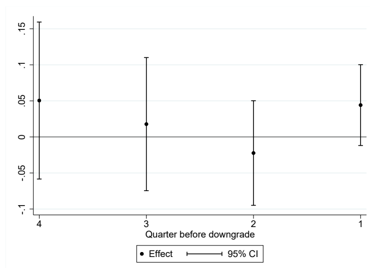


(ii) Log change of credit

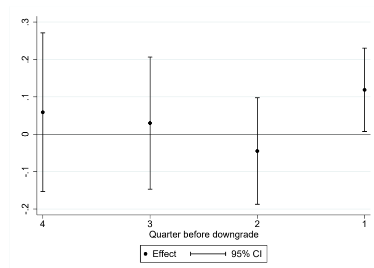
BW = 15 days



BW = 10 days

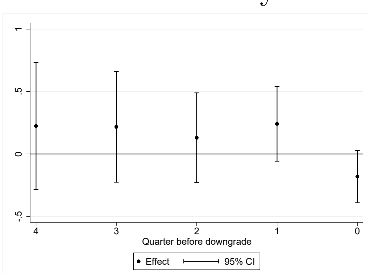


BW = 5 days

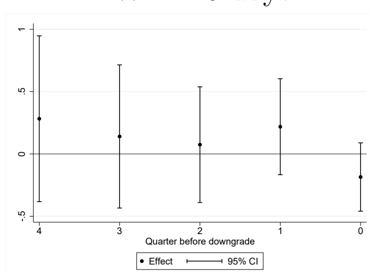


(iii) Quality of guarantee

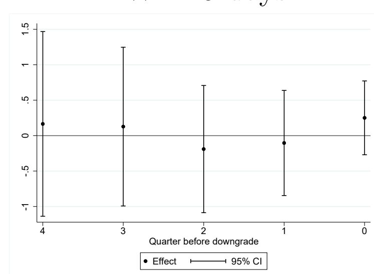
BW = 15 days



BW = 10 days

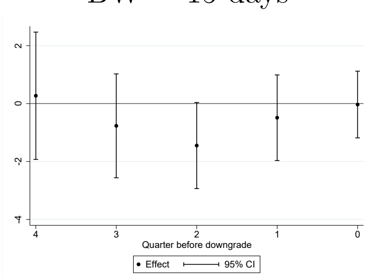


BW = 5 days

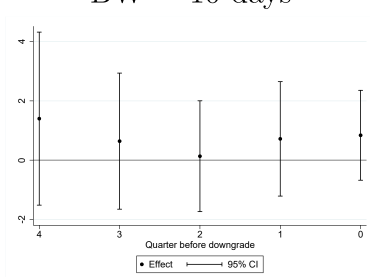


(iv) Loss Given Default (pp)

