# The impact of bank regulation on the cost of credit: Evidence from a discontinuity in capital requirements

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**Abstract.** We study the effect on credit relationships of the Small and Medium Enterprises Supporting Factor (SME-SF), a regulatory risk weight reduction on small loans to SMEs. Employing a regression discontinuity design and matched bank-firm data from Italy, we find that a 1 percent drop in capital requirements causes an average 13 basis points reduction in the cost of credit. Moreover, with a novel measure of bank regulatory capital scarcity, we show that the drop is larger for banks facing tighter constraints. Furthermore, the drop is larger for firms with low switching costs, while the sharp assignment rule may have led to the rationing of marginal borrowers. Such findings indicate that the entire distribution of firms and banks' characteristics plays a crucial role in determining the impact of regulatory capital changes.

*Keywords*: Capital requirements, SME, Cost of credit, Credit access, Switching costs.

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# I Introduction

Bank regulators have employed minimum capital requirements to ensure bank solvency since the introduction of the Basel Accord framework in the 1980s. More recently, minimum capital requirements have become part of the macroprudential policy toolkit, which includes countercyclical changes in mandatory capital buffers to moderate lending booms in good times and mitigate lending busts in bad times (Claessens, 2015).

Minimum capital requirements aim to bring bank leverage closer to the socially optimal level. Banks may engage in excessive leverage because of moral hazard, either induced by limited liability and managerial discretion (Jensen and Meckling, 1976; Myers and Majluf, 1984), or by the distorted incentives arising from deposit insurance and the implicit or explicit government safety net. Imposing minimum capital requirements increases shareholders' stake, thereby reducing the ex ante incentive to gamble with insured deposits (Kareken and Wallace, 1978; Keeley, 1990).

If capital and debt are not perfect substitutes, capital requirements may come at a cost. If bank capital is more costly than debt (Diamond and Rajan, 2000), imposing minimum capital requirements may result in higher interest rates and reduced credit supply. Even though there have been many attempts at assessing the magnitude of such costs, for example through model-based simulations (e.g. Kashyap, Stein, and Hanson, 2010; Miles, Yang, and Marcheggiano, 2012) and through investigation of the effect of negative shocks to banks' capital (e.g. Berger and Udell, 1994; Peek and Rosengren, 2000; Behn, Haselmann, and Wachtel, 2016), little consensus has emerged.

We provide direct evidence on such cost in a quasi-experimental setting, exploiting the Small and Medium Enterprises Supporting Factor (SME-SF), a regulatory capital discount targeting small loans, aimed to shield European SMEs from the adverse effects of the tougher regulation coming with Basel III. From the impact of such discount on the spread on SMEs revolving credit facilities we infer that a 1 percentage point decrease in minimum capital requirements causes a 13 basis points drop in the cost of bank credit.

We exploit the unique framework, the availability of a rich set of firm-level proxies of switching costs, and a novel measure of banks' capital scarcity to gauge the extent to which the pass-through is heterogeneous across firms and banks. First, we show that: (i) borrowers with multiple healthy credit relationships drive the average effect; (ii) better borrowers (high EBITDA) get about a 40 percent larger discount; (iii) worse borrowers (high drawn over granted credit ratio or high risk score) get a 50 to 60 percent smaller discount. Second, banks whose shadow cost of capital is higher apply a 20 percent larger than average discount to their borrowers. Third, while there is no impact on credit quantities on average, credit to weaker borrowers grows less after the introduction of the SME-SF. Such evidence suggests that the sharp assignment cut-off could engender undesirable effects. Overall, we highlight that the effects of changes in capital requirements on credit depend on the entire distribution of borrowers' and lenders' characteristics.

We base our analysis on a rich dataset on bank loans to firms from the Italian Credit Register, matched with firm and bank characteristics and covering the period around the introduction of the SME-SF. The SME-SF entered into force on the 1st of January, 2014 through Article 501(1) of the Capital Requirements Regulation (CRR) and reduced risk weights of eligible exposures by 23.81 percent. Considering a corporate loan to an SME with a risk weight of 100 percent and a minimum capital requirement of eight percent of risk weighted assets, the reduction in the minimum capital requirement is approximately two percent.

