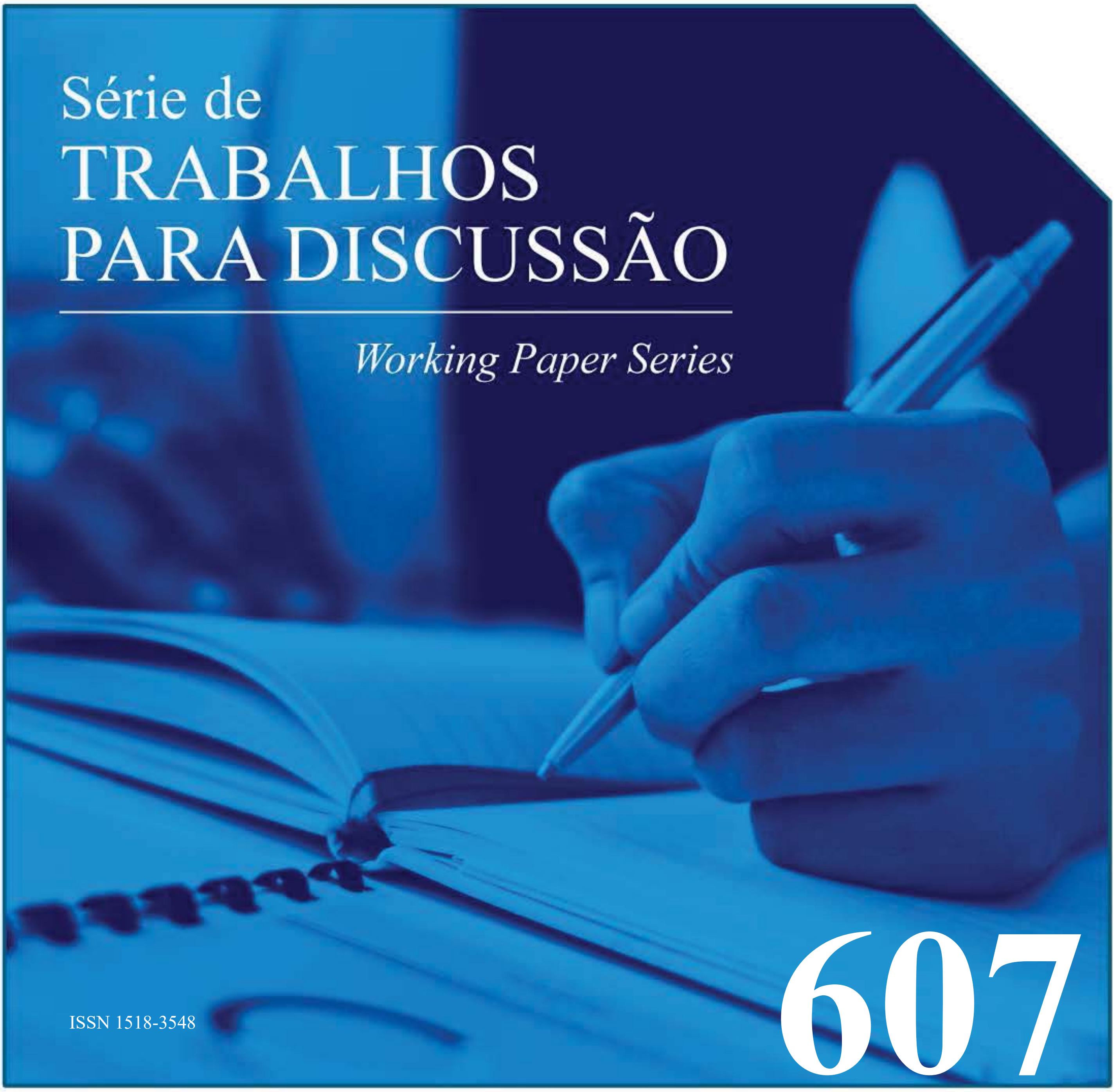

Novembro 2024

Macroprudential, Monetary Policy and Credit Supply: evidence from matched bank-firm loan-level data in Brazil

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Non-Technical Summary

This paper investigates the role of Reserve Requirements (RRs) in managing domestic credit cycles in Brazil. Historically, RRs were a key monetary policy tool, but their use declined with the adoption of inflation targeting in advanced economies. However, in emerging markets like Brazil, RRs have remained in use, especially during and after the global financial crisis (GFC) and the COVID-19 pandemic.

We use loan-level data from the Central Bank of Brazil's credit registry, covering the period 2008-2015 and focus on firms with multiple bank relationships, introducing firm-quarter fixed effects to control for credit demand shifts. For identification, we build a counterfactual variable that captures bank-level exposure to RRs, essential for disentangling credit supply effects of RRs from those of the policy rate. Additionally, the research explores the asymmetries in the lending channel of RRs by comparing the effects of tightening versus loosening and synergies with the policy rate.

We find RRs are an effective instrument in managing credit booms and busts through credit supply. Tightening RRs during booms constrains credit supply, while loosening RRs during busts stimulates it. The study identifies a potent channel operating in the supply of credit during episodes of RRs tightening and loosening, with strong synergies with monetary policy during tightening, to counter credit booms. Bank heterogeneity plays a key role in the RRs' lending channel, with foreign and government banks partially mitigating the effects of the policy. Stronger banks (with more capital) mitigate effects of RRs tightening on credit supply. Economic policy uncertainty (EPU) is also found to be a crucial factor in the transmission mechanism, influencing banks' response to RRs changes.

Moreover, our results challenge the conventional macroprudential index approach, suggesting that focusing on policy intensity and banks' exposure is relevant and leads to stronger estimates of macroprudential policy effects on the credit cycle. Importantly, the study finds that real effects on employment are not economically significant during RRs tightening or loosening and synergies with interest-rate policy are relevant during tightening episodes.

Sumário Não-Técnico

Este artigo investiga o papel dos recolhimentos compulsórios na gestão dos ciclos de crédito domésticos no Brasil. Historicamente, o compulsório foi um instrumento importante de política monetária, mas seu uso diminuiu com a adoção do sistema de metas de inflação nas economias avançadas. No entanto, em mercados emergentes como o Brasil, o compulsório contracíclico permaneceu em uso, especialmente durante e após a crise financeira global de 2008 e a pandemia de COVID-19.

Utilizamos dados em nível de empréstimo do Sistema de Classificação de Risco do Banco Central do Brasil (SCR), abrangendo o período de 2008 a 2015, e nos concentramos em empresas com múltiplos relacionamentos bancários, introduzindo efeitos fixos de empresa-tempo para controlar possíveis mudanças na demanda de crédito. Para identificação dos efeitos, construímos uma variável contrafactual que captura a exposição de cada banco às mudanças no compulsório, fundamental para dissociar os efeitos na oferta de crédito do compulsório daqueles relacionados à taxa de juros. Além disso, a pesquisa explora as assimetrias no canal de empréstimos, comparando os efeitos de aperto versus afrouxamento do compulsório e as sinergias com a taxa básica de juros (Selic).

Descobrimos que os recolhimentos compulsórios são instrumentos eficazes na gestão de ciclos de crédito. Aumentar o compulsório na fase de expansão do ciclo limita a oferta de crédito, enquanto reduzir o compulsório na fase de retração do ciclo a estimula. O estudo identifica um canal potente na oferta de crédito durante episódios de aumento e redução do compulsório e fortes sinergias com a política monetária durante o período expansionista do ciclo de crédito. A heterogeneidade bancária desempenha um papel fundamental no canal de empréstimos, com bancos estrangeiros e governamentais mitigando parcialmente os efeitos da política macroprudencial. Bancos mais capitalizados mitigam os efeitos do aumento do compulsório na oferta de crédito. A incerteza na política econômica também se mostra um fator crucial no mecanismo de transmissão, influenciando a resposta dos bancos às mudanças no compulsório.

Além disso, nossos resultados desafiam a abordagem convencional do índice macroprudencial, sugerindo que focar na intensidade da política e na exposição dos bancos é relevante e leva a estimativas mais robustas dos efeitos da política macroprudencial no ciclo de crédito. O estudo ainda conclui que a sinergia com a taxa de juros é particularmente relevante durante a fase contracionista e os efeitos reais no emprego não são economicamente significativos após aumentos ou reduções do compulsório.

Macroprudential, Monetary Policy Synergies and Credit Supply: evidence from matched bank-firm loan-level data in Brazil

BY RODRIGO BARBONE GONZALEZ‡, BERNARDUS F. NAZAR VAN DOORNIK§, JOÃO BARATA R. B. BARROSO†

This paper estimates the impact of countercyclical reserve requirements (RRs) on credit. We explore differential bank exposure to RRs in matched bank-firm loan-level data from Brazil, where RRs have been used extensively to pursue financial stability. We find that, after tightening RRs, more exposed banks reduce credit to firms; after loosening, they expand credit supply. During booms, private domestic banks with lower capital adequacy are more responsive to a tightening of RRs and to a simultaneous tightening of the short-term policy rate. We also find that higher levels of economic policy uncertainty weaken this channel, and real effects in employment are modest.

Keywords: Reserve requirements, credit supply, monetary policy, macroprudential policy.

JEL Codes: E51, E52, E58, G21, G28

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Reserve Requirements (RRs) were the main monetary policy instrument for much of the twentieth century, but started falling into disuse as advanced economies introduced inflation targeting regimes. Since the great financial crisis (GFC), central banks have added regulatory instruments and unconventional policies to the short-term policy rate to better accommodate the mandates of financial stability and macroeconomic stabilization (Reinhart and Rogoff, 2013, Greenwood, Hanson, and Stein, 2016). In emerging markets (EMs), which more commonly face financial crises, RRs were never discontinued and have been deployed before, during, and after the GFC to slow credit growth and smooth the credit cycle (see Montoro and Moreno, 2011, Cordella et al., 2014). For example, during the COVID-19 pandemic, 53 EMs eased RRs - making RRs the most widely deployed instrument in EMs on that period complementing policy rate cuts (Gopinath, 2020, IMF, 2020).

Despite this broad use of RRs, there is scarce evidence from loan-level data about the effectiveness of RRs to manage domestic credit cycles or about potential synergies with the use of the short-term policy rate. In line with theory, we show that RRs are an effective instrument to manage credit booms and busts through credit supply, i.e., tightening RRs during booms constrains the supply of credit to firms, while loosening RRs during busts stimulates it. We also show higher levels of economic policy uncertainty weaken this channel. During booms, private domestic banks with lower capital ratios are more responsive to a tightening of RRs, and their simultaneous use with the policy rate leads to even stronger responses.

For identification, we turn to Brazil, a country where RRs have been used extensively to pursue financial stability (BCB, 2011, Montoro and Moreno, 2011, Pereira da Silva and Harris, 2012). In Brazil, bank credit in local currency is the major source of firms' external finance, and comprehensive high-quality bank-firm data on credit are available. We build a panel using quarterly loan-level data from 2008 to 2015 from "Sistema de Informações de Crédito" (SCR), the Central Bank of Brazil (BCB) credit registry database, covering virtually all loans to private sector non-financial firms.

Relative to other empirical studies that focus on macroprudential policy and its synergies with monetary policy, we take three steps to identify several effects of interest. First, using loan-level data and firm-quarter fixed effects (FE), we focus on firms with multiple bank relationships. This strategy grants a relevant degree of control over shifts in credit demand, therefore avoiding confounding effects from the correlation of credit demand and supply following policy decisions targeting the credit market. Second, we explore a counterfactual variable that captures bank-level exposure to changes in RRs. Indeed, in our sample, RRs are fixed independently for different types of deposits, and banks can be differentially exposed depending on their ex-ante deposit mix. Having a bank-level exposure to RRs is crucial to disentangle the credit supply effects of the policy rate from those of RRs. It is also crucial to estimate interaction effects between the two policies, revealing policy complementarities. Third, we test the effectiveness of policy loosening versus tightening to identify asymmetries in the lending channel of RRs.

Preview of the paper. We find a potent channel operating in the supply of credit during episodes of loosening and tightening of RRs, along with strong synergies with monetary policy to counter credit booms. To identify these synergies, we interact banks' exposure to RRs with an indicator of monetary policy surprises – where surprises are inferred from interest rate derivatives immediately after each monetary policy announcement in Brazil (see Kuttner, 2001, Gertler and Karadi, 2015). Relative to the same firm and quarter, a private domestic bank subject to a 1 percentage point (pp) higher increase in RRs, i.e., tightening, decreases credit supply by 1.68 pp and by 1.90 pp if simultaneously subjected to a (one standard deviation) surprise policy rate tightening.

We find that bank heterogeneity is relevant in the RR lending channel and show that foreign and government banks can partially mitigate the effects of the policy. Banks with more capital also mitigate the related effects on credit supply, but only following RR tightening. After controlling for bank heterogeneity, we do not find differences in policy effectiveness comparing episodes of tightening versus loosening.

Economic policy uncertainty (EPU) is also found to be an important state variable in the transmission mechanism. Banks more exposed to RR loosening extend 19% less credit when the Brazilian EPU index from Baker, Bloom, and Davis (2016) is one standard deviation higher.

These results are robust and not driven by other local or global variables possibly correlated with the credit cycle, such as changes in the balance of payments, commodity prices, global liquidity, risk aversion, or by any influential quarter of policy action.

The empirical literature on macroprudential policy typically relies on time-varying aggregate indices to estimate the policy effects on the credit cycle, but the indices are mute to policy intensity and banks' differential exposure to policy interventions. We put the macroprudential index approach to the test and find results that are qualitatively similar but of significantly lower magnitude than in other studies.

Importantly, during tightening, firms do not insulate from RRs and synergies with monetary policy are relevant and significant. Real effects on employment are not economically significant during tightening or loosening of RRs.

The theoretical case for RRs as a macroprudential policy tool. RRs are often expressed as a pigouvian tax on banks (Stein, 2012). This regulatory tax withdraws liquid funds from banks during tightening and represents a cash injection during loosening. Kashyap and Stein (2012) build a model where central banks can change not only short-term policy rates, but also the interest on and quantity of reserves. In their integrated framework, RRs become a tool to face financial stability risks, granting monetary policy more independence to manage inflation-output cycles.[2,3] Richter, Schularick, and Shim (2019) corroborate this view by finding limited effects of macroprudential policy (including RRs) on output and inflation.[4]

The pigouvian tax approach to macroprudential policies is the most common in the macro literature. The key idea is that a pecuniary externality arises when private agents do not fully internalize the effects of their borrowing decisions in market prices and collateral value (see Kehoe and Levine, 1993, Lorenzoni, 2008, Benigno et al., 2013).[5] Bianchi and Mendoza (2018) and Jeanne and Korineck (2019) find that a countercyclical tax is the optimal macroprudential policy, preventing overborrowing while improving welfare.

In certain EMs, the policy rate can be acyclical or procyclical depending on their vulnerability to external conditions. In particular, "fear of floating" might constraint central banks to respond to inflation with the policy rate (Calvo and Reinhart, 2002). In these cases, when external conditions are binding[6], authorities commonly rely on RRs (particularly on foreign

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[2] "(…) by broadening the scope of reserve requirements, the central bank can simultaneously pursue two objectives: it can manage the inflation-output tradeoff using a Taylor-type rule, and it can regulate the externalities created by socially excessive short-term debt issuance on the part of financial intermediaries" (Kashyap and Stein, 2012).
[3] There is a large literature evaluating the declining role of RRs as a monetary policy instrument following the adoption of Taylor-type rules in advanced economies. Indeed, in this context, RRs are seen as inefficient (see Sellon and Weiner, 1996). Several papers estimate this channel of RRs and corroborate these findings (see Glocker and Towbin, 2012, Areosa and Coelho, 2013), but we focus on the macroprudential role of RRs.
[4] Reinhart and Rogoff (2013) also support the view that multiple goals should be backed by multiple instruments and highlight the role of RRs to influence credit provision.
[5] Several quantitative models focusing on macroprudential policy followed (see Bianchi, 2011, Gertler, Kiyotaki, and Queralto, 2012). Bianchi and Mendoza (2018) present a dynamic equilibrium model of financial crises and find that a state-contingent tax (such as countercyclical RRs) reduces crisis probability and severity. Cantú, Gondo, Martinez (2024) present empirical evidence of this mechanism and effects of RRs in the probability and severity of a crisis.
[6] Differently from policy rate changes, tightening (loosening) RRs are unlikely to attract capital inflows (outflows). Brei and Moreno (2019) find that tightening the policy rate simultaneously increases lending and deposit rates. On the other hand, tightening RRs increase lending rates but decreases deposit rates, hence discouraging carry trades.

deposits) as a substitute for the short-term policy rate (Federico, Vegh, and Vuletin, 2014). Agénor, Alper, and da Silva (2018) present a model exploring this channel and find a countercyclical rule for RRs addresses credit exuberance in the context of "fear of floating".

Contributions to the literature on macroprudential policy with RRs. We show that RRs are effective as a state-contingent tax to manage *domestic* credit booms and busts through their impact on credit supply. This result corroborates the theoretical literature, and the policy rationale just mentioned. The use of loan-level data is key for this result. It allows us to control for demand shifts that could otherwise bias the estimated supply effects (Araujo et al., 2020).[7] To the best of our knowledge, this is the first loan-level paper to estimate the effects of RRs on *domestic* credit cycles[8] disentangling and documenting synergies with monetary policy using loan-level data. It is also the first paper to connect higher levels of economic uncertainty with lower effectiveness of macroprudential policy and to assess the role of bank capital in mitigating RR tightening.

Most empirical papers using loan-level data explore the impact of RRs on dollar denominated deposits and focus on the global financial cycle. Camors et al. (2019) use loan-level data to explore a tightening of RRs in Uruguay, where RRs are implemented as a "tax" on dollar denominated deposits with negative effects on the credit supply of banks. Epure et al. (2018) use loan-level data from Romenia and find that macroprudential policy (including RRs) moderates household credit growth in foreign currency when the VIX is low. The conclusion from this literature (which complements our results) is that RRs can alleviate spillovers of the *global* financial cycle and address policy makers "fear of floating".[9] However, countries such as Brazil

[7] Araujo et al. (2020) show that macroprudential policy studies using microdata present estimates up to three times larger than those using aggregate data because of credit demand shifts and leakage.
[8] Cerutti, Claessen, and Laeven (2017) and Fendoglu (2017) find that RRs as well as several other macroprudential instruments negatively correlate with credit growth in a cross-country sample of EMs.
[9] Mora (2014) and Alper et al. (2018) corroborate these findings using bank-level data from Lebanon and Turkey, respectively.

and China (Chang et al., 2019) have banking sectors with low levels of foreign debt and dollar-denominated deposits and thus offer an opportunity to study RRs targeting mostly the *domestic* credit cycle.

