# Borrowing Beyond Bounds: How Banks Pass On Regulatory Compliance Costs<sup>\*</sup>

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#### Abstract

Banks in the euro area must inform supervisors about exposures to individual counterparties that exceed 10% of the bank's capital. Using a granular dataset that combines banks' loan and security exposures, we test whether banks pass on the cost of complying with the large-exposure framework to borrowers above the threshold. We show that after a lowering of the reporting threshold, small banks react by shifting more exposures just below the threshold. Moreover, they charge a sizable 67 basis point interest rate premium for large exposures, relative to firms just below the threshold. This premium is more pronounced for borrowers with fewer banking relationships and hence fewer outside options. In response, when firms approach their bank's large exposure threshold, they become more likely to borrow from other banks. Despite the "large-exposure penalty", we find no statistical evidence for bunching below the threshold, suggesting that there are substantial frictions that prevent firms from switching to higher-capital banks to reduce interest expenses.

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## 1 Introduction

Regulatory reforms usually impose (explicit or implicit) private costs on some parts of the economy that must be weighed against the expected social benefits. Part of the problem that regulators face is that regulated institutions might try to evade these costs, e.g., by passing them on to consumers, suppliers, competitors, or the like. To avoid unintended consequences, it is therefore important for policymakers to assess the potential of such "regulatory leakage" ex ante.

In this paper, we attempt to find out whether banks pass on the costs of large-exposure (LEX) regulation to their corporate borrowers. Following guidelines set by the Basel Committee on Banking Supervision (BCBS), banks must report their largest clients to the supervisory authority. More precisely, all exposures to individual counterparties (or groups of connected counterparties) that exceed 10% of the bank's Tier 1 capital must be reported. If reporting is costly for banks and they can pass on the cost to their borrowers, one could expect to observe higher interest rates (and/or higher fees, stricter covenants, etc.) on loans above the threshold than on those that stay below it. Furthermore, banks could strategically maintain exposures to single clients below the threshold, which would result in "bunching" of exposures.

Although LEX regulation is pervasive across the developed world, we are among the first researchers to study its effects on bank- and firm-level outcomes empirically.<sup>1</sup> Our analysis reveals that small banks are particularly sensitive to LEX reporting requirements. After a reform that lowered the reporting threshold, they strategically shift exposures below the critical value. In addition, they charge an interest premium of approximately 67 basis points relative to firms just below the threshold. A look at the cross-section of firms reveals that this premium arises among firms with a number of banking relationships below the median, i.e., those who have fewer alternative creditors. Our preferred interpretation is therefore that banks with particularly high compliance costs (i.e., smaller banks) pass these costs on to borrowers with the weakest bargaining position.<sup>2</sup>

Whether reporting large clients is costly for banks in the first place is not obvious. After all, in the digital age, many compliance tasks can be automated, so a mere reporting requirement might not impose large administrative costs on banks, at least at the margin (i.e., once an automation rule has been implemented). Moreover, since central banks and other supervisory

<sup>&</sup>lt;sup>1</sup>According to a survey by the International Monetary Fund in 2013, 86 out of 97 surveyed countries have imposed limits on large exposures (IMF (2013)).

<sup>&</sup>lt;sup>2</sup>The European Banking Authority (EBA) acknowledged in a report in 2016 that lowering the LEX reporting threshold "would have a bigger impact on smaller institutions" (EBA (2016)).

authorities maintain comprehensive credit registers, banks already report the near-universe of counterparties in their credit portfolio anyway — including the largest ones.

However, there are several reasons to conclude that the LEX reporting requirement causes substantial costs for reporting banks. For example, a survey conducted by the European Banking Authority (EBA) in 2021 revealed that banks consider LEX reporting the 8<sup>th</sup> most costly reporting requirement out of 58, and the result is driven mainly by the smallest banks in the sample (see EBA (2021)). One reason the study cites is that banks must identify "groups of connected counterparties", that is, evaluate their overall exposure to an entire corporate group, rather than individual legal counterparties (which might be subsidiaries). This nontrivial task also qualitatively distinguishes the LEX reporting requirement from general reporting of credit exposures for the purpose of credit registers.

Moreover, while preparing a reform of the LEX framework in 2013, the Basel Committee invited commercial banks to comment on an early draft.<sup>3</sup> These comments reveal that banks were, in fact, concerned about a number of elements that would increase their cost of regulatory compliance with the reporting requirement. Consistently with our findings, small banks were particularly concerned with a planned (and later abandoned) reduction in the reporting threshold from 10% to 5% of Tier 1 capital and the (later implemented) requirement to report the 20 largest borrowers. They warned of a "clear inflation of institutional operating expenditure for the necessary logistics" and a "significant increase in processing cost resulting from [...] continuous monitoring during the quarter."

In addition to administrative costs, there may be other, more implicit costs associated with reporting and highlighting large clients separately. For example, banks may be reluctant to flag their largest clients to the supervisor because they fear to invite more scrutiny in the future. *In this paper, we remain agnostic about the specific type of reporting cost.* Instead, we let the data speak by taking advantage of a reform of the Capital Requirements Regulation (CRR) in 2019 that lowered the reporting threshold for all banks in proportion to their Tier 2 capital. A simple event study shows that banks experienced a significant drop in cumulative abnormal stock returns around the policy announcement date. Importantly, the drop was significantly larger for banks with a larger share of Tier 2 capital in total capital, i.e., those banks whose reporting threshold would be most affected. This finding suggests that, regardless of *why* LEX reporting is costly, the market's verdict is in line with the costly-reporting hypothesis.

For our main analysis, we combine the European credit register (*AnaCredit*) with bank-level Security Holdings Statistics (*SHSG*) to compute each bank's total exposure (loans plus securi-

<sup>&</sup>lt;sup>3</sup>Banks' comments are publicly available at https://www.bis.org/publ/bcbs246/comments.htm.

ties) vis-à-vis each counterparty. This allows us to document that after the reform, small banks (with total assets below the 25<sup>th</sup> percentile) moved exposures from just above the critical value to just below. We interpret this behavior as an attempt to avoid reporting these counterparties to the supervisor.

We then employ a standard Regression Discontinuity (RD) approach to check for discrete jumps in interest rates at the 10% LEX threshold. In a first set of results, we show that despite the sharp regulatory cut-off, there is no statistically significant clustering of exposures below the LEX threshold — neither before nor after the reform; the results of the Cattaneo *et al.* (2018) manipulation test (a variant of the famous McCrary (2008) test) do not allow us to reject the null hypothesis that the distribution of exposures around the 10% threshold is continuous. This finding is further confirmed by an alternative manipulation test following Bugni and Canay (2021).

The negative result of these manipulation tests strengthens the validity of our identification assumptions when we compare the interest rates of borrowers just below the 10% threshold to those just above. We report results across a wide range of different modeling parameters (e.g., fixed effects, functional form, kernel functions, bandwidths), and we find that in our most conservative specification there is a statistically significant 67 basis point interest rate premium on large exposures. In robustness tests with fixed effects for banks, borrower industry, and credit rating, the magnitude and statistical significance are even more pronounced.

