Bank Capital Requirements and Loan Pricing: Loan-level Evidence from a Macro Prudential Within-Sector Policy[*]

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Abstract:

This paper investigates the consequences on loan spreads of a within-sector macro prudential measure in Brazil that raised regulatory bank capital for targeted auto-loans (long maturities and high loan-to-values). Our results show that Brazilian banks, after the regulatory measure, increased spreads charged on the same borrower for targeted auto loans, while there is no robust evidence of spread changes for untargeted ones. Finally, the later withdrawal of the regulatory capital measure was similarly followed by lower spreads charged on auto loans whose capital charges decreased. Nevertheless, this reduction in spreads was smaller than the original increase.

Keywords: bank capital requirement; macro prudential policy; auto loans; loan spreads

JEL Classification: G21; G28

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1. Introduction

After the international financial crisis of 2007/2008, financial regulation started to be designed with a new macro prudential dimension (e.g. Hanson et al., 2011). An important macro prudential tool brought to the forefront of the debate refers to countercyclical capital requirements, which boost capital requirements in booms, providing additional buffers to be consumed in downturns. The countercyclical capital buffer of Basel III is an example of such a tool (BCBS, 2010b). Its objectives are not only to increase the banking sector resilience to future downturns but also to lean against the credit cycle. The impact of such policies has also deserved closer academic inspection (e.g. Aiyar et al., 2014; Jimenez et al., 2013). More recently, the policy of varying capital requirements only on lending to sectors that may be exhibiting particular exuberance has also been discussed and used in some countries (BoE, 2014; CGFS, 2012). Such sectoral capital requirements focus on the relative risks stemming from such apparent exuberance and, therefore, try to lean against that specific lending.

The experience of the Brazilian auto loan credit market during the years of 2009 and 2010 is an example of a sector evolution that generated macro prudential concerns due to a rapid and unbalanced expansion towards riskier loans (e.g. long maturities and high loan-to-values). To cope with these concerns, a new Brazilian bank capital regulation was established at the end of 2010, with a format close to sectoral capital requirements, but not exactly the same (BCB, 2010). In a novel within-sector policy, capital requirement was raised for particular targets within the consumer auto loan sector, with those targets being precisely new loans with long maturities and high LTVs. Was the macro prudential capital policy effective in leaning against the riskier lending within the auto loan sector? The empirical results of this paper indicate that this goal was achieved in the sense that that banks charged higher loan spreads in response to higher capital requirements.

The following transmission mechanisms could have been involved in the banks' spread responses. First, higher capital requirements increase the optimal target for banks' capital ratios (e.g. Berrospide and Edge, 2010; Francis e Osborne, 2012; Hancock and Wilcox, 1993 and 1994).[1] The need to constitute more capital may be then addressed by charging higher lending spreads.[2] Additionally, the higher (future) capital position increases banks' total financing costs due to the presence of financial frictions (e.g. Admati, 2011), which are passed to lending spreads.[3,4] However, the relevance and true intensity of those financial frictions and, therefore, of the increase in banks' loan spreads is a matter of substantial debate in the recent literature about bank capital regulation (e.g. BCBS, 2010a; MAG, 2010; Hanson *et al*., 2010; Miles *et al*., 2013). This paper contributes to this debate by providing evidence of material effects of bank capital requirement on loan spreads: an increase of 2.19 percentage points in spreads for an additional capital charge of 8.25% of bank assets. This sort of result is new in the literature since previous studies on loan spreads, as those mentioned above, gauge the consequences of increases in *actual* capital and not in capital requirements.[5]

The main contributions of this study, however, pertain to the within-sector nature of the capital requirement policy investigated, which brings novel features to the traditional analysis of the impact of bank capital shocks. First, the fact that the macro prudential capital policy was applied on a loan-level basis naturally motivates the question of how

[1] That does not mean that banks have *ex-ante* actual capital equal to minimum capital requirements, but simply that capital requirements are binding restrictions on banks' capital decisions. The cited papers show empirical evidence of that.
[2] Banks that need to rebuild their capital structure are likely to sacrifice reputational capital by reneging on their implicit commitment not to exploit their monopoly power over borrowers (e.g. Boot et al., 1993)
[3] These financial frictions include tax benefits of debt finance, implicit guarantee subsidies and asymmetric information about banks' conditions (e.g. Admati et al. 2011; Freixas and Rochet, 2008). These frictions are not contemplated in the idealized world of the Miller-Modigliani theorem (Modigliani and Miller, 1958), in which leverage is irrelevant to the cost of bank finance.
[4] Besides passing this higher cost to borrowers through increases in loan spreads, banks may also adopt other strategies, such as cutbacks in operational expenditures through productivity increases, but those may be feasible only in the medium to long run. Higher capital requirements may also imply in higher credit rationing by banks.
[5] Therefore, our results are not directly comparable to previous studies, so that we develop an alternative methodology to check whether our estimations make economic sense.

differently the spreads of targeted and untargeted loans varied after the regulatory measure. If banks disregard the new regulatory distinction between targeted and untargeted loans for capital allocation purposes, the additional capital cost could be passed-through to all types of auto loans. If, instead, banks consider in their risk-based pricing the cost of allocated regulatory capital, then they would increase the spreads mainly of targeted loans as a result of the new regulation.[6] In that case, a natural control group for an empirical investigation of the Brazilian experiment is the set of untargeted auto loans, which didn't suffer any capital requirement increase, and the focus of the analysis becomes whether the spreads charged on targeted loans grew in relation to those of untargeted ones in the same sector (by means of a difference-in-difference investigation). This paper finds econometric evidence of this behavior, controlling for several explanatory variables, including loan features, which suggests that higher capital requirements indeed increase financing costs of banks and also that this cost was offset on a regulatory capital allocation basis. Furthermore, from a bank cross-section analysis exploring estimates of the costs of bank capital and bank debt and actual capital ratios, there is additional evidence that the spread increase in targeted loans was driven by higher bank financing costs imposed by the higher capital requirements.

However, a caveat in the previous discussion is that the set of untargeted loans may have also been indirectly affected by demand shifts driven by the macro prudential measure. Migration of demand from targeted (now even more expensive) to untargeted loans (the traditional substitution effect) could also increase the spread of loans not targeted by the regulation.[7,8] However, if demand shifts are limited, we expect the set of untargeted loans to perform better as a control group and not to be charged higher

[6] See BCBS (2009) for possible channels though which regulatory capital allocation may influence internal economic capital allocation. Some banks may simply use the minimum regulatory capital charges for internal management purposes, including pricing profitability analysis.
[7] This could occur because of more, and possibly riskier, borrowers demanding untargeted loans.
[8] In a similar fashion, some degree of pass-through of the higher bank financing costs to untargeted loans could also increase the spreads charged on the latter.

spreads. Indeed, this paper does not find robust evidence of spread spillovers to set of untargeted auto loans. This is an important result because undesired spillovers driven by substitution effects are a high concern in the debate of macro-prudential policies. The result is also new because, while both Aiyar et al. (2014) and Jimenez et al. (2013) investigate credit substitution between banks differently affected by macro prudential measures, this paper is the first to implicitly consider substitution effects between loan types.

The macro prudential capital measure was mostly withdrawn on November, 2011, almost a year after its introduction. This paper shows that the regulatory capital release was, similarly, associated to lower spreads charged on auto loans whose capital charges decreased, although this reduction was smaller than the original increase. The asymmetric impact as of the adoption and withdrawal of the regulatory capital measure points to the long lasting effects that macro prudential policies may have due to their signaling impact for example. Notice that this sort of result is only obtainable when macro-prudential policies vary in comparable ways, which is not always the case (e.g. Jimenez et al., 2013).[9]

This paper speaks to the empirical literature on the effects of bank capital shocks on bank lending. This literature traditionally faces the challenge of disentangling supply from demand effects (e.g. Bernanke and Lown, 1991; Cornett et al. 2011; Gambacorta and Mistrulli, 2004, Ivashina and Scharfstein, 2010, among many others). Indeed, poor economic conditions may produce bank losses and decrease bank capital and, at the same time, generate smaller number and amounts of loans being granted due to fewer lending opportunities. One way to deal with this challenge is to use “natural experiments” where the shock to capital is unrelated to lending opportunities. Houston et al. (1997), Peek and

[9] The regulatory events analyzed in Jimenez et al. (2013) with regard to dynamic provision in Spain include its adoption, changes in the parameters of the formula and changes in ceilings and floors allowed. Their varying nature makes their magnitude incomparable. Aiyar et al. (2014) investigate the effect of incremental variations of bank capital requirements in UK but do not examine this issue.

Rosengreen (1997) and Puri et al. (2011) are examples of that approach, in which capital shocks and affected supply occur at different parts of the bank holding company[10]. Aiyar et al. (2014), Berger and Udell (1994), Brinkmann and Horvitz (1995), Jimenez et al. (2013) and this paper are also examples of that approach, but in which capital shocks derive from specific regulatory changes. However, regulatory actions may still be partly endogenous to characteristics of bank lending so that some of these studies try to control for bank cross-sectional variation of credit demand.

To further control for demand effects, this paper uses loan-level data and fixed effects, as in Jimenez et al., (2012, 2013).[11] Loan-level data come from a comprehensive public credit register that comprises information on basically all outstanding auto loans in the Brazilian economy. Fixed effects at the borrower level, borrower-bank level and further controls for unobserved loan-type heterogeneity are also used extensively in the following sections. That means that we compare, before and after the regulatory measure, the spreads charged on the same borrower from the same bank on similar new auto loans. Notice that this is particularly important for analyzing within sector-capital requirements since migration of demand may clearly change unobserved borrower characteristics of the groups of targeted and untargeted loans within each bank.