Another large strand of empirical literature makes use of macroprudential indices in cross-country data (see Cerutti, Claessens, and Laeven, 2017, Epure et al., 2018, Cizel et al, 2019, Gambacorta and Murcia, 2020). These papers often find policies targeting the borrower, such as loan-to-value limits, are more effective than the ones targeting the banks, such as RRs (see Akinci and Olmstead-Rumsey, 2018, Alam et al., 2019). They also find macroprudential policy is more effective during tightening - in booms - than loosening - in busts (see Cerutti, Claessen, and Laeven, 2017). Yet, the macroprudential index approach does not properly account for policy intensity or differential bank exposure. Our approach overcomes such difficulties and helps to qualify this literature.

We take these issues of asymmetries and effectiveness to loan-level data. After properly controlling for bank heterogeneity and firm-level demand within each firm-quarter, we find that the credit channel is just as potent during tightening as during loosening of RRs for private domestic banks in Brazil. Private domestic banks with higher capital ratios are insulated from policy tightening, suggesting substitution between capital and liquidity (see Acosta-Smith et al., 2019). We also find a stronger policy channel using a variable sensitive to policy intensity than with an alternative mimicking the macroprudential index approach from the earlier literature. In other words, cross-country studies can overestimate the role of asymmetries and underestimate the role of RRs.

Finally, we explore real effects on employment and find modest effects, in line with Richter, Schularick, and Shim (2019).

Contributions to lending channel literature. We contribute to the lending channel literature by disentangling the effects of interest rate and RR policies and identifying their synergies. While macroprudential policy may have modest effects on inflation and output (see Richter, Schularick, and Shim, 2019), monetary policy is known to have strong effects on credit supply (see Jimenez et al., 2012). Thus, disentangling these effects on credit requires both loan-level panel data, and credible identification strategies. Our key strategy allows for a large degree of model saturation with fixed effects and simultaneous interactions between RRs and other macroeconomic factors - in a sense implementing a "horserace" between policy synergies (RRs and interest rate) and confounding macroeconomic conditions possibly correlated with greater bank exposure to RRs (see Jimenez et al., 2014).

To measure the combined policy effects, we interact our bank-level exposure to RRs with monetary policy. We focus on monetary policy surprises extracted from high-frequency interest rate derivatives (see Kuttner, 2001) and implement a "horserace" between the combined policy and local and global shocks. In line with Bruno, Shim, and Shin (2017), we find that the combined policy has real effects on firms' credit borrowing only during tightening of RRs.

Our results have implications for policymakers trying to unfreeze credit markets using RRs as several EMs did during the GFC and during the COVID-19 pandemic. EMs that face constraints to use the policy rate counter-cyclically or that have reached their effective lower bound can still stimulate credit supply by relying on RRs alone.

Contributions to the literature on EPU. We find banks are less responsive to RRs when EPU is high. There are several strands of the literature related to the role of policy uncertainty. The key theoretical mechanism at play is the lower amplification of demand shocks when uncertainty is high as economic agents become more cautious.

Bloom (2009) shows that economic uncertainty shocks can reduce aggregate demand by making firms more cautious in their investment and hiring decisions. The same mechanism results in less effective stimulus policy in the immediate aftermath of uncertainty shocks (Bloom et al., 2018). By reducing the amplification mechanism of stimulus policy, these shocks reduce the sensitivity of the credit cycle as well. In the empirical front, Wu and Suardi (2021) show banks become more conservative after uncertainty shocks, reducing credit supply, increasing loan spreads, and tightening contract terms. Asharaf and Shen (2019) also argue EPU affects bank loan pricing by changing the default risk of borrowers and document higher loan spreads after uncertainty shocks. They argue that banks tighten credit supply in such cases likely because of a higher probability of default in their portfolios.

Our paper also documents negative credit supply effects but turns to the effectiveness of stimulus policy. We offer new empirical evidence in response to Bloom et al (2018), by showing that policy stimulus is less effective when policy uncertainty is high. From a policy perspective, this means that policy makers trying to unfreeze credit markets, such as during COVID-19, must ease RRs more aggressively if EPU is high.

The paper proceeds as follows. Section I discusses the main policy developments with RRs in Brazil; presents the counterfactual variable used throughout this paper to identify exposure to RRs; and defines the high-frequency indicator for monetary policy surprises. Section II describes the data and identification strategy. Section III discusses the results and Section IV concludes.

I. RRs in Brazil

RRs were rarely changed in Brazil prior to the great financial crisis. Indeed, before 2008, the last change in RRs happened in 2003. In September 2008, the ratio of RRs in Brazil was high by international standards. It represented on average 23% of total liabilities subject to RRs (LRR)[10] while Montoro and Moreno (2011) reported emerging market ratios below 15% and developed market ratios below 5%. Vault cash and balances deposited at the BCB by September 2008 stood at BRL 253 bn (USD 149bn or 8.2% of GDP). The BCB reduced RRs to the historically low levels of 18% in November of the same year in response to a liquidity squeeze in the interbank market and a strong credit crunch (BCB, 2008). This was the main and the first of many interventions with RRs between 2008 and 2015 (Table I).

Insert Table I about here

Credit started recovering in early 2010 (Figure I), but the steep credit growth was characterized by low origination standards, such as high debt-to-income ratio on auto loans and payroll lending as detailed in the Financial Stability Report (BCB, 2010). In response, the BCB raised RRs close to its pre-GFC levels in March 2010.

Figure I

However, year-over-year (yoy) credit growth remained strong (Figure I), and the BCB tightened RRs again in December 2010 (Table I). Therefore, we highlight three main policy events in our sample - September 2008, March, and December 2010 - but the sample is rich in other interventions between 2008 and 2015 (Table I) including another loosening cycle in mid-2012.[11]

[10] Total liabilities subject to RRs (LRR) are core liabilities, including demand deposits, times deposits and savings Table I presents the components as well as RR rates.
[11] Table I is not exhaustive, beyond changes in RR ratios, changes in deductibles and smaller adjustments have happened in every quarter between 2008 and 2015.

RR(count) - at the right-hand side (RHS) of Figure I - represents our (counterfactual) variable of interest, calculated using system-wide totals. We will define this variable shortly. It is worth noticing the financial stability and countercyclical nature of RRs, with changes in RRs following the evolution of aggregate bank credit. The correlation between these two time-series is 0.50 (Figure I). As a reference, the correlation between credit growth and CPI inflation is far more modest at 0.14.

Relative to other macroprudential policies implemented during the same period, changes in RRs were the only ones directly affecting credit to firms.[12] In terms of scope and depth, they were the most important tool used in Brazil in this period (BCB, 2011, Pereira da Silva and Harris, 2012).

The use of RRs has been balanced in episodes of tightening (when aggregate quarterly changes in RRs are positive) and loosening (when these changes are negative) between 2008 and 2015. The gray areas in Figure II represent quarters of loosening policy with RRs[13] and the black line represents the evolution of overnight monetary policy rate in Brazil, the Selic (RHS). The correlation between these two variables is 0.31.

Insert Figure II about here

[12] There are two other macroprudential policies in this window, but they targeted credit to individuals and not firms. The policies were a 90% LTV limit on housing finance in 2013 (Araujo, Barroso, and Gonzalez, 2020) and increased capital requirement, risk weights, on auto loans of very long maturities in 2010 (Martins and Schechtman, 2013).

[13] Notice that negative quarterly changes in the counterfactual variable, RR(count), are considered loosening episodes and not RR(count) *per se* - Figure III. The 2008 quarters preceding the first macroprudential intervention in September 2008 are counted as tightening quarters because of an intervention raising RRs for bank conglomerates taking interbank deposits from non-bank financial subsidiaries. This intervention was simply closing a regulatory arbitrage opportunity and was not motivated by financial stability concerns.

During this period, the BCB managed mainly four RR components: RRs on demand deposits (unremunerated), on savings (remunerated according to the savings accounts reference rate), on time deposits (remunerated at the overnight policy rate), and an additional component comprised of these three subcomponents. The additional component (including all its subcomponents) was remunerated at the overnight policy rate (Selic). The BCB also managed RR deductibles, conditional deductibles, exemption thresholds, eligible liabilities, and remuneration of RRs.[14] For example, before 2008, the ratio of RRs on time deposits was 15% (Table I), i.e., for each bank, 15% of the total amount of time deposits was forcibly deposited at the BCB. In September 2008, this ratio was reduced to 13.5% thus introducing liquidity to the banking system (loosening).

Bank-level exposure to RRs, Counterfactual RRs. RRs affected mostly banks with larger shares of core liabilities, which tend to be the bigger banks in Brazil (Takeda, Rocha and Nakane, 2015). Similarly, the smaller banks in our sample were unaffected by changes in RRs because of a deductible. Moreover, *each bank's particular mix of deposits* was relevant to identify the effects of changes in RRs, because the related components and subcomponents were not necessarily changed at the same time (see Table I). This implies that RR policy has had both time-varying and cross-sectional variance.

To capture both, we build a counterfactual variable relying on the rules available before the first intervention in September 2008. Because of constant changes in RRs, comparing current and counterfactual RRs is useful to summarize the impacts of regulatory changes on each bank in one figure. The counterfactual variable is the same used in Camors et al. (2019) and is

[14] Table I is not exhaustive and only presents changes of RRs' rates. Also notice that two other components are used for short periods in Brazil: RRs on short FX positions of banks and RRs on deposits from leasing companies (within bank conglomerates). See Cavalcanti and Vonbun (2013) for all details.

straightforward to calculate. The RR ratios, deductibles, conditional deductions, and exemptions are calculated for each bank and quarter based on the *old rules* available before September 2008 ($\text{Counterfactual}_{b,t-1}$) and on the *current rules* ($\text{Current}_{b,t-1}$) in every quarter. For each bank, the difference between these two variables relative to its total liabilities ($\text{Liabilities}_{b,t-1}$) becomes our variable of interest $\Delta\text{ResReq}_{b,t-1}$ used throughout this paper. Thus, $\Delta\text{ResReq}_{b,t-1}$ is the change in the mean RRs *net* of the change in a counterfactual scenario of unchanged policy. See equation (1) to (3).

$$\text{Counterfactual}_{b,t-1} = \sum_{j=1}^{J} \emptyset_{j,2008}\ x\ \left(\bar{D}_{b,j,t-1} - \text{deduct}_j\right) - \text{deduct}_{j,2008} \qquad (1)$$

$$\text{Current}_{b,t-1} = \sum_{j=1}^{J} \emptyset_{j,t-1}\ x\ (\bar{D}_{b,j,t-1} - \text{deduct}_j) - \text{deduct}_{j,t-1} - \text{deduct}_{j,k,t-1} \qquad (2)$$

$$\Delta\text{ResReq}_{b,t-1} = 100 * \left[\Delta\left(\frac{\text{Current}_{b,t-1} - \text{Counterfactual}_{b,t-1}}{\text{Liabilities}_{b,t-1}}\right)\right], \qquad (3)$$

where b refers to bank, t to quarter, j to one of the four RR categories, k to bank regulatory capital bucket, $\emptyset_{j,t-1}$ to the current rules (i.e., the ratio of RRs of category j in t-1), and $\emptyset_{j,2008}$ to the counterfactual rules (i.e., ratio of RRs available before September 2008 for each category j[15]). Meanwhile, $\bar{D}_{j,t-1}$ refers to the average volume of deposits in category j in t-1, deduct_j[16] to the exemption threshold (or the minimum volume of deposits in category j above which RRs become binding), $\text{deduct}_{j,t-1}$ to a fixed deductible applied to the absolute value of RRs of each category j in

[15] The RR ratios available between August 2003 and September 2008 were: 15% on time deposits, 45% on demand deposits, 20% on savings; and in the additional components (8% on demand and time deposits, and 10% on savings). See Table I.
[16] This deductible is unchanged in the sample and was 30M for time deposits and 44M for demand deposits.

t-1, $\text{deduct}_{j,2008}$ to a fixed deductible applied to the absolute value of RRs of each category *j* before September 2008,[17] and $\text{deduct}_{j,k,t-1}$ to the deductible created in February 2020, which takes different values pending on the bank regulatory capital bucket k.[18]

In equation (3), we use the variation in counterfactual reserves to filter out the determinants of RRs other than the regulatory changes, capturing only macroprudential "taxation" at the bank level. This variable is calculated for each bank and quarter, and therefore measures the relative amount of cash being injected (during busts) or withdrawn (during booms) as a result of the change in reserve requirement rules. It therefore captures banks' overall exposure to changes in RRs. While equation (3) measures differential "taxation" at the bank level, a possible concern is that the liability mix may change towards less affected deposits. We address this concern in the robustness section.

Identifying surprises in the policy rate and interactions with RRs. Changes in the policy rate can have strong effects on credit supply. Therefore, we introduce the overnight policy rate (Selic) and its interaction with bank-level exposure to RRs to explore the combined effects or synergies. To the extent that monetary policy and macroprudential policy with RRs may also co-move, this strategy disentangles their effects on credit supply – hence delivering a more credible identification of the lending channel of RRs and its synergies with monetary policy.

One related concern is that the BCB responds to expected macroeconomic developments; thus, changes in Selic are not exogenous to the credit cycle. Moreover, banks may anticipate

[17] For example, category *j* term deposits had a deductible of 300M (Circular BCB n. 3262, 2004) before September 2008. Thus, $\text{deduct}_{j,2008}$ is always 300M, and $\text{deduct}_{j,t-1}$ takes the value of 300M until it is increased to 700M (Circular BCB n. 3408, 2008) and 2000M (Circular BCB n. 3410, 2008).

[18] For term deposits, three *k* buckets were created. Financial institutions with regulatory capital below 2bn had a deductible of 2bn, those with regulatory capital between 2bn and 5bn had a deductible of 1.5bn, and those with more than 5bn of regulatory capital had a deductible of zero (Circular BCB n. 3485, 2010, Article 5). Both *k* thresholds and the related deductible available for the *k* bucket were adjusted within our sample. All these changes are captured in $\Delta\text{ResReq}_{b,t-1}$.

changes in the policy rate and adjust credit policy in advance. The forward-looking behavior of these two agents warrants one to focus on the unexpected variation of monetary policy to identify the policy effects.

To this end, following Kuttner (2001), we decompose changes in the overnight policy rate into two additive components: an unexpected component or surprise (Δi_t^s), proxied by the one-day change in interest rate derivatives immediately after each Monetary Policy announcement; and an expected component (Δi_t^e), reflecting the difference between Δi_t^s and the announced change in the policy rate (Δi). See equation (4):

$$\Delta i_t = \Delta i_t^s + \Delta i_t^e \tag{4}$$

The immediate reaction of interest rate derivatives, or the one-day adjustment in the price of these contracts, captures the extent of market "surprise" to the announcement made in the previous day. Conversely, the difference between the surprise and the announcement change is already incorporated in the derivative price of the previous day, i.e., it is "expected" or anticipated (Kuttner, 2001).

In Brazil, the Monetary Policy Committee (Copom) meeting makes all the announcements when markets are already closed. There are no *ad hoc* announcements in the sample, i.e., all announcements of changes in the policy rate (Selic, Δi) followed Copom meetings. [19] Surprises (Δi_t^s) are abundant across the sample albeit their magnitude tends to be much lower than the related expected component. They revolve around zero in the sample and are relatively balanced between loosening and tightening episodes. In Figure III, we present quarterly aggregated surprises together

[19] Differently from Kuttner (2001), we use the changes in the 30-day interest-rate swap and not in the 30-day future as the proxy for surprises. The choice is for convenience since future contracts must be adjusted by the remaining days to maturity whereas the swaps represent at each day a reference (fixed) risk-free rate for the following 30 days naturally eliminating this issue.

with the change in Selic. The difference between the hollowed and the colored area is the expected change in the policy rate. In Appendix A.1, we present the policy rate stance before and after each Copom announcement in Brazil and the related unanticipated content.

Insert Figure III about here

Since 2006, there have been eight Copom meetings per year in Brazil. Hence, we accumulate two one-day changes in the interest rate derivatives (one every 45 days) to build the quarterly surprise proxy, Δi_t^s. We also use quarterly changes in the overnight policy rate (Δi) for consistency. The high-frequency strategy to identify unexpected variation in monetary policy decisions has been used in many empirical papers (e.g., Gurkaynak, Sack and Swanson, 2005, Bernanke and Kuttner, 2005, Gilchrist and Zakrajsek, 2012). Yet, Gertler and Karadi (2015) and Miranda-Agrippino and Ricco (2020) show that autocorrelation across surprises identified using this strategy could be an issue. In Appendix A.2, we follow Miranda-Agrippino and Ricco (2020) but find no evidence of within-quarter or within-year auto-correlation using Δi_t^s.[20] In Figure IV, we show the small correlation between aggregated RR(count), changes in quarter-over-quarter (qoq) policy rates, and policy surprises.

Insert Figure IV about here

[20] The F-value of these regressions is below one even after the introduction of other possibly correlated contemporaneous announcements (e.g., employment and inflation). Using Δi, we find some autocorrelation, but the F-value of these regressions remains below ten.

II. Data and Identification Strategy

The main database used in this paper is the credit register of the BCB ("Sistema de Informações de Créditos"). We augment the data with macroeconomic and bank controls as well as firm controls from the formal employment registry of the Brazilian Ministry of Labor and Employment ("Relação Anual de Informações Sociais - RAIS)". The final sample spans all 30 calendar quarters from 2008Q1 to 2015Q2.

A. *Data Description*

The credit registry contains detailed and comprehensive[21] information about the underlying credit contracts, including credit amounts, ex-ante risk classification (which connects to each loan provision for non-performing loans), and information on loan performance, i.e., delinquency. We further aggregate these credit contracts at the bank-firm-quarter level to calculate total credit exposure. We follow the dynamics of each bank-firm pair throughout the sample. The main dependent variable is the real growth rate[22] of the bank-firm total credit exposure (in log terms) winsorized at the 1st and 99nt percentiles.[23]

We exclude from the sample financial firms as well as loans that are not originated by commercial banks (8%). Moreover, we focus on credit in local currency and drop observations with at least one loan indexed to currencies other than the Brazilian Real (BRL). They represented less than 0.5% of the loans.

[21] Up to December 2011, the credit registry covered all loans greater than BRL 5,000 (USD 3,000 in 2011), and, after that, all loans greater than BRL 1,000 (USD 425 in 2014). We keep only loans greater than BRL 5,000 in this paper for consistency.

[22] Total firm-bank credit exposure is first presented in constant BRL. It is then put in log format and quarterly differences are taken.

[23] Apart from dummies, all other controls and the dependent variable are winsorized.

We also focus on multiple bank relationship firms for identification of credit supply using the firm-quarter FE estimator (see Jimenez et al., 2014). This step restricts the original sample to 85 per cent of firms in terms of total credit extended by all financial institutions.[24] After these steps, we end up with more than 25M observations.