Moreover, and in line with our hypothesis that banks strategically incorporate regulatory costs into rates above the threshold, we find that the results described above hold only among non-syndicated loans. In the subsample of syndicated loans, where individual banks have much less control over loan terms, our analysis shows no significant discontinuity.

To further strengthen our results, we introduce a natural control group: We re-estimate the RD model on a larger sample of banks for which, by construction of the LEX framework, the 10% LEX reporting threshold is not binding.<sup>4</sup> The fact that the corresponding RD estimate is statistically indistinguishable from zero lends further credibility to our findings.

Similarly, we present a placebo test in which we check for a discontinuity in interest rates around the *new* LEX threshold in the *pre-reform* sample, and vice versa. In both cases, as expected, our tests are negative.

Our results have important implications for the availability of credit in the real economy. In particular, the above-mentioned results are almost exclusively driven by small banks and firms with only few (namely, below-median) existing bank relationships. These borrowers, despite

<sup>&</sup>lt;sup>4</sup>The effective LEX threshold for this set of banks is *lower* than 10%.

meeting the technical "large exposure" definition, might not be able to easily substitute from bank loans at their main bank (and, for instance, issue bonds) if they reach the LEX threshold. In fact, we show that when firms approach the LEX threshold at their existing bank, they are more likely to add new bank relationships in the future. In the presence of switching costs, this suggests that banks' behavior around large exposure thresholds might not just result in a neutral redistribution of surplus, but could affect overall efficiency in the allocation of credit to bank-dependent borrowers.

## 2 Related Literature

Although there is a vast literature on the side effects of banking regulation in general, largeexposure regulation in particular has not yet been thoroughly studied. Although some theoretical research features LEX limits in simulations or policy counterfactuals (e.g., Coen and Coen (2019)), the only empirical paper, to our knowledge, that studies large-exposure regulation is Kosenko and Michelson (2022) who use Israeli loan level data to show that large exposure limits force large borrowers to explore alternative sources of financing (including other banks), thereby increasing banks' asset commonality and systemic risk. More precisely, the authors show that the probability of switching from single-bank lending relationships to multiple-bank lending is decreasing in the distance between the borrower's actual exposure at the original lender and the regulatory limits on the bank's large credit exposures. In section 5.3 below, we follow a similar approach, but instead of focusing on the prevailing regulatory upper bound (25% for EU banks), we study the 10% reporting threshold.

Another closely related paper is Ivanov *et al.* (2022). Using US Dealscan data, they find that an unanticipated change in supervisory coverage of syndicated loan deals led to lower interest rate spreads and longer maturities for excluded deals. Similarly to the spirit of this paper, the authors attribute their finding to banks passing on the reduced cost of regulation to their borrowers.

More generally, our paper is also related to Alvero *et al.* (2022), who use a structural model that predicts bunching in the bank size distribution to estimate the true cost of complying with the Dodd-Frank Act in the United States. Although our model is based on a reduced form method, we share this revealed preference approach with the authors. They conclude that the regulatory costs that banks incur are "substantial, but significantly lower than banks' self-reported estimates." This finding emphasizes the need for regulators and academics to "watch what [banks] do, not what they say."

In a narrower sense, our paper is most closely related to the literature on unintended consequences of bank disclosure requirements. Examples include Nicoletti and Zhu (2022), who analyze a rule implemented in 2015 (TRID) that simplified US banks' disclosures provided to prospective mortgage borrowers, and Kim *et al.* (2022) who investigate the impact of bank disclosure regulations on local business activities in response to the 2005 Community Reinvestment Act (CRA) reform. The former find a decrease in the probability of approval of affected mortgage applications, and they attribute it to the hypothesis that TRID reduced the relative attractiveness of investing in closed-end mortgages.

Kim *et al.* (2022), on the other hand, documents that after the CRA reform (which exempted some banks from mandatory disclosure requirements for geographic loan distribution), affected banks reduced their lending to poorer areas with a high proportion of racial minority population. What these papers have in common with ours is that banks' cost of reporting is the driving force behind the unintended consequences. However, they differ from our setting in that we are concerned with *confidential* disclosures to the bank supervisor, not public disclosures to other market participants.

Finally, one of the contributions of this paper is to combine banks' loan exposures with security exposures, i.e., bond and stock holdings. While loans have traditionally been the core business of European banks, security holdings have recently gained importance. Darmouni and Papoutsi (2022) document that as of 2019, banks in the euro area hold a sizable share (approx. 10%) of outstanding European corporate bonds, especially among unrated issuers.

## 3 The Large Exposures Framework

Large-exposure regulation is not a new idea. In fact, the Basel Committee on Banking Supervision made its first proposal on measuring and controlling large credit exposures as early as 1991. In 2014, however, the Committee added new standards to the overall Basel framework of banking regulation. The objective, according to Basel Committee (2014), was to "limit the maximum loss a bank could face in the event of a sudden counterparty failure to a level that does not endanger the bank's solvency".

To achieve this goal, the LEX framework defines an upper bound for banks' overall exposures to individual counterparties (more precisely: groups of connected counterparties), i.e., the sum of all debt, equity, and derivative exposures towards a given consolidated counterparty. This large exposure limit is set at 25% of a bank's Tier 1 capital, albeit after various credit risk mitigation (CRM) techniques have been applied to the original exposures.<sup>5</sup> Exposures in excess of the 25% limit (after CRM) require supervisory approval and shall be rare and temporary.

What usually draws less attention than the 25% LEX limit is a supervisory reporting requirement: According to the LEX framework, *banks must report all their large exposures, that is, all exposures that exceed 10% of their Tier 1 capital.*<sup>6</sup> Importantly, this reporting threshold is not sensitive to credit risk mitigation. Instead, for the definition of large exposures, it is the actual accounting value of the exposure that matters. Furthermore, in addition to all their large exposures, banks must also report their 20 largest exposures, even if they do not meet the LEX definition above (i.e., even if they do not exceed the 10% threshold). In the following, we will exploit this "dual" reporting requirement for identification.

In the European Union, the LEX framework was implemented as part of the Capital Requirements Regulation (CRR) in 2013 and largely followed the definitions and terms of the Basel Committee blueprint. The key difference with respect to the Basel proposal lies in the definition of banks' eligible capital base. Initially, the European version of the LEX framework defined the LEX threshold (10%) and the LEX limit (25%) not as shares of Tier 1 capital alone, but the base additionally included Tier 2 capital up to a third of Tier 1 capital. This deviation from the original Basel proposal allowed banks to maintain higher (and report fewer) exposures than the Basel framework stipulated. However, in 2019, the original CRR was amended and the capital base for both thresholds was reduced to Tier 1 capital only. As a result, as shown in Figure 1, the LEX reporting threshold was reduced by up to one third for more than half of the banks in our sample. Although the Regulation entered into force in June 2019, banks only had to comply with the new LEX definition and limits as of June 2021.

This time lag between the announcement and the effective implementation of the reform allows us to gauge whether (a) banks and (b) stock market participants actually perceived the LEX reporting requirement as costly. Figure 2 shows the distribution of exposures around the critical value of 10% of Tier 1 capital before (blue) and after (red) the implementation of the CRR reform. The two histograms reveal that banks shifted some of the mass of exposures from just above the new threshold to just below. As shown in Figure 3, this behavior is particularly pronounced among small banks (i.e., those with total assets below the 25<sup>th</sup> percentile). Echoing calculations by the EBA (2016), this suggests that small banks perceived the LEX reporting requirement as particularly costly, and actively tried to prevent treated exposures from becoming large in the legal sense.