Finally, it is worth remarking that, differently to most of the bank capital literature, our focus is on prices rather than on quantities. This is useful because prices are likely a better indicator of the whole behavior (intensive and extensive margins) of credit supply than purely individual loan amounts, which mostly reflect the intensive margin.[12,13] Indeed, in the Brazilian regulatory experiment, the average auto loan size hardly varied following the regulatory events, whereas total credit to new auto loans clearly declined

[10] In the case of Peek and Rosengreem (1997) and Puri et al. (2011) they also occur at different countries.
[11] It also uses a short sample time period with no major macroeconomic changes and time dummies.
[12] We do not have data on loan applications that could allow a proper investigation of the extensive margin of credit supply (e.g. Jimenez et al., 2012 and 2013).
[13] The fact that few studies investigate loan spreads seems to be a consequence of the scarcity of data on individual loan prices.

after the capital increase. This reduction, though not directly captured in our estimations, is consistent with the spread increases for targeted loans identified in the paper. The focus on prices makes this paper also related to the empirical literature on loan pricing policies (e.g. Hubbard et al., 2002, Santos, 2011). In particular, our difference-in-difference analysis is close to Santos (2011), who investigates corporate loan pricing behavior of US banks following the subprime crisis.

The remainder of the paper is organized as follows. Section 2 explains the novel macro prudential within-sector capital measure adopted in Brazil, section 3 presents and discusses the methodology, section 4 describes and characterizes the data, section 5 presents and discusses the results and section 6 concludes.

2. The novel macro prudential within-sector capital measure

The experience of the Brazilian auto loan market during the years of 2009 and 2010 is an example of a sector evolution that generated concerns of prudential nature. The rapid expansion of new auto loan credit, accompanied by extension of loan maturities, greater loan to value and, at the same time, decreasing spreads (see figures 3, 4, 5 and 6 of the appendix) naturally raised preoccupation.[14] The underlying origin of those movements could perhaps be tracked to the higher risk-taking incentives prompted by the abundant liquidity transmitted into Brazilian credit markets by international capital flows (Silva and Harris, 2012). On its turn, the particular manifestation in the auto-loan sector might be related to an environment of fierce competition allied to a perception of opportunities for regulatory arbitrage.[15] To cope with concerns with the formation of unbalances, a new Brazilian bank capital regulation was established on December, 3rd of

[14] At the end of 2010, the Brazilian auto loan sector represented 25% of all consumer loans with non-earmarked funds and 13% of all loans with non-earmarked funds.
[15] The Brazilian auto loan market had also benefitted tremendously in 2004 from legal reforms that simplified the sale of repossessed cars used as collateral (e.g. Assunção et. al., 2013).

2010, with a format close to sectoral capital requirements, but not exactly the same (BCB, 2010). Capital requirement was raised for particular targets within the consumer auto loan sector, with those targets being new loans with long maturities and high LTVs. More specifically, risk weights were doubled, from 75% to 150%, for the universe of auto loans presented in table 1. This translated into an additional capital charge of 8.25% (=11%×75%) of the outstanding balance of targeted auto loans.[16] The remaining auto loans did not suffer any capital increase and continued to be weighted 75%.[17] Such within-sector capital requirement policy was largely unexpected to market participants since it was the first capital-based macro prudential instrument implemented in Brazil.

Table 1: Universe of loans targeted by new regulation

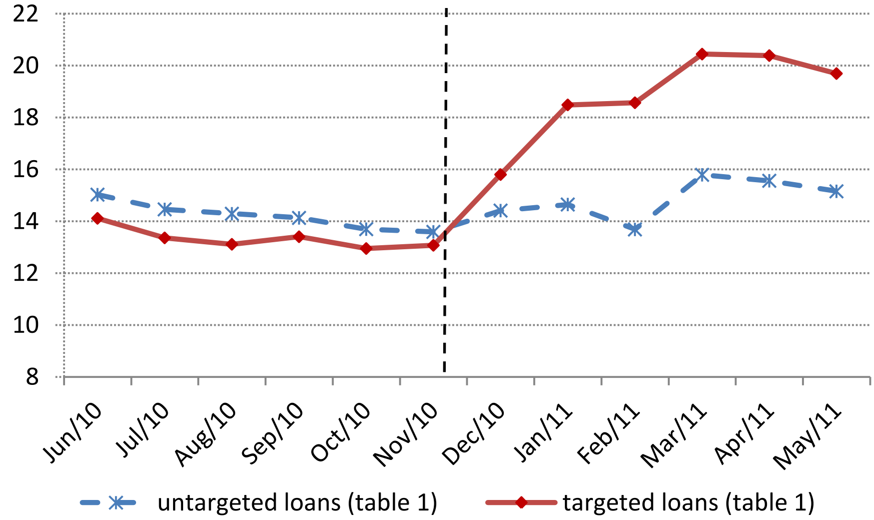
Maturity (months)	(24-36]	(36-48]	(48-60]	>60
LTV(%)	>80	>70	>60	All

At the previously mentioned figures, one can notice on December, 2010 a sharp contraction in the monthly volume of new auto loans, with a somewhat stabilization thereafter, a clear reversion in the trajectories of maturity and LTV, both with a reduction tendency thereafter, and a remarked rise in spreads after the new regulation too. The behavior of lending spreads and credit volumes, according to whether loans were targeted or not by the new regulation, can be seen in the following figures 1 and 2.[18] At figure 1, on December 2010, we notice a sharp increase of spreads of new targeted loans relatively to new untargeted loans. That would be consistent with banks passing largely to targeted loans their higher funding costs derived from the higher capital requirements. Alternative

[16] Currently in Brazil, the capital charge for each credit exposure is 11% multiplied by its risk weight. Credit risk internal models approach has not been adopted in Brazil yet.
[17] The additional required capital for loans granted after December, 3th of 2010 needed to be in place on July 1st, 2011. Although around seven months were given for banks adjust their reactions, the pricing response was immediate as the next paragraph informs.
[18] Figures 1 and 2 are computed based on a slightly different universe of auto-loans (excluding loans with missing data) from the one underlying the computation of figures 6 and 3. Therefore, loan spread levels and total credit levels are not exactly the same between the two sets of figures.

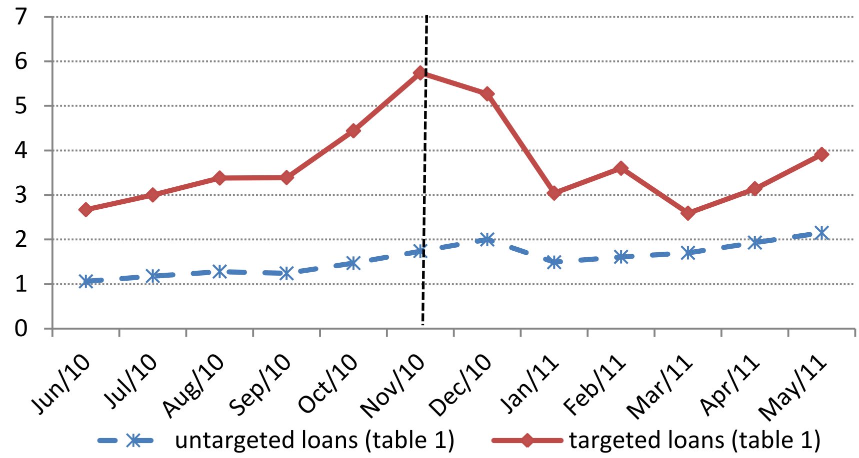
explanations for the relative spread increase based on supposedly increasing credit risk of targeted loans in comparison to untargeted ones are unlikely given that both groups of loans were following parallel trajectories of probability of becoming non-performing previous to the new regulation (see figure 7 of the appendix[19]). Notice also at figure 2 that total credit to targeted loans acquires a downward tendency from December 2010 until March 2011, whereas total credit to untargeted loans maintains a moderate increase pattern since June 2010 and during most of the time shown. Therefore, the movements of targeted loans, with increase in spreads and decrease in volumes, is likely to be supply driven.

Figure 1: Loan spread charged on new auto loans (monthly average - %)



[19] On the other hand, after the new regulation it is nevertheless possible that the credit risk of targeted loans has increased comparatively due to the higher spread charged itself.

Figure 2: Total credit to new auto loans (R$ billions)



On November 11th, 2011, regulation was changed again, abolishing most of the previous risk weight increases for auto loans (BCB, 2011). More specifically, previously affected auto loans satisfying the criteria of table 2 (which are a strict subset of table 1) returned to the 75% weight, translating into a capital charge reduction of 8.25%. Auto loans with maturities greater than 60 months remained risk-weighted high, at 150%, and auto loans with maturities shorter than 24 months loans remained risk-weighted low, at 75%.[20] In contrast to the introduction of the regulation, figures 3, 4 and 5 of the appendix do not show any remarked change in the trajectories of total credit of new auto loans, their maturities or LTVs around the time of the regulatory change, on November, 2011. On the other hand, figure 8 of the appendix shows, after January, 2012, a reduction in the spread gap between new loans that were targeted by the regulatory capital release and new untargeted loans. That would be consistent with banks passing largely to targeted loans their lower funding costs derived from the smaller capital requirements.[21]

[20] Risk weights were also altered for consumer credit loans, depending on their maturities.

[21] The fact that the average spread of untargeted loans has increased in that period is related to a higher realization of auto loan credit risk then. More importantly, however, is the fact that the average spread of targeted loans has not followed the same movement.

Table 2: Universe of loans targeted by regulatory capital release			
Maturity (months)	(24-36]	(36-48]	(48-60]
LTV(%)	>80	>70	>60

3. Methodology

Making use of a time period covering the implementation of the novel within-sector capital measure in Brazil, we estimate the following equation that explains the spread $Loan\_spread_{i,b,l,t}$ charged on borrower *i* by bank *b* for the new auto loan *l* at time *t*.

$$\text{Loan\_spread}_{i,b,l,t} = c + \gamma\cdot \text{ Loan targeted(table\_1)}_{l} + \alpha\cdot \text{ New regulation}_{t} + \beta\cdot \text{ New regulation}_{t} \times \text{Loan targeted(table\_1)}_{l} + \text{bank controls}_{b,t-1} + \text{loan controls}_{l} + \text{time controls}_{t} + \text{fixed effect}_{i,b} + \text{error term}_{i,b,l,t} \qquad (1)$$

where New regulation_t = 1 after December, 3rd, 2010 and 0 before, Loan $\text{targeted(table\_1)}_l$ = 1 if new loan l fits within the criteria of table 1 and 0 otherwise.