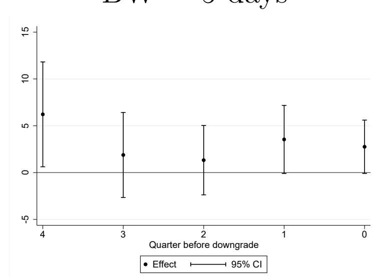
BW = 15 days



BW = 10 days



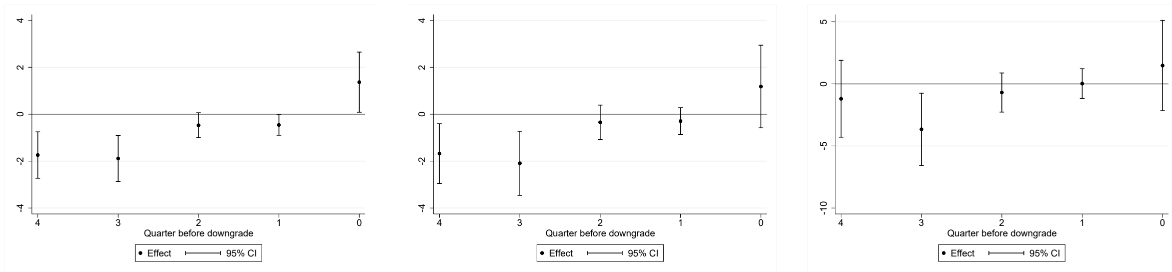
BW = 5 days



(b) 60-day cutoff

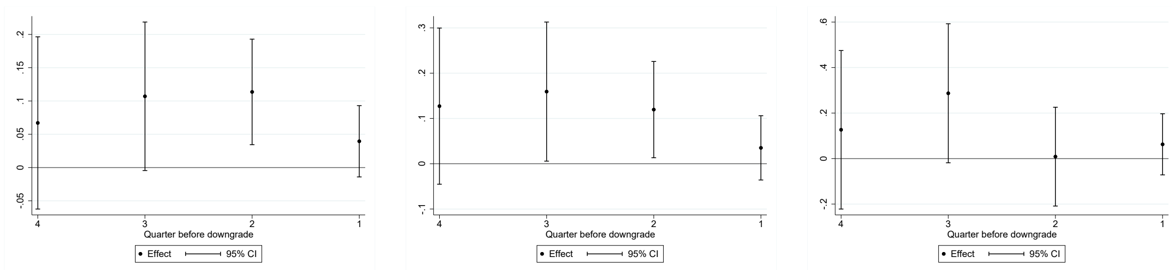
(i) Provision-credit ratio (pp)

BW = 15 days | BW = 10 days | BW = 5 days



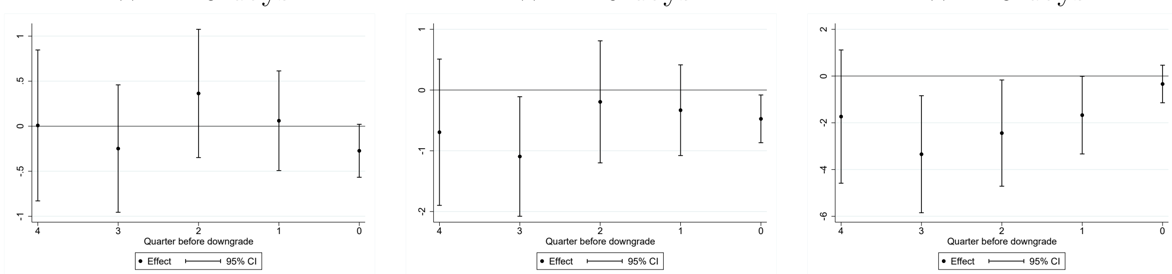
(ii) Log change of credit

BW = 15 days | BW = 10 days | BW = 5 days



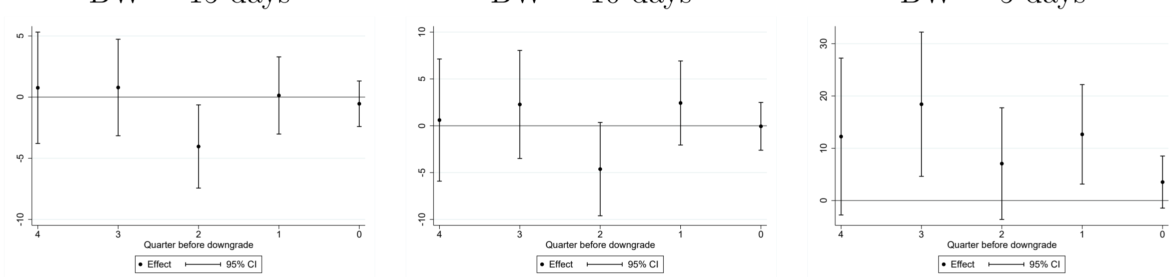
(iii) Quality of guarantee

BW = 15 days | BW = 10 days | BW = 5 days



(iv) Loss Given Default (pp)

BW = 15 days | BW = 10 days | BW = 5 days



(c) 90-day cutoff

Notes: Fuzzy RDD estimates for different bandwidth choices. Estimations include bank-, firm sector-, and firm size- fixed effects. Robust standard errors are clustered at the bank-firm level. Estimations are restricted to bank-firm relationships that receive new credit in any period 8 quarters ahead of period 0. Estimations for quality of guarantee and loan-loss given default are restricted to credit relationships with guarantees. Source: Authors' elaboration based on SFC's data.

“Donut hole” test

To further explore the possible manipulation of agents, we report “donut hole” estimations as a second test. Essentially, this exercise evaluates the sensitivity of our main results, presented in Section 4.1, to observations located very close to the cutoffs. In particular, we are interested in the sensibility of results when credits at the cutoff are excluded since there we observe a discontinuity in the distribution of the running variable.