<sup>&</sup>lt;sup>5</sup>Eligible CRM techniques include accounting for collateral or guarantees and allow banks to decrease the regulatory value of some exposures to ensure they remain below the 25% limit.

<sup>&</sup>lt;sup>6</sup>The templates (COREP C27–C31) that contain the items to be reported are displayed in Appendix A.



Figure 1: Reduction of LEX threshold in %



Figure 2: Exposure distribution before and after CRR reform



Figure 3: Exposure distribution before and after the CRR reform (small banks) This sample contains only banks in the bottom quartile of the total asset distribution.

Furthermore, to analyze the stock market response to the CRR reform in 2019, we conduct a standard event study of banks' cumulative abnormal stock returns (CAR) in a narrow window around the reform announcement in May 2019. For each of the 75 publicly listed banks in our sample, we obtain daily stock price data from Yahoo Finance and calculate abnormal stock returns as the difference between the daily net return of each stock and that of the STOXX Europe 600 index. We then aggregate these abnormal returns over the three trading days following the announcement. The histogram in Figure 4 plots the distribution of CARs during the event window. The average bank experiences a cumulative abnormal stock price decline of 80 basis points, and most of the distribution is in negative territory, which lends support to the hypothesis that LEX reporting is costly.

However, the 2019 CRR reform also included elements that are not related to the treatment of large exposures. Therefore, to improve the explanatory power of this exercise, we regress each bank's CAR on the ratio of Tier 2 to Tier 1 capital, which determines by how much the bank's LEX threshold will be tightened by the reform (see also Figure 1). In other words, it measures bank-level exposure to the LEX threshold reduction included in the CRR reform. The estimated regression coefficient is highly statistically significant (p-value 0.008) and suggests that a one-standard-deviation increase in the Tier2/Tier1 ratio is associated with a decrease in CAR of almost one-third of a standard deviation. We take these correlations as further suggestive evidence that the stock market (a) perceives LEX reporting requirements as costly and (b) even distinguishes between banks of different exposures to the reform.

Finally, note that voluntary compliance with the tighter threshold *even before June 2021* would suggest that reporting large exposures is *not* really costly for banks. However, as Figure 5 indi-



Figure 4: Cumulative abnormal return in percentage points



Figure 5: Number of newly reported exposures per bank

cates, the number of newly reported exposures per bank only jumps considerably in June 2021, when the new and tighter reporting threshold became mandatory. Admittedly, this pattern is also consistent with banks being indifferent between reporting or not during the grace period. But at the very least, it does not suggest to reject the hypothesis of costly reporting.

It is therefore only natural to ask whether the reporting requirement has a distortionary impact on banks' lending behavior and, eventually, credit supply to bank-dependent borrowers.

## 4 Data

To compute banks' total exposures to each counterparty, we combine two proprietary datasets that are available at the European Central Bank (ECB): The *Group-level Security Holdings Statistics* (SHSG) and the still relatively young European *Analytical Credit Datasets* (AnaCredit). The former contains security-by-security information on the security holdings (debt and equity) of the largest banking groups in the euro area (EA), reported separately for each subsidiary at quarterly frequency. The latter is a credit register that contains each loan made by banks resident in the EA (including their domestic or foreign branches) or the branches of foreign banks in the EA to legal entities (no natural persons or households), as long as the amount owed exceeds  $25,000 \in$ . In addition, AnaCredit includes information on syndicated loans with a detailed breakdown of how loan shares are allocated between participating banks. Data are available on a monthly basis since September 2018.

We are able to link those two resources both on the banks' side (via ECB-internal identifiers) and, crucially, on the counterparty side (via a firm's *Legal Entity Identifier* (LEI)). To reflect the regulatory requirements as closely as possible, we aggregate banks' exposures at the counterparty *group* level, making use of corporate structure information in the ECB's *Register of Institutions and Affiliates Database* (RIAD).

After converting foreign currency amounts into EUR, we obtain a dataset whose unique identifier is a bank-firm pair in a specific quarter with information about the total loan exposure and the total bond and equity exposure that the bank has to the counterparty. Moreover, to compute the ratio of individual exposures to a bank's Tier 1 capital, we also leverage quarterly supervisory bank balance sheet data from the ECB's Single Supervisory Mechanism (SSM). Finally, we add long-term credit ratings for entire companies or single security issues, based on a composite ECB dataset containing ratings from Standard & Poor's, Moody's, Fitch, and DBRS.

In this study, we only focus on counterparties from the non-financial corporate sector (NFC). Not only do these represent the vast majority of banks' exposures, but banks can also more actively influence the terms of corporate loans, whereas they are price-takers in the market for government debt. Furthermore, focusing on non-financial corporations has the advantage that the lack of derivative exposures in our dataset is less problematic, as most exposures to these firms are debt or equity exposures.<sup>7</sup>

<sup>&</sup>lt;sup>7</sup>Among reported large exposures to financial corporations (including banks), the value of derivative exposures makes up 5% of the total on average. For non-financial corporations, the share is only 0.7%.

The final dataset consists of 1,149 banks and 91,931 NFC counterparties during the period 2018q3–2023q2. Although we do not observe the universe of euro-area banks, our sample covers approximately 80% of the aggregate euro area banking sector in terms of total assets throughout the entire sample period.

In line with the legal definition described in the previous section, we classify an exposure as large if the sum of the bank's loan, bond, and stock holdings of a given counterparty exceeds 10% of the bank's Tier 1 capital (before June 2021: 10% of  $Tier1 + min(Tier2, \frac{1}{3}Tier1)$ ). For simplicity, in the following we use the term "exposure ratio" for the ratio of a bank's total exposure to a counterparty relative to the relevant capital measure at the time.

To a large extent, we can even cross-validate our classification of large exposures by comparing our data with the actual reports that banks submit to the SSM. However, this comparison is complicated by the fact that the SSM and our dataset do not share a common counterparty identifier. Except for a few firms with a reported LEI number in the SSM dataset, we cannot match the exposures we classify as large with the actually reported ones in a reliable way. Nevertheless, a manual check of counterparties' names reveals that our bottom-up approach manages to cover a large fraction of reported exposures.

Our classification is, however, imperfect for two reasons: First, we do not observe banks' derivative exposures. Hence, in cases where derivatives constitute a substantial part of a bank's total exposure to a counterparty, this could lead us to systematically misclassify large exposures as non-large. Fortunately, the average share of derivative exposures in total reported exposures to NFC is negligible (0.7%), so we are confident that missing derivative exposures are not a problem for our LEX classification.

Second, AnaCredit does not cover loan exposures of banks' subsidiaries residing outside the euro area. If, for instance, a French bank has a subsidiary in India with large exposures to Indian companies, these would have to be reported to the SSM, but we cannot observe them. As long as these bank-firm pairs are not recorded in our dataset at all, this is not a problem. Only firms that borrow from a banking group through multiple subsidiary banks simultaneously (e.g., the French headquarters and the Indian subsidiary) could become problematic because we would only observe part of the total exposure.