For computational reasons, we sample the data from the original database by firm tax ID, i.e., we first collect a 10 per cent random sample of firms ever present in the credit registry and withdraw their complete credit histories from all banks that ever lent to these firms. We exclude firms with less than two quarters of data. After this process, we end up with a working sample of 2,595,398 observations encompassing 90,440 firms and 83 commercial banks across 30 quarters.

From these data, we build the following firm controls (firm$_{f,t-1}$): the ex-ante (quarterly lagged) total firm credit (firm credit$_{f,t-1}$) in log terms, a dummy variable in case the firm is in default, i.e. if it has at least one loan in arrears for more than 90 days against any financial system player in *t-1* (firm default$_{f,t-1}$), and the (log of) the number of formal employees (n employees$_{f,t-1}$). We also build risk$_{b,f,t-1}$, the weighted average provision for non-performing loans assigned by each bank to all its loans against the same firm in *t-1*. The latter is the only control available at the bank-firm-quarter dimension - Table II.

Insert Table II about here

[24] Identification of bank supply is superior with firm-quarter fixed effects, but a possible concern is that multiple bank relationship (MBR) firms are fundamentally different from single bank relationship (SBR) firms leading to misrepresentative results. Degryse et al. (2019) shows that MBR firms are much smaller than SBR in Belgium and this translates into different dynamics in loan outcomes. The average number of employees in our firm level MBR sample is 9.39 (exp(2.24) on Table I) and, in the complete sample, about 8 with a similar standard deviation 3.6 and similar median of 8 employees Moreover, in Belgium only 46 per cent of credit is extended to MBR firms. Thus, we do not find substantial differences between these two samples and we focus on MBR for identification. In this respect, our sample is closer to the one in Spain, where MBR is just as representative and banks provide most of the credit in the economy (Jimenez et al., 2014).

We use the following bank controls ($\text{bank}_{b,t-1}$) common to the bank lending channel literature to assess bank's strength: the core capital-to-assets ratio (capital_{t-1}), the natural logarithm (ln) of bank's assets (size_{t-1}), the liquid to total assets ratio (liquidity_{t-1}), the share of foreign liabilities to core liabilities (fx liab_{t-1}), the share of non-performing loans to total credit (npl_{t-1}), and two dummy variables for banks with foreign (foreign_{t-1}) and government (gov_{t-1}) control.[25]

As defined before, the main variable of interest is the bank ex-ante exposure to changes in RRs, $\Delta\text{ResReq}_{b,t-1}$. This variable averages -1.64 with a standard deviation of 2.61 in the bank-level sample. Notice that the median is close to zero (Table II). At the loan level, the average is -1.38 and the standard deviation 2.65.

The macro controls (macro_{t-1}) are the consumer price index (IPCA, ΔCPI_{t-1}), GDP growth (ΔGDP_{t-1}), and changes in the current account to GDP ratio ($\Delta\text{CA/GDP}_{t-1}$). We also use two monetary policy proxies: the quarterly change in the overnight policy rate in Brazil (Selic, Δi_{t-1}) and quarterly aggregated policy rate surprises (Δi^s_{t-1}) extracted from interest rate derivatives around MP announcements (Kuttner, 2001).

B. *Identification Strategy*

The baseline and most saturated regression to identify the effects of RRs on credit supply is (5):

$$\Delta\ln(\text{credit})_{b,f,t:t+1} = \Delta\text{ResReq}_{b,t-1} + \text{risk}_{b,f,t-1} + \text{bank}_{b,t-1} + \alpha_{f,t}\ , \tag{5}$$

where $\Delta\text{ResReq}_{b,t-1}$ is bank-level exposure to RRs from equation (3), $\text{bank}_{b,t-1}$ are bank controls, $\text{risk}_{b,f,t-1}$ is the risk control, and $\alpha_{f,t}$ are firm-quarter fixed effects (one for each firm-

[25] There is a large literature showing that Δi_{t-1} has differential effects on the credit supply of larger banks (see Kashyap and Stein, 2000), banks with more capital and liquidity ratios (see Jimenez et al., 2012), NPL (see Accornero et al., 2017), non-core liabilities (e.g, Hahm, Shin and Shin, 2013), foreign (e.g, Morais, Ruiz and Peydro, 2019) and government banks (e.g, Bonomo and Martins, 2016).

(year:quarter) pair), which fully control for credit demand shifts (Jimenez at al., 2014). We also run less saturated versions of equation (5) with firm and macro-controls as well as separate regressions on quarters following loosening and tightening of RRs. Parameters are omitted for simplicity.

Loan-level data are aggregated in the bank, firm, and quarter dimensions.[26] Because $\alpha_{f,t}$ absorbs firm-level mean credit growth, we can confidently alleviate concerns about simultaneous causality between firms' credit demand and banks' credit supply (Khwaja and Mian, 2008). This is a powerful and necessary control since RR policy is motivated by excessive credit appetite. Put differently, our variable of interest, $\Delta\mathrm{ResReq}_{b,t-1}$, is supposed to be orthogonal to the error term conditional on $\alpha_{f,t}$ (Jimenez at al., 2014). $\Delta\mathrm{ResReq}_{b,t-1}$ reflects how much more differentially affected ("taxed") banks b lend to firm f, and the related effects on credit supply are compositional.

We account for the interactions between bank controls and $\Delta\mathrm{ResReq}_{b,t-1}$ in equations (6) and (7). In equation (7), we add the interaction with policy rate surprises, which captures the effects of synergies between monetary and macroprudential policy or the combined effects of these policies.

$$\begin{aligned}\Delta\ln(\mathrm{credit})_{b,f,t:t+1} = {} & \Delta\mathrm{ResReq}_{b,t-1} + \mathrm{risk}_{b,f,t-1} + \mathrm{bank}_{b,t-1} \\ & + \Delta\mathrm{ResReq}_{b,t-1} * \mathrm{bank}_{b,t-1} + \alpha_{f,t}\,, \end{aligned} \tag{6}$$

$$\begin{aligned}\Delta\ln(\mathrm{credit})_{b,f,t:t+1} = {} & \Delta\mathrm{ResReq}_{b,t-1} + \mathrm{risk}_{b,f,t-1} + \mathrm{bank}_{b,t-1} \\ & + \Delta\mathrm{ResReq}_{b,t-1} * \mathrm{bank}_{b,t-1} \\ & + \Delta\mathrm{ResReq}_{b,t-1} * \Delta\mathrm{i}^{s}_{t-1} + \alpha_{f,t}\,, \end{aligned} \tag{7}$$

[26] Consequently, the dependent variable represents, credit growth of firm f against two or more banks b simultaneously lending to f. All credit lines, drawn and undrawn amounts between b and f are considered altogether.

For identification of the combined effect of both policies, we also add global and local macro-variable (Z_{t-1}) interactions in equation (8), which work as a “horserace” between $\Delta \text{ResReq}_{b,t-1} * \Delta \text{i}^{S}_{t-1}$ and macro-variables possibly correlated with $\Delta \text{i}^{S}_{t-1}$. The term $\Delta \text{ResReq}_{b,t-1} * z_{t-1}$ captures these interactions.

$$\begin{aligned} \Delta \ln(\text{credit})_{b,f,t:t+1} = {} & \Delta \text{ResReq}_{b,t-1} + \text{risk}_{b,f,t-1} + \text{bank}_{b,t-1} \\ & + \Delta \text{ResReq}_{b,t-1} * \text{bank}_{b,t-1} \\ & + \Delta \text{ResReq}_{b,t-1} * \Delta \text{i}^{S}_{t-1} \\ & + \Delta \text{ResReq}_{b,t-1} * \text{z}_{t-1} + \alpha_{f,t} \end{aligned} \tag{8}$$

where z_{t-1} can be one-year changes in the current account to GDP ratio ($\Delta CA/GDP_{t-1}$); one-year changes in the US short shadow rate (Δi^{US}_{t-1}); US equity volatility index (VIX) - e.g. Rey (2015); one-year changes in commodity prices (Δcommodity prices$_{t-1}$) - e.g. Drechsel and Tenreyro (2018); the economic policy uncertainty index for Brazil (EPU_{t-1} from Baker, Bloom, and Davis, 2016) - e.g. Bordo, Duca, and Kock (2016); CPI inflation (ΔCPI_{t-1}); GDP growth (ΔGDP_{t-1}); or an index of other macroprudential policies contemporaneously implemented in Brazil (not including RRs), Macropru policy$_{t-1}$. These policies, as already explained, target individuals’ and not firms’ credit.

Whereas all previous regressions make use of the bank-level exposure $\Delta \text{ResReq}_{b,t-1}$, we use the macro-variable X_{t-1} in equation (9) to explore the role of policy intensity.

$$\begin{aligned} \Delta \ln(\text{credit})_{b,f,t:t+1} = {} & \Delta \text{ResReq}_{b,t-1} + \text{risk}_{b,f,t-1} + \text{bank}_{b,t-1} \\ & + \text{X}_{t-1} * \text{bank}_{b,t-1} + \alpha_{f,t}\ , \end{aligned} \tag{9}$$

where X_{t-1} can be $\Delta \text{RR(count)}_{t-1}$, i.e., $\Delta \text{ResReq}_{b,t-1}$, the counterfactual variable, aggregated to reflect the whole banking system, or X_{t-1} can be the macroprudential index

ResReq_{t-1}. The index is very common in empirical cross-country studies. We follow Cerutti, Claessens, and Laeven (2017) and build an index that increases by 1 when a new tightening of RRs is in place and contracts by 1 when a new loosening is in place, thus ignoring policy intensity. A less saturated version with firm and macro-controls is also presented for both $\Delta\text{RR(count)}_{t-1}$ and ResReq_{t-1}.

Firms can insulate themselves from tightening of RRs by resorting to less affected banks (Jimenez et al., 2017). It is therefore important to account for this possible equilibrium effect.[27] In this spirit, to assess the net effect of RRs and synergies with policy rate surprises on firms' credit borrowing, we estimate equation (10) at the firm level. The most saturated firm-level equation that achieves this goal is:

$$\begin{aligned}\Delta\ln(\text{credit})_{f,t:t+1} = {} & \Delta\text{ResReq}_{f,t-1} + \text{risk}_{f,t-1} + \text{bank}_{f,t-1} \\ & + \Delta\text{ResReq}_{f,t-1} * \text{bank}_{f,t-1} \\ & + \Delta\text{ResReq}_{f,t-1} * \Delta\text{i}^{S}_{t-1} \\ & + \text{firm}_{f,t-1} + \alpha_f + \alpha_t, \end{aligned} \tag{10}$$

where all bank controls ($\text{bank}_{f,t-1}$), $\Delta\text{ResReq}_{f,t-1}$, and $\text{risk}_{f,t-1}$ are weight averaged using the ex-ante bank-firm total credit exposure, α_t are time FEs, and α_f are time-invariant firm FEs. In the absence of firm-time FEs, $\text{firm}_{f,t-1}$, α_f, and α_t altogether control for credit demand shifts. The same strategy of equation (10) is also used to explore changes in $\text{employment}_{f,t:t+1}$ and related effects in the number of $\text{hired}_{f,t:t+1}$ and $\text{fired}_{f,t:t+1}$[28] individuals.

[27] In Jimenez et al. (2017) firms fully mitigate the tightening of dynamic provisions in Spain by resorting to less affected banks. Other studies show that firms can fully (Jimenez et al., 2020) or partially (Iyer et al., 2014) insulate credit borrowing from negative shocks to their banks.

[28] Because number of fired and hired individuals are discrete non-negative counting variables, we use the high-dimensional Poisson pseudomaximum likelihood (PPML) estimator from Correia, Guimarães, and Zylkin (2020), but all other controls, fixed effects and two-way clustering strategy remain the same. The estimator is robust to heteroskedasticity, including from zero-inflated dependent variables.

Robustness

In the Appendix, we carry out several exercises. We build a bank-level fixed effects panel and regress quarterly changes in savings, term, and time deposits against our bank-level exposure to RRs to alleviate concerns that banks are quickly adjusting their liabilities towards less affected deposits and evading macroprudential policy with RRs (A.3). We replicate our baseline estimates using the overnight policy rate (Selic) directly instead of monetary policy surprises (A.4). We exclude policy influential quarters, i.e. those with very strong changes in RRs, such as 2008Q3-Q4 and 2010Q1-Q2 (Figure I), to alleviate concerns that results are driven by few episodes of RRs changes (A.4) – Barroso et al. (2020). We also introduce additional controls and interactions with variables that capture each bank ex-ante deposit mix (A.6), which alleviates concerns that results are driven, for instance, by greater exposure to demand deposits or any other "taxable" liability.

III. Results

A. Loan-level analysis

We start by estimating the lending channel of RRs in Table III using equation (5). The variable of interest, $\Delta \text{ResReq}_{b,t-1}$, captures state-contingent changes in RRs and represents banks' differential exposure to changes in RRs. In columns (1) to (3), we use all 30 quarters in our sample. A 1 pp increase (decrease) in this variable, tightening (loosening), is associated with an average decrease in bank-firm credit of 0.52 to 0.56 pp in the following quarter (columns 1 and 2). The average effect of a 1 pp (which is close to one standard deviation or 0.97) increase in the short funds rate (Selic) is 0.71 pp. If we consider a comparable interest rate surprise, [29] the effect (from column 2) would be a credit contraction of 0.63 pp.

Insert Table III about here

A 1 pp GDP growth in the previous year correlates to a 0.51 to 0.62 pp increase in credit. Higher credit growth is associated with ex-ante bigger, more capitalized banks, and those less dependent on foreign liabilities. Riskier loans to smaller and more indebted firms, and firms in default, show lower credit growth, and we find no significant results for the dummy variable loosening that identifies the policy quarters with aggregate negative changes in RRs.

In column (3), we introduce firm-quarter FEs to better control for credit demand shifts, we observe modest changes in the estimates of $\Delta\text{ResReq}_{b,t-1}$ as compared to the previous columns. According to Oster (2019), this suggests that this variable is orthogonal to unobservable firm-level credit shifts identified as credit demand (see Jimenez et al. 2012). While countercyclical policy objectively responds to credit demand shifts, the extent to which banks are differentially "taxed" via RRs is found to be exogenous to firms, which creates room for (unbiased) firm-level analysis of the related credit supply channel. Relative to the same firm and quarter, banks facing an additional 1 pp change in RRs respond with a -0.67 pp credit supply change in the following quarter.

In column (4), we restrict the sample to the 16 quarters where aggregate (system-wide) changes in RRs are negative, i.e., to quarters of loosening policy (in busts); and, in column (5), to the 14 quarters of positive changes in RRs, i.e., tightening quarters (in booms). We present the

[29] One standard deviation of quarterly changes in interest rate surprises is 0.14 pp.

difference between both in column (6). In line with Cerutti, Claessens, and Laeven (2017) and Fendoglu (2017), the tightening phase of RRs is apparently stronger on average. Moreover, banks ex-ante share of non-performing loans (NPL) is not associated with lower credit growth during policy tightening (in booms) as it is during loosening (in busts), column (6).

In Table IV, we estimate equation (6) and present bank heterogeneities one at a time. Government and foreign banks mitigate some of the effects of RRs on credit. Whereas foreign banks strongly respond to the policies of their headquarters' country (Morais, Ruiz, and Peydró, 2019), government banks in Brazil respond countercyclically (Bonomo, Brito, and Martins, 2015) and are less sensitive to RRs. Alternatively, private domestic banks are more responsive. Relative to the same firm and quarter, private domestic banks facing an additional 1 pp change in RRs respond with -1.39 pp credit supply change in the following quarter (column 9), twice as much as the average bank (-0.67 pp as seen in column 1).

Insert Table IV about here

In Table V, we estimate equation (7), introducing interactions with the policy rate and bank heterogeneities during loosening and tightening of RRs. We find strong evidence of synergies between RRs and policy rate surprises. These synergies are present in loosening and tightening. Relative to the same firm-quarter, a private domestic bank subject to a 1 pp increase in RRs (tightening) decreases credit supply by 1.68 pp more (column 9) and by 1.9 pp[30] more if simultaneously subjected to (one standard deviation) policy rate tightening (column 9).

[30] The standard deviation of policy rate surprises is 0.14. Thus, 1.60*0.14=0.22, which leads us to 1.9 after adding the baseline of 1.68.

Insert Table V about here

Importantly, after controlling for bank heterogeneities, we find no significant difference between loosening and tightening of RRs (column 10). In other words, the asymmetric stronger response observed in the tightening episodes of Table III and found in several papers (see Cerutti, Claessen, and Laeven, 2017) is not driven by private domestic banks in Brazil but rather by government and foreign banks, which are less responsive to RRs (columns 6 and 9). Foreign banks, in particular, remain unreactive during busts, fully mitigating the effects of loosening policy (column 6).

We also find that bank capital alleviates tightening of RRs (column 9) in booms but not loosening (column 6) in busts. This result is statistically and economically significant. Whereas the average private domestic bank subjected to a 1 pp tightening in RRs decreases credit supply by 1.68 pp, stronger banks (one standard deviation above the mean of the core capital/assets ratio) decrease credit by 0.85 pp. This heterogenous response of stronger banks alleviating macroprudential policy tightening is documented in Camors et al. (2019) and entails substitution between capital and liquidity (Acosta-Smith et al., 2019).[31]

Banks more exposed to RRs could hypothetically be more exposed to other macroeconomic conditions or policies, leading to biased estimates of the effectiveness of RRs and of related synergies with monetary policy on credit supply. We alleviate these concerns by estimating equation (8), in another horserace exercise between $\Delta \mathrm{i}_{t-1}^{s} * \Delta \mathrm{ResReq}_{b,t-1}$ and interactions of RRs

[31] Acosta-Smith et al. (2019) offer theoretical and empirical results for capital and liquidity substitution. The authors show that a higher capital ratio induces the bank to hold less liquidity for two reasons. First, higher capital reduces the threshold level above which liquidity holdings are not sufficient to cover withdrawals. Second, higher capital increases the unit price of long-term assets, which reduces the loss the bank incurs in the case of selling its long-term assets. Thus, higher capital levels incentivize banks to hold less liquid assets.

with several possibly correlated local (Table VI) and global (Table VII) macroeconomic variables. The results found in Table VI (column 8) are similar to those of the most saturated regression in Table V (column 3). In other words, the effects of RRs on private domestic banks and related synergies with the policy rate have not been substantially affected by determinants of monetary policy such as changes in inflation and GDP growth, or a worsening in Brazil current accounts, or economic political uncertainty (EPU$_{t-1}$).