Coefficient β of the interaction *New* $\textit{regulation}_t \times \textit{Loan targeted}_l$ is the parameter of most interest in (1). It measures the relative impact of the regulatory capital increase on the spread charged on auto loans that have been applied higher risk weights in comparison to untargeted loans. If banks price each loan based largely on its regulatory capital allocation rather than on internal economic capital modeling (e.g BCBS, 2009), then banks will increase the spread after the new regulation mostly of targeted loans (table 1), that is β>0. Also, as the reduction of credit supply for targeted auto loans (with now higher risk weights) helps, to a greater extent, banks to cope with the higher overall capital requirement than a corresponding reduction in untargeted auto loans, we expect banks to increase more the spread of targeted loans after the new regulation than of untargeted ones, which leads again to $\beta>0$.

Coefficient α of the variable *New regulation* represents the spread increase suffered by untargeted auto loans after the new regulation. Some pass-through of the higher bank total financing costs to untargeted loans and migration of demand from targeted (now even more expensive) to untargeted loans after the new regulation (the traditional substitution effect) may also produce increases in the spread of untargeted loans, which would be consistent with a positive sign also for α.[22] In other words, loans outside the criteria of table 1 may also be indirectly affected. Although a precise identification of those effects is unfeasible in equation (1) because there is no control group for the set of untargeted loans, we expect them not to get entangled with aggregate factors that affect loan spreads over time and are not related to the introduction of the new regulation. The latter are likely better captured by the time control variables.

Loan spread is computed as the difference between the loan lending rate and the Brazilian basic interest rate (daily SELIC). Bank controls comprise indicators of bank financial position, bank risk and bank efficiency. They include capital to assets ratio (*Capital*), logarithm of total assets (*Lassets*), holdings of cash and marketable securities over total assets (*Liquidity*), bank reserves over total assets (*Reserves*), non-performing auto loans (*Npl*) and return on assets (*Roa*), among others. Time dummies (one for each month) capture the phase of the business cycle and secular trends. The set of loan controls include the logarithm of loan amount (*Lamount*), the logarithm of loan maturity (*Lmaturity*) and loan-to-value (LTV). Larger or longer loans, or loans with smaller relative collateral, may represent higher credit risk, so the effect of those variables on loan spreads can be positive. On the other hand, these loan controls are jointly determined

[22] Those factors may also reduce the original impact for targeted loans but, since targeted and untargeted loans are not perfect substitutes, the expectation for $\beta>0$ remains unaltered. Finally, notice at (1) that *Loan targeted* controls for discrete differences between the groups of targeted and untargeted loans, whereas the loan controls deal with continuous differences between the two groups.

with loan spreads and also reflect credit demand characteristics. For example, more expensive loans may be associated to less demand for larger amounts and a preference for shorter maturities, so that negative signs may also be found. We estimate our models both with and without loan controls[23].

In a similar fashion, the indicator variable *Loan targeted* is possibly determined jointly with loan spreads and reflects also borrowers' decisions. However, it lies at the core of the analysis and it is not meaningful to estimate (1) without it. To address concerns about the influence of its endogeneity (and again of the loan controls too) on our estimations, we adopt a matched loan approach. It means that, when performing the fixed effect transformation, we additionally only consider auto loans that fit all within or all without the criteria of table 1 (no migration). Notice that this approach is equivalent to adding a loan type dimension to the fixed effects of equation (1). Finally, in a further robustness control, matched loans are restricted to be sufficiently close, so as to disregard borrowers whose characteristics may vary too much along the period analyzed.

The regulatory capital release, occurred at the end of 2011, canceled most of the previous auto loan risk weight increases. This naturally motivates the question of whether spreads of loans targeted by the regulatory release decreased in comparison to loans whose risk weights remained unaltered. If so, it is also natural to ask whether this relative spread decrease has been of similar magnitude to the original relative spread increase when the new regulation was introduced. That is an important issue to the debate of whether macro-prudential measures could have asymmetric impacts with regard to their implementations and their withdrawals. Equation (2) below is adopted to investigate the consequences on spreads of the regulatory release.

[23] Notice that, as only new loans are considered, there is no option to use lags of the loan variables as instruments.

$$\text{Loan\_spread}_{i,b,l,t} = c + \zeta\cdot \text{ loan targeted(table\_1)}_l + \gamma\cdot \text{ loan targeted(table\_2)}_l + \alpha\cdot \text{ regulatory release}_t + \beta\cdot \text{ regulatory release}_t \times \text{ loan targeted(table\_2)}_l + \text{bank controls}_{b,t-1} + \text{loan controls}_l + \text{time controls}_t + \text{borrower-bank fixed effect}_{i,b} + \text{error term}_{i,b,l,t} \quad (2)$$

where regulatory release_t = 1 after November, 11th 2011 and 0 otherwise, loan $\text{targeted(table\_j)}_l$ = 1 if loan l fits within the criteria of table j and 0 otherwise, j=1,2.[24]

The regulatory release only reduces the risk weights of loans contained in the criteria of table 2, so that these loans are expected to suffer most of the impact of the capital release, having their spreads decreased.[25] The relative impact of this decrease in relation to loans that have retained the same risk weights is captured in coefficient β, for which we expect a negative sign. Also, the magnitude of this coefficient β can be compared to the magnitude of the analogous estimate of β in equation 1. Because immediately before the regulatory release, the effect of the original capital increase of December, 2010 might have already been partly mitigated (due for instance to substitutions effects), the comparison of β's (difference-in-differences of spreads) estimated around their respective regulatory events seems a proper way to compare between their effectiveness. On its turn, coefficient α, similarly to the discussion of equation (1), may capture a partial pass-through of the reduction of bank total financing costs to the set of untargeted loans and also substitution effects related to the migration of demand away from unaffected loans to the group of loans whose risk weights have decreased. Both of those effects would be consistent with a negative sign for α. Although a precise identification of those effects is difficult in our regressions, an insignificant coefficient α suggests they are limited, particularly if this insignificancy is maintained under different types of time controls (e.g. time dummies, linear tends).

[24] Notice that variable *Loan targeted(table_2)* controls at (2) for discrete differences between loans that experience reduction in risk weights and others, whereas *Loan targeted(table_1)* controls for loans that have higher risk weights before the regulatory release.

[25] Analogous arguments as of the capital requirement increase apply here.

4. Data and sample characterization

The data sources for this paper come from Brazilian Public Credit Register (SCR-Credit Information System) and the accounting database of Brazilian financial institutions (COSIF), both owned and managed by Central Bank of Brazil. The former provides information on loan interest rates and loan controls, whereas the latter provides information on most of bank controls. The sample used in the estimation of equation (1) comprises new auto loans granted in the period from June 2010 to May 2011, whereas estimation of equation (2) involves a similar sample from July 2011 to March 2012.[26] We work with monthly data.

SCR is a huge repository of data of all loans above R$5000 (five thousand *reais*) of financial institutions in Brazil.[27] However, it contains vary little borrower-level information, so that we cannot appropriately control for the multifaceted aspects of borrowers' creditworthiness. Therefore, we adopt a similar strategy to Santos (2011) and use borrower or borrower-bank fixed effects in most of the regressions. Such fixed effects help control for unobserved borrower and bank characteristics. Consequently, most estimations are based only on borrowers that have taken out at least two loans (from the same bank, in the case of borrower-bank fixed effects) in the period analyzed. Borrowers left out in those cases may be individuals that have acquired only one automobile during the period analyzed or more than one provided only one was financed, so that the impact of a possible sample bias introduced in the estimations with fixed effects is difficult to gauge. Notice also that, as the sample period comprises at most only one year for the estimations of equations (1) and (2), we do not expect characteristics of the same

[26] We stop the sample in March 2012 not to be contaminated by the new loan pricing policies adopted by Brazilian government-owned banks from April 2012 onwards.
[27] The reporting threshold was later lowered and is currently R$1000 (one thousand *reais*).

borrower to change in a meaningful way along that period[28]. Nevertheless, in some robustness exercises, we add the restriction that the multiple loans from the same bank to the same borrower be apart at most a specific number of months.

The sample related to the estimation of equation (1) has 3,017,387 new auto loans, 416 financial institutions[29] and 2,900,618 borrowers, after the removal of outliers and observations with missing data. From those, 39,311 borrowers have taken out at least two auto loans from the same financial institution during the sample period. For 10,647 borrowers, such loans were taken out only before the new regulation; for 8,406 borrowers, such loans were taken out only after the new regulation, and for 20,611 borrowers, at least one loan was taken out before and one after the new regulation. Of those last borrowers, 14,258 borrowers have taken out the same type of loan both before and after the new regulation, whereas 3,286borrowers have migrated from targeted loan(s) to untargeted one(s) and 3,466 borrowers have migrated in the opposite direction after the new regulation. There are 207 financial institutions that have granted at least two loans to the same borrower during the sample period.

The characterization of this sample in terms of loan variables is depicted at table 3. All loan variable differences between the two time periods (before and after new regulation) are significant at the 1% level due to the large number of observations. Average loan spreads after the new regulation are remarkably higher, which can be attributed to the behavior of targeted loans previously depicted at figure 1. Average loan amount, maturity and LTV are slightly lower after the new regulation, which is consistent with demand responses to the higher spreads prevailing in that period. However, their reductions being small in relative terms may contribute to reduce the potential impact of their endogeneity on our estimations. The number of sample loans also only marginally

[28] Moreover, usual macro explanatory factors for consumer credit risk, such as unemployment rate, the basic interest rate and GDP growth expectations did not suffer any abrupt change in that period.
[29] In the remainder of the paper, the term bank will be used to designate any financial institution allowed to grant loans.

changes between the two time periods, although the total number of loans (which includes loans with missing data) decreases substantially, consistent with the contraction in the total volume of new auto loans shown in figure 3 for example.[30] The percentage of targeted loans becomes smaller after the new regulation, which may be related to a higher preference among borrowers and/or banks for untargeted loans in the second period.

[Table 3]

The sample related to the estimation of equation (2) has 2,868,785 new auto loans, 440 financial institutions and 2,769,042 borrowers, after the removal of outliers and observations with missing data. From those, only 25,634 borrowers have taken out at least two auto loans from the same financial institution during the sample period. For 7,171 borrowers, loans were taken out only before the new regulation; for 5,625 borrowers, loans were taken out only after the new regulation, and for 13,847 borrowers, at least one loan was taken out before and one after the new regulation. Of those last borrowers, 9,276 borrowers have taken out the same type of loan both before and after the new regulation, whereas 2,765 borrowers have migrated from untargeted loan(s) to targeted one(s) and 1,823 borrowers have migrated in the opposite direction after the modification of the regulation. There are 155 financial institutions that have granted at least two loans to the same borrower during the sample period.