These two caveats imply that every exposure we classify as large is in fact large (and therefore must be reported to the SSM), but not necessarily vice versa. Importantly, these "missing" large exposures would only be problematic if the amount we miss is small. In that case, the observed exposure ratio might still fall into the narrow bandwidth around the regression discontinuity, but on the wrong (namely, left-hand) side of the cut-off, potentially biasing our results.

Panel A: Exposure level										
	Ν	Min	Median	Max	Mean	SD				
Total exposure in '000 EUR	1,332,323	1.0e-05	1800	1.9e+07	1.6e+04	8.8e+04				
Exposure ratio	1,332,323	5.7e-13	.0102	.9976	.0164	.0325				
Syndicated	686,098	0	0	1	.0983	.2977				
New loan	1,332,323	0	0	1	.0834	.2764				
Average interest rate	664,911	0	2.002	12.68	2.348	1.696				
Average interest rate (new loans)	101,435	0	1.87	12.68	2.294	2.005				
Average original maturity	615,369	.0795	8.1	80.05	10.71	8.737				
Average original maturity (new loans)	97,995	.0795	3	80.05	6.406	9.388				
	Panel B: B	ank level								
	Ν	Min	Median	Max	Mean	SD				
Total assets in bln EUR	1,149	.0056	2.2	2,398	26	121				
Tier 1 capital in bln EUR	1,149	.0025	.18	98	1.6	6				
# of NFC large exposures per bank	1,149	0	0	40	1.3	3.2				

**Table 1: Summary Statistics** 

Interest rates and original loan maturities are calculated as averages (weighted by total exposure amounts) across all loans within the same bank-firm pair at the same time.

If, on the other hand, the missing portion of a large exposure is substantial, the observation is dropped from the local regression analysis anyway.

To ensure that our results are not driven by outliers, we trim the dataset by discarding observations with interest rates below the first and above the 99<sup>th</sup> percentile. Furthermore, we drop a small number of observations where the exposure ratio exceeds 100%.<sup>8</sup> Finally, since a regression discontinuity design only uses a limited number of observations around the threshold anyway, we restrict our data query to observations with an exposure ratio of at least 1%. In doing so, we can increase the speed of our data queries without sacrificing explanatory or statistical power.

Table 1 contains summary statistics for the main exposure- and bank-level variables of interest in our study. The table shows, among other things, that the average exposure to the NFC sector in our sample amounts to approximately  $\leq 16m$  (the median is considerably lower at  $\leq 1.8m$ ), which translates into an average exposure ratio of 1.64%. Moreover, the table reveals that the average bank only has 1.3 large exposures to the non-financial corporate sector.

<sup>8</sup>Remember that credit risk mitigation may allow even those exposures to comply with the 25% LEX limit.

## 5 Empirical Strategy & Results

In this section, we investigate whether the requirement to report large exposures to the supervisor affects banks' lending behavior in terms of quantities (loan amounts) and prices (interest rates). We then turn to the firm side and study how firms react to potential discontinuities in loan terms around LEX thresholds.

#### 5.1 Manipulation Tests

To assess whether banks actively try to avoid crossing the 10% large exposure threshold, we perform a manipulation test based on density discontinuity. This class of tests builds on Mc-Crary (2008) and is commonly used for robustness checks in the RD literature. In short, the idea is to estimate the density of the data around a cut-off value of the variable of interest and check for a discontinuity of that density at the cut-off. If such a discontinuity is detected, this may be taken as evidence of manipulation.<sup>9</sup> In our test, the density is estimated using local polynomials as in Cattaneo *et al.* (2020).<sup>10</sup>

We focus our analysis on banks with more than 20 reported large exposures. These are the only banks for which an exposure ratio of 10% is the relevant threshold that triggers the reporting requirement. To understand why, remember that banks must report their largest 20 exposures in any case (see Section 3 for details). Therefore, if a bank has less than 20 large exposures, then at the margin even a new exposure with a ratio below 10% must be reported (as soon as it exceeds the previously 20<sup>th</sup>-largest exposure of that bank). Although most of the banks in our sample usually report less than 20 large exposures, there are also 148 banks that report more than 20 large exposures (including exposures to governments, households, and financial corporations). In other words, there are still a considerable number of data points to be used in this restricted sample.

The result of our test is visualized in Figure 6. The solid red and blue lines represent the estimated density (surrounded by 95% confidence bands) in terms of the exposure ratio to the left and right of the 10% threshold, respectively. The confidence bands clearly overlap at the threshold and we cannot reject the null hypothesis that the density is continuous (p-value = 0.48). In

<sup>&</sup>lt;sup>9</sup>Educational test results provide the textbook example for such manipulation. If there is an upward jump in the frequency distribution of students right above the grade cut-off required for passing an exam, the examiner was probably lenient and pushed a number of students above the bar that would otherwise have marginally failed.

<sup>&</sup>lt;sup>10</sup>The corresponding Stata routine is described in detail in Cattaneo *et al.* (2018).



Figure 6: Estimated density on both sides of the cut-off

other words, we cannot conclude with sufficient statistical confidence that banks systematically keep exposures below the reporting threshold.<sup>11</sup>

To confirm the result of the Cattaneo *et al.* (2018) tests above, we test the same null hypothesis (continuity of the density of the exposure ratio around 10%) using an alternative procedure proposed by Bugni and Canay (2021). Instead of relying on estimates of the density around the cut-off value, this test "exploits the fact that a certain functional of order statistics of the data is approximately binomially distributed under the null hypothesis." Again, the null cannot be rejected at any conventional significance level (p-value = 0.76). For the sake of completeness, we also fail to detect bunching among the control group of banks with fewer than 20 large exposures, as expected.

At first sight, these findings seem to suggest that reporting large exposures is not particularly costly for banks and, therefore, should not distort their behavior in terms of credit supply to large borrowers. However, the observation is also consistent with a scenario where reporting large exposures *is* costly, but banks can pass on the cost to their borrowers, for example, in the form of higher interest rates or shorter loan maturities. To explore whether this is the case, we now zoom in on the interest rates that banks charge their borrowers on both sides of the LEX threshold.

<sup>&</sup>lt;sup>11</sup>Note that this finding does not contradict the earlier result that small banks shifted exposures below the new threshold. The latter is a before-after comparison whereas the manipulation tests were performed on a pooled cross-section.

#### 5.2 Regression Discontinuity Analysis

There are two fundamental traditions in the RD literature: One, the so-called *global polynomial approach*, relies on fitting higher-order polynomials across the entire support of the running variable (here: exposure ratio), allowing for a discontinuity at a cut-off chosen by the researcher. The advantage of this approach is a relatively good fit of the underlying data distribution and hence a relatively small bias. However, as argued by Gelman and Imbens (2019), controlling for global higher-order polynomials can lead to "noisy estimates, sensitivity to the degree of the polynomial, and poor coverage of confidence intervals."