Under the assumption that potential confounding factors do not change discontinuously at the € 1.5 million threshold, we employ the SME-SF eligibility rule to estimate the effect of capital requirements on lending rates with a Regression Discontinuity Design (RDD).[1] Through the RDD, we compare credit relationships that are very similar before the reform, but face different risk weights once the SME-SF is implemented. To support the validity of such a design, we provide evidence that firms', banks' and relationships' characteristics do not vary discontinuously at the SME-SF threshold, and that there is no bunching of credit relationships below the threshold in the two years before and the year after policy implementation. The absence of manipulation is not surprising as the estimated drop in the interest rates amounts to a saving in the annual cost of credit of a few thousand Euros for credit lines that are drawn for more than one million Euro. Moreover, we run placebo tests for non-SME and SME relationships before the SME-SF

[1] The approach, introduced by Thistlethwaite and Campbell (1960), is commonly both in labor economics and in empirical corporate finance. For a few example in the last field, see Chava and Roberts (2008), Keys et al. (2010), Agarwal et al. (2017), Rodano, Serrano-Velarde, and Tarantino (2018) and Becker, Opp, and Saidi (2021).

and find no evidence of spurious effects.[2]

Our baseline analysis shows an average 13 basis points decrease in the cost of credit for a 1 percentage point decrease in capital requirements. This average effect may only partially reflect the benefit accruing to banks from the marginal capital requirements relaxation for several reasons. First, if borrowers face significant frictions to switch between lenders, banks can exercise monopoly power and retain a fraction of the surplus. The change in the cost of credit will then reflect only such fraction, which in turn depends on the size of switching costs.[3] Second, the SME-SF may be more beneficial to the borrowers of banks whose regulatory capital is relatively scarcer. In the absence of the SME-SF these banks would raise the cost of credit more than capital-abundant banks as a result of the tighter Basel III regulation.

We exploit our rich dataset to analyze the relevance of these two heterogeneity dimensions. To test for the role of switching costs in influencing the degree of the pass-through across firms we employ different literature-based proxies. To measure the regulatory capital scarcity we instead employ a novel variable based on unique supervisory information at the bank level. When Basel III became effective, banks were given some time to adjust to the more restrictive capital definition. During the phase-in period they had to report to the supervisor what their capital would have been if the new definition were to be applied at once. We employ the difference between such fully phased-in capital and the transitory capital as our measure of relative scarcity. The intuition is that the larger the difference, the more significant an adjustment the bank had to do to revert to the desired buffer level by the end of the phase-in period (Repullo and Suarez, 2013).

Under the assumption that banks transfer the entire benefit of the capital discount to borrowers that enjoy low switching costs, we find that banks may be happy to pay up to 16 cents for each euroof regulatory capital saved. Moreover, we observe that banks

[2] As regards manipulation, we must also bear in mind that firm can only manipulate by not using credit. If they need credit, such a manipulation is costly.

[3] The importance of switching costs for the dynamic of credit and its cost has been documented, for example, by Ioannidou and Ongena (2010); Barone, Felici, and Pagnini (2011); Allen, Clark, and Houde (2019). For theoretical works on the effect of banks monopoly power on the cost of credit, we refer instead to Sharpe (1990) and Petersen and Rajan (1995). Moreover, as low switching costs should be an important driver of the pass-through, a fixed effect identification strategy would result in a larger effect due to sample selection. The pass-through would be identified only for firms with multiple relationships. These firms are likely to be less captive than single-bank borrowers and would therefore receive greater discounts (Ioannidou and Ongena, 2010). In the Appendix we demonstrate that this intuition holds true, documenting an increase in the point estimate when we restrict the estimation to the sub-sample of firm with multiple bank relationships and we show that such increase can be fully attributed to sample selection.

tend to increase granted credit less on those eligible lines whose utilization is closer to the maximum granted amount, although we do not find any significant average effect on the amount of credit granted. This result suggests that risk-weight rules incentivizing credit provision may have unintended effects on some groups of borrowers when based on sharp cut-offs. From a policy perspective, the subsequent decision of substituting the SME-SF cut-off with a smoother tapering of the discount was appropriate.