Table B1 presents the effect of the SARC scheme on the provision-to-credit ratio in quarters $j = -4, ..., 8$ relative to downgrade (as in Table 4), but now excluding observations that lie exactly at the cutoff ($\tilde{X}_{bft} = 0$). According to the results, when credits at the 30-, 60-, and 90-day cutoffs are excluded, the positive effect on provisioning costs holds as in Section 4.1.

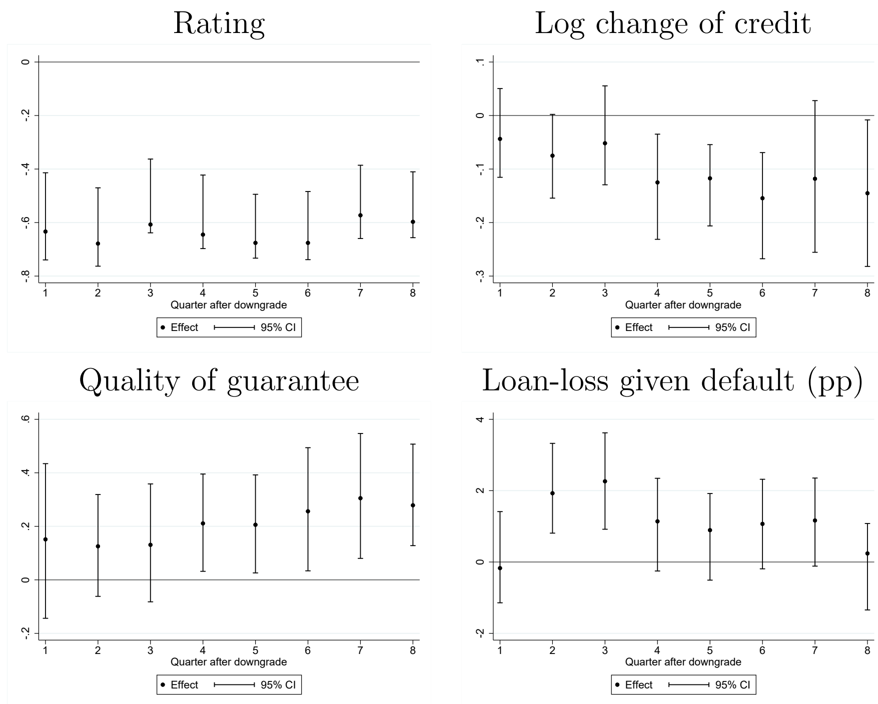
Table B1: “Donut hole” test for the effect of the SARC on the provision-to-credit ratio (pp)

j	(1) 30-day cutoff	(2) 60-day cutoff	(3) 90-day cutoff
-4	-0.0542	-0.637	-1.796***
	(0.189)	(0.459)	(0.495)
-3	-0.0604	-0.120	-1.853***
	(0.154)	(0.413)	(0.587)
-2	-0.0176	-0.280	-0.581**
	(0.106)	(0.246)	(0.292)
-1	-0.0413	-0.258	-0.503**
	(0.0900)	(0.194)	(0.283)
$j = 0$	**0.608*** **	**6.792*** **	**3.253*** **
	(0.125)	**(0.344)**	**(0.777)**
1	1.959***	13.82***	19.91***
	(0.349)	(1.345)	(2.487)
2	3.089***	16.58***	18.62***
	(0.594)	(1.498)	(3.513)
3	3.617***	17.27***	17.12***
	(0.639)	(1.745)	(4.804)
4	4.550***	11.29***	12.83***
	(0.746)	(1.793)	(3.830)
5	4.368***	13.07***	14.50***
	(0.685)	(1.756)	(4.103)
6	5.013***	11.39***	14.89***
	(0.745)	(2.167)	(3.242)
7	4.442***	10.15***	13.08***
	(0.796)	(2.192)	(2.875)
8	4.174***	9.828***	12.28***
	(0.803)	(1.771)	(2.940)
Effective Observations (left) in $j = 0$	23,277	4,521	2,165
Effective Observations (right) in $j = 0$	6,635	2,092	1,637
Bandwidth in $j = 0$	6.510	6.865	9.548