The alternative, and nowadays more widely used, strategy is based on *local linear regressions*. As the name suggests, the researcher fits two regression lines on each side of the cut-off, but only within a small neighborhood ("bandwidth"). The modern consensus among econometricians seems to be that local linear regressions have preferable properties, both in a purely statistical sense and when it comes to the identification and interpretation of estimated treatment effects (see, e.g., Hahn *et al.* (2001)). In our main analysis, we therefore follow the local linear regression approach. However, we find qualitatively very similar results using the global polynomial approach, which we report in Appendix B.1.

As a baseline specification, we estimate the following simple equation.

$$y_{bft} = \alpha + \beta \times LEX_{bft} + \gamma \times ExpRatio_{bft} + \delta \times LEX_{bft} \times ExpRatio_{bft} + \varepsilon_{bft}$$

where  $y_{bft}$  is the average interest rate of *new* loans from bank *b* to firm *f* at time *t*, and *ExpRatio*<sub>bft</sub> the total exposure of bank *b* to firm *f* at time *t*, divided by the relevant capital measure of bank *b* at time *t*. *LEX*<sub>bft</sub> is an indicator variable that takes the value 1 if the exposure ratio of bank *b* to firm *f* exceeds 10% at time *t*, and zero otherwise. To make the estimated coefficients easy to interpret, we center the running variable around zero in all regressions by subtracting 0.1 from the exposure ratio. The RD estimator then simply corresponds to the estimate  $\hat{\beta}$  and measures the difference between the (linear) conditional expectation function on the right-hand side and that on the left-hand side of the cut-off (both evaluated at the cut-off). As is standard in the RD literature, we allow the slopes of the two estimated regression lines to differ.

Of course, a key input parameter of every RD design is the chosen bandwidth around the cut-off that determines which observations are used for estimation. The general trade-off is that using a larger bandwidth yields more precise estimates (since more data points are used for estimation), but the chosen functional form (here: linear) becomes a worse approximation of the underlying data, which creates bias. To navigate this trade-off, we follow the procedure pro-

	Before CF	RR reform	After CR	R reform
Sample	(1) < 20 LEX	$(2) \\ \geq 20 \text{ LEX}$	(3) < 20 LEX	$(4) \\ \geq 20 \text{ LEX}$
RD_Estimate	0.215 (0.180)	0.0771 (0.232)	-0.116 (0.233)	0.671** (0.327)
Kernel Bandwidth N (left) N (right) p-value	Uniform 0.028 1,452 738 0.232	Uniform 0.025 569 329 0.739	Uniform 0.030 1,204 549 0.619	Uniform 0.029 534 284 0.040
Robust p-value	0.341	0.921	0.568	0.080

Table 2: Baseline RD results

\* p < 0.10, \*\* p < 0.05, \*\*\* p < 0.01

posed by Calonico *et al.* (2020) for an optimal data-driven bandwidth selection that minimizes an approximation of the conditional mean squared error of the RD point estimate. However, for transparency reasons, we also plot the main RD estimate and confidence intervals as a function of bandwidth choice in Appendix B.2.

#### 5.2.1 Baseline RD results

In our baseline specification (see Table 2 and Figure 7), we weight all observations that fall within the chosen bandwidth equally (i.e., with a uniform kernel). Appendix B.3 also contains our results for a specification with triangular kernels which attach higher weights to observations closer to the threshold. With triangular kernel weighting, we find even larger point estimates and smaller standard errors, so our baseline results can be understood as a conservative estimate. Furthermore, we report not only conventional p-values, but also a heteroskedasticity-robust version based on nearest-neighbor matching (see Calonico *et al.* (2014)).

The first thing to notice is the sign and magnitude of the coefficient in columns (2) and (4). The RD estimate for the interest rate in the period after the CRR reform is positive and amounts to 67 basis points. Given the average interest rates reported in Table 1, these are economically large effects, ranging around 30% of the unconditional average. Both conventional and bias-corrected RD estimates are significantly different from zero, at least at the 10 percent level. Importantly, this is only the case in the sample of banks with more than 20 large exposures, that is, those for which the 10% threshold is the relevant trigger of the reporting requirement. For banks with fewer than 20 LEX in columns (1) and (3), all estimates are statistically indistin-



Figure 7: Local linear regression plots

guishable from zero. Interestingly, there is no measurable discontinuity in interest rates *before* the CRR reform that reduced the threshold.

The combination of a sharp cut-off with a regulatory reform that shifts that very cut-off enables us to perform a further placebo test where we apply the "new" (that is, post-reform) definition of the LEX threshold to the sample before the reform was implemented, and vice versa. If our interpretation that banks pass on the cost of the reporting requirement to their borrowers is correct, we should not find a statistically significant discontinuity at these placebo thresholds. In Appendix B.4 we show that this is indeed the case.

To further test the robustness of our findings, the Appendix contains results for several alternative specifications, including a simple comparison of *average* new interest rates in the neighborhood of the 10% threshold (rather than local linear regression) in Appendix B.5. The difference in means is somewhat smaller ( $\approx$  50bp) than in the baseline specification with local linear regression, but statistically highly significant.

Note that we did not include any additional control variables or fixed effects in the baseline analysis. The reason is that, in an ideal scenario, the identifying assumption behind RD designs is the quasi-random treatment assignment to units in the close neighborhood around the threshold. Therefore, observations that fall into this neighborhood should be sufficiently similar to each other even without controlling for other covariates.

However, it is perfectly acceptable to control for fixed effects in an RD design, and it can even improve the efficiency of estimates. We report the results of specifications with fixed effects for bank, borrower industry, and borrower rating in Appendix B.6. The positive impact of LEX status on new interest rates is robust to all these fixed effects and becomes even more statistically significant.

			Averag	e interest ra	te (new loa	ns)	
	(1) Syndicated	(2) Not syndicated	(3) Small	(4) Medium	(5) Large	$\begin{array}{c} (6) \\ N^{Banks} \leq Median \end{array}$	(7) N <sup>Banks</sup> >Median
RD_Estimate	1.154	0.782**	1.495**	0.230	0.821	1.313**	0.298
	(0.830)	(0.320)	(0.746)	(0.363)	(0.624)	(0.536)	(0.358)
Kernel	Uniform	Uniform	Uniform	Uniform	Uniform	Uniform	Uniform
Bandwidth	0.023	0.036	0.029	0.040	0.034	0.036	0.030
N (left)	67	646	91	420	264	316	342
N (right)	39	295	74	204	83	138	181
p-value	0.164	0.015	0.045	0.526	0.188	0.014	0.406
Behuet n value	0.176	0.058	0.041	0.206	0.244	0.026	0.520

#### Table 3: Baseline RD results for various subsamples

Standard errors in parentheses

\* p < 0.10, \*\* p < 0.05, \*\*\* p < 0.01

#### 5.2.2 Heterogeneity across loan types and bank size

In the following, we repeat our basic regression discontinuity analysis for different subsamples of the data to narrow down the driving forces behind the observed discontinuities.

An implicit assumption behind our interpretation that banks pass on the cost of the LEX reporting requirement to their counterparties is that banks have sufficient bargaining power vis-à-vis their clients. On the contrary, if a bank has no bargaining power over loan terms, we should not expect a discontinuity in interest rates or loan maturities around the LEX threshold. A simple way to test this is to compare syndicated loans with traditional bilateral loans.