*Related literature*: This paper contributes to the literature on the impact of minimum capital requirements on the supply of credit to firms (Aiyar, Calomiris, and Wieladek, 2016; Behn, Haselmann, and Wachtel, 2016; Jiménez et al., 2017; Mayordomo and Rodríguez-Moreno, 2018) and the one trying to quantify the costs of capital regulation for banks (e.g. Kashyap, Stein, and Hanson, 2010; Miles, Yang, and Marcheggiano, 2012; Kisin and Manela, 2016; Plosser and Santos, 2018; Glancy and Kurtzman, 2022), providing a novel view into the distributive effects of capital regulation at the relationship-level.

Our assessment of the average pass-through is considerably larger than the one suggested by Kisin and Manela (2016), who derive the shadow cost of capital requirements from the extent to which banks exploit a costly loophole in regulation.[4] Our estimates are also larger than those presented in the two quasi-experimental studies by Plosser and Santos (2018) and Glancy and Kurtzman (2022). We suggest that this difference is due to the context of our study. In contrast with Plosser and Santos (2018), which exploits variation from Basel I and II, we focus on the European implementation of Basel III. Basel III introduced a more significant tightening of capital regulation, which may have had a non-linear effect on the cost of regulatory capital for banks. Regarding Glancy and Kurtzman (2022), we focus on the universe of banks operating in Italy, which may face higher funding costs than the large US banks on which they focus instead.

Uniquely, we highlight how the effect of capital regulation on the cost of credit depends on the entire distribution of firms' and banks' characteristics, lending support to the conclusions of recent theoretical works such as Ambrocio and Jokivuolle (2017); Bahaj and Malherbe (2020); Harris, Opp, and Opp (2020). We show evidence of how firms' cost of switching between lenders can influence the pass-through, suggesting a significant

[4] For a more in depth discussion of the modeling assumptions that are important to explain the small estimates by Kisin and Manela (2016) we refer to Plosser and Santos (2018)'s introduction. In brief, Kisin and Manela (2016)'s calculation assumes that banks can move on-balance sheet assets freely and at a low cost assets to off-balance sheet conduits; relaxation of such hypothesis may reconcile the discrepancy between our findings and theirs.

and under-explored link between the capital requirements literature and the literature on the effects of monopoly power within the context of credit relationships (for the latter, see Santos and Winton, 2008, 2019).[5] Furthermore, we document that banks expecting a greater drop in regulatory capital from the Basel III reform decrease rates more, showing a direct link between the heterogeneity in the shadow cost of capital and the pass-through.

Our approach to measuring the shadow cost, similarly to the one in Plosser and Santos (2018), does not rely on a difference-in-difference plus fixed effects strategy. The latter approach has been recently subject to methodological revision (see, e.g. De Chaisemartin and d'Haultfoeuille, 2020) and has been called into question in its corporate finance applications (Berg, Reisinger, and Streitz, 2021; Paravisini, Rappoport, and Schnabl, 2022).[6]

Finally, our results shed light on the effectiveness of risk weights as a policy instrument. Targeted changes in risk weights are increasingly being employed as a macro-prudential policy instrument(see, e.g. Altunbas, Binici, and Gambacorta, 2018). We add to the growing literature on the effects of such policies, e.g. Akinci and Olmstead-Rumsey (2018), Mayordomo and Rodríguez-Moreno (2018) and Lecarpentier et al. (2020) for the SME-SF in particular. While Mayordomo and Rodríguez-Moreno (2018) and Lecarpentier et al. (2020) study the effect of the SME-SF on credit access, we complement their analysis by studying the impact on the cost of credit. We also show evidence suggestive of an unintended effect, because exposures close to the eligibility threshold to firms with higher switching costs grow less than others. In this sense, our findings are in line with those of Becker, Opp, and Saidi (2021), who show that sharp changes in risk weights buckets applied to insurance companies' assets lead to strategic manipulation by intermediaries.