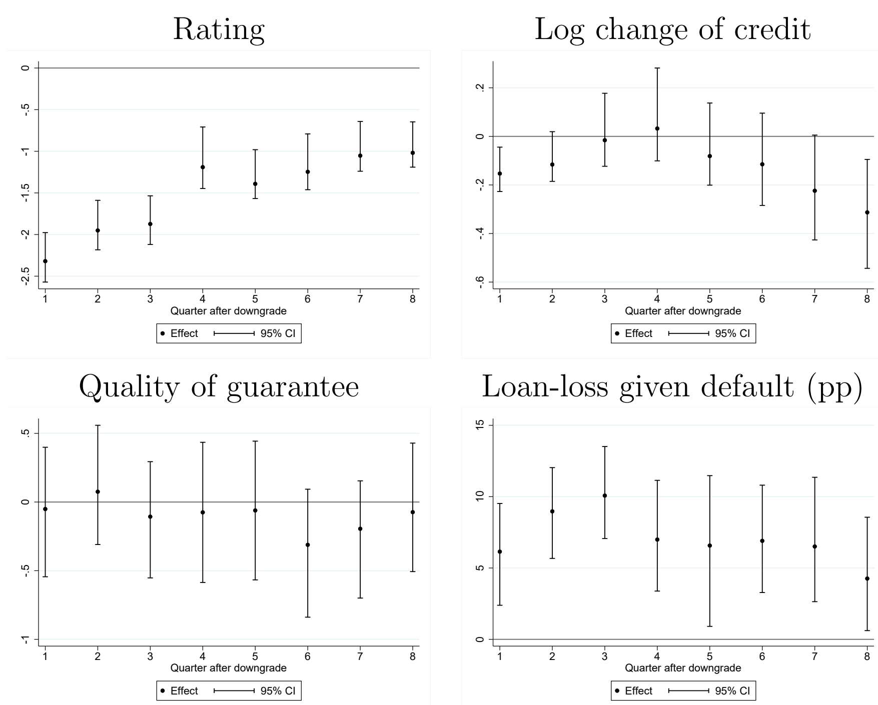
Notes: Fuzzy RDD estimates for $j = -4, ..., 8$. Bandwidth choices and significance levels calculated according to the robust, bias-corrected method of Calonico et al. (2014) and Calonico et al. (2019). Estimations include bank-, firm sector-, and firm size- fixed effects. Robust standard errors clustered at the bank-firm level in parentheses. Estimations exclude loans with a distance to the cutoff less than or equal to 0 and are restricted to new credits, i.e., bank-firm relationships that have increased credit in any period between t and $t + 8$. *** p<0.01, ** p<0.05, * p<0.1. Source: Authors' elaboration based on SFC's data.

Next, we present the “donut hole” test for the effects of the higher provision requirement on loan conditions. First, Figure B3 plots the IRFs presented in Figure 3, excluding credits located just at past-due day thresholds. According to this test, excluding observations at the cutoffs does not affect our main findings: higher provisioning cost triggers banks to offer a lower loan amount (30- and 60-day cutoffs), demand higher quality guarantees (30-day cutoff), and impose a higher provision coverage through LGD (60-day cutoff). Finally, Figure B3 shows the IRFs of the effect of treatment on the probability of receiving new credit, 1–8 quarters ahead downgrade (as in Figure 4), when credits at the cutoffs are excluded. We also find that the negative effects on the probability found in Section 4.1 are not altered by the exclusion of debtors at the cutoffs.

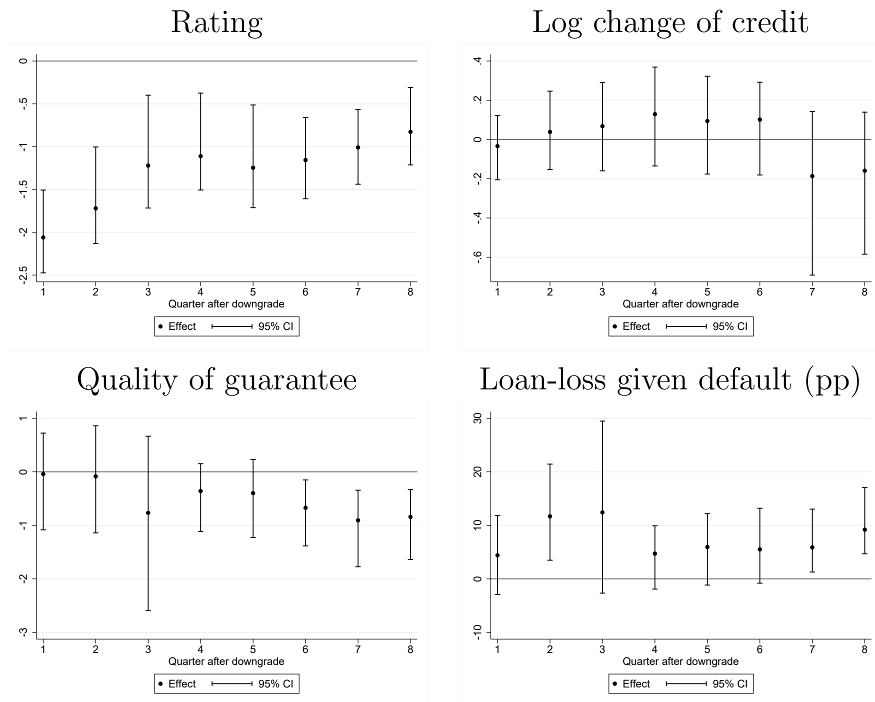
Figure B3: “Donut hole” test for the effects on new credit conditions