In a syndicated loan, several lenders join forces (usually coordinated by one or more "lead arrangers") to offer borrowers larger credit volumes than any single bank would be willing or able to offer otherwise. In return, participating banks sacrifice the power to set the terms of the loan (say, interest rates) freely. In fact, from individual banks' points of view, a syndicated loan is a much more standardized asset than a bilateral loan. The evidence we present in columns (1) and (2) of Table 3 is consistent with that. The large exposure interest rate premium disappears in a statistical sense once we restrict the sample to only syndicated loans. Instead, our results apply only to loans contracted directly between the bank and the respective borrower, which is consistent with our interpretation.

Of course, *a priori* there is no reason to believe that the regulatory cost associated with the LEX reporting requirement should be homogeneous across banks. For example, the cost of reporting might be relatively lower for a large bank with a vast compliance division than for

smaller ones. To explore bank heterogeneity, we apply our RD design separately for small, medium-sized, and large banks.<sup>12</sup>

The table reveals that the positive effect on interest rates for new loans is driven exclusively by small banks. The point estimates in column (3) are much larger and the estimates are more precise than in the average baseline results reported in Table 2 above. This finding could mean one of two things: Either (a) small banks are the only ones that care about reporting large exposures, for example because their relative regulatory burden is higher; or (b) they are the only ones that are *able* to pass on the regulatory cost to their borrowers.

In an attempt to distinguish between these two interpretations, in columns (6) and (7) we repeat the analysis separately for firms below and above the median in terms of the number of bank relationships (the median in our sample is 2). If banks can, in fact, pass on the compliance cost to large borrowers, the effect should be more pronounced for counterparties that have "nowhere else to go", i.e., those with fewer outside options and thus less bargaining power. In contrast, firms with many existing bank relationships will find it easier to turn to other banks for their financing needs where they might not qualify as LEX and hence get a better deal.

Our estimates are consistent with this prior; the RD coefficient is much larger and exhibits higher statistical significance in the subsample of firms with fewer banking relationships. Firms with more existing banking relationships, on the other hand, do not appear to pay a discontinuous premium if they are marginally above their banks' LEX threshold.

These last two results have important implications for how to think about the impact of the LEX reporting requirement on overall efficiency. If the firms affected by their banks' behavior were only the largest corporations with easy access to capital markets and multiple bank relationships, our findings might not be worrying for policymakers. In that case, the discontinuity in interest rates around LEX thresholds that we document would only reflect a redistribution from borrowers to lenders. However, by construction of the regulatory thresholds, small banks' large exposures are also relatively small. Indeed, an eyeball inspection of our data suggests that the counterparties of affected loans are often regional small and medium-sized enterprises (SMEs) that are large relative to their regional bank, but small relative to the distribution of firms. These firms are what the literature often describes as bank-dependent, and hence the "threat" of becoming a large exposure for their bank (and incurring the costs we document in this paper) could have substantial effects on the credit allocation to these firms.

We conclude this section with a simple back-of-the-envelope calculation of the annual increase in interest expenses that a typical firm experiences if it ends up marginally above its

<sup>&</sup>lt;sup>12</sup>Small (large) banks are those with total assets below the 25<sup>th</sup> (above the 75<sup>th</sup>) percentile in a given quarter; medium-sized banks are those in between the two percentiles.

bank's LEX threshold, relative to borrowing from another bank with slightly more capital, *ce*teris paribus. If we ignore the caveat that RD designs generally measure highly local effects and are numerically sensitive to many modeling parameters, our baseline effect of 67 basis points translates into additional interest expenses of almost 190,000€ per year for the median firm.<sup>13</sup> In the subsample of small banks from column (3) of Table 3, the 1.5 percentage point premium instead translates into an annual interest expense premium of 30,000€ for the average borrower. These figures should be taken with a grain of salt, but fall into a region that we consider reasonable in thinking about regulatory compliance costs. For instance, in the aforementioned survey by the EBA (2021), banks also report estimates of the monetary cost that they incur due to different supervisory reporting requirements. For LEX reporting, the average (median) estimates are EUR 2.9 million (EUR 790k) for the entire sample and EUR 1.5 million (EUR 305k) for small banks.

#### 5.3 Firm-level analysis

The previous section shed light on banks' behavior around their large exposure thresholds. Instead, this final section focuses on the firm side. If a firm is aware that their bank will charge an interest rate premium on loans above its LEX threshold, it may be discouraged from additional borrowing from that bank. Instead, it might try to borrow from alternative funding sources, including capital markets (i.e., by issuing bonds or raising equity) or other banks. Our dataset allows us to investigate whether firms open new bank relationships once they get close to their bank's LEX thresholds.

Inspired by a similar setup in Kosenko and Michelson (2022), we define a dummy variable  $1{New bank}$  that takes on the value 1 whenever we observe a new loan from a bank that had no outstanding loan with the respective firm *f* before (and zero otherwise). We then regress this dummy onto a lagged firm-level measure of distance from the relevant LEX threshold, defined as  $Distance_{ft} = 0.10 - min_b(ExpRatio_{bft})$ . Formally, we estimate the following linear probability model:

$$\mathbb{1}\{New \ bank\}_{ft} = \alpha_f + \gamma_t + \beta Distance_{f,t-1} + \delta N(Banks)_{f,t-1} + \theta Distance_{f,t-1} \times N(Banks)_{f,t-1} + \varepsilon_{ft} + \delta N(Banks)_{f,t-1} + \delta N(Ba$$

To do so, we first collapse the dataset to the firm-quarter level. Importantly, we keep the minimum exposure ratio for each firm across all banks from which the firm borrows in a given

<sup>&</sup>lt;sup>13</sup>The median exposure in the sample used in the baseline specification is approximately EUR 28 million.

	Below LEX threshold	Above LEX threshold
	(1) 1{New bank}	(2) 1{New bank}
Distance <sub>t-1</sub>	-1.578*** (0.100)	0.0915 (0.0746)
$N(Banks)_{t-1}$	-0.0310*** (0.00190)	-0.0651*** (0.00993)
$\begin{array}{l} Distance_{t-1} \\ \times N(Banks)_{t-1} \end{array}$	0.281*** (0.0219)	-0.0600** (0.0298)
$Log(Borrowing)_{t-1}$	-0.0353*** (0.00221)	-0.0173 (0.0207)
Firm FE	$\checkmark$	$\checkmark$
Time FE	$\checkmark$	$\checkmark$
N	460,159	15,310

Table 4: Do firms open new bank relationships when they approach the LEX threshold?

Heteroskedasticity-robust standard errors in parentheses

\* p < 0.10, \*\* p < 0.05, \*\*\* p < 0.01

quarter. That way, our distance measure captures each firm's *maximum distance from the LEX thresholds of all their banks*. This is the most conservative metric for how close a firm is to breaching the LEX threshold with any of their banks — in other words, how much "headroom" it has left.