The paper proceeds as follows: Section II provides background information on Basel III, with special focus on the SME-SF and the transitory measure relaxing the novel capital standards; Section III describes our data; Section IV explains our identification

[5] As regards the effects of capital regulation, the only important exception we are aware of is Corbae and D'Erasmo (2021), which uses a large, general equilibrium model of dynamic monopolistic competition between lenders to track the effects of regulation on lending concentration and ultimately on the cost and availability of credit. A growing literature is instead tackling the importance of banks' monopoly power for the transmission of monetary policy, highlighting similar results (see, e.g. Agarwal et al., 2023; Drechsler, Savov, and Schnabl, 2017; Wang et al., 2020; Benetton and Fantino, 2021).

[6] Other empirical banking studies employing RDD techniques are Rodano, Serrano-Velarde, and Tarantino (2018), which studies access to credit over the cycle through a firm-level discontinuity in the assignment of credit ratings, and Becker, Opp, and Saidi (2021), which focuses on insurer's balance sheets instead of corporate loans and exploits risk-weight discontinuities at the instrument level.

strategy; Section V illustrates and interprets the results; Section VI concludes.

# II Institutional Background

The three key elements of capital requirements are: Minimum regulatory capital ratios, risk weights for each asset or asset class, and rules defining what counts as capital from a prudential perspective. After the Global Financial Crisis, the Basel Committee on Banking Supervision approved new capital standards (Basel III) with the purpose of increasing the quantity and quality of the capital buffer that banks need to hold against their risk weighted assets. The European Union adopted the new standards in June 2013, with application starting on January 1, 2014;[7] some of the measures entered into force immediately while others gradually.[8]

The framework put forth by the Basel Committee requires banks to hold at least 4.5 percent of risk weighted assets in Common Equity Tier 1 (CET1),[9] and increases the minimum Tier 1 capital requirement from 4 to 6 percent while leaving the overall requirement at 8 percent. Under Basel III banks are also required to hold two additional buffers: the Capital Conservation Buffer and the Countercyclical Capital Buffer. The first consists of an additional CET1 buffer of 2.5 percent of risk weighted assets; the second is a CET1 buffer that varies between 0 to 2.5 percent of risk weighted assets depending on cyclical conditions in the credit market.[10] Finally, the new rules tightened the capital definitions, to grant uniform, high quality buffers for loss absorption.

Considering that under the previous framework (Basel II) banks were required to hold

[7] See the European Commission's Online References at https://ec.Europa.eu/info/law/banking-prudential-requirements-directive-2013-36-eu_en.

[8] On Basel III and its implementation, see the Basel Committee's "Basel III: A global regulatory framework for more resilient banks and banking systems" at https://www.bis.org/publ/bcbs189.pdf, and their updated summary in "High-level summary of Basel III reforms" at https://www.bis.org/bcbs/publ/d424_hlsummary.pdf.

[9] The definition of CET1 includes "Common shares issued by the bank that meet the criteria for classification as common shares for regulatory purposes (or the equivalent for non-joint stock companies); Stock surplus (share premium) resulting from the issue of instruments included CET1; Retained earnings; Accumulated other comprehensive income and other disclosed reserves; Common shares issued by consolidated subsidiaries of the bank and held by third parties (ie minority interest) that meet the criteria for inclusion in CET1 capital [...] and Regulatory adjustments applied in the calculation of CET1" (Basel Committee, 2011, Global Regulatory Framework Report's p.13). Additional Tier1 includes other types of shares; Tier2 capital includes some subordinated debt instruments.

[10] These figures are the fully phased-in buffers; the time-line of implementation is described in the "Basel III phase-in arrangements" document by the Basel Committee at https://www.bis.org/bcbs/basel3/basel3_phase_in_arrangements.pdf. We will provide more details regarding the transition to the new regulatory regime in the last part of this Section.

an overall 8 percent capital buffer, while under the new fully phased-in rules the buffer would be at least 10.5 percent, European banks and other stakeholders raised the concern that the reform would lead to an excessive tightening of the credit supply, particularly to SMEs, hampering the recovery of the EU economy.[11]

In response to this concern, the EU capital regulation adopting Basel III in the EU (Capital Requirements Regulation - Capital Requirements Directive IV, CRR-CRD IV henceforth) introduced measures to smooth the transition. We will make avail of two such measures in this study: The Small and Medium Enterprise Supporting Factor (SME-SF), which will help us identify the effects of capital regulation changes; the transitory regime for the adoption of the more stringent definition of capital, which will help us investigate bank-level heterogeneity.