The characterization of this second sample in terms of loan variables is depicted at table 4. Given their respective levels, average loan spread and loan amount are rather stable between the two periods, whereas average maturity and LTV are slightly lower after the regulatory release. The fact that the average spread is not smaller in the second period, in spite of the lower prevailing risk weights, may be related to the higher occurrence of non-performing auto loans in that period. Also, the number of loans is

[30] Problems with missing data are more intensive before the new regulation since there was no requirement then to distinguish loans according to the criteria of table 1.

smaller in the second period, both within the sample that excludes loans with missing data and in total. Contrary to our expectations, the percentage of targeted loans according to table 2 becomes lower after the regulatory capital release.

[Table 4]

The auto-loan sector is highly concentrated with three banks detaining more than three quarters and ten banks detaining around 95% of the number of new loans in our first sample. Those numbers are also valid for both periods before and after the new regulation. Bank concentration is somewhat smaller in the second sample. Four banks detain a bit less than three quarters and ten banks detain around 90% of the number of loans in our second sample. Those numbers are again stable along the periods before and after the regulatory release. Nevertheless, on a more detailed level, the auto loan market share held by each bank, as depicted in out samples, varies along the four periods aforementioned, leading to changes in the ranking of importance of some banks.

5. Results

5.1 The effect of the capital requirement increase

In order to ascertain if the new regulation was a contributing factor for the relative increase in targeted loan spreads, at table 5 we estimate equation (1), adding sequentially different sets of controls. Since borrower characteristics are a key component for any risk analysis underlying pricing decisions, model (1) is estimated with (only) borrower fixed effects. Because our sample does not contain borrower-level variables, fixed effects are the only way to account for borrower characteristics. Model (1) indicates that the spread of a targeted loan increased 3.12 p.p. higher than the spread of an untargeted loan after the regulation, for the same borrower. This is the magnitude of the coefficient of the interaction *New regulation* $\times$ *Loan targeted*, which is significant at the 1% confidence level. The 3.12 p.p. represents a percentage increase of 22.5% to the average spread level prevailing before the new regulation (table 3). Since bank characteristics also play a role on banks' loan pricing policies, bank controls are added in model (2). The coefficient of the interaction changes only slightly and remains highly significant. To control for the fact that borrowers may have taken out loans from different banks after the new regulation, borrower-bank fixed effects are included in model (3). As expected, this reduces our sample significantly. Model (3) indicates that the spread charged from the same bank on the same borrower increased after the new regulation 2.26 p.p. higher for a targeted loan in comparison to an untargeted one. That effect remains significant at 1%. Because loan features are also key determinants of auto loan's riskiness and since some of them may be jointly determined with loan spreads, it is particularly important to examine what happens when loan controls are included in the regression. Model (4) shows that the magnitude of the interaction equals 2.39 p.p., remaining highly significant, while the quality of the adjustment rises substantially (adjusted-R^2). Finally, at model (5) the set of controls is augmented to include time dummies that could capture the influence of the business cycle and secular trends. The coefficient of interaction changes only slightly and remains highly significant. Results are robust when time dummies are replaced by linear trends or when loan controls are added before time controls (not shown).

[Table 5]

The coefficients of *Loan targeted* and of *Lmaturity*, whenever present at table 5, are always negative and significant. The reason may be demand-driven: more expensive loans could be associated to a lower preference for longer maturities and, therefore, targeted loans. Alternatively, the negative signs could also point to a context of a sharp expansion of supply for long-term auto loans before the new regulation. Notice that such expansion was behind the prudential concerns discussed in the introduction.[31] The models of table 5 also display a positive significant sign for *New regulation*, albeit at only 10% level in model (2). That suggests that the spreads of untargeted loans also increased after the new regulation. Model (5) shows this increase equal to 0.78 p.p., therefore around 25% of the total spread increase of targeted loans (3.11 p.p. = 0.78 p.p. + 2.33 p.p.). We examine whether this increase for untargeted loans is robust to controls for further sets of unobserved heterogeneities at tables 6 and 7.

The signs of the (lagged) bank controls, omitted in the interest of space at table 5, are generally consistent with our expectations. We generally find positive significant effects for *Npl, Liquidity, Reserves* and *Roa,* and negative significant signs for *Lassets* and *Capital*. The sign of *Npl* may mean that banks that experience high credit risk levels require compensation in terms of greater spreads. Similarly, a bank with high *Liquidity* or high *Reserves* may choose higher spreads to compensate for the low return of its liquid assets and reserve deposits, explaining the positive signs. On the other hand, the positive sign for *Roa* could have an endogenous reasoning, that is, that higher profitability is generally obtained with the help of higher spreads. As for the negative effect of *Lassets,* larger banks are more diversified, suggesting lower portfolio risk, and may also have better access to funding markets, suggesting lower funding costs. Both of these implications lead to lower spreads. Finally, banks that generally work with higher *Capital* may be perceived as having an improved financial position, leading to lower costs of funding and, at the same time, reduced need to build up financial capital. Both of these implications lead again to lower spreads.[32]

[31] Furthermore, the negative sign for *Lamount* could be explained by a smaller demand for larger loans when spreads are higher. On the other hand, the positive significant sign of *LTV* is consistent with the higher credit risk posed by relatively smaller collaterals.

[32] Notice that this is not in opposition to the positive effect of the *unexpected* regulatory capital increase on loan spreads (the main focus of the paper).

Table 6 examines the robustness of our estimates to sequentially adding further controls for unobserved effects, while maintaining in the regressions the full set of observable variables. Model (3) is our baseline model with borrower-bank fixed effects, identical to model (5) of table 5. Models (1) and (2) have fewer controls for unobserved effects than model (3), whereas models (4) to (6) have more. Model (1) does not control for any unobservable characteristic, in particular, for borrower characteristics. This allows model (1) estimates to be based on the full set of auto loan borrowers, who may have taken out only one loan during the sample period. Notice that the number of observations used in this estimation is almost 40 times the corresponding number of the baseline model. The coefficient of the interaction *New regulation* $\times$ *Loan targeted* is higher than those of table 5, 3.52 p.p., while the coefficient of *New regulation* becomes insignificant. Model (2) has borrower fixed effects and, therefore, its estimates are based on borrowers with multiple auto loans but not necessarily from the same bank. The number of observations is close to three times the corresponding number of the baseline model. The coefficient of the interaction is 2.87 p.p., already closer to that of the baseline model. Baseline model (3) is based on borrowers with multiple auto loans from the same bank and was already commented previously. On its turn, model (4) is estimated based on only those borrowers who have taken out loans before and after the new regulation, from the same bank, in order to reduce concerns with sample selection. This reduces our sample to around half of the baseline sample, but adjusted-R^2 is higher than in model (3). All shown estimates (including the interaction coefficient) are also very close in terms of significancy and magnitude to those of the baseline model, apart from the effect of *New regulation* which becomes insignificant. Therefore, it is not possible to argue, according to model (4), that untargeted loans also experienced higher spreads after the new regulation.

[Table 6]

To help address concerns about the endogeneity of loans controls and of the *Loan targeted* indicator, a matched loan-type approach is adopted in model (5) of table 6. The model only considers (for the same borrower and bank) auto loans that all fit within or without the criteria of table 1[33]. This rules out, for example, borrowers that have migrated from targeted loans before the new regulation (e.g. long maturity and/or high LTV) to untargeted loans after the new regulation (e.g. short maturity and/or low LTV). Therefore, this approach controls, in particular, for the possibility that banks have priced their loans differently according to whether or not borrowers have changed their loan type. Notice that the presence or not of change for each individual borrower may add information to banks about the riskiness of this borrower. Most shown estimates at model (5), including the interaction coefficient, are again close in terms of significancy and magnitude, to those of models (3) and (4), and *New regulation* remains insignificant.

Even after matching loan types, unobserved borrower characteristics may vary along time affecting our results. Therefore, at model (6), matched loans are restricted to be sufficiently close (at most 90 days apart) or, otherwise, not considered for the estimation. With such short distance, the more similar becomes the risk profile of the borrower at the points in time when he takes out new auto loans. The resulting estimation is based on the lowest number of observations of all models of table 6 (more than 7.5 times smaller than in the baseline model) but *New regulation* $\times$ *Loan targeted* remains significant at the 1% confidence level (with a magnitude only slightly smaller than in the previous models). According to model (6), banks have increased 2.19 p.p. the spread charged on the same borrower for auto loans that fit within the criteria of table 1 after the new regulation was introduced. That is still a sizable effect, as the average targeted loan spread increased a parcel of 2.73 p.p. from November 2010 to the next month (figure 1). The 2.19 p.p. spread increase represents a percentage increase of 15.82% to the average spread level prevailing before the new regulation (table 3) and also corresponds to an increase of 0.26

[33] In this case, because fixed effects are employed, the effect of *Loan_targeted* cannot be identified.

p.p. for every additional capital charge of 1%. There was no significant increase for spreads of loans outside the criteria of table 1, according to models (4) to (6). It is also interesting to note that despite the use of increasingly smaller samples in models (4) to (6), adjusted-R^2 higher than in the baseline model are obtained. Results of model (6) are qualitatively similar when larger time distances are used to select loans taken out in neighboring dates.

Because of the potential endogeneity of loan controls, the same models of table 6 are estimated without them at table 7. In the new estimates, the coefficient of the interaction *New regulation* $\times$ *Loan targeted* is always significant at the 1% level, with magnitudes not distant from those of the corresponding models of table 6. Also importantly, as in the previous table, except for model (3), the coefficient of *New regulation* is never significant. It was already insignificant in table 6, except for models (2) and (3). Furthermore, this insignificancy is generally maintained in both tables if the time dummies are replaced by linear trends (results not shown). Therefore, the combined evidence of tables 6 and 7 does not allow us to conclude that the spread of untargeted loans has also increased due to the introduction of the new regulation. Consequently, substitution effects related to the migration of demand from targeted loans to untargeted loans should have been limited. This may suggest that higher-risk borrowers, with a strong preference for long maturities and low LTVs, tend to focus more on their monthly debt-to-income ratios than actually on prices. An insignificant coefficient of *New regulation* is also an indication of an inexistent or limited pass-through of higher bank total financing costs to the set of loans not targeted by the regulation. This suggests that internal capital allocations that serve as input to bank risk-based pricing policies *respect* regulatory capital allocations.