We present the result of this linear probability model in Table 4, where we also include the lagged number of banks a firm borrows from, its interaction with our lagged distance measure, and fixed effects for firms and quarters. Note that we also include lagged total borrowing to control for possible firm-level growth trends that could jointly affect distance to the LEX threshold and the number of banks the firm borrows from. The two columns represent two different samples; one for firms with a positive lagged distance measure (i.e., they were below the LEX threshold of at least one of their banks), and one with a negative lagged distance measure (i.e., they had already breached the LEX threshold of even their "slackest" bank).

Column (1) reveals a strong negative and highly statistically significant effect. Intuitively, when firms get closer to the LEX threshold of their marginal bank (that is, the bank with the least exhausted capacity), they are considerably more likely to start borrowing from other banks. In addition, the sign and magnitude of the interaction term with  $N(Banks)_{t-1}$  mean that this effect is particularly strong for firms with relatively few banks and vanishes for firms with many banks. In fact, a quick back-of-the-envelope calculation suggests that for a firm with initially

two banks, a one-standard-deviation decrease in lagged distance increases the probability of adding a new bank by 1.7 percentage points. Considering that, on average, 6.6% of firms add a new bank each quarter, this is a significant increase.

Consistent with the one-sided deterrence of LEX thresholds, we only find a significant response to distance from the threshold when firms are below the threshold. In column (2), for firms that are already large exposures at every bank they borrow from, distance to the threshold does not matter for firm's propensity to open new bank relationships.

In Appendix B.7, we show that the result of our linear probability model above is qualitatively robust to using a conditional logit model instead. The weaknesses of linear probability models are well known (e.g., predicted values outside of [0,1] or misspecification due to the linearity assumption). However, we follow Timoneda (2021) who argues that with fixed effects and rare events (the unconditional mean of  $1{New bank}$  is 0.066), linear probability models outperform conditional logit models.

## 6 Conclusion

Large-exposure regulation for banks was designed to reduce risk concentration in banks' credit portfolios and increase transparency for supervisory authorities. However, to the extent that banks perceive reporting their largest clients as costly, LEX reporting requirements could hamper credit provision to (relatively) large borrowers, especially those that are particularly bankdependent.

Although large-exposure regulation is in place in almost all developed countries, there is surprisingly little scholarly work on its economic effects. In this paper, we analyzed a rich and novel dataset to shed light on banks' behavior around large exposure thresholds. In particular, we investigated whether banks systematically keep exposures below the critical value and whether interest rates on new loans exhibit discontinuities at the threshold.

Our findings suggest that firms classified as large exposures (and hence reported to the supervisor) pay an interest rate premium compared to firms just below the threshold of approximately 67 basis points. For the average loan in our estimation sample, this magnitude translates into a 30% higher annual interest rate.

We show that these results are driven by non-syndicated loans where individual banks have agency over loan terms, and thus where regulatory considerations can be reflected in their lending behavior. The effect is particularly pronounced among small banks and among firms that have at most the median number of banking relationships in our sample. Therefore, the companies affected most are also relatively small and bank-dependent. Moreover, we find that firms are more likely to open new bank relationships when they approach the LEX threshold at their existing banks from below, especially if they have few bank relationships to begin with.

Taken together, these results form a coherent picture: Faced with the requirement to report their large exposures, small banks manage to pass on some of the reporting cost to those borrowers via higher interest rates. The reason they are able to do so (while larger banks cannot) is that their borrowers have fewer outside options available. Anticipating banks' pricing behavior, firms respond by expanding their network of bank relationships and thus escape the large-exposure premium.

In light of these robust discontinuities, the lack of a statistically significant concentration of exposures below LEX thresholds is a stark sign of frictions in bilateral loan markets. If there were perfect information and no switching costs, borrowers would know the entire menu of available interest rates and could choose to borrow from banks with more capital to avoid the "large exposure penalty". However, this kind of shopping behavior would imply systematic bunching below LEX thresholds, which we show to be counterfactual.

One possible implication of our findings is that LEX disclosure rules (and banks' response to them) favor large banks over smaller ones. For example, when firms with strong growth prospects decide which bank to borrow from, they might consider that they will have more "headroom" (before approaching the LEX threshold) in larger banks than in smaller ones.

In addition, since banks might also pass on regulatory costs in other ways than through interest rates (e.g., higher fees, stricter covenants, more collateral, etc.), our findings might only be part of the story.

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# Appendix

# A LEX Reporting Templates

C 27.00	- Identif	ication of	the counter	erparty (LE	1)						
COUNTERPARTY IDENTIFICATION											
Code	Name	LEI code	Residence of the counterparty	Sector of the counterparty	NACE code	Type of counterparty					
010	020	030	040	050	060	070					

C 28.00 - Exposures in the non-trading and trading book (LE 2)

C	COUNTERPA	RTY							ORIGI	AL EXPO	SURES					
	Transactions						Direct expo	sures			Indirect exposures					
		Transactions where there						Off b	alance sheet	items				Off ba	lance sheet	items
Code	Group or individual	is an exposure to underlying assets	Total original exposure	<i>Of which: defaulted</i>	Debt instruments	Equity instrument s	Derivatives	Loan commit- ments	Financial guarantees	Other commit- ments	Debt instruments	Equity instruments	Derivative s	Loan commit- ments	Financial guarantees	Other commit- ments
010	020	030	040	050	060	070	080	090	100	110	120	130	140	150	160	170

C 29.00	C 29.00 - Detail of the exposures to individual clients within groups of connected clients (LE 3)																			
	COUNT	ERPARTY								ORIGI	IAL EXPO	SURES								
		Transactio				Direct exposures							Indirect exp	osures						
	Group	ns where there is an	ns where here is an Type of	s where ere is an Type of	ere is an Type of	n Type of						Off ba	alance sheet	items				Off ba	lance sheet	items
Code	Group code	exposure to underlying assets	Type of connection	Total original exposure	<i>Of which: defaulted</i>	Debt instruments	Equity instrument s	Derivatives	Loan commit- ments	Financial guarantees	Other commit- ments	Debt instruments	Equity instruments	Derivative s	Loan commit- ments	Financial guarantees	Oth com me			
010	020	030	040	050	<u>050</u> 060 070 080 090 100 110 120 130 140 150 160 170 18															
C 20 0	0 - Motu	urity buc	ets of the		oc in the	non-trad	ing and t	rading b	ook (LE	(1)				•						

C 30.00 - Maturity buckets of the exposures in the non-trading and trading book (LE 4)

COUNTER PARTY											MATURIT	Y BUCKETS C	OF THE EXPO	SURE	
Code	Up to 1 Month	Greater than 1 month up to 2 Months	Greater than 2 months up to 3 Months	Greater than 3 months up to 4 Months	Greater than 4 months up to 5 Months	Greater than 5 months up to 6 Months	Greater than 6 months up to 7 Months	Greater than 7 months up to 8 Months	Greater than 8 months up to 9 Months	Greater than 9 months up to 10 Months	Greater than 10 months up to 11 Months	Greater than 11 months up to 12 Months	Greater than 12 months up to 15 Months	Greater than 15 months up to 18 Months	Great than mont up to Mont
010	020	030	040	050	060	070	080	090	100	110	120	130	140	150	160