## II.1 The SME Supporting Factor

The SME-SF is a 23.81 percent discount on the risk weight that applies to loans granted to firms with turnover below € 50 million, provided that the total exposure of the lender to each eligible firm is below € 1.5 million. The magnitude of the SME-SF exactly counteracted the maximum overall increase in capital requirements implied by the additional Capital Conservation Buffer.[12]

The Capital Conservation Buffer was gradually phased in between 2016 and 2019, but the SME-SF became effective on January 1, 2014. Consequently, capital requirements for outstanding and new eligible exposures to SMEs were de facto lowered with respect to the pre-CRR/CRD IV framework. To give an example of the SME-SF effect on minimum capital requirements, we consider an average capital requirement of 8 percent and a pre-SME-SF risk weight of 100 percent. After the implementation of the SME-SF, the minimum capital requirement on an SME's credit line utilized for € 1.6 million would be unchanged at € $128,000$. Instead, the minimum requirement on a € 1.4 million SME exposure would amount to € $85,000$, taking the SME-SF into account. Such stark change in minimum capital requirements at the SME-SF eligibility threshold provides ground to

[11] For a more detailed comparison between the Basel II and Basel III regimes, we refer to Gatzert and Wesker (2012). Regarding the concern of European stakeholders about the strictness of Basel III's rules, see Recital 44 of the "Regulation (EU) No 575/2013 of the European Parliament and of the Council" available at https://eur-lex.Europa.eu/legal-content/EN/TXT/?uri=celex%3A32013R0575.

[12] A 23.81 percent reduction in a pre-reform risk weighted exposure of 100 would exactly compensate for the increase in the capital ratio: from the $0.08*100$ implied by Basel II, to the equivalent $0.105*76.19$ under the fully phased-in Basel III regime.

expect an effect on loan pricing.

Anecdotal evidence suggests that the SME-SF did influence credit supply for targeted SMEs. According to the Intesa San Paolo Bank[13] response to the Call for Evidence on the SME-SF by the European Banking Authority (EBA):

> Despite being difficult to quantify the exact price reduction triggered by the application of the SMEs supporting factor, a direct relation between the SMEs SF and the credit price is easy to draw as the cost of regulatory capital is one of the key components of the credit pricing models. The possibility of applying the SF on the eligible SMEs exposures significantly reduces the cost of regulatory capital for such exposures; this capital relief ensures a direct (positive) effect of the SF on the credit price for SMEs borrowers.

In the same vein, the German Banking Industry Committee responded that:

> The SMEs Supporting Factor reduces own funds requirements and cuts the cost of capital. This is all the more important the higher interest rates climb, because customer price sensitivity then also increases. If interest rates are expected to rise, cost of capital is thus likely to become more important [...] A lower cost of capital increases profit margins and makes SME loans more attractive.

Even so, the initial effort by the EBA (EBA, 2016) to evaluate the effect of the SME-SF on lending has returned no strong evidence in favor of an immediate effect. However, the EBA's analysis is based on survey data and, for this reason, it cannot fully disentangle supply from demand, or account for the confounding effects of other aspects of Basel III implementation in Europe.

Two recent studies tackled such identification problems using micro-data, and both found evidence of a positive effect of the SME-SF on lending. The first, Mayordomo and Rodríguez-Moreno (2018), finds that the SME-SF contributes to easing credit constraints of medium-sized firms. The second, Lecarpentier et al. (2020), finds instead a lagged, positive overall effect on credit supply, stronger for very small loans of small and micro firms. As both these works find evidence of an effect on credit supply conditions, we argue

[13] One of the largest Italian banking groups.

that the SME-SF provides a promising testing ground to improve our understanding of the effects of minimum capital requirements regulation. In particular, the effect of the SME-SF on loan rates is still not explored. Our objective in this paper is the investigation of this aspect, which gives us a chance to learn more about the broader issue of the cost of capital requirements to banks.

## II.2 The New Capital Ratio and the Transitory Regime