(a) 30-day cutoff



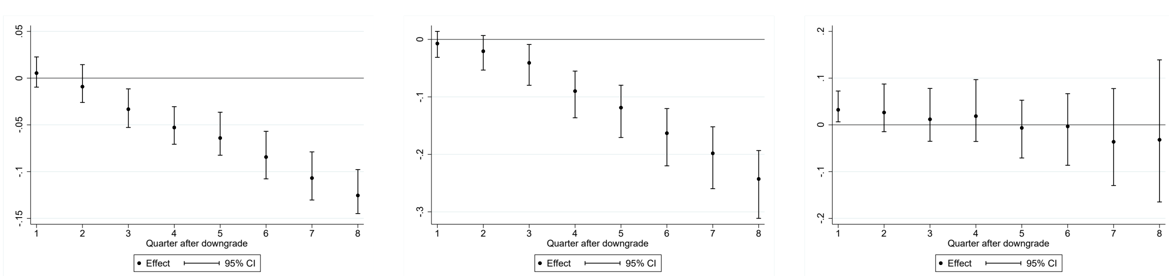
(b) 60-day cutoff



(c) 90-day cutoff

Notes: Fuzzy RDD estimates. Bandwidth choices and significance levels calculated according to the robust, bias-corrected method of Calonico et al. (2014) and Calonico et al. (2019). Estimations include bank-, firm sector-, and firm size- fixed effects. Robust standard errors are clustered at the bank-firm level. Estimations exclude loans with a distance to the cutoff less than or equal to 0 and are restricted to bank-firm relationships that have increased credit in any period between t and $t+j$. Estimations for quality of guarantee and loan-loss given default are restricted to credit relationships with guarantees. Source: Authors' elaboration based on SFC's data.

Figure B4: "Donut hole" test for the effects on the probability of receiving new credit



(a) 30-day cutoff (b) 60-day cutoff (c) 90-day cutoff

Notes: Fuzzy RDD estimates. Bandwidth choices and significant levels calculated according to the robust, bias-corrected method of Calonico et al. (2014) and Calonico et al. (2019). Estimations include bank-, firm sector-, and firm size- fixed effects and exclude loans with a distance to the cutoff less than or equal to 0. Robust standard errors are clustered at the bank-firm level. Source: Authors' elaboration based on SFC's data.