#### C 31.00 - Maturity buckets of the exposures to individual clients within groups of connected clients (LE 5)

COUNTE	ERPARTY		MATURITY BUCKETS OF THE EXPOSUR									SURE			
Code	Group code	Up to 1 Month	Greater than 1 month up to 2 Months	Greater than 2 months up to 3 Months	Greater than 3 months up to 4 Months	Greater than 4 months up to 5 Months	Greater than 5 months up to 6 Months	Greater than 6 months up to 7 Months	Greater than 7 months up to 8 Months	Greater than 8 months up to 9 Months	Greater than 9 months up to 10 Months	Greater than 10 months up to 11 Months	Greater than 11 months up to 12 Months	Greater than 12 months up to 15 Months	Great than mont up to Mont
010	020	030	040	050	060	070	080	090	100	110	120	130	140	150	160

# **B** Robustness Checks

# **B.1** Global Polynomial Approach

	Before CF	RR reform	After CRR reform				
Sample	(1)	(2) > 20 L FX	(3) < 20 I FX	(4) > 20 L FX			
bampie		<u>&gt;</u> 20 LLX		<u>&gt;</u> 20 LLA			
RD_Estimate	0.201 (0.176)	-0.252 (0.243)	0.0710 (0.225)	0.816** (0.327)			
Kernel	Triangular	Triangular	Triangular	Triangular			
Bandwidth	0.100	0.100	0.100	0.100			
N (left)	43,273	29,013	10,155	8,568			
N (right)	1,699	1,120	1,024	755			
p-value	0.253	0.300	0.752	0.013			
Robust p-value	0.891	0.207	0.794	0.010			

Table 5: Third-order polynomial across entire support of *ExpRatio* 

\* p < 0.10,\*\* p < 0.05,\*\*\* p < 0.01



(a) Banks with fewer than 20 LEX

(b) Banks with at least 20 LEX

# **B.2** Bandwidth sensitivity



# **B.3** Triangular kernel

	Before CF	RR reform	After CR	R reform
Sample	(1) < 20 LEX	$(2) \\ \geq 20 \text{ LEX}$	(3) < 20 LEX	$(4) \\ \geq 20 \text{ LEX}$
RD_Estimate	0.0197	0.0599	-0.179	0.854**
	(0.201)	(0.212)	(0.221)	(0.339)
Kernel	Triangular	Triangular	Triangular	Triangular
Bandwidth	0.024	0.036	0.039	0.030
N (left)	1,128	891	1,903	579
N (right)	647	457	684	296
p-value	0.922	0.778	0.418	0.012
Robust p-value	0.855	0.942	0.371	

Table 6: Baseline RD results with triangular kernel

Standard errors in parentheses

\* p < 0.10, \*\* p < 0.05, \*\*\* p < 0.01

			Avera	ge interest rat	e (new loans)		
	(1) Syndicated	(2) Not syndicated	(3) Small	(4) Medium	(5) Large	(6) $N^{Banks} \leq Median$	(7) N <sup>Banks</sup> >Mediar
RD_Estimate	1.069	0.798**	1.241*	0.408	0.955	1.663***	0.385
	(0.880)	(0.351)	(0.652)	(0.387)	(0.705)	(0.631)	(0.341)
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Bandwidth	0.028	0.035	0.041	0.042	0.037	0.029	0.040
N (left)	85	615	153	458	299	208	527
N (right)	43	284	96	206	88	110	239
p-value	0.225	0.023	0.057	0.292	0.175	0.008	0.258
Robust p-value	0.216	0.040	0.065	0.278	0.256	0.009	0.339

#### Table 7: RD results: Heterogeneity

Standard errors in parentheses

\* p < 0.10,\*\* p < 0.05,\*\*\* p < 0.01

### **B.4** Placebo

Table 8:	Placebo	test with	new thr	reshold	in pre-	period	(and	vice	versa)

	New definit	tion, before reform	Old definition, after reform		
	(1)	(2)	(3)	(4)	
RD_Estimate	-0.240	-0.203	0.208	0.288	
	(0.228)	(0.217)	(0.245)	(0.243)	
Kernel	Uniform	Triangular	Uniform	Triangular	
Bandwidth	0.022	0.029	0.045	0.056	
N (left)	499	673	1,039	1,522	
N (right)	344	419	368	438	
p-value	0.292	0.349	0.394	0.237	
Robust p-value	0.290	0.298	0.501	0.303	

Standard errors in parentheses

\* p < 0.10,\*\* p < 0.05,\*\*\* p < 0.01

## **B.5** Mean comparison

	Banks with less than 20 LEX		Banks with at least 20 LEX		
	(1)	(2)	(3)	(4)	
RD_Estimate	-0.0196	0.00917	0.458***	0.309***	
	(0.121)	(0.113)	(0.170)	(0.113)	
Kernel	Uniform	Triangular	Uniform	Triangular	
Bandwidth	0.010	0.015	0.011	0.028	
N (left)	697	1,145	383	1,161	
N (right)	517	777	257	634	
p-value	0.871	0.935	0.007	0.006	
Robust p-value	0.689	0.736	0.009	0.020	

Table 9: A simple comparison of average interest rates around the LEX threshold

\* p < 0.10, \*\* p < 0.05, \*\*\* p < 0.01

### **B.6** Fixed effects

Table 10: RD results after absorbing fixed effects (uniform kernel)

	Average interest rate (new loans)		
	(1) Bank FE	(2) Industry FE	(3) Rating FE
RD_Estimate	0.724**	0.972***	$0.587^{*}$
	(0.310)	(0.369)	(0.327)
Kernel	Uniform	Uniform	Uniform
Bandwidth	0.019	0.018	0.029
N (left)	322	288	523
N (right)	196	180	277
p-value	0.019	0.009	0.073
Robust p-value	0.026	0.006	0.125

Standard errors in parentheses

\* p < 0.10, \*\* p < 0.05, \*\*\* p < 0.01

	Average interest rate (new loans)		
	(1) Bank FE	(2) Industry FE	(3) Rating FE
RD_Estimate	0.791*** (0.285)	0.942*** (0.318)	0.841** (0.337)
Kernel	Triangular	Triangular	Triangular
Bandwidth	0.025	0.028	0.029
N (left)	446	484	530
N (right)	246	262	281
p-value	0.006	0.003	0.013
Robust p-value	0.016	0.004	0.016

Table 11: RD results after absorbing fixed effects (triangular kernel)

\* p < 0.10, \*\* p < 0.05, \*\*\* p < 0.01

# **B.7** Logit regression

	Below LEX threshold	Above LEX threshold
	(1)	(2)
	$1{New bank}$	$1{New bank}$
$Distance_{t-1}$	-14.53*** (0.973)	2.735 (1.682)
$N(Banks)_{t-1}$	-0.228*** (0.0110)	-0.824*** (0.0625)
$\begin{array}{l} Distance_{t-1} \\ \times N(Banks)_{t-1} \end{array}$	2.078*** (0.124)	-0.890*** (0.228)
$Log(Borrowing)_{t-1}$	-0.200*** (0.0144)	-0.332 (0.312)
Firm FE	$\checkmark$	$\checkmark$
Time FE	$\checkmark$	$\checkmark$
N	196,596	5,595

Table 12: Firm-level regression: Logit model

\* p < 0.10, \*\* p < 0.05, \*\*\* p < 0.01