[Table 7]

5.2 Digging into the estimated spread effects

Previous section has discussed, among other estimations, an increase of 2.19 percentage points in credit spreads (model 6 of table 6) for an additional capital charge of 8.25% of bank assets. However, the magnitudes of the estimated βs in tables 6 and 7 do not lead to straightforward comparison with the spread increases due to higher capital forecasted by recent papers on bank capital regulation (e.g. BCBS, 2010a; MAG, 2010; Hanson *et al*., 2010; Miles *et al*., 2013). The main reason is that this recent literature generally gauges the consequences of increases in actual capital and not in capital requirements, as in our case.[34] Therefore, in order to check whether the previous estimations make economic sense, we build upon a simple methodology used in this same literature to develop our own proxies for the expected spread impacts.[35] This would further assure us that the increase on loan spreads estimated in the previous section is indeed driven by higher bank financing costs.

In this section, we try to relate the previously estimated spread increases and bank financing costs by computing the expected magnitude of bank spread reactions based on a simple accounting approach (e.g. BCBS 2010a; Elliott, 2009). More specifically, the accounting approach assumes that, following the increase in risk weights for targeted auto loans, banks work towards maintaining their capital ratios (capital over risk-weighted assets) unaltered. This requires higher actual capital in the new equilibrium. The new capital, obtained for example through retained earnings or issuances of new equity, is further assumed to replace bank debt so that the size of total assets remains constant.

[34] In this literature, the capital increase is usually assumed homogenous across the whole portfolio, and not applied to just a part of it, although this might not constitute an obstacle to simple comparison as proposition 1 ahead reveals. This literature also generally refers to an increase of 1% in capital over risk weighted assets while ours deals with a corresponding increase of 75%. To the extent that the transmission mechanism is linear, that difference would not constitute an obstacle to simple comparison either.

[35] Note, as discussed in the introduction, that the papers that do study variations in capital requirements (e.g. Aiyar et al., 2014; Jimenez et al., 2013) examine their consequences on the amount of credit supply and not on its price. So their results are difficult to compare to ours.

Because the costs of bank capital and of bank debt are also assumed constant and capital is more expensive than debt, the financing costs of banks increase. Banks are assumed to entirely offset this cost increase by charging larger spreads on loans targeted by the new regulation (consistent with results of the previous section). Under those assumptions the following result ensues.

Proposition 1: If a set of targeted loans suffers a risk weight increase of p% while the capital over risk weighted assets ratio (cr), the return on capital before taxes roe, the cost of debt r, the size of total assets A, the amount of targeted loans T and the spread charged on untargeted loans remain all constant[36], then the increase in spreads on targeted loans is given by:

Δspread = p × cr × (roe –r)

Proof: see appendix.

Notice that the increase in spreads does not depend on the proportion of targeted loans to total loans. Because the increase in spreads is only applied to targeted loans, it is smaller the larger the amount of targeted loans. At the same time, the spread increase is larger the greater the growth of bank capital, which is, on its turn, proportional to the amount of targeted loans. Therefore, the two effects cancel out.

Although proposition 1 is based on rather simplifying assumptions, it is useful to provide an expected order of magnitude for the increase in targeted loan spreads. Making use of the data encompassing one year before the introduction of the new regulation, the average cr, roe (before taxes) and r of the of Brazilian banking system are estimated as

[36] A spread increase and T constant could hold simultaneously if the loan demand is perfectly inelastic.

14%, 23.1% and 8.3%, respectively[37]. These figures, together with p=75% (risk weight changing from 75% to 150%) results into Δspread= 1.6 p.p. for the Brazilian auto loan case, according to proposition 1. Compared, for example, to the previous referred estimation of 2.19 p.p. (model 6 of table 6), the forecasting error is 0.59 p.p., which one can show to be basically the same magnitude of the standard deviation of the monthly time series of average auto loan spreads before the new regulation (figure 1). The fact that proposition 1 forecasts a magnitude consistent with our previous estimations further points to the economic sense of our empirical results.

A more interesting use of proposition 1 is to apply it on a bank level but, in this case, one should be wary that its assumptions may be simply too unrealistic for some banks. Banks whose capital ratios were very high before the new regulation are not likely to have been constrained by capital requirements in the past and therefore may accept a decrease in their ratios as a result of higher risk weights. Public banks may act upon the belief that a smaller roe after the new regulation may be accepted (or even stimulated as part of a more general social policy) by the government shareholder, and therefore calibrate their spreads accordingly. Finally, in deciding their spread reactions, subsidiaries of foreign banks could take into account not only their domestic bank variables but also the capital ratio, roe and r of the consolidated holding company in ways not so easy to assess.[38] In the remainder of this section, proposition 1 is applied on a bank level only to private national banks with non-excessive capital ratios[39].

The goal of our bank-level analysis is to rank banks according to their proposition-implied Δspread figures and then compare this ranking with the coefficients β estimated

[37] Development banks and other specific type of banks are excluded from the underlying data that supports those estimates.
[38] Also, banks who were already involved in atypical situations before the new regulation, such as insolvency problems, decision to withdraw from the auto loan market, may have their reactions even more difficult to assess.
[39] Capital ratio above 15% is considered excessive. The Brazilian regulatory minimum is 11%.

for each bank separately.[40] For that purpose, the models of tables 6 and 7 are re-estimated for restricted samples of auto loans granted by individual banks. The estimation is conducted for each of three largest banks in our sample, which comprise together more than three quarters of the number of loans. The sufficient number of loans pertained to each one of these three banks allows the re-estimations of the last models of tables 6 and 7 to attain significancy in the parameters of interest. Furthermore, these three banks happen to be private national banks with non-excessive capital ratios, which allows a less problematic application of proposition 1, as discussed before. The three banks are ordered according to their proposition-implied Δspread (bank 3 has the greatest Δspread). The resulting estimated coefficients β and α of banks 1 to 3 are shown in tables 8 (with loan controls) and 9 (without loan controls). The Δspread figures of each bank are shown below the bank labels. The interaction coefficient β is highly significant for each bank and each model. On the other hand, coefficient α becomes insignificant once a sufficient set of controls for unobservables is applied (from model 4 onwards for banks 1 and 2 or from model 2 onwards for bank 3). These results are consistent with those of the previous section. More important, however, is to note that the magnitude of β increases from bank 1 to bank 3 in every model of tables 8 and 9, but the first. Therefore, in the universe of the largest banks in our sample, larger capital ratios and/or more expensive costs of capital relative to debt generally imply a higher relative spread increase of targeted loans after the new regulation. Furthermore, a closer inspection on the estimated bank β's reveals still that their relation (across banks) to the expected impact figures (Δspread) is close to linear in every model, except for the models 1 and 6.[41] Overall, the cross-section analysis allowed by tables 8 and 9 reinforces the thesis that the observed increase on targeted loan spreads is driven by higher bank financing costs.

[40] Because of the rather simplifying assumptions underlying the proposition, it is unlikely to be useful in forecasting the magnitude of spread increases on a bank-level.

[41] In model 1, β diminishes from bank 2 to bank 3, whereas in model 6, β growth pattern from bank 1 to bank 3 seems to be more of a quadratic form.

[Table 8]

[Table 9]

5.3 The effect of the regulatory capital release

The effect of the regulatory capital release that decreased risk weights of auto loans satisfying the criteria of table 2 is investigated through the estimations of equation (2) contained in table 10. Estimations are carried out sequentially adding controls for unobserved effects in the same manner as in table 6.[42] The numbers of observations employed by models of table 10 are generally somewhat smaller than those of the corresponding models at table 6 because of the slightly shorter time period covered. We find in all models but (1) and (2) that banks charged significantly smaller spreads after the regulatory release on their auto loans whose capital requirements decreased (table 2), comparatively to untargeted loans. In fact, the coefficient of *Regulatory release* $\times$ *Loan targeted* is significant at the 1% level in these models). According to the baseline model (3), the spread charged by the same bank on the same borrower did not varied significantly for an untargeted loan and decreased 0.46 p.p. for a loan targeted by the regulatory release. That is the lowest highly significant estimate of the interaction coefficient in table 10. Also importantly, the absolute magnitudes of the *Regulatory release* $\times$ *Loan targeted* at table 10 are rather smaller (at most 35%) than the magnitudes of the coefficient of *New regulation* $\times$ *Loan targeted* in the corresponding models of table 6. Therefore, when the impacts are measured locally near the policies, the cancelation of the previous capital requirement increase had a smaller impact on spreads than the original capital increase. This could be related to a more precautionary behavior adopted by banks as a result of market pressures and of the signaling effect of the original regulatory within-sector capital measure. In that sense, the absence of a response of the

[42] When there is no migration and borrower fixed effects are employed, the effects of *Loan_targeted(table_1)* and *Loan_targeted (table 2)* cannot be identified.

same magnitude when previous capital requirement increase is withdrawn may indeed contain evidence that the original increase was effective beyond the introduction of the higher requirement itself.

[Table 10]

The second lowest and still highly significant estimate for *Regulatory release* $\times$ *Loan targeted* is obtained by model (6).[43] According to this model, after the regulatory capital release, banks have decreased 0.65 p.p. the spread charged on the same borrower for auto loans that fit within the criteria of table 2. That is less than 30% of the previously referred estimation of 2.19 p.p. of model (6) of table (6) for the spread increase as of the introduction of the new regulation. The 0.65 p.p. spread decrease also represents a percentage decrease of just 3.75% from the average spread level prevailing before the regulatory change (table 4).

The same models of table 10 are estimated without loan controls at table 11. The significance levels (and the magnitudes) of the interaction coefficient are similar to those of the corresponding models of table 10.Additionally, the coefficient of *Regulatory release* is insignificant in almost all models of tables 10 and 11.[44] As a result, there is no strong evidence that auto loans that continued to receive the same risk weight have been charged different or lower spreads. This result also indirectly informs that substitution effects related to migration of demand away from untargeted loans to loans whose risk weights have decreased may have been limited with regard to the regulatory capital release. Similarly, there is no strong evidence that banks have partially passed through to untargeted loans the reduction of total financing costs derived from the capital requirement decrease.