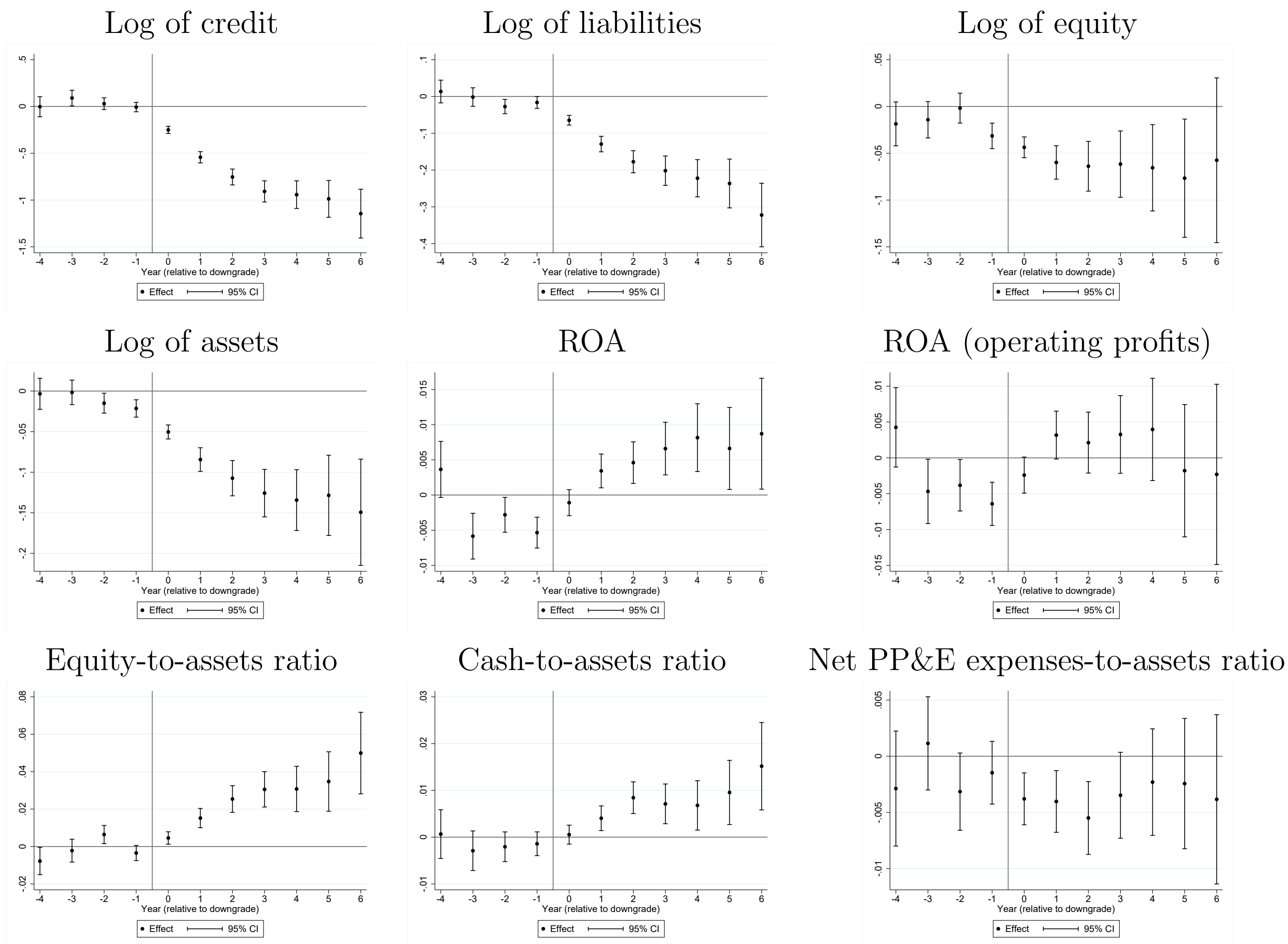
C DiD robustness checks

The results regarding the effects of the SARC scheme presented in Section 4.2 use, as comparison group, the *"not-yet-but-eventually"* treated firms. Further, to increase comparability in line with the RDD exercises, estimations restrict the sample to firms within the 10-day vicinity of the triggering threshold. This appendix shows the sensitivity of results to these parameters.

Figure C1 displays the results when treated firms are compared to *"not-yet-but-eventually"* treated firms without the above-mentioned restriction of loans being in the immediate neighborhood of the cutoff. Results indicate that our main exercise is not affected by this sample restriction: both the magnitudes and directions of the effects are highly similar to those presented in Figure 5. On the other hand, Figure C2 presents the results of Section 4.2, adding never-treated firms to the comparison group. In contrast to the previous exercise, the results significantly differ from our main results. In particular, this exercise shows that, prior to the downgrade event, treated firms persistently show higher credit and indebtedness relative to assets compared to never downgraded firms. The above is not surprising since it is reasonable to expect that firms that always remained in the top rating are less fragile.