Insert Table VI about here

Nonetheless, we find positive economically and statistically significant effects in the interaction between RRs and the Baker, Bloom, and Davis (2016) EPU$_{t-1}$ index for Brazil. Private domestic banks subjected to 1pp lower RRs increase credit by 1.50 pp. If EPU$_{t-1}$ is one standard deviation (71 points) higher, they increase credit by 1.22 pp. This result suggests that loosening of RRs could be less effective to support credit under increased EPU, which mitigates about 19% of the average policy response. In our sample, EPU reaches the highest levels during the GFC and in 2015, when a corruption scandal called “car wash” became public, implicating many politicians and leading to a presidential impeachment. These periods also coincide with major loosening policy quarters (Figure V).

Insert Figure V about here

Bloom et al. (2018) developed a model showing that stimulus policy is temporarily less effective when uncertainty shocks hit the economy, because firms become more cautious in responding to price changes. Ashraf and Shen (2019) also document negative credit supply effects in the syndicated loan market when EPU is high and in election years. Our results echo this literature and show that macroprudential policy with RRs is also less effective when EPU is high, suggesting that banks also become more cautious under policy uncertainty.

RRs have been used in EMs to insulate currencies from global shocks, i.e., the "fear of floating" channel. RRs on foreign currency liabilities together with capital controls were common to attenuate credit booms fueled by global liquidity (Camors et al., 2019). These surges of capital inflows to the banking sector are also associated with periods of low implied volatility as measured by VIX (Rey, 2015, Epure et al., 2018). This usage of RRs in Brazil was restricted to banks' FX short positions (far more modest than foreign currency liabilities) and only between 2011 and 2013 (Table I). Yet, we interact the changes in the Fed Funds Ratio, US short shadow rate, and VIX with bank-level exposure to RRs and all other bank controls (Table VII). These results are mostly in line with the baseline ones. We also control for commodity prices and for a macroprudential policy index comprised of all macroprudential policies but RRs (Macropru Policy$_{t-1}$),[32] and the results remain unchanged. Hence, the global financial cycle, "fear of floating" and other macroprudential policies implemented in Brazil do not drive our estimates, providing additional evidence that RRs are effective to smooth *domestic* credit cycles.

Insert Table VII about here

The role of policy intensity. In Table VIII, we take the system-wide counterfactual variable and a derived macroprudential index for RRs, the most typical approach in the empirical literature, and

[32] None of these macro-prudential policies target directly or indirectly credit to firms. Still, possible "spillovers" from macro-prudential policies on auto loans and housing finance to firms' credit are not relevant.

take these to loan-level data by estimating equation (9). In columns (1) and (2), we introduce the system-wide (countercyclical) variable, the same used in Figures I, II and V. While this proxy reflects the intensity of the policy, it ignores the cross-sectional differences across banks we explored in the previous tables.

Insert Table VIII about here

The results we find in columns (1) and (2) are qualitatively similar to those of the previous tables. In column (1), a one standard deviation increase in the (aggregate) counterfactual would lead to a 2 pp decrease in credit from private domestic banks.[33]

In columns (3) and (4), we reproduce the results with a macroprudential index for RRs like the index from Cerutti, Claessens, and Laeven (2017), i.e., the index increases by 1 when a new tightening of RRs is in place and contracts by 1 when a new loosening is in place thus ignoring policy intensity. Again, the results are qualitatively similar, but this time a one standard deviation increase in the index would lead to a 1.43 pp decline in credit from private domestic banks.[34] This weakened result helps to qualify prior empirical research and suggests that accounting for policy intensity is relevant. Conversely, not accounting for policy intensity leads to an underestimation of the policy effects.

B. Firm-level analysis

We implement firm-level regressions from equation (10) in Tables IX and X. Firms associated with more exposed banks are not insulated from loosening or tightening of RRs (Table IX). This result is important. If firms can easily substitute credit by resorting to banks with more capital or other unaffected or less affected financial intermediaries following a tightening of RRs

[33] From summary statistics (Table II), the standard deviation of this variable is 2.01. Thus, -1.008*2.01 = 2.02 pp.
[34] From summary statistics (Table II), the standard deviation of this variable is 2.77. Thus, -0.518*2.77 = 1.43 pp.

(see Iyer et al., 2014, Jimenez et al., 2020), the transmission channel would "leak" and not be effective to dampen credit booms. However, we find strong results at firm level in both loosening and tightening periods. Synergies with policy rate surprises are statistically weaker during loosening of RRs in line with Bruno, Shim, and Shin (2017).

Insert Table IX about here

In Table X (Panel A), we replicate the firm level regressions, taking as dependent variable the changes in employment. Results are not statistically or economically significant. In Panel B, we explore the underlying data available on RAIS, i.e., the number of hired and fired individuals at the firm level. Because these are discrete non-negative counting variables, we use the high-dimensional Poisson pseudomaximum likelihood (PPML) estimator from Correia, Guimarães, and Zylkin (2020). We find no statistically significant changes in hiring behavior, but we find effects in firing. For each additional 1 pp loosening of reserve requirements faced by their bank, firms fire 1.6% fewer workers due to bank relationships and 2% fewer due to relationships with private domestic banks. For the average firm connected to private domestic banks, an RR loosening of 8.33 pp would be needed to prevent an additional firing (which is larger than three standard deviations of our bank-level exposure to RRs).[35] Thus, real effects of macroprudential policy with RRs are modest and not economically significant as in Richter, Schularick, and Shim (2019).

Insert Table X about here

[35] We take the average firm-level number of fired individuals of 6 (Table II) and coefficient 0.02 from Table IX column 4, Panel B (bottom), and calculate 1/(6*0.02).

C. Robustness

Banks could hypothetically adjust their deposits towards liabilities less affected by changes in RRs. The tightening of time deposits for instance was more pronounced in our sample, which could lead to an increase in demand deposits or savings. In Appendix A.3, we present a bank-level panel and regress quarterly changes in savings, demand and term deposits against the lagged counterfactual variable using the same controls from the loan-level panel and a similar setting. We find no significant results in loosening or tightening episodes. Thus, banks are not circumventing the policy, and the effects on credit supply estimated at the loan-level are not biased.

In Appendix A.4, we replicate Table V using the changes in the quarterly overnight policy rate (Selic) instead of policy rate surprises. The results are mostly in line with the previous ones. A private domestic bank simultaneously subjected to a 1pp tightening of RRs and one standard deviation (0.97) tightening of the policy rate decreases credit supply by 2.02 pp, in contrast to 1.9 pp from interest rate surprises (Table V).

We exclude the main policy quarters in Appendix A.5. First, we replicate the main results of the paper with policy rate surprises (column 1) and Selic (column 2). Second, we remove the main policy quarters from the sample (columns 3 and 4), i.e. we remove possibly influential quarters in which RRs policy cuts and hikes were large – 2008Q4, 2009Q1, 2010Q1, 2010Q2, 2010Q4 and 2011Q1 (Figure I). The main results remain unchanged and are not driven by few possibly influential quarters.

Whereas in Appendix A.3, we show banks do not circumvent RR policy adjusting their liability mix, in Appendix A.6 and A.7, we reproduce Tables III and V, respectively, but introducing controls that represent banks' ex-ante liability structure, i.e., their share of savings, time and demand deposits in log form. Introducing these variables helps to alleviate concerns that the credit supply effects are driven by bank's underlying liability mix and not by changes in RRs. However, our baseline results remain qualitatively and quantitatively similar.

Another concern is that banks' excess reserves – voluntary reserves hold at the BCB – could influence the lending channel, in which case they could introduce omitted variable bias or correlate with our variable of interest, influencing its estimates. Voluntary reserves were unremunerated and represented on average 0.08% of banks' total liabilities between 2008 and 2015[36]. To address these issues, we run baseline Table III introducing the variable excess reserves to total liability (exc reserves$_{b,t}$) as an independent variable in Appendix A.8. The related estimates are not statistically significant, and the variable of interest remains unchanged. In Appendix A.9, we interact the variable of interest with exc reserves$_{b,t}$, but this heterogeneity is also not statistically significant, and the overall interactions are close to those of Table IV.

IV. Conclusion

We study the effects of countercyclical reserve requirement policies on domestic credit supply in Brazil. Although RRs have been broadly studied, particularly targeting spillovers from the global financial cycle, we turn to the *domestic* credit cycle. We also significantly improve the identification strategy of previous studies by using comprehensive loan-level data and a bank-level exposure, which is sensitive to policy intensity and banks' differential exposure. We bring to this dataset issues of effectiveness, asymmetric transmission (in loosening versus tightening), synergies with the short-term policy rate, and the impact of economic policy uncertainty. We find strong credit supply effects at the loan and firm-level but limited real effects on employment. Our

[36] In December 2021, the BCB created a deposit-taking facility for financial institutions, remunerating voluntary reserves. These reserves increased substantially since.

results are weaker when a policy index is used, which helps to qualify several papers looking at the same issues using this alternative approach.

We find the transmission to credit supply depends on several factors. While private domestic banks respond similarly to loosening and tightening policy, government and foreign banks are less sensitive to these policies. Similarly, banks with higher capital ratios are insulated from tightening episodes. We also find evidence of synergies with the short-term policy rate but this is only statistically significant for firms only during tightening. We document that EPU weakens the transmission of RRs to bank-level credit supply, a result that points to more conservative behavior of financial institutions in response to loosening policy when policy uncertainty is high.

Our results have implications for policymakers in EMs trying to curb excessive credit growth during booms even when external conditions do not allow interest-rate usage. They are also relevant for policymakers trying to unfreeze credit markets during crisis, such as the COVID-19 pandemic. Importantly, RRs can stimulate credit supply even when the policy rate is unchanged.

There are several promising avenues for future exploration considering these findings. Firstly, a more granular exploration of the differential responses of private domestic, government, and foreign banks to RR policies could provide valuable insights into the dynamics of the domestic credit cycle. This could include studying the specific factors that make certain banks more sensitive to these policies. Secondly, the role of economic policy uncertainty in weakening the transmission of RRs to bank-level credit supply warrants further investigation. The research could be extended to understand how banks' behavior changes in response to policy uncertainty and how this impacts the effectiveness of RRs. Thirdly, the limited real effects on employment observed in this study suggest that there may be other factors at play that are not captured by credit supply effects. Future research could aim to identify these factors and understand their interactions with credit supply and employment. Lastly, the potential synergies between RRs and the short-term policy rate could be further explored, particularly in the context of economic crises. This could involve examining how these synergies could be leveraged to stimulate credit supply and support economic recovery.

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Figures and Tables

Table I: Changes in RRs

Period		Demand deposits	Time deposits	Savings accounts: Housing	Savings accounts: Rural	Additional: Demand deposits	Additional: Time deposits	Additional: Savings deposits
2003	Feb	60%	15%	20%	20%	8%	8%	10%
	Aug	45%	"	"	"	"	"	"
2008	May	"	"	"	"	"	"	"
	Jul	"	"	"	"	"	"	"
	Sep	"	"	"	"	"	"	"
	Oct	42%	"	"	"	5%	5%	"
	Nov	"	"	"	15%	"	"	"
2009	Jan	"	"	"	"	"	4%	"
	Sep	"	13.5%	"	"	"	"	"
2010	Mar	"	15%	"	"	8%	8%	"
	Jun	43%	"	"	16%	"	"	"
	Dec	"	20%	"	"	12%	12%	"
2011	Apr	"	"	"	"	"	"	"
	Jun	"	"	"	17%	"	"	"
	Jul	"	"	"	"	"	"	"
2012	Jul	44%	"	"	"	6%	"	"
	Sep	"	"	"	"	0%	"	"
	Oct	"	"	"	"	"	11%	"
	Dec	"	"	"	"	"	"	"
2013	Jul	"	"	"	18%	"	"	"
2014	Jul	45%	"	"	19%	"	"	"
	Out	"	"	"	13%	"	"	"
2015	Jun	"	"	24.5%	15.5%	"	"	5.5%

* RRs on foreign exchange short positions were introduced in April 2011 and removed in July 2013. Another non-recurrent component focusing on interfinancial deposits from leasing companies was introduced in May 2008 and removed in January 2009.