[43] It is interesting to note that the behavior of the magnitude of the interaction coefficient across models (1) to (6) tends in general to display an ascending pattern at table 10, in contrast to a general descending pattern in table 6.

[44] And this insignificancy is generally maintained in both tables if the time dummies are replaced by linear trends (results not shown)

[Table 11]

6. Conclusion

This paper investigates the consequences on loan spreads of a within-sector macro prudential capital-based measure in Brazil. Due to concerns related to a possibly too fast and unbalanced expansion of the auto-loan sector, regulatory capital requirements were raised (8.25% additional capital charge) for auto-loans with specific long maturities or high LTVs. This paper shows that banks, after the new regulation, increased spreads charged on the same borrower for auto loans targeted by the regulatory capital increase. In comparison to the set of untargeted loans, the increase was, on the bank average, at least of 2.19 p.p. or, in percentage terms, around 15%. This result is highly statistically significant and robust to a variety of controls for unobserved heterogeneities and to sub-sample estimations. On the other hand, evidence on the increase of spreads charged for the set of untargeted auto loans is not strong, given the lack of robustness. In theory, spillovers to the set of untargeted auto loans could be caused by partial pass-through of higher total financing costs also to these loans (supply effect) and/or by migration of demand from targeted loans to untargeted ones (demand effect). We conclude, therefore, that those spillovers, if present, have been limited. This indirectly suggests that, with regard to supply effects, banks respected regulatory capital allocations in their pricing policies and, with regard to demand effects, high risk borrowers may have focused more on their monthly capacity to service their debt rather than on interest rates. Also, this paper shows that, in the universe of the largest banks in the auto loan sector, the spread increase on targeted loans was higher the more expensive the bank cost of capital relative to debt and/or the larger the bank capital ratio. Finally, this paper reveals that the later withdrawal of the regulatory capital increase was, similarly, associated to lower spreads charged on the same borrower for auto loans whose capital charges decreased.

Nevertheless, this reduction in spreads was smaller than the original increase. This could be associated to a more precautionary behavior adopted by banks after the introduction of the macro prudential policy and that lasted beyond the regulatory capital release.

The results of this paper are important in connection to the implementation of Basel III. In particular, the adopted macro-prudential measure is similar to the functioning of the yet-to-be implemented countercyclical capital buffers. Indeed, Brazilian banks were given approximately seven months after the announcement of the capital increase to cope with the new requirements, whereas the capital release produced effects immediately. Both characteristics are in close proximity to the functioning of the future Basel III countercyclical capital buffers. On the other hand, the extension to which the results for auto-loans generalize to other credit sectors or other countries is an open empirical question.

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8. Appendix: figures and tables

Figure 3: Credit to new auto loans (R$ bill)

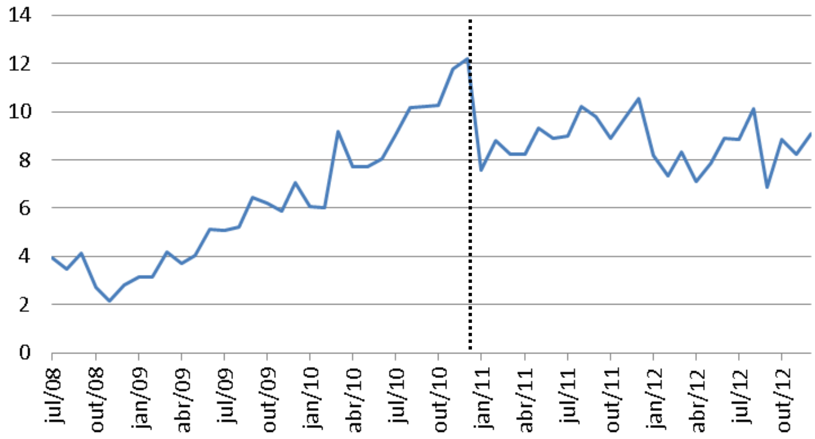

Figure 4: New auto loans by maturity (share - %)

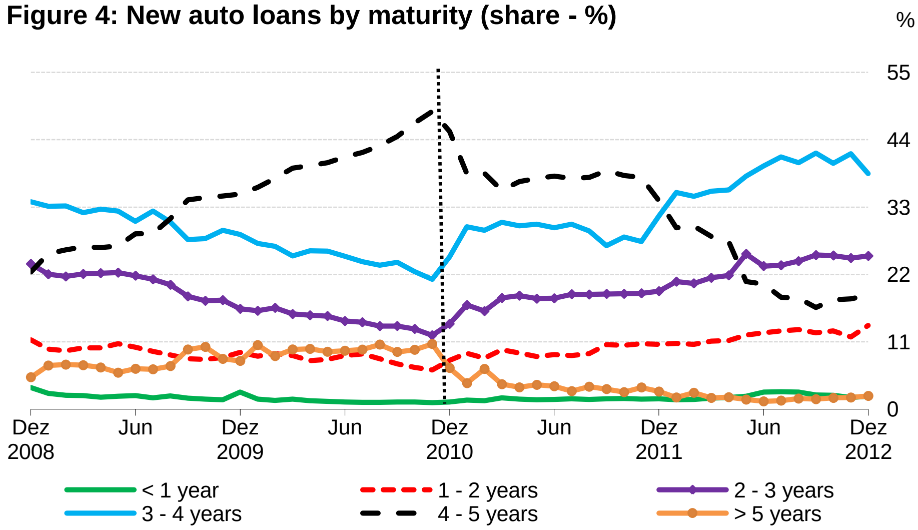

Figure 5: New auto loans by LTV (share - %)

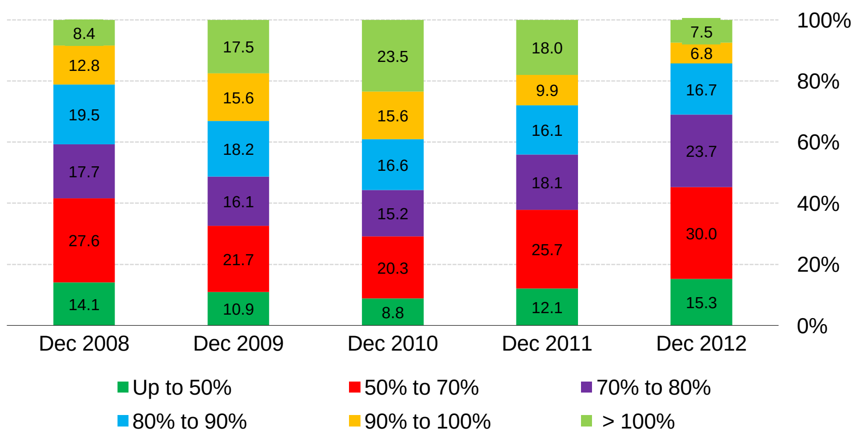

Figure 6: Loan Spread (monthly average - %)

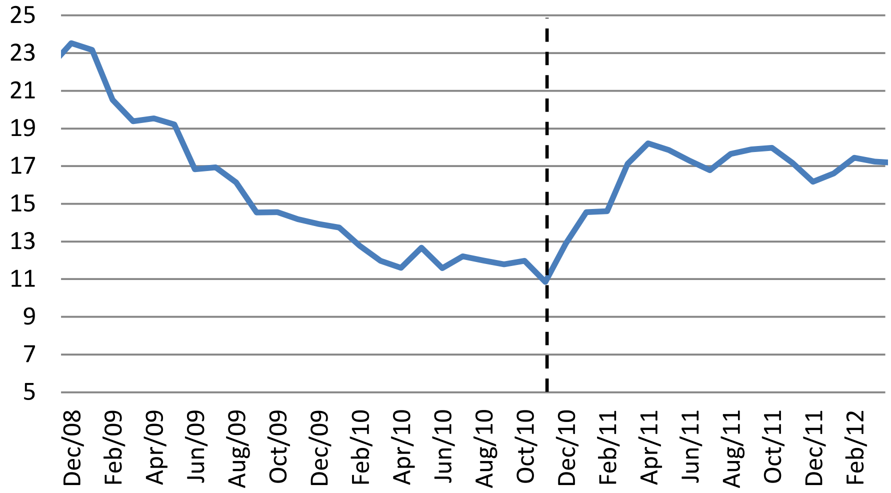

Figure 7: Proportion of loans non-performing one year ahead from those granted in current month - targeted x untargeted loans

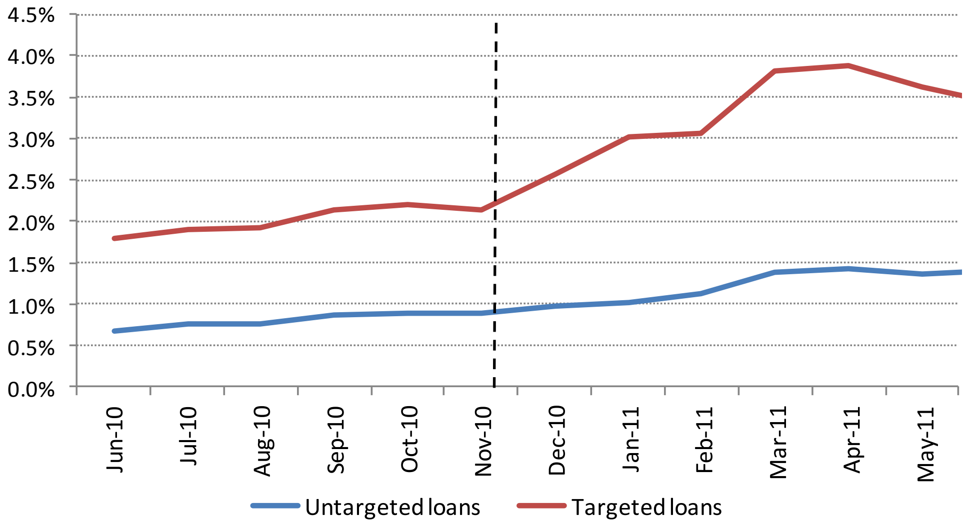

Figure 8: Loan spread (monthly average - %)