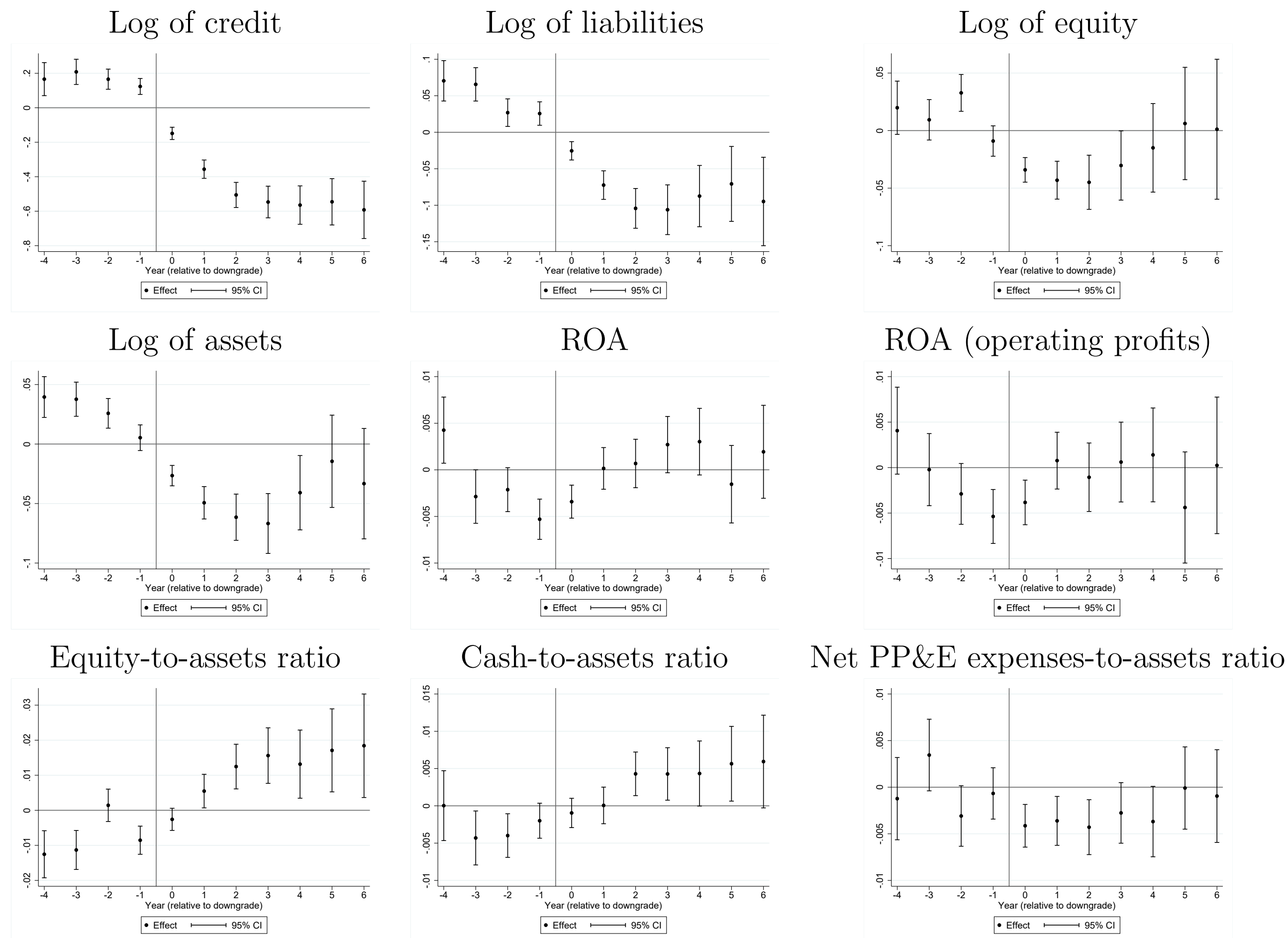
Taking together the above results, we conclude that our best comparison group are firms with similar historical arrears days that eventually get a downgrade (our main results presented in Section 4.2). In particular, if we compare downgraded firms to corporates that have always remained in the top rating, results might be highly contaminated since we would compare healthy firms with more fragile ones.

Figure C1: Effects on firms' performance without the sample restriction for default days



Notes: DiD estimates based on Callaway and Sant'Anna (2021). For each estimation, the sample is winsorized with percentiles 1 and 99. Estimates include sector and size fixed effects. Robust standard errors are clustered at the firm level. Source: Authors' elaboration based on data from the Colombian Business Registry.

Figure C2: Effects on firms' performance using not yet and never treated firms as comparison group



Notes: DiD estimates based on Callaway and Sant'Anna (2021). For each estimation, the sample is winsorized with percentiles 1 and 99. Estimates include sector and size fixed effects. Robust standard errors are clustered at the firm level. Source: Authors' elaboration based on data from the Colombian Business Registry.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