Source: Central Bank of Brazil

Figure I. Credit growth and changes in RRs

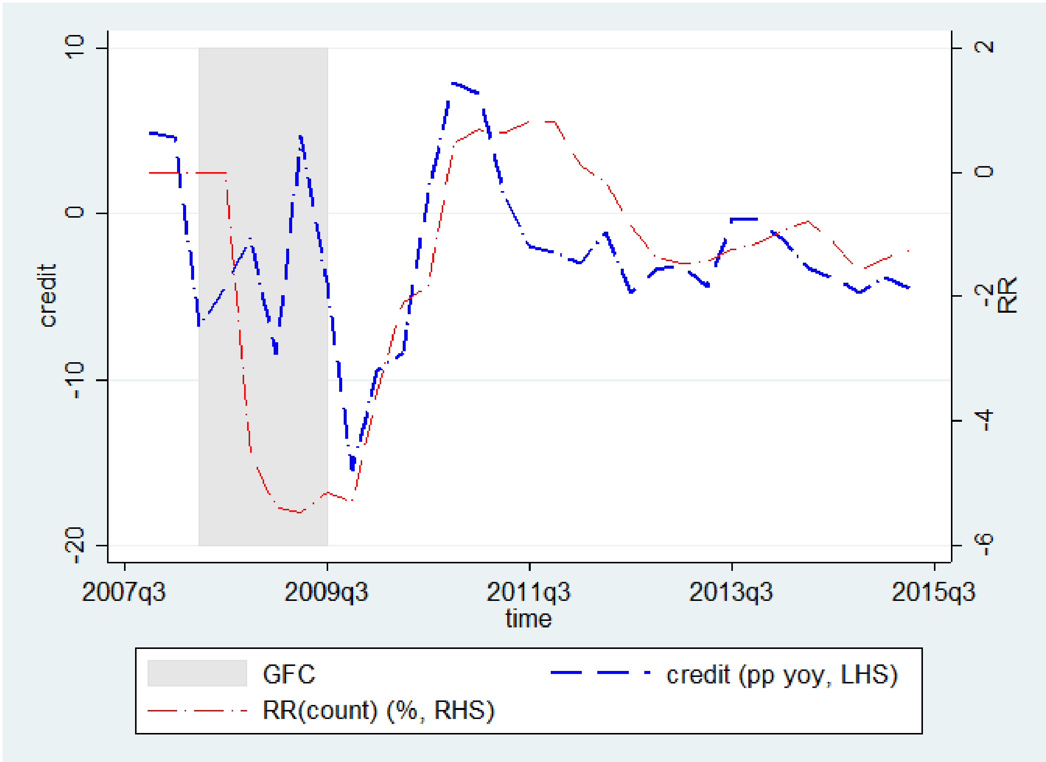

Figure II. Loosening of macroprudential policy and monetary policy

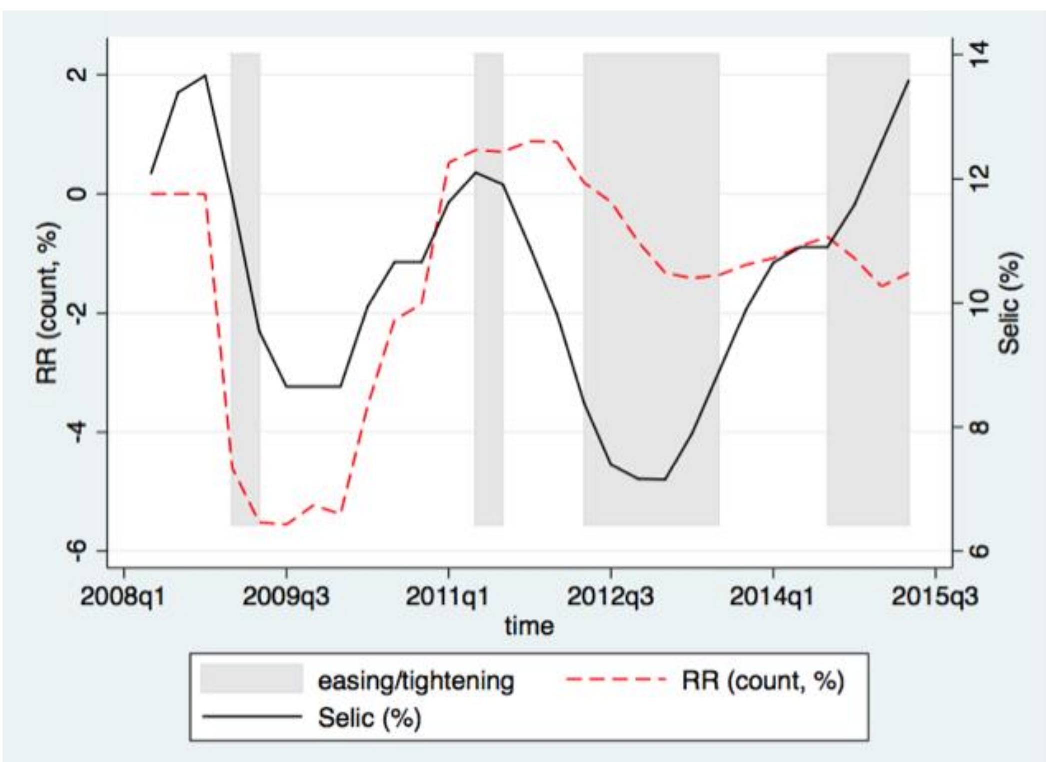

Figure III: Monetary policy Surprises

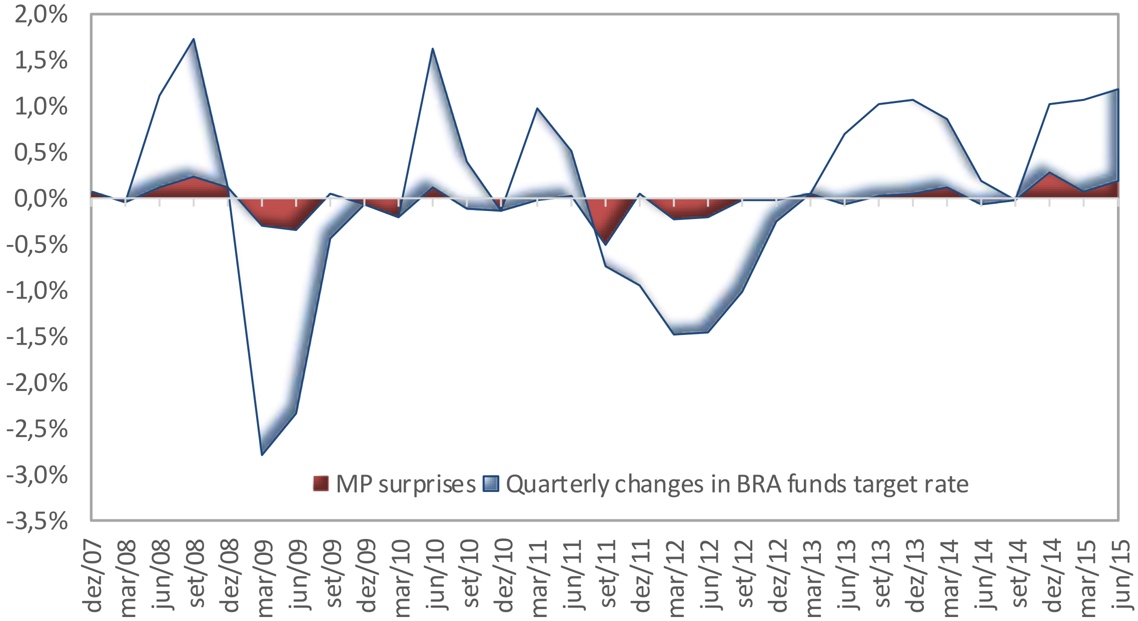

Figure IV. Changes in RRs and monetary policy

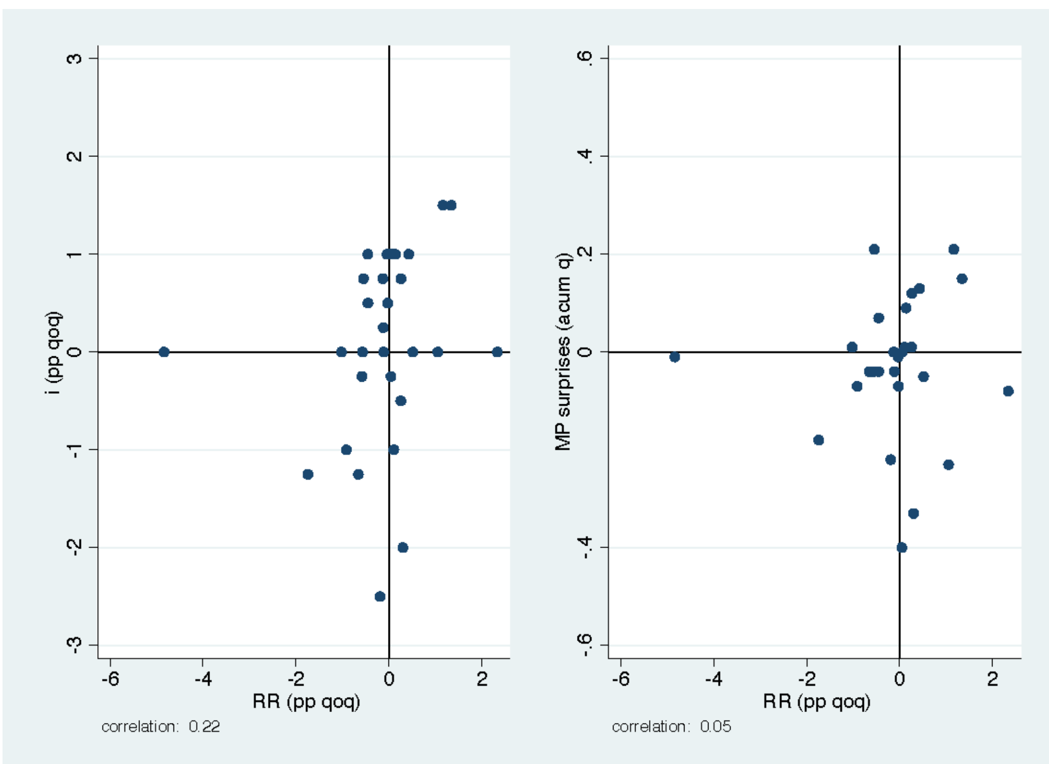

Figure V. Changes in RRs and Economic Policy Uncertainty

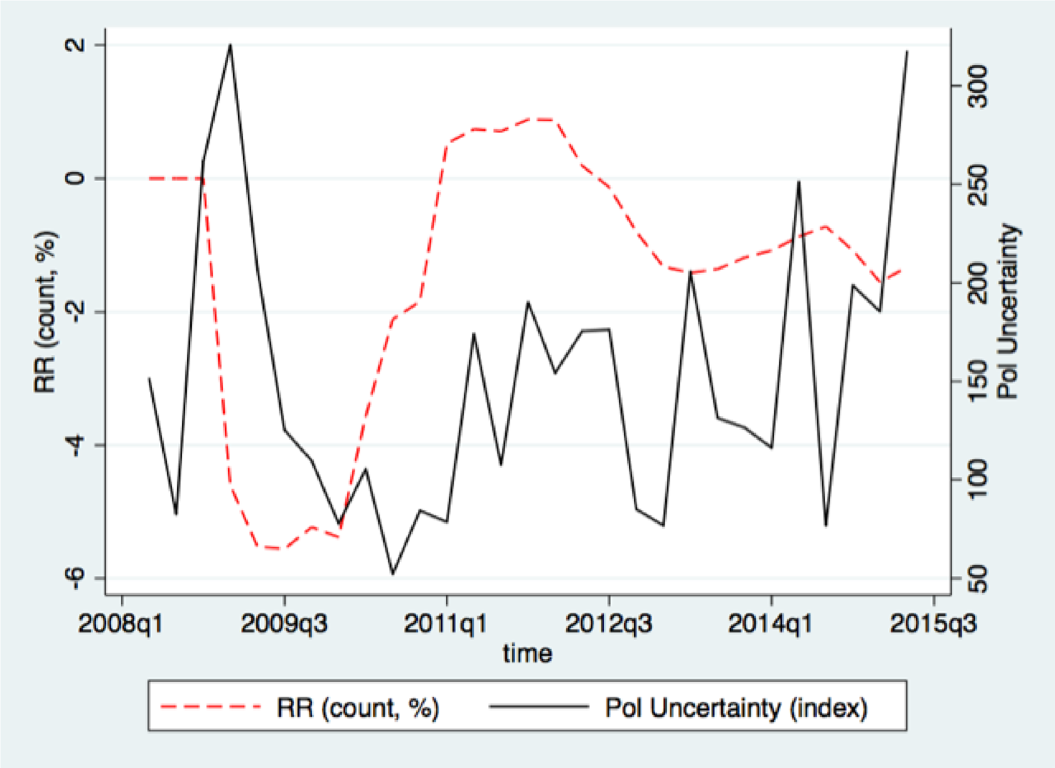

Table II: Summary

	Unit	p25	p50	mean	p75	sd
Loan Dependent						
Δln(credit)$_{b,f,t:t+1}$	p.p.	-15.38	-5.38	-0.78	4.56	44.14
Exposure to RRs						
ΔResReq$_{b,t-1}$	% counterfactual	-2.08	-0.84	-1.38	0.00	2.65
Loan Control						
Risk$_{b,f,t-1}$	Ln (1 + %)	0.41	0.50	0.88	1.10	1.02
N observations	2,595,398					
Firm Dependent						
Δln(credit)$_{f,t:t+1}$	p.p.	-12.50	-4.12	-0.51	8.89	26.86
Δemployment$_{f,t:t+1}$	$2*(n_{t+1} - n_t)/(n_{t+1} + n_t)$	-.069	0	-.017	.058	0.42
n hired$_{f,t:t+1}$	count	0	1	6.31	3	83.49
n fired$_{f,t:t+1}$	count	0	1	6.00	3	81.12
Firm Controls						
firm credit$_{f,t-1}$	Ln	12.83	14.03	14.21	15.38	2.02
n employees$_{f,t-1}$	Ln	1.39	2.08	2.24	2.89	1.28
firm default$_{f,t-1}$	0/1	0.00	0.00	0.07	0.00	0.26
N observations	1,029,426					
N firms	90,440					
Bank Controls						
ΔResReq$_{b,t-1}$	% counterfactual	-2.46	-0.04	-1.64	0.00	2.71
size$_{b,t-1}$	Ln (BRL Millions)	21.19	22.56	22.56	23.55	2.14
capital$_{b,t-1}$	% of assets	9.49	13.63	16.26	19.19	11.32
liquidity$_{b,t-1}$	% of assets	15.06	22.14	25.34	32.85	13.82
npl$_{b,t-1}$	% of credit	2.33	4.78	5.63	7.03	6.22
fx liab$_{b,t-1}$	% of deposits	0.06	9.79	43.29	32.94	116.31
foreign$_{b,t-1}$	0/1	0.00	0.00	0.25	0.00	0.43
gov $_{b,t-1}$	0/1	0.00	0.00	0.15	0.00	0.36
N observations	1,670					
N banks	83					
Macro Variables						
loosening$_{t-1}$	0/1	0.00	1.00	0.53	1.00	0.51
Δi $_{t-1}$	p.p. (qoq)	-0.25	0.00	0.05	0.75	0.97
$\Delta i^{s}_{\ t-1}$	accum 3m	-0.07	-0.01	-0.03	0.06	0.14
ΔGDP $_{t-1}$	p.p. (yoy)	-1.51	0.41	0.71	3.28	3.07
ΔCPI $_{t-1}$	p.p (qoq)	-0.33	0.21	0.13	0.41	0.67
ΔCA/GDP $_{t-1}$	p.p (yoy)	-50.54	-16.70	-22.41	8.87	36.81
Pol. Uncertainty$_{t-1}$	index	85.04	141.38	153.20	190.21	71.03
$\Delta i^{US}_{\ t-1}$	p.p (yoy)	-1.67	-0.65	-1.04	-0.40	1.05
VIX$_{t-1}$	index	16.07	19.68	22.24	25.49	9.89
Δcommodity prices$_{t-1}$	p.p (qoq)	-9.20	-2.41	2.34	26.60	27.02
Macropru policy$_{t-1}$	index	-1.00	2.50	2.27	5.00	2.68
ResReq$_{t-1}$	index	-1.00	0.00	-0.27	2.00	2.77
ΔRR (count) $_{t-1}$	% counterfactual	-1.84	-1.08	-1.42	0.00	2.01
N quarters	30					

Table III: the lending-channel of RRs at the loan level

Dependent: $\Delta\ln(credit_{b,f,t:t+1})$	(1)	(2)	(3)	(4) loosening (in busts)	(5) tightening (in booms)	(6) difference (5)-(4)
	all quarters					
$\Delta ResReq_{b,t-1}$	-0.562**	-0.526*	-0.673***	-0.501**	-0.840***	-0.339*
	(0.227)	(0.258)	(0.163)	(0.184)	(0.182)	(0.170)
$risk_{b,f,t-1}$	-3.231***	-3.246***	-1.405***	-1.317***	-1.532***	-0.215
	(0.316)	(0.318)	(0.185)	(0.267)	(0.143)	(0.206)
$size_{b,t-1}$	1.487***	1.491***	1.251***	1.258***	1.074**	-0.184
	(0.339)	(0.360)	(0.269)	(0.345)	(0.386)	(0.430)
$capital_{b,t-1}$	0.173**	0.182**	0.181***	0.163*	0.150	-0.013
	(0.072)	(0.071)	(0.064)	(0.086)	(0.115)	(0.134)
$liquidity_{b,t-1}$	-0.006	0.005	-0.039	-0.052	-0.046	0.005
	(0.078)	(0.077)	(0.054)	(0.071)	(0.074)	(0.091)
$npl_{b,t-1}$	-0.205	-0.200	-0.178	-0.357*	0.155	0.512*
	(0.203)	(0.249)	(0.138)	(0.181)	(0.210)	(0.261)
fx $liab_{b,t-1}$	-0.105**	-0.106**	-0.080***	-0.078***	-0.076**	0.002
	(0.044)	(0.041)	(0.026)	(0.023)	(0.030)	(0.023)
$gov_{b,t-1}$	0.775	0.794	2.179**	2.032*	2.330	0.298
	(0.866)	(0.917)	(0.880)	(1.063)	(1.543)	(1.857)
$foreign_{b,t-1}$	-2.341***	-2.381***	-1.442*	-2.744**	0.259	3.002
	(0.737)	(0.745)	(0.844)	(0.980)	(1.489)	(1.806)
ΔGDP_{t-1}	0.625***	0.512***				
	(0.153)	(0.180)				
ΔCPI_{t-1}	-0.457	-0.701				
	(0.617)	(0.705)				
$\Delta CA/GDP_{t-1}$	-0.000	-0.004				
	(0.018)	(0.015)				
Δi_{t-1}	-0.711**					
	(0.276)					
Δi^{s}_{t-1}		-4.532***				
		(0.878)				
loosening	0.459	0.584				
	(0.474)	(0.431)				
firm $credit_{f,t-1}$	-8.773***	-8.799***				
	(0.570)	(0.567)				
n $employees_{f,t-1}$	3.469***	3.475***				
	(0.306)	(0.307)				
firm $default_{f,t-1}$	-5.483***	-5.467***				
	(0.594)	(0.581)				

continued

Observations	2,595,398	2,595,398	2,595,398	1,440,168	1,155,230	2,595,398
R-squared	0.065	0.065	0.411	0.412	0.411	0.412
Seasonal effects & Macro-controls$_{t-1}$	Yes	Yes	<>	<>	<>	<>
Firm FEs & Controls$_{f,t-1}$	Yes	Yes	<>	<>	<>	<>
Firm*Quarter FE	No	No	Yes	Yes	Yes	Yes
Risk$_{f,b,t-1}$	Yes	Yes	Yes	Yes	Yes	Yes
Bank Controls$_{b,t-1}$	Yes	Yes	Yes	Yes	Yes	Yes
N firms	90440	90440	90440	81817	76601	90440
N banks	83	83	83	82	81	83
N quarters	30	30	30	16	14	30
Cluster			bank & quarter			

Notes: This table presents the lending channel of Reserve Requirements (RRs). For each bank and quarter, $\Delta ResReq_{b,t-1}$, represents differential bank-level exposure to RRs. The dependent variable is the change in the natural logarithm (ln) of the total credit exposure of bank *b* against firm *f* between *t* and *t+1*, $\Delta \ln(credit_{b,f,t:t+1})$. The macro-controls are the consumer price index (ΔCPI_{t-1}), GDP growth (ΔGDP_{t-1}), and yearly changes in the current-account/GDP ($\Delta CA/GDP_{t-1}$). The bank controls are core capital-to-assets ratio ($capital_{b,t-1}$), the natural logarithm of banks' assets ($size_{b,t-1}$), liquid-to-total assets ratio ($liquidity_{b,t-1}$), share of non-performing loans to total credit ($npl_{b,t-1}$), foreign currency-to-core liabilities ratio (fx $liab_{b,t-1}$), a dummy variable for banks with foreign ($foreign_{b,t-1}$) and government ($gov_{b,t-1}$) control. Firm controls are: (ln) of total firm credit (firm $credit_{f,t-1}$), (ln) of the number of its employees (n $employees_{f,t-1}$) and a dummy variable that takes the value of 1 if firm *f* is in default, i.e., if it has at least one loan in arrears for more than 90 days against any financial system player in *t-1* (firm $default_{f,t-1}$). This information is promptly available to all banks in the credit registry. We use a risk control, $risk_{f,b,t-1}$, which is the weighted average provision assigned by each bank to all its loans against the same firm in *t-1*. To proxy for interest rate policy, we take the quarterly changes in the overnight policy rate (Selic, Δi_{t-1}) in model (1) and policy surprises, i.e., the (quarterly) accumulated one-day changes in the 30-day interest rate swap immediately after each Copom meeting (Δi^{s}_{t-1}) in model (2). We use firm-quarter fixed effects (FEs) to control for credit demand shifts in models (3) to (5). In models (1) and (2), all macro-controls are estimated, and we rely on firm observables and (time invariant) firm FEs for demand control. In model (4), we restrict the sample to the quarters following loosening policies with RRs; and, in model (5), to quarters following tightening policies. Model (6) presents the difference between (5) and (4). Standard errors are two-way clustered at the bank and time (year:quarter) dimension. Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table IV: Bank heterogeneities at the loan level

Dependent: $\Delta\ln(\text{credit}_{b,f,t:t+1})$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\Delta ResReq_{b,t-1}$	-0.673***	-1.021***	-0.792***	-0.666**	-0.600***	-0.682***	-0.770***	-0.981***	-1.394***
	(0.163)	(0.302)	(0.162)	(0.247)	(0.158)	(0.157)	(0.232)	(0.162)	(0.325)
$\Delta ResReq_{b,t-1}$ * $size_{b,t-1}$		-0.218*							-0.117
		(0.125)							(0.084)
$\Delta ResReq_{b,t-1}$ * $capital_{b,t-1}$			0.062						0.024
			(0.038)						(0.019)
$\Delta ResReq_{b,t-1}$ * $liquidity_{b,t-1}$				-0.001					-0.011
				(0.021)					(0.018)
$\Delta ResReq_{b,t-1}$ * $npl_{b,t-1}$					-0.066				-0.066
					(0.069)				(0.040)
$\Delta ResReq_{b,t-1}$ * fx $liab_{b,t-1}$						0.018**			0.018
						(0.009)			(0.013)
$\Delta ResReq_{b,t-1}$ * $gov_{b,t-1}$							0.352		1.014***
							(0.318)		(0.276)
$\Delta ResReq_{b,t-1}$ * $foreign_{b,t-1}$								1.055***	1.206***
								(0.293)	(0.270)
Observations	2,595,398	2,595,398	2,595,398	2,595,398	2,595,398	2,595,398	2,595,398	2,595,398	2,595,398
R-squared	0.411	0.411	0.411	0.411	0.411	0.411	0.411	0.412	0.412

Notes: This table presents bank heterogeneities related to the lending channel of RRs. For each bank and quarter, $\Delta ResReq_{b,t-1}$, represents differential bank-level exposure to RRs. The dependent variable is the change in the natural logarithm (ln) of bank *b* total credit exposure against firm *f* between *t* and *t+1*, $\Delta\ln(\text{credit}_{b,f,t:t+1})$. All models have the risk, $risk_{f,b,t-1}$, and bank controls ($size_{b,t-1}$, $liquidity_{b,t-1}$, $npl_{b,t-1}$, fx $liab_{b,t-1}$, $foreign_{b,t-1}$, $gov_{b,t-1}$) as well as firm-quarter FEs. Apart from the dummy variables, government and foreign control, all other variables have been de-meaned. Standard errors are two-way clustered at the bank and time (year:quarter) dimension. Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table V: Combined policies at the loan level