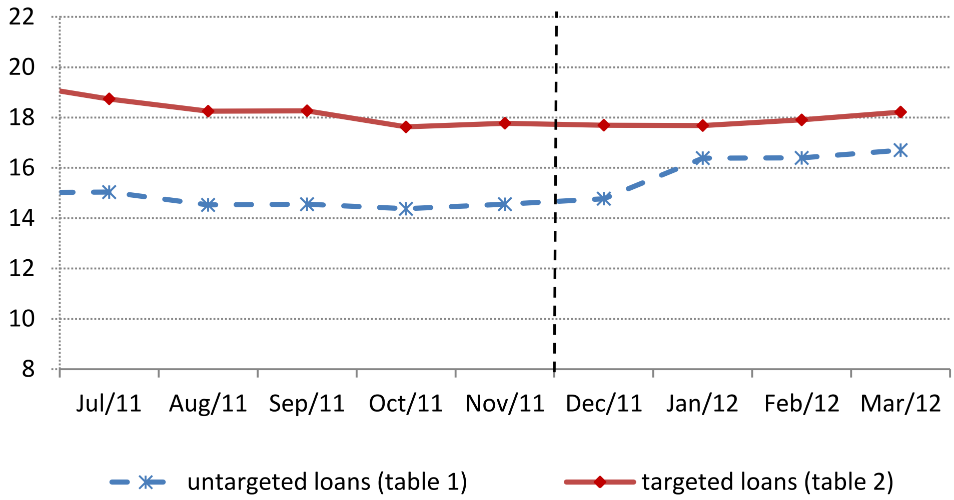

Table 3: Loan descriptive statistics: sample used in models for equation 1

Loan variables	Before new regulation	After new regulation	Difference	t-statistic
Loan spread (annual rate)	13.84	18.05	4.21***	440
Amount (R$)	21,808	21,021	-787***	-38.44
Maturity (months)	48.74	47.04	-1.70***	119.71
LTV	0.85	0.81	-0.04***	-111.01
Targeted loans of table 1 (%)	71.62	65.28	-6.34***	-118.74
Number of sample loans	1,494,760	1,522,627	27,867	
Total number of loans	2,200,116	1,909,206	-290,910	

Table 4: Loan descriptive statistics: sample used in models for equation 2

Loan variables	Before regulatory release	After regulatory release	Difference	t-statistic
Loan spread (annual rate)	17.34	17.52	0.17***	15.85
Amount (R$)	21,144	21,509	365***	17.99
Maturity (months)	46.45	45.02	-1.43***	-98.22
LTV	0.8	0.77	-0.03***	-78.69
Targeted loans of table 2 (%)	61.59	55.61	-5.98***	102.82
Number of sample loans	1,523,426	1,345,359	-178067	
Total number of loans	1,540,920	1,362,174	-178746	

Table 5: Models for equation (1)

Dependent variable: Loan_spread	(1)	(2)	(3)	(4)	(5)
Loan targeted$_1$	-1.7934***	-1.7604***	-1.4148***	-2.1690***	-2.1041***
	(0.0652)	(0.0681)	(0.0982)	(0.1208)	(0.1194)
New regulation$_t$	1.5806***	1.2798***	1.0124***	1.1124***	0.7819***
	(0.0642)	(0.0838)	(0.1449)	(0.1333)	(0.2237)
New regulation$_t$ x Loan targeted$_1$	3.1176***	3.0770***	2.2579***	2.3890***	2.3263***
	(0.0820)	(0.0838)	(0.1190)	(0.1095)	(0.1080)
Lamount				-2.9219***	-2.9318***
				(0.0738)	(0.0730)
Lmaturity				-3.0192***	-3.0943***
				(0.1633)	(0.1617)
LTV				11.1838***	11.3562***
				(0.2997)	(0.2977)
Borrower fixed effects	Yes	Yes	-	-	-
Borrower-bank fixed effects	-	-	Yes	Yes	Yes
Bank controls	No	Yes	Yes	Yes	Yes
Loan controls	No	No	No	Yes	Yes
Time controls	No	No	No	No	Yes
Number of observations	221,561	201,008	70,017	70,017	70,017
R^2 (adj)	0.0808	0.1771	0.0916	0.2775	0.2975

Note: *, ** and *** indicate coefficients statistically significant at 10%, 5% and 1%, respectively. Robust standard errors are in brackets.

Loan targeted$_1$ = Loan targeted (table 1)

Table 6: Robustness on models for equation (1)

Dependent variable: Loan_spread	(1)[1]	(2) [2]	(3)	(4)	(5)	(6)
Loan targeted$_1$	-3.6445***	-2.8392***	-2.1041***	-2.3126***		
	(0.6401)	(0.0777)	(0.1194)	(0.1590)		
New regulation$_t$	0.2889	0.3817***	0.7819***	0.2715	0.1538	0.1155
	(0.4071)	(0.1331)	(0.2237)	(0.3431)	(0.4310)	(0.5401)
New regulation$_t$ x Loan targeted$_1$	3.5236***	2.8742***	2.3262***	2.3945***	2.3279***	2.1941***
	(0.3521)	(0.0720)	(0.1080)	(0.1243)	(0.1330)	(0.2008)
Lamount	-5.1858***	-3.4862***	-2.9318***	-2.6067***	-2.7564***	-2.9532***
	(0.2566)	(0.0420)	(0.0730)	(0.1027)	(0.1284)	(0.1933)
Lmaturity	-2.9913***	-3.8182***	-3.0943***	-3.0254***	-2.9447***	-2.5677***
	(1.0333)	(0.1062)	(0.1617)	(0.2083)	(0.2758)	(0.4226)
LTV	18.4962***	13.5840***	11.3562***	11.8598***	12.3913***	11.4875***
	(1.3469)	(0.1594)	(0.2977)	(0.3964)	(0.5105)	(0.7878)
Fixed effects	No	borrower	borrower-bank	borrower-bank	borrower-bank	borrower-bank
Before and after new regulation	No	No	No	Yes	Yes	Yes
Matched by loan type (no migration)	No	No	No	No	Yes	Yes
Short distance between matched loans	No	No	No	No	No	Yes
Bank controls	Yes	Yes	Yes	Yes	Yes	Yes
Time controls	Yes	Yes	Yes	Yes	Yes	Yes
Loan controls	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	2,746,173	200,860	70,017	37,020	23,305	9,097
R^2 (adj)	0.5804	0.4986	0.2975	0.3293	0.3657	0.3381

Note: *, ** and *** indicates statistically significance at the 10%, 5% and 1% level, respectively. Robust standard errors are in brackets.

[1] Standard errors adjusted for 101 clusters in bank. [2] Bank dummies are also included.

Loan targeted$_1$ = Loan targeted (table 1)

Table 7: Robustness on models for equation (1)

Dependent variable: Loan_spread	(1)[1]	(2)[2]	(3)	(4)	(5)	(6)
Loan targeted$_1$	0.6110	-1.6005***	-1.3353***	-1.2552***		
	(0.6672)	(0.0662)	(0.0975)	(0.1312)		
New regulation$_t$	-0.1657	0.1409	0.6999***	0.0278	-0.1052	-0.1689
	(0.4381)	(0.1550)	(0.2560)	(0.3958)	(0.5033)	(0.6150)
New regulation$_t$ x Loan targeted$_1$	3.9418***	3.0933***	2.1982***	2.1402***	2.0521***	2.1151***
	(0.2769)	(0.0809)	(0.1179)	(0.1344)	(0.1405)	(0.2121)
Fixed effects	No	borrow er	borrow er-bank	borrow er-bank	borrow er-bank	borrow er-bank
Before and after new regulation	No	No	No	Yes	Yes	Yes
Matched by loan type (no migration)	No	No	No	No	Yes	Yes
Short distance betw een matched loans	No	No	No	No	No	Yes
Bank controls	Yes	Yes	Yes	Yes	Yes	Yes
Time controls	Yes	Yes	Yes	Yes	Yes	Yes
Loan controls	No	No	No	No	No	No
Number of observations	2,746,173	200,860	70,017	37,020	23,305	9,097
R^2 (adj)	0.2236	0.2461	0.1079	0.1605	0.1934	0.1663

Note: *, ** and *** indicates statistically significance at the 10%, 5% and 1% level, respectively. Robust standard errors are in brackets.

[1] Standard errors adjusted for 101 clusters in bank. [2] Bank dummies are also included.

Loan targeted$_1$ = Loan targeted (table 1)

Table 8: Models by bank for equation (1)

Dependent variable: Loan_spread		(1)[1]	(2)[2]	(3)	(4)	(5)	(6)
New regulation$_t$							
	Bank 1	1.1649***	1.2885***	1.2764***	0.7999	0.1649	-0.2189
		(0.0427)	(0.3250)	(0.3249)	(0.5193)	(0.6830)	(0.8401)
		[1,248,902]	[87,846]	[42,725]	[22,543]	[13,845]	[5,373]
	Bank 2	0.3358***	1.0069***	0.8262**	0.3037	0.7080	0.4649
		(0.0436)	(0.3233)	(0.3345)	(0.5208)	(0.7346)	(0.8889)
		[603,859]	[49,883]	[16,471]	[9,045]	[5,799]	[2,207]
	Bank 3	-0.9821***	-0.5679	-0.1681	0.0254	-1.9122	-1.9421
		(0.0493)	(0.9556)	(1.0200)	(1.5021)	(1.2207)	(1.4270)
		[358,677]	[21,975]	[2,278]	[1,093]	[815]	[411]
New regulation$_t$ x Loan targeted$_1$							
	Bank 1	3.0129***	1.5112***	1.5230***	1.5593***	1.5042***	1.4047***
	(Δspread =0.82)	(0.0223)	(0.1493)	(0.1493)	(0.1757)	(0.1887)	(0.2915)
		[1,248,902]	[87,846]	[42,725]	[22,543]	[13,845]	[5,373]
	Bank 2	4.5746***	2.8075***	2.8569***	2.8628***	2.8414***	2.2043***
	(Δspread =1.65)	(0.0265)	(0.1664)	(0.1694)	(0.1946)	(0.2047)	(0.2876)
		[603,859]	[49,883]	[16,471]	[9,045]	[5,799]	[2,207]
	Bank 3	4.3276***	4.2937***	4.1330***	4.4335***	4.7050***	5.0664***
	(Δspread = 2.00)	(0.0363)	(0.5905)	(0.6605)	(0.7352)	(0.7328)	(0.9375)
		[358,677]	[21,975]	[2,278]	[1,093]	[815]	[411]
Fixed effects		No	borrower	borrower-bank	borrower-bank	borrower-bank	borrower-bank
Before and after new regulation		No	No	No	Yes	Yes	Yes
Matched by loan type (no migration)		No	No	No	No	Yes	Yes
Short distance between matched loans		No	No	No	No	No	No
Bank controls		Yes	Yes	Yes	Yes	Yes	Yes
Time controls		Yes	Yes	Yes	Yes	Yes	Yes
Loan controls		Yes	Yes	Yes	Yes	Yes	Yes