Dependent: $\Delta \ln(\text{credit}_{b,f,t:t+1})$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	all quarters			loosening (in busts)			tightening (in booms)			dif (9)-(6)
$\Delta ResReq_{b,t-1}$	-0.673***	-0.731***	-1.471***	-0.501**	-0.567**	-1.529***	-0.840***	-0.935***	-1.677***	-0.141
	(0.163)	(0.165)	(0.328)	(0.184)	(0.215)	(0.382)	(0.182)	(0.178)	(0.471)	(0.554)
$\Delta ResReq_{b,t-1}$ * Δi^{s}_{t-1}		-1.204*	-1.611**		-1.979***	-2.900**		-1.479**	-1.598**	1.302
		(0.629)	(0.693)		(0.233)	(1.030)		(0.609)	(0.709)	(1.235)
$\Delta ResReq_{b,t-1}$ * $size_{b,t-1}$			-0.128			-0.216			-0.072	0.145
			(0.088)			(0.124)			(0.134)	(0.164)
$\Delta ResReq_{b,t-1}$ * $capital_{b,t-1}$			0.020			-0.016			0.073**	0.088**
			(0.020)			(0.033)			(0.034)	(0.041)
$\Delta ResReq_{b,t-1}$ * $liquidity_{b,t-1}$			-0.015			-0.014			-0.001	0.013
			(0.018)			(0.017)			(0.029)	(0.032)
$\Delta ResReq_{b,t-1}$ * $npl_{b,t-1}$			-0.064			-0.078			-0.072	0.006
			(0.041)			(0.059)			(0.053)	(0.080)
$\Delta ResReq_{b,t-1}$ * fx $liab_{b,t-1}$			0.019			0.027*			0.003	-0.023
			(0.012)			(0.012)			(0.019)	(0.019)
$\Delta ResReq_{b,t-1}$ * $gov_{b,t-1}$			1.056***			0.899***			1.156**	0.257
			(0.280)			(0.298)			(0.503)	(0.537)
$\Delta ResReq_{b,t-1}$ * $foreign_{b,t-1}$			1.213***			1.435***			1.078***	-0.357
			(0.271)			(0.272)			(0.233)	(0.332)
Observations	2,595,398	2,595,398	2,595,398	1,440,168	1,440,168	1,440,168	1,155,230	1,155,230	1,155,230	2,595,398
R-squared	0.411	0.411	0.412	0.412	0.412	0.413	0.411	0.411	0.412	0.413
N firms	90440	90440	90440	81817	81817	81817	76601	76601	76601	90440
N banks	83	83	83	82	82	82	81	81	81	83
N quarters	30	30	30	16	16	16	14	14	14	30

Notes: This table presents the lending channel of RRs and the interactions with policy rate surprises (Δi^{s}_{t-1}). For each bank and quarter, $\Delta ResReq_{b,t-1}$, represents differential bank-level exposure to RRs. Policy rate surprises are (quarterly) accumulated one-day changes in the 30-day interest rate swaps immediately after each Copom meeting (Δi^{s}_{t-1}). The dependent variable is the change in the natural logarithm (ln) of bank *b* total credit exposure against firm *f* between *t* and *t+1*, $\Delta \ln(\text{credit}_{b,f,t:t+1})$. All models have the risk, $risk_{f,b,t-1}$, and bank controls ($size_{b,t-1}$, $liquidity_{b,t-1}$, $npl_{b,t-1}$, fx $liab_{b,t-1}$, $foreign_{b,t-1}$, $gov_{b,t-1}$) as well as firm-quarter FEs. Apart from the dummy variables, government and foreign, all other variables have been de-meaned. In models (4) to (6), we restrict the sample to the quarters following loosening policies with RRs and, in models, (7) to (9) to quarters following tightening policies. In model (10), we present the differences between models (9) and (6). We introduce all bank interactions in models (3), (6) and (9) and interactions with policy surprises in models (2),(3),(5),(6),(8) and (9). Standard errors are two-way clustered at the bank and time (year:quarter) dimension. Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table VI: Local macro-interactions (at the loan level)

Dependent: $\Delta\ln(\text{credit}_{b,f,t:t+1})$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\Delta ResReq_{b,t-1}$	-0.673***	-0.731***	-0.671***	-0.701***	-0.722***	-0.643***	-0.694***	-1.503***
	(0.163)	(0.165)	(0.154)	(0.160)	(0.164)	(0.145)	(0.145)	(0.341)
$\Delta ResReq_{b,t-1}$ * Δi^{s}_{t-1}		-1.204*					-0.976	-1.292*
		(0.629)					(0.632)	(0.653)
$\Delta ResReq_{b,t-1}$ * ΔCPI_{t-1}			0.016				0.073	0.050
			(0.172)				(0.213)	(0.196)
$\Delta ResReq_{b,t-1}$ * ΔGDP_{t-1}				-0.062			0.007	-0.036
				(0.038)			(0.059)	(0.067)
$\Delta ResReq_{b,t-1}$ * $\Delta CA/GDP_{t-1}$					0.005*		0.002	0.001
					(0.003)		(0.004)	(0.005)
$\Delta ResReq_{b,t-1}$ * EPU_{-1}						0.005***	0.005***	0.004**
						(0.002)	(0.002)	(0.002)
Observations	2,595,398	2,595,398	2,595,398	2,595,398	2,595,398	2,595,398	2,595,398	2,595,398
R-squared	0.411	0.411	0.411	0.411	0.411	0.411	0.411	0.412
$\Delta ResReq_{b,t-1}$ * Bank $Controls_{t-1}$	No	No	No	No	No	No	No	Yes
$\Delta ResReq_{b,t-1}$ * BRA Macro $Control_{t-1}$	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents the lending channel of RRs, interactions with policy rate surprises (synergies) and a horserace exercise between these synergies and interactions between RRs exposure and macroeconomic conditions from Brazil that could have possibly influenced the transmission channel. All models have the risk, $risk_{f,b,t-1}$, and bank controls ($size_{b,t-1}$, $liquidity_{b,t-1}$, $npl_{b,t-1}$, fx $liab_{b,t-1}$, $foreign_{b,t-1}$, $gov_{b,t-1}$) as well as firm-quarter FEs. In model (8), we also introduce bank controls interacted with $\Delta ResReq_{b,t-1}$. The BRA macro-controls are quarterly CPI (ΔCPI_{t-1}), the yearly GDP growth (ΔGDP_{t-1}), the yearly changes in current-accounts/GDP ($\Delta CA/GDP_{t-1}$), and the economic policy uncertainty (EPU_{t-1}) index from Baker, Bloom and Davis (Pol. $Uncertainty_{t-1}$) for Brazil. Standard errors are two-way clustered at the bank and time (year:quarter) dimension. Apart from the dummy variables, government and foreign control, all other variables have been de-meaned. Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table VII: Global macro-interactions (at the loan level)

Dependent: $\Delta\ln(credit_{b,f,t:t+1})$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\Delta ResReq_{b,t-1}$	-0.673***	-0.731***	-0.701***	-0.763***	-0.704***	-0.678***	-0.748***	-1.554***
	(0.163)	(0.165)	(0.151)	(0.148)	(0.161)	(0.147)	(0.143)	(0.344)
$\Delta ResReq_{b,t-1}$ * Δi^{s}_{t-1}		-1.204*					-0.564	-1.248*
		(0.629)					(0.686)	(0.708)
$\Delta ResReq_{b,t-1}$ * Δi^{US}_{t-1}			-0.253				-0.162	-0.132
			(0.205)				(0.163)	(0.197)
$\Delta ResReq_{b,t-1}$ * VIX_{t-1}				0.027			0.020	0.011
				(0.018)			(0.019)	(0.025)
$\Delta ResReq_{b,t-1}$ * Δcommodity $prices_{t-1}$					-0.006		-0.000	-0.005
					(0.005)		(0.004)	(0.004)
$\Delta ResReq_{b,t-1}$ * Macropru $policy_{t-1}$						-0.008	0.064	-0.007
						(0.068)	(0.062)	(0.058)
Observations	2,595,398	2,595,398	2,595,398	2,595,398	2,595,398	2,595,398	2,595,398	2,595,398
R-squared	0.411	0.411	0.411	0.411	0.411	0.411	0.411	0.412
$\Delta ResReq_{b,t-1}$ * Bank $Controls_{t-1}$	No	No	No	No	No	No	No	Yes
$\Delta ResReq_{b,t-1}$ * Global Macro $Control_{t-1}$	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents the lending channel of RRs, interactions with policy rate surprises (synergies) and a horserace exercise between these synergies and interaction with global macroeconomic controls that could have possibly influenced the transmission channel. All models have the risk, $risk_{f,b,t-1}$, and bank controls ($size_{b,t-1}$, $liquidity_{b,t-1}$, $npl_{b,t-1}$, fx $liab_{b,t-1}$, $foreign_{b,t-1}$, $gov_{b,t-1}$) as well as firm-quarter FEs. In model (8), we introduce bank controls interacted with $\Delta ResReq_{b,t-1}$. The global controls are: changes in the US short shadow rates (Δi^{US}_{t-1}), the US stock volatility index (VIX_{t-1}), changes in the commodity price index (Δcommodity $prices_{t-1}$), and the macroprudential index for Brazil (Macropru $policy_{t-1}$). Standard errors are two-way clustered at the bank and time (year:quarter) dimension. Apart from the dummy variables, government and foreign control, all other variables have been de-meaned. Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table VIII: The lending channel of RRs with macro proxies

Dependent: $\Delta\ln(credit_{b,f,t:t+1})$	(1)	(2)	(3)	(4)
	$X_{t-1} = \Delta RR(count)_{\mathbf{t-1}}$		X_{t-1} = ResReq$_{t-1}$	
X_{t-1}	-1.008***		-0.518**	
	(0.161)		(0.205)	
X_{t-1} * $size_{b,t-1}$	-0.160*	-0.129*	-0.152	-0.126
	(0.086)	(0.075)	(0.127)	(0.103)
X_{t-1} * $capital_{b,t-1}$	-0.000	0.010	-0.035	-0.019
	(0.045)	(0.033)	(0.041)	(0.032)
X_{t-1} * $liquidity_{b,t-1}$	-0.043	-0.033	-0.019	0.000
	(0.032)	(0.026)	(0.034)	(0.025)
X_{t-1} * $npl_{b,t-1}$	-0.046	-0.016	-0.071	-0.063
	(0.097)	(0.089)	(0.104)	(0.075)
X_{t-1} * fx $liab_{b,t-1}$	-0.015	-0.009	-0.006	-0.002
	(0.016)	(0.012)	(0.016)	(0.010)
X_{t-1} * $gov_{b,t-1}$	1.413***	1.572***	0.551	0.666*
	(0.393)	(0.351)	(0.377)	(0.346)
X_{t-1} * $foreign_{b,t-1}$	1.653***	1.797***	0.795*	0.896**
	(0.376)	(0.463)	(0.390)	(0.427)
Observations	2,595,398	2,595,398	2,595,398	2,595,398
R-squared	0.066	0.412	0.065	0.411
X_{t-1} * Bank Controls$_{t-1}$	Yes	Yes	Yes	Yes
Firm FE & Firm Controls$_{t-1}$	Yes	<>	Yes	<>
Firm*Quarter FE	No	Yes	No	Yes

Notes: In this table, we present an alternative estimation of the lending channel of RRs using macro (and not bank-level) proxies of the exposure to RRs and interactions with bank characteristics. In models (1) and (2), we take the same counterfactual variable used across this paper but aggregated at the time dimension (and not at the bank-level), i.e., system-wide RRs. In models (3) and (4), we use an index for RRs, like the one from Cerutti, Claessens, and Laeven (2017). This index shows (+1) when one additional tightening instrument related to RRs is active and (-1) when a loosening instrument is active (cumulatively). All models have risk, $risk_{f,b,t-1}$, bank controls ($size_{b,t-1}$, $liquidity_{b,t-1}$, $npl_{b,t-1}$, fx $liab_{b,t-1}$, $foreign_{b,t-1}$, $gov_{b,t-1}$), and interactions of bank controls with the macro-proxies X_{t-1}. We use firm-quarter fixed effects (FEs) to control for credit demand shifts in models (2) and (4). In models (1) and (3), all macro-controls are estimated, and we rely on firm observables and (time invariant) firm FEs for demand control. Apart from the dummy variables, government and foreign control, all other variables have been de-meaned. Standard errors are two-way clustered at the bank and time (year:quarter) dimension and presented in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table IX: Firm level estimates: credit

Dependent: $\Delta\ln(credit_{f,t:t+1})$	(1)	(2) all quarters	(3)	(4)	(5) loosening (in busts)	(6)	(7)	(8) tightening (in booms)	(9)	(10) dif (9)-(6)
$\Delta ResReq_{f,t-1}$	-0.499***	-0.840***	-1.157***	-0.495**	-0.500*	-1.029**	-0.767***	-1.280***	-1.354**	-0.325
	(0.148)	(0.238)	(0.336)	(0.176)	(0.260)	(0.402)	(0.210)	(0.359)	(0.611)	(0.672)
$\Delta ResReq_{f,t-1} * \Delta i^s_{t-1}$		-0.792	-0.830**		0.902	-1.344		-0.708	-1.080*	0.264
		(0.498)	(0.404)		(0.923)	(0.915)		(0.501)	(0.520)	(0.916)
Observations	1,029,426	1,029,426	1,029,426	572,554	572,554	572,554	456,872	456,872	456,872	1,029,426
R-squared	0.213	0.214	0.216	0.265	0.265	0.266	0.282	0.283	0.284	0.274
$\Delta ResReq_{f,t-1} * \Delta i^s_{t-1}$	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	Yes
$\Delta ResReq_{f,t-1}$ * Bank $Controls_{f,t-1}$	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	Yes
Quarter FEs	No	No	Yes	No	No	Yes	No	No	Yes	Yes
N firms	90440	90440	90440	81817	81817	81817	76601	76601	76601	90440
N max banks	67	67	67	66	66	66	64	64	64	67
N quarters	30	30	30	16	16	16	14	14	14	30

Notes: This table presents effects of RRs on firm-level credit intake and interactions with policy rate surprises. For each bank and quarter, $\Delta ResReq_{f,t-1}$, represents exposure to RRs. However, $\Delta ResReq_{f,t-1}$ is weighted averaged to the firm-level using the ex-ante credit exposure of each bank-firm pair. All bank controls and the risk control are similarly weighted to the firm-quarter dimension. Policy rate surprises are the (quarterly) accumulated one-day changes in the 30 days interest rate swaps immediately after each Copom meeting (Δi^s_{t-1}). The dependent variable is the change in the natural logarithm (ln) of the total credit exposure of firm *f* between *t* and *t+1*, $\Delta\ln(credit_{f,t:t+1})$. All models have weighted risk and bank controls as well as firm controls and firm FEs. Apart from the dummy variables, government and foreign control, all other variables have been de-meaned. In models (4) to (6), we restrict the sample to the quarters following loosening policies with RRs and, in models (7) to (9) to tightening policies. We interact $\Delta ResReq_{f,t-1}$ with all bank controls and policy rate surprises in models (2), (3), (5), (6), (8) and (9). We introduce quarter FEs in models (3), (6) and (9), and we use the macro-controls and seasonal dummies in all remaining models. In model (10), we present the difference between models (9) and (6). Standard errors are clustered at the maximum bank, i.e., the bank to which the firm is mostly exposed in *t-1* and the time dimension. Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table X: Firm level estimates: employment, hiring and firing ratios

Panel A Dependent:	(1)	(2)	(3)	(4)	(5)	(6)
Δemployment$_{f,t:t+1}$	all quarters		loosening (in busts)		tightening (in booms)	
ΔResReq$_{f,t-1}$	0.001	0.001	0.001	0.002	0.000	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
ΔResReq$_{f,t-1}$ * Δi^s_{t-1}	0.002	0.002	-0.004	-0.010	0.003	0.003
	(0.003)	(0.003)	(0.009)	(0.007)	(0.003)	(0.003)
Observations	1,029,426	1,029,426	572,554	572,554	456,872	456,872
R-squared	0.179	0.179	0.235	0.235	0.266	0.267
Panel B Dependent:	(1)	(2)	(3)	(4)	(5)	(6)
Number of hired$_{f,t:t+1}$	all quarters		loosening (in busts)		tightening (in booms)	
ΔResReq$_{f,t-1}$	-0.005	-0.008	-0.002	0.002	-0.011	-0.015
	(0.006)	(0.007)	(0.010)	(0.015)	(0.009)	(0.011)
ΔResReq$_{f,t-1}$ * Δi^s_{t-1}	-0.102***	-0.100***	-0.099*	-0.107**	-0.101**	-0.101**
	(0.036)	(0.036)	(0.051)	(0.047)	(0.041)	(0.040)
Observations	1,029,426	1,029,426	572,554	572,554	456,872	456,872
Pseudo R-squared	0.872	0.872	0.878	0.878	0.880	0.880
Number of fired$_{f,t:t+1}$	all quarters		loosening (in busts)		tightening (in booms)	
ΔResReq$_{f,t-1}$	0.009	0.011	0.016***	0.020***	0.012*	0.013
	(0.006)	(0.007)	(0.005)	(0.006)	(0.007)	(0.010)
ΔResReq$_{f,t-1}$ * Δi^s_{t-1}	0.042	0.044	0.040	0.073	0.049*	0.045*
	(0.027)	(0.028)	(0.050)	(0.069)	(0.025)	(0.026)
Observations	1,029,426	1,029,426	572,554	572,554	456,872	456,872
Pseudo R-squared	0.892	0.892	0.898	0.898	0.896	0.896
ΔResReq$_{f,t-1}$ *Bank Controls$_{f,t-1}$	No	Yes	No	Yes	No	Yes
N firms	90440	90440	81817	81817	76601	76601
N max banks	67	67	66	66	64	64
N quarters	30	30	16	16	14	14

Notes: In Panel A, the dependent variable is the change in the number of employees in firm *f* between *t* and *t+1* divided by the average number of employees in *t* and *t+1*, thus constrained to -2 and 2. In Panel B, the number of individuals hired or fired by firm *f* between *t* and *t+1* are the dependent variables. Because these are discrete counting variables, we use the high-dimensional Poisson pseudomaximum likelihood (PPML) estimator from Correia, Guimarães, and Zylkin (2020), which is robust to heteroskedasticity, particularly with large number of zeros. In Panels A and B, all models have weighted risk and bank controls as well as firm controls, firm and quarter FEs. Models (2), (4) and (6) have bank control interactions with ΔResReq$_{f,t-1}$. Standard errors are clustered at the maximum bank, i.e., the bank to which the firm is mostly exposed in *t-1*, and the time dimension. Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Appendix

Appendix A.1: Policy rate surprises in the sample and Copom dates

Date	New target	Prior Target	Announced change (%)	Unanticipated change (%)	Anticipated change (%)
07 March 2007	12.75%	13.00%	-0.25%	0.01%	-0.26%
18 April 2007	12.50%	12.75%	-0.25%	0.01%	-0.26%
06 June 2007	12.00%	12.50%	-0.50%	-0.13%	-0.37%
18 July 2007	11.50%	12.00%	-0.50%	-0.04%	-0.46%
05 September 2007	11.25%	11.50%	-0.25%	-0.05%	-0.20%
17 October 2007	11.25%	11.25%	0.00%	0.06%	-0.06%
05 December 2007	11.25%	11.25%	0.00%	0.00%	0.00%
23 January 2008	11.25%	11.25%	0.00%	-0.02%	0.02%
05 March 2008	11.25%	11.25%	0.00%	-0.02%	0.02%
16 April 2008	11.75%	11.25%	0.50%	0.16%	0.34%
04 June 2008	12.25%	11.75%	0.50%	-0.03%	0.53%
23 July 2008	13.00%	12.25%	0.75%	0.13%	0.62%
10 September 2008	13.75%	13.00%	0.75%	0.08%	0.67%
29 October 2008	13.75%	13.75%	0.00%	-0.08%	0.08%
10 December 2008	13.75%	13.75%	0.00%	0.07%	-0.07%
21 January 2009	12.75%	13.75%	-1.00%	-0.16%	-0.84%
11 March 2009	11.25%	12.75%	-1.50%	-0.06%	-1.44%
29 April 2009	10.25%	11.25%	-1.00%	-0.01%	-0.99%
10 June 2009	9.25%	10.25%	-1.00%	-0.32%	-0.68%
22 July 2009	8.75%	9.25%	-0.50%	-0.01%	-0.49%
02 September 2009	8.75%	8.75%	0.00%	0.02%	-0.02%
21 October 2009	8.75%	8.75%	0.00%	-0.03%	0.03%
09 December 2009	8.75%	8.75%	0.00%	-0.02%	0.02%
27 January 2010	8.75%	8.75%	0.00%	-0.05%	0.05%
17 March 2010	8.75%	8.75%	0.00%	-0.18%	0.18%
28 April 2010	9.50%	8.75%	0.75%	0.09%	0.66%
09 June 2010	10.25%	9.50%	0.75%	0.06%	0.69%
21 July 2010	10.75%	10.25%	0.50%	-0.05%	0.55%
01 September 2010	10.75%	10.75%	0.00%	-0.02%	0.02%
20 October 2010	10.75%	10.75%	0.00%	0.01%	-0.01%
08 December 2010	10.75%	10.75%	0.00%	-0.09%	0.09%
19 January 2011	11.25%	10.75%	0.50%	-0.01%	0.51%
02 March 2011	11.75%	11.25%	0.50%	0.00%	0.50%
20 April 2011	12.00%	11.75%	0.25%	-0.06%	0.31%
08 June 2011	12.25%	12.00%	0.25%	0.02%	0.23%
20 July 2011	12.50%	12.25%	0.25%	-0.01%	0.26%
31 August 2011	12.00%	12.50%	-0.50%	-0.39%	-0.11%
19 October 2011	11.50%	12.00%	-0.50%	-0.01%	-0.49%
30 November 2011	11.00%	11.50%	-0.50%	0.02%	-0.52%
18 January 2012	10.50%	11.00%	-0.50%	-0.04%	-0.46%
07 March 2012	9.75%	10.50%	-0.75%	-0.14%	-0.61%
18 April 2012	9.00%	9.75%	-0.75%	-0.06%	-0.69%
30 May 2012	8.50%	9.00%	-0.50%	0.02%	-0.52%
11 July 2012	8.00%	8.50%	-0.50%	-0.03%	-0.47%

continued

29 August 2012	7.50%	8.00%	-0.50%	-0.04%	-0.46%
10 October 2012	7.25%	7.50%	-0.25%	-0.03%	-0.22%
28 November 2012	7.25%	7.25%	0.00%	-0.01%	0.01%
16 January 2013	7.25%	7.25%	0.00%	-0.02%	0.02%
06 March 2013	7.25%	7.25%	0.00%	-0.02%	0.02%
17 April 2013	7.50%	7.25%	0.25%	-0.16%	0.41%
29 May 2013	8.00%	7.50%	0.50%	0.16%	0.34%
10 July 2013	8.50%	8.00%	0.50%	-0.01%	0.51%
28 August 2013	9.00%	8.50%	0.50%	0.01%	0.49%
09 October 2013	9.50%	9.00%	0.50%	0.06%	0.44%
27 November 2013	10.00%	9.50%	0.50%	0.03%	0.47%
15 January 2014	10.50%	10.00%	0.50%	0.15%	0.35%
26 February 2014	10.75%	10.50%	0.25%	-0.03%	0.28%
02 April 2014	11.00%	10.75%	0.25%	0.03%	0.22%
28 May 2014	11.00%	11.00%	0.00%	-0.03%	0.03%
16 July 2014	11.00%	11.00%	0.00%	0.00%	0.00%
03 September 2014	11.00%	11.00%	0.00%	0.01%	-0.01%
29 October 2014	11.25%	11.00%	0.25%	0.22%	0.03%
03 December 2014	11.75%	11.25%	0.50%	-0.01%	0.51%
21 January 2015	12.25%	11.75%	0.50%	0.06%	0.44%
04 March 2015	12.75%	12.25%	0.50%	0.01%	0.49%
29 April 2015	13.25%	12.75%	0.50%	0.06%	0.44%
03 June 2015	13.75%	13.25%	0.50%	0.05%	0.45%

Notes: Data from BM&F Bovespa and Central Bank of Brazil. The strategy of decomposing policy rate changes into two additive components (expected and unexpected) using derivatives' data one-day after each Monetary Policy Committee Announcement mimics Kuttner (2001).

Appendix A.2: Auto-correlation in the instruments used for the short-term policy rate

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent:		Δi^{s}_{t-1}			Δi_{t-1}	
Δi^{X}_{t-1}	-0.11	-0.19	-0.18	0.25*	0.04	0.03
	(0.09)	(0.12)	(0.12)	(0.14)	(0.14)	(0.13)
Δi^{X}_{t-2}	0.04	-0.04	0.01	0.39**	0.21	0.33**
	(0.08)	(0.10)	(0.11)	(0.16)	(0.14)	(0.16)
Δi^{X}_{t-3}			-0.03			0.58***
			(0.12)			(0.13)
Δi^{X}_{t-4}			-0.05			-0.21
			(0.10)			(0.17)
Δi^{X}_{t-5}			0.14			-0.20
			(0.12)			(0.16)
Δi^{X}_{t-6}			-0.09			-0.22
			(0.11)			(0.17)
Δi^{X}_{t-7}			-0.02			0.14
			(0.14)			(0.16)
Δi^{X}_{t-8}			0.20			0.08
			(0.14)			(0.16)
Δi^{X}_{t-9}			0.05			-0.02
			(0.13)			(0.14)
Δi^{X}_{t-10}			-0.16			-0.15
			(0.12)			(0.13)
Δi^{X}_{t-11}			-0.01			0.13
			(0.11)			(0.11)
Constant	-0.00	-0.01	-0.01	-0.00	-0.14***	-0.09**
	(0.00)	(0.01)	(0.01)	(0.00)	(0.04)	(0.04)
Observations	90	90	90	90	90	90
R-squared	0.11	0.26	0.34	0.31	0.44	0.62
R-squared (adj)	-0.04	0.03	0.01	0.19	0.27	0.42
F-value	0.83	0.78	0.98	1.55	2.85	5.18
Seasonal effects	Yes	Yes	Yes	Yes	Yes	Yes
Other Announcements	No	Yes	Yes	No	Yes	Yes

Notes: In this Table, we evaluate possible auto-correlation across monetary policy surprises and correlation between these surprises and other contemporaneous announcements. COPOM meetings happen every 45 days, and we aggregate two of those over to build the quarterly (Δi^{s}_{t-1}), used in most of this paper. We follow Gertler and Karadi (2015) and use monthly data to investigate possible auto-correlation between surprises. In column (1), we regress each (monthly) surprise against surprises from the previous two months (using zero if there is no announcement). In column (2), we introduce possibly correlated contemporaneous economic data announcements (e.g., Miranda-Agrippino and Ricco (2020)), including monthly inflation (IPCA), unemployment, FX and real activity (IBC-Br), as well as GDP and inflation forecasts from the BCB Focus survey published in the week before. In column (3), we extend auto-regressive components that round up to one year. In columns (4) to (6), we reproduce the strategy using monthly changes in the short-term policy rate (instead of surprises) for comparison. Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.

Appendix A.3: Are banks circumventing RR policy adjusting deposits' composition?

Panel A:	(1)	(2)	(3)	(4)	(5)	(6)
Dependent: Δln(time$_{b,t:t+1}$).	All quarters		loosening (in busts)		tightening (in booms)	
ΔResReq$_{b,t-1}$	0.029	-0.108	-0.621	-0.769	0.409	0.442
	(0.387)	(0.415)	(0.522)	(0.509)	(0.515)	(0.532)
Observations	1,670	1,670	862	862	798	798
R-squared	0.150	0.184	0.203	0.229	0.251	0.262

Panel B:	(1)	(2)	(3)	(4)	(5)	(6)
Dependent: Δln(savings$_{b,t:t+1}$)	All quarters		loosening (in busts)		tightening (in booms)	
ΔResReq$_{b,t-1}$	0.035	0.012	-0.098	-0.106	0.127	0.112
	(0.087)	(0.080)	(0.115)	(0.104)	(0.108)	(0.102)
Observations	1,670	1,670	862	862	798	798
R-squared	0.490	0.506	0.428	0.438	0.578	0.589

Panel C:	(1)	(2)	(3)	(4)	(5)	(6)
Dependent: Δln(demand$_{b,t:t+1}$)	All quarters		loosening (in busts)		tightening (in booms)	
ΔResReq$_{b,t-1}$	0.232	0.164	-0.311	0.293	0.975	0.255
	(0.718)	(0.449)	(0.847)	(0.641)	(1.061)	(0.996)
Observations	1,670	1,670	862	862	798	798
R-squared	0.061	0.092	0.127	0.136	0.130	0.159

Seasonal effects & Macro-controls$_{t-1}$	Yes	<>	Yes	<>	Yes	<>
Bank FE & Bank Controls$_{t-1}$	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Bank & Firm Controls $_{t-1}$	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	No	Yes	No	Yes	No	Yes
N banks	83	83	75	75	78	78
N quarters	30	30	16	16	14	14

Notes: In this Table, we build a bank-quarter panel with bank and quarter FEs to evaluate if RR policy is leading to compositional changes in banks' deposits in the following quarter, in which case estimates of ΔResReq$_{b,t-1}$ would be downward biased in the previous tables. All models have bank FEs and bank controls. Risk and firm-level controls are introduced weighted at the bank-level. In Panel A, the dependent variable is the change in the natural logarithm (ln) of time deposits of bank *b* between *t* and *t+1*, Δln(time$_{t:t+1}$). Similarly, in Panel B and C, the dependents are the change in the natural logarithm of savings, Δln(savings$_{t:t+1}$), and demand, Δln(demand$_{t:t+1}$), respectively. We use the same seasonal and macro-controls including the monetary policy surprise instrument (Δi^s_{t-1}) in models (1), (3) and (5) and quarter FEs in models (2), (4) and (6). Only loosening quarters are used in models (3) and (4) and only tightening quarters in models (5) and (6). Standard errors are two-way clustered at the bank and time (year:quarter) dimension and presented in parentheses: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Appendix A.4: The combined policy using changes in the policy rate at the loan level

Dependent: $\Delta\ln(\text{credit}_{b,f,t:t+1})$	(1)	(2) all quarters	(3)	(4)	(5) loosening (in busts)	(6)	(7)	(8) tightening (in booms)	(9)	(10) dif (9)-(6)
$\Delta\text{ResReq}_{b,t-1}$	-0.673***	-0.737***	-1.508***	-0.501**	-0.543**	-1.530***	-0.840***	-0.932***	-1.715***	-0.090
	(0.163)	(0.162)	(0.338)	(0.184)	(0.195)	(0.313)	(0.182)	(0.161)	(0.486)	(0.557)
$\Delta\text{ResReq}_{b,t-1}$ * Δi_{t-1}		-0.195*	-0.283***		-0.163	-0.236		-0.226*	-0.317**	-0.081
		(0.107)	(0.098)		(0.107)	(0.187)		(0.107)	(0.107)	(0.185)
$\Delta\text{ResReq}_{b,t-1}$ * $\text{size}_{b,t-1}$			-0.146			-0.230*			-0.092	0.139
			(0.088)			(0.117)			(0.131)	(0.159)
$\Delta\text{ResReq}_{b,t-1}$ * $\text{capital}_{b,t-1}$			0.020			-0.007			0.071*	0.078*
			(0.019)			(0.031)			(0.033)	(0.040)
$\Delta\text{ResReq}_{b,t-1}$ * $\text{liquidity}_{b,t-1}$			-0.015			-0.010			-0.002	0.008
			(0.017)			(0.014)			(0.031)	(0.034)
$\Delta\text{ResReq}_{b,t-1}$ * $\text{npl}_{b,t-1}$			-0.074*			-0.095			-0.079	0.016
			(0.041)			(0.066)			(0.056)	(0.088)
$\Delta\text{ResReq}_{b,t-1}$ * $\text{fx liab}_{b,t-1}$			0.020			0.026*			0.004	-0.022
			(0.013)			(0.014)			(0.019)	(0.019)
$\Delta\text{ResReq}_{b,t-1}$ * $\text{gov}_{b,t-1}$			1.089***			0.899***			1.201**	0.301
			(0.272)			(0.262)			(0.472)	(0.507)
$\Delta\text{ResReq}_{b,t-1}$ * $\text{foreign}_{b,t-1}$			1.185***			1.390***			1.071***	-0.319
			(0.264)			(0.234)			(0.235)	(0.318)
Observations	2,595,398	2,595,398	2,595,398	1,440,168	1,440,168	1,440,168	1,155,230	1,155,230	1,155,230	2,595,398
R-squared	0.411	0.411	0.412	0.412	0.412	0.413	0.411	0.411	0.412	0.413
N firms	90440	90440	90440	81817	81817	81817	76601	76601	76601	90440
N banks	83	83	83	82	82	82	81	81	81	83

Notes: This table reproduces Table V, but we take the policy rate (and not policy rate surprises) in the interactions. For each bank and quarter, $\Delta\text{ResReq}_{b,t-1}$ represents bank-level exposure to RRs. Interest rate policy is measured as the quarterly changes in the overnight policy rate (Selic, Δi_{t-1}). The dependent variable is the change in the natural logarithm (ln) of the total credit exposure of bank *b* against firm *f* between *t* and *t+1*, $\Delta\ln(\text{credit}_{b,f,t:t+1})$. All models have the risk, $\text{risk}_{f,b,t-1}$, and bank controls ($\text{size}_{b,t-1}$, $\text{liquidity}_{b,t-1}$, $\text{npl}_{b,t-1}$, $\text{fx liab}_{b,t-1}$, $\text{foreign}_{b,t-1}$, $\text{gov}_{b,t-1}$) as well as firm-quarter FEs. In models (4) to (6), we restrict the sample to quarters following loosening policies with RRs and, in models, (7) to (9) to quarters following tightening policies. In model (10), we present the differences between models (9) and (6). We introduce all bank interactions in models (3), (6) and (9) and the combined policy in models (2),(3),(5),(6),(8) and (9). Standard errors are two-way clustered at the bank and time (year:quarter) dimension. Apart from the dummy variables, government and foreign control, all other variables have been de-meaned. Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Appendix A.5: Robustness on influential quarters

Dependent: $\Delta\ln(credit_{b,f,t:t+1})$	(1)	(2)	(3)	(4)
	baseline		(-) main quarters	
$\Delta ResReq_{b,t-1}$	-1.471***	-1.508***	-1.573***	-1.585***
	(0.328)	(0.338)	(0.416)	(0.418)
$\Delta ResReq_{b,t-1}$ * Δi^s_{t-1}	-1.611**		-1.762**	
	(0.693)		(0.756)	
$\Delta ResReq_{b,t-1}$ * Δi_{t-1}		-0.283***		-0.241*
		(0.098)		(0.119)
$\Delta ResReq_{b,t-1}$ * $size_{b,t-1}$	-0.128	-0.146	-0.185*	-0.200**
	(0.088)	(0.088)	(0.093)	(0.092)
$\Delta ResReq_{b,t-1}$ * $capital_{b,t-1}$	0.020	0.020	0.007	0.006
	(0.020)	(0.019)	(0.025)	(0.024)
$\Delta ResReq_{b,t-1}$ * $liquidity_{b,t-1}$	-0.015	-0.015	-0.010	-0.010
	(0.018)	(0.017)	(0.017)	(0.019)
$\Delta ResReq_{b,t-1}$ * $npl_{b,t-1}$	-0.064	-0.074*	-0.025	-0.035
	(0.041)	(0.041)	(0.050)	(0.053)
$\Delta ResReq_{b,t-1}$ * fx $liab_{b,t-1}$	0.019	0.020	0.022**	0.023*
	(0.012)	(0.013)	(0.011)	(0.012)
$\Delta ResReq_{b,t-1}$ * $gov_{b,t-1}$	1.056***	1.089***	1.030***	1.062***
	(0.280)	(0.272)	(0.351)	(0.333)
$\Delta ResReq_{b,t-1}$ * $foreign_{b,t-1}$	1.213***	1.185***	1.201***	1.179***
	(0.271)	(0.264)	(0.327)	(0.322)
Observations	2,595,398	2,595,398	2,136,214	2,136,214
R-squared	0.412	0.412	0.413	0.413
N firms	90440	90440	90275	90275
N banks	83	83	83	83
N quarters	30	30	24	24
Cluster	bank & quarter			

Notes: This table presents robustness exercises concerning influential quarters. All models have the risk, $risk_{f,b,t-1}$, and bank controls ($size_{b,t-1}$, $liquidity_{b,t-1}$, $npl_{b,t-1}$, fx $liab_{b,t-1}$, $foreign_{b,t-1}$, $gov_{b,t-1}$) as well as firm-quarter FEs. In models (3) to (4), we exclude the quarters with strong changes in RRs and the following one, i.e., 2008Q4, 2009Q1, 2010Q1, 2010Q2, 2010Q4, 2011Q1. In models (1), (3), we use policy rate surprises (Δi^s_{t-1}) as proxy of interest rate policy. In the remaining models, we use the quarterly change in the overnight policy rate (Δi_{t-1}). We introduce bank controls interacted with $\Delta ResReq_{b,t-1}$ in all models. Standard errors are two-way clustered at the bank and time (year:quarter) dimension. Apart from the dummy variables, government and foreign control, all other variables have been de-meaned. Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Appendix A.6: Robustness: introducing controls for ex-ante bank-level savings, demand and time deposits in Table III