Note: *, ** and *** indicates statistically significance at the 10%, 5% and 1% level, respectively. Robust standard errors are in parentheses. Number of observations are in brackets.
[1]Standard errors adjusted for 101 clusters in bank. [2]Bank dummies are also included.
Loan targeted$_1$ = Loan targeted (table 1)

Table 9: Models by bank for equation (1)

Dependent variable: Loan_spread		(1)[1]	(2) [2]	(3)	(4)	(5)	(6)
New regulation$_t$							
	Bank 1	0.5988***	1.0014***	0.9924***	0.2729	-0.4441	-0.5641
		(0.0575)	(0.3629)	(0.3628)	(0.5770)	(0.7622)	(0.9352)
		[1,248,902]	[87,846]	[42,725]	[22,543]	[13,845]	[5,373]
	Bank 2	0.3621***	1.0884***	0.7475*	0.1007	0.8481	0.4745
		(0.0628)	(0.3963)	(0.4088)	(0.6162)	(0.8757)	(1.0126)
		[603,859]	[49,883]	[16,471]	[9,045]	[5,799]	[2,207]
	Bank 3	-0.9591***	-1.0745	-0.6331	0.8610	-1.0200	-1.4091
		(0.0756)	(1.2674)	(1.3899)	(2.4356)	(1.6819)	(1.9117)
		[358,677]	[21,975]	[2,278]	[1,093]	[815]	[411]
New regulation$_t$ x Loan targeted$_1$							
	Bank 1	3.5146***	1.4174***	1.4256***	1.3441***	1.2328***	1.3393***
	(Δspread =0.82)	(0.0274)	(0.1610)	(0.1610)	(0.1864)	(0.1950)	(0.3007)
		[1,248,902]	[87,846]	[42,725]	[22,543]	[13,845]	[5,373]
	Bank 2	4.4752***	2.8092***	2.8147***	2.6377***	2.6739***	2.2511***
	(Δspread =1.65)	(0.0344)	(0.1914)	(0.1964)	(0.2240)	(0.2371)	(0.3428)
		[603,859]	[49,883]	[16,471]	[9,045]	[5,799]	[2,207]
	Bank 3	3.7111***	4.3102***	4.3164***	5.2614***	5.0018***	5.3543***
	(Δspread = 2.00)	(0.0500)	(0.7880)	(0.8503)	(0.9235)	(0.7949)	(1.1327)
		[358,677]	[21,975]	[2,278]	[1,093]	[815]	[411]
Fixed effects		No	borrow er	borrow er-bank	borrow er-bank	borrow er-bank	borrow er-bank
Before and after new regulation		No	No	No	Yes	Yes	Yes
Matched by loan type (no migration)		No	No	No	No	Yes	Yes
Short distance betw een matched loans		No	No	No	No	No	Yes
Bank controls		Yes	Yes	Yes	Yes	Yes	Yes
Time controls		Yes	Yes	Yes	Yes	Yes	Yes
Loan controls		No	No	No	No	No	No

Note: *, ** and *** indicates statistically significance at the 10%, 5% and 1% level, respectively. Robust standard errors are in brackets. Number of observations are in brackets.

[1]Standard errors adjusted for 101 clusters in bank. [2]Bank dummies are also included.

Loan targeted$_1$ = Loan targeted (table 1)

Table 10: Models for equation (2)

Dependent variable: Loan_spread	(1)[1]	(2)[2]	(3)	(4)	(5)	(6)
Loan targeted (table 1)	-1.1535***	-0.7351***	-0.6505**	-0.5794		
	(0.6452)	(0.1546)	(0.2696)	(0.3794)		
Loan targeted (table 2)	-0.4087	-0.4176**	-0.0540	-0.0947	-0.0004	0.2188
	(0.5495)	-0.1462	(0.2574)	(0.3641)	(0.4225)	(0.5173)
Regulatory release$_t$	0.0560	-0.0331	-0.0059	0.3132	0.5460	0.4554
	(0.1915)	(0.1195)	(0.2016)	(0.2817)	(0.3459)	(0.3630)
Regulatory release$_t$ x Loan targeted (table 2)	-0.4191	-0.0893	-0.4582***	-0.7157***	-0.8215***	-0.6486***
	(0.3725)	(0.0737)	(0.1253)	(0.1480)	(0.1560)	(0.1903)
Lamount	-4.7305***	-3.1692***	-3.6687***	-3.6855***	-3.5465***	-3.6504***
	(0.3841)	(0.0469)	(0.0898)	(0.1286)	(0.1662)	(0.2048)
Lmaturity	-3.6578***	-5.0705***	-4.4536***	-4.4491***	-4.5471***	-4.7079***
	(0.9124)	(0.1085)	(0.1946)	(0.2589)	(0.3429)	(0.4189)
LTV	19.1458***	15.3315***	15.1285***	15.1881***	14.6573***	15.0589***
	(1.9072)	(0.1519)	(0.2883)	(0.3963)	(0.5192)	(0.6741)
Fixed effects	No	borrow er	borrow er-bank	borrow er-bank	borrow er-bank	borrow er-bank
Before and after regulatory release	No	No	No	Yes	Yes	Yes
Matched by loan type (no migration)	No	No	No	No	Yes	Yes
Short distance betw een matched loans	No	No	No	No	No	Yes
Bank controls	Yes	Yes	Yes	Yes	Yes	Yes
Time controls	Yes	Yes	Yes	Yes	Yes	Yes
Loan controls	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	2,660,465	178,170	50,120	26,380	16,505	10,828
R^2 (adj)	0.5277	0.4731	0.3182	0.3243	0.3051	0.3122

Note: *, ** and *** indicates statistically significance at the 10%, 5% and 1% level, respectively. Robust standard errors are in brackets.
[1]Standard errors adjusted for 101 clusters in bank. [2]Bank dummies are also included.

Table 11: Models for equation (2)

Dependent variable: Loan_spread	(1)[1]	(2)[2]	(3)	(4)	(5)	(6)
Loan targeted (table 1)	-0.5956	-1.2480***	-1.3780***	-1.4996***		
	(0.5511)	(0.1856)	(0.3311)	(0.4746)		
Loan targeted (table 2)	4.1356***	1.9871***	2.0197***	2.1823***	1.8037***	2.2490***
	(0.4557)	-0.1849	(0.3318)	(0.4719)	(0.5487)	(0.6993)
Regulatory release$_t$	0.1699	-0.2896**	-0.2899	-0.2560	0.2143	0.1742
	(0.2169)	(0.1419)	(0.2379)	(0.3394)	(0.4096)	(0.4274)
Regulatorty release$_t$ x Loan targeted (table 2)	-0.7507	0.1365	-0.4844***	-0.7214***	-0.7849***	-0.6200***
	(0.3129)	(0.0881)	(0.1507)	(0.1765)	(0.1857)	(0.2270)
Fixed effects	No	borrow er	borrow er-bank	borrow er-bank	borrow er-bank	borrow er-bank
Before and after regulatory release	No	No	No	Yes	Yes	Yes
Matched by loan type (no migration)	No	No	No	No	Yes	Yes
Short distance betw een matched loans	No	No	No	No	No	Yes
Bank controls	Yes	Yes	Yes	Yes	Yes	Yes
Time controls	Yes	Yes	Yes	Yes	Yes	Yes
Loan controls	No	No	No	No	No	No
Number of observations	2,660,465	178,170	50,120	26,380	16,505	10,828
R^2 (adj)	0.1955	0.2083	0.0153	0.0229	0.0219	0.0218

Note: *, ** and *** indicates statistically significance at the 10%, 5% and 1% level, respectively. Robust standard errors are in brackets.

[1]Standard errors adjusted for 101 clusters in bank. [2]Bank dummies are also included.

Proof of proposition 1:

We use the following notations:

$CR \equiv C/RWA$, where C is the actual bank capital and RWA is the risk-weighted assets of the bank

$ROE \equiv (spread \cdot T + R - r \cdot D)/C$, where T is the total amount of targeted loans, r is the average cost of bank debt, $D \equiv A - C$ is the size of the bank debts and R comprises all bank revenues apart from the interest income of the set of targeted loans (shown above as spread · T) minus the all bank operational expenses.

Throughout the following, the variables with subscript “1” refer to the period before the introduction of the new regulation and the variables with subscript “2” refer to the period after the new regulation.

Let us first compute what is the new actual bank capital required given the higher capital requirement:

By assumption $T_1 = T_2 \equiv T$, $C_1/RWA_1 = C_2/RWA_2$ and $RWA_2 = RWA_1 + p \cdot T$

Then $C = (RWA_1 + p \cdot T) \cdot C_1 / RWA_1$ so that $\Delta C \equiv C_2 - C_1 = p \cdot T \cdot C_1/ RWA_1$ (1)

ΔC is the increase in actual capital required after the new regulation.

Let us now compute the spread increase in order to maintain ROE unaltered:

By assumption $R_1 = R_2 \equiv R$, $r_1 = r_2 \equiv r$, $ROE_1 = ROE_2 \equiv ROE$ and $D_2 = D_1 - \Delta C$ (because A is constant)

Expressing spread as a function of ROE , we have:

$spread_1 = (ROE \cdot C_1 - R + r \cdot D_1) / T$ and $spread_2 = (ROE \cdot (C_1 + \Delta C) - R + r \cdot (D_1 - \Delta C)) / T$

Then $\Delta spread \equiv spread_2 - spread_1 = \Delta C \cdot (ROE - r) / T$ (2)

Δspread is the expected increase in spreads on targeted loans based on the assumptions of the proposition.

Using (1) and (2), we have $\Delta spread = p \cdot T \cdot C_1/ RWA_1 \cdot (ROE - r) / T = p \cdot CR \cdot (ROE - r)$ q.e.d.