Dependent: $\Delta\ln(\text{credit}_{b,f,t:t+1})$	(1)	(2)	(3)	(4) loosening (in busts)	(5) tightening (in booms)	(6) difference (5)-(4)
	all quarters					
$\Delta ResReq_{b,t-1}$	-0.553*	-0.513*	-0.649***	-0.325*	-0.958**	-0.632**
	(0.277)	(0.301)	(0.229)	(0.176)	(0.327)	(0.280)
$risk_{b,f,t-1}$	-3.227***	-3.240***	-1.417***	-1.302***	-1.564***	-0.262
	(0.320)	(0.328)	(0.189)	(0.270)	(0.156)	(0.213)
$size_{b,t-1}$	0.555	0.495	0.870	-0.589	2.368	2.958
	(1.166)	(1.131)	(1.372)	(1.044)	(2.493)	(2.509)
$capital_{b,t-1}$	0.170**	0.179**	0.171**	0.141	0.125	-0.016
	(0.082)	(0.079)	(0.066)	(0.087)	(0.114)	(0.127)
$liquidity_{b,t-1}$	-0.035	-0.023	-0.067	-0.100	-0.064	0.036
	(0.090)	(0.086)	(0.065)	(0.087)	(0.085)	(0.104)
$npl_{b,t-1}$	-0.179	-0.176	-0.113	-0.295	0.253	0.547
	(0.259)	(0.278)	(0.186)	(0.216)	(0.287)	(0.331)
fx $liab_{b,t-1}$	-0.095**	-0.095**	-0.073**	-0.055**	-0.086*	-0.031
	(0.041)	(0.042)	(0.028)	(0.024)	(0.041)	(0.040)
$gov_{b,t-1}$	0.710	0.730	2.198**	2.036*	2.335	0.299
	(0.859)	(0.927)	(0.951)	(1.122)	(1.597)	(1.942)
$foreign_{b,t-1}$	-2.403**	-2.441**	-1.251	-2.777**	0.814	3.591
	(1.059)	(1.071)	(1.162)	(1.073)	(2.288)	(2.497)
ΔGDP_{t-1}	0.621***	0.501***				
	(0.157)	(0.181)				
ΔCPI_{t-1}	-0.481	-0.739				
	(0.618)	(0.709)				
$\Delta CA/GDP_{t-1}$	0.002	-0.002				
	(0.018)	(0.016)				
Δi_{t-1}	-0.747**					
	(0.290)					
Δi^{s}_{t-1}		-4.849***				
		(0.985)				
loosening	0.472	0.605				
	(0.486)	(0.447)				
firm $credit_{f,t-1}$	-8.760***	-8.785***				
	(0.566)	(0.561)				
n $employees_{f,t-1}$	3.473***	3.479***				
	(0.307)	(0.307)				
firm $default_{f,t-1}$	-5.469***	-5.450***				
	(0.581)	(0.561)				

continued

$\ln(\text{demand}_{b,t-1})$	0.561	0.626*	0.762*	1.397***	0.202	-1.195
	(0.365)	(0.365)	(0.421)	(0.398)	(0.705)	(0.748)
$\ln(\text{savings}_{b,t-1})$	0.245	0.243	0.175	0.348**	-0.023	-0.371
	(0.149)	(0.146)	(0.161)	(0.159)	(0.246)	(0.242)
$\ln(\text{time}_{b,t-1})$	-0.386	-0.397	-1.072	-0.921	-1.577	-0.656
	(1.205)	(1.187)	(1.221)	(1.048)	(2.252)	(2.260)
Observations	2,595,398	2,595,398	2,595,398	1,440,168	1,155,230	2,595,398
R-squared	0.065	0.065	0.411	0.413	0.411	0.412
Seasonal effects & Macro-controls$_{t-1}$	Yes	Yes	<>	<>	<>	<>
Firm FEs & Controls$_{f,t-1}$	Yes	Yes	<>	<>	<>	<>
Firm*Quarter FE	No	No	Yes	Yes	Yes	Yes
Risk$_{f,b,t-1}$	Yes	Yes	Yes	Yes	Yes	Yes
Bank Controls$_{b,t-1}$	Yes	Yes	Yes	Yes	Yes	Yes
N firms	90440	90440	90440	81817	76601	90440
N banks	83	83	83	82	81	83
Cluster	bank & quarter					

Notes: This table is a robustness exercise of Table III. It presents the lending channel of Reserve Requirements (RRs) in the presence of bank controls that reflect the ex-ante share of savings, time and demand deposits. For each bank and quarter, $\Delta\text{ResReq}_{b,t-1}$ represents differential exposure to RRs. The dependent variable is the change in the natural logarithm (ln) of the total credit exposure of bank *b* against firm *f* between *t* and *t+1*, $\Delta\ln(\text{credit}_{b,f,t:t+1})$. The macro-controls are the consumer price index (ΔCPI_{t-1}), GDP growth (ΔGDP_{t-1}), and yearly changes in the current-account/GDP ($\Delta\text{CA/GDP}_{t-1}$). The bank controls are core capital-to-assets ratio ($\text{capital}_{b,t-1}$), the natural logarithm of banks' assets ($\text{size}_{b,t-1}$), liquid-to-total assets ratio ($\text{liquidity}_{b,t-1}$), share of non-performing loans to total credit ($\text{npl}_{b,t-1}$), foreign currency-to-core liabilities ratio ($\text{fx liab}_{b,t-1}$), a dummy variable for banks with foreign ($\text{foreign}_{b,t-1}$) and government ($\text{gov}_{b,t-1}$) control. Firm controls are: (ln) of total firm credit ($\text{firm credit}_{f,t-1}$) and (ln) of the number of its employees ($\text{n employees}_{f,t-1}$). We also use a dummy variable in case the firm is in default, i.e., if it has at least one loan in arrears for more than 90 days against any financial system player in *t-1* ($\text{firm default}_{f,t-1}$). This information is promptly available to all banks in the credit registry. We use a risk control, $\text{risk}_{f,b,t-1}$, which is the weighted average provision assigned by each bank to all its loans against the same firm in *t-1*. To proxy for interest rate policy, we take the quarterly changes in the overnight policy rate (Selic, Δi_{t-1}) in model (1) and policy surprises, i.e., the (quarterly) accumulated one-day changes in the 30-day interest rate swap immediately after each Copom meeting (Δi^{s}_{t-1}) in model (2). We use firm-quarter fixed effects (FEs) to control for credit demand shifts in models (3) to (5). In models (1) and (2), all macro-controls are estimated, and we rely on firm observables and (time invariant) firm FEs for demand control. In model (4), we restrict the sample to the quarters following loosening policies with RRs; and, in model (5), to quarters following tightening policies. Model (6) presents the difference between (5) and (4). Standard errors are two-way clustered at the bank and time (year:quarter) dimension. Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Appendix A.7: Robustness: introducing controls for ex-ante bank-level savings, demand and time deposits in Table V.

Dependent: $\Delta\ln(credit_{b,f,t:t+1})$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	all quarters			loosening (in busts)			tightening (in booms)			dif (9)-(6)
$\Delta ResReq_{b,t-1}$	-0.649***	-0.700***	-1.483***	-0.325*	-0.365*	-1.388***	-0.958**	-1.061***	-1.865**	-0.466
	(0.229)	(0.227)	(0.390)	(0.176)	(0.172)	(0.324)	(0.327)	(0.325)	(0.682)	(0.720)
$\Delta ResReq_{b,t-1}$ * Δi^s_{t-1}		-0.978	-1.420**		-1.055**	-2.224**		-1.510**	-1.620**	0.604
		(0.626)	(0.668)		(0.475)	(0.996)		(0.624)	(0.729)	(1.241)
$\ln(demand_{b,t-1})$	0.762*	0.724*	0.885**	1.397***	1.361***	1.314**	0.202	0.139	0.339	-0.975
	(0.421)	(0.420)	(0.368)	(0.398)	(0.430)	(0.483)	(0.705)	(0.700)	(0.628)	(0.763)
$\ln(savings_{b,t-1})$	0.175	0.167	0.133	0.348**	0.339*	0.332**	-0.023	-0.033	-0.072	-0.405
	(0.161)	(0.164)	(0.160)	(0.159)	(0.161)	(0.139)	(0.246)	(0.248)	(0.264)	(0.267)
$\ln(time_{b,t-1})$	-1.072	-1.054	-1.263	-0.921	-0.895	-0.787	-1.577	-1.570	-1.979	-1.193
	(1.221)	(1.222)	(1.127)	(1.048)	(1.068)	(0.976)	(2.252)	(2.255)	(2.204)	(2.227)
Observations	2,595,398	2,595,398	2,595,398	1,440,168	1,440,168	1,440,168	1,155,230	1,155,230	1,155,230	2,595,398
R-squared	0.411	0.411	0.412	0.413	0.413	0.413	0.411	0.411	0.412	0.413
N firms	90440	90440	90440	81817	81817	81817	76601	76601	76601	90440
N banks	83	83	83	82	82	82	81	81	81	83
N quarters	30	30	30	16	16	16	14	14	14	30

Notes: This table is a robustness exercise of Table V. It presents the lending channel of RRs and the interactions with policy rate surprises (Δi^s_{t-1}) in the presence of bank controls that reflect the ex-ante share of savings, time and demand deposits. For each bank and quarter, $\Delta ResReq_{b,t-1}$ represents differential bank-level exposure to RRs. Policy rate surprises are (quarterly) accumulated one-day changes in the 30-day interest rate swaps immediately after each Copom meeting (Δi^s_{t-1}). The dependent variable is the change in the natural logarithm (ln) of bank *b* total credit exposure against firm *f* between *t* and *t+1*, $\Delta\ln(credit_{b,f,t:t+1})$. All models have the risk, $risk_{f,b,t-1}$, and bank controls ($size_{b,t-1}$, $liquidity_{b,t-1}$, $npl_{b,t-1}$, fx $liab_{b,t-1}$, $foreign_{b,t-1}$, $gov_{b,t-1}$) as well as firm-quarter FEs. Apart from the dummy variables, government and foreign, all other variables have been de-meaned. In models (4) to (6), we restrict the sample to the quarters following loosening policies and, in models (7) to (9), we restrict the sample to quarters following tightening policies. In model (10), we present the differences between models (9) and (6). We introduce all bank interactions in models (3), (6) and (9) and interactions with policy surprises in models (2),(3),(5),(6),(8) and (9). Standard errors are two-way clustered at the bank and time (year:quarter) dimension. Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Appendix A.8: Robustness: introducing controls for ex-ante bank-level excess reserves in Table III

Dependent:	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta \ln(\text{credit}_{b,f,t:t+1})$	all quarters			loosening (in busts)	tightening (in booms)	difference (5)-(4)
$\Delta \text{ResReq}_{b,t-1}$	-0.538**	-0.500**	-0.670***	-0.497**	-0.843***	-0.346*
	(0.224)	(0.222)	(0.170)	(0.186)	(0.196)	(0.174)
$\text{risk}_{b,f,t-1}$	-3.232***	-3.248***	-1.403***	-1.321***	-1.505***	-0.183
	(0.330)	(0.324)	(0.192)	(0.269)	(0.175)	(0.210)
$\text{size}_{b,t-1}$	1.546***	1.551***	1.335***	1.316***	1.175**	-0.141
	(0.373)	(0.371)	(0.303)	(0.369)	(0.413)	(0.435)
$\text{capital}_{b,t-1}$	0.195***	0.205***	0.210***	0.188*	0.174	-0.014
	(0.066)	(0.068)	(0.072)	(0.096)	(0.106)	(0.127)
$\text{liquidity}_{b,t-1}$	-0.012	-0.001	-0.049	-0.060	-0.056	0.004
	(0.069)	(0.066)	(0.054)	(0.074)	(0.072)	(0.093)
$\text{npl}_{b,t-1}$	-0.248	-0.244	-0.233	-0.383*	0.047	0.430*
	(0.218)	(0.215)	(0.147)	(0.191)	(0.176)	(0.228)
$\text{fx liab}_{b,t-1}$	-0.107***	-0.108***	-0.083***	-0.078***	-0.084**	-0.006
	(0.035)	(0.034)	(0.025)	(0.023)	(0.032)	(0.022)
$\text{gov}_{b,t-1}$	0.660	0.676	2.042**	2.020*	1.897	-0.123
	(0.983)	(0.991)	(0.901)	(1.078)	(1.365)	(1.554)
$\text{foreign}_{b,t-1}$	-2.642***	-2.692***	-1.843**	-3.004**	-0.290	2.715*
	(0.832)	(0.837)	(0.824)	(1.056)	(1.197)	(1.510)
ΔGDP_{t-1}	0.630***	0.514***				
	(0.146)	(0.112)				
ΔCPI_{t-1}	-0.566	-0.818				
	(0.616)	(0.627)				
$\Delta \text{CA/GDP}_{t-1}$	0.003	-0.001				
	(0.012)	(0.012)				
Δi_{t-1}	-0.722**					
	(0.267)					
Δi^{s}_{t-1}		-4.690**				
		(1.784)				
loosening	0.452	0.579				
	(0.546)	(0.470)				
$\text{firm credit}_{f,t-1}$	-8.784***	-8.810***				
	(0.573)	(0.565)				
$\text{n employees}_{f,t-1}$	3.465***	3.471***				
	(0.290)	(0.294)				
$\text{firm default}_{f,t-1}$	-5.502***	-5.485***				
	(0.660)	(0.634)				

continued

exc reserves$_{b,t}$	-2.426 (2.643)	-2.495 (2.551)	-3.193 (2.691)	-1.829 (1.273)	-5.201 (5.099)	-3.372 (4.657)
Observations	2,595,398	2,595,398	2,595,398	1,440,168	1,155,230	2,595,398
R-squared	0.065	0.065	0.411	0.412	0.411	0.412
Seasonal effects & Macro-controls$_{t-1}$	Yes	Yes	<>	<>	<>	<>
Firm FEs & Controls$_{f,t-1}$	Yes	Yes	<>	<>	<>	<>
Firm*Quarter FE	No	No	Yes	Yes	Yes	Yes
Risk$_{f,b,t-1}$	Yes	Yes	Yes	Yes	Yes	Yes
Bank Controls$_{b,t-1}$	Yes	Yes	Yes	Yes	Yes	Yes
N firms	90440	90440	90440	81817	76601	90440
N banks	83	83	83	82	81	83
Cluster				bank & quarter		

Notes: This table is another robustness exercise of Table III. It presents the lending channel of Reserve Requirements (RRs) in the presence of the bank control excess reserves (exc reserves$_{b,t}$), which captures the voluntary (unremunerated) "excess" reserves of each bank deposited at the BCB as a share of its total liabilities. For each bank and quarter, ΔResReq$_{b,t-1}$ represents differential exposure to RRs. The dependent variable is the change in the natural logarithm (ln) of the total credit exposure of bank *b* against firm *f* between *t* and *t+1*, Δln(credit$_{b,f,t:t+1}$). The macro-controls are the consumer price index (ΔCPI$_{t-1}$), GDP growth (ΔGDP$_{t-1}$), and yearly changes in the current-account/GDP (ΔCA/GDP$_{t-1}$). The bank controls are core capital-to-assets ratio (capital$_{b,t-1}$), the natural logarithm of banks' assets (size$_{b,t-1}$), liquid-to-total assets ratio (liquidity$_{b,t-1}$), share of non-performing loans to total credit (npl$_{b,t-1}$), foreign currency-to-core liabilities ratio (fx liab$_{b,t-1}$), a dummy variable for banks with foreign (foreign$_{b,t-1}$) and government (gov$_{b,t-1}$) control. Firm controls are: (ln) of total firm credit (firm credit$_{f,t-1}$) and (ln) of the number of its employees (n employees$_{f,t-1}$). We also use a dummy variable in case the firm is in default, i.e., if it has at least one loan in arrears for more than 90 days against any financial system player in *t-1* (firm default$_{f,t-1}$). This information is promptly available to all banks in the credit registry. We control for risk, risk$_{f,b,t-1}$, which is the weighted average provision assigned by each bank to all its loans against the same firm in *t-1*. To proxy for interest rate policy, we take the quarterly changes in the overnight policy rate (Selic, Δi_{t-1}) in model (1) and policy surprises, i.e., the (quarterly) accumulated one-day changes in the 30-day interest rate swap immediately after each Copom meeting (Δi^s_{t-1}) in model (2). We use firm-quarter fixed effects (FEs) to control for credit demand shifts in models (3) to (5). In models (1) and (2), all macro-controls are estimated, and we rely on firm observables and (time invariant) firm FEs for demand control. In model (4), we restrict the sample to the quarters following loosening policies with RRs; and, in model (5), to quarters following tightening policies. Model (6) presents the difference between (5) and (4). Standard errors are two-way clustered at the bank and time (year:quarter) dimension. Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Appendix A.9: Robustness: introducing excess reserves in Table IV

Dependent: $\Delta\ln(\text{credit}_{b,f,t:t+1})$	(1)	(2)	(3)	(4)
$\Delta ResReq_{b,t-1}$	-0.670***	-0.731***	-0.673***	-1.370***
	(0.170)	(0.158)	(0.229)	(0.305)
$\Delta ResReq_{b,t-1}$ * exc $reserves_{b,t}$		-0.895	-0.950	0.077
		(0.883)	(0.855)	(0.592)
exc $reserves_{b,t}$	-3.193	-3.702	-3.772	-3.519
	(2.677)	(3.117)	(3.032)	(2.769)
$\Delta ResReq_{b,t-1}$ * $size_{b,t-1}$				-0.100
				(0.079)
$\Delta ResReq_{b,t-1}$ * $capital_{b,t-1}$				0.029
				(0.019)
$\Delta ResReq_{b,t-1}$ * $liquidity_{b,t-1}$			-0.009	-0.010
			(0.017)	(0.016)
$\Delta ResReq_{b,t-1}$ * $npl_{b,t-1}$				-0.081**
				(0.037)
$\Delta ResReq_{b,t-1}$ * fx $liab_{b,t-1}$				0.019*
				(0.010)
$\Delta ResReq_{b,t-1}$ * $gov_{b,t-1}$				1.073***
				(0.247)
$\Delta ResReq_{b,t-1}$ * $foreign_{b,t-1}$				1.194***
				(0.304)
Observations	2,595,398	2,595,398	2,595,398	2,595,398
R-squared	0.412	0.411	0.412	0.412
N firms	90440	90440	90440	90440
N banks	83	83	83	83
N quarters	30	30	30	30
Cluster		bank & quarter		

Notes: This table presents bank heterogeneities related to the lending channel of RRs including excess reserves (exc $reserves_{b,t}$), which captures the voluntary (unremunerated) "excess" reserves of each bank deposited at the BCB as a share of its total liabilities. For each bank and quarter, $\Delta ResReq_{b,t-1}$, represents differential bank-level exposure to RRs. The dependent variable is the change in the natural logarithm (ln) of bank b total credit exposure against firm f between t and $t+1$, $\Delta\ln(\text{credit}_{b,f,t:t+1})$. All models have the risk, $risk_{f,b,t-1}$, and bank controls ($size_{b,t-1}$, $liquidity_{b,t-1}$, $npl_{b,t-1}$, fx $liab_{b,t-1}$, $foreign_{b,t-1}$, $gov_{b,t-1}$) as well as firm-quarter FEs. Apart from the dummy variables, government and foreign control, all other variables have been de-meaned. Standard errors are two-way clustered at the bank and time (year:quarter) dimension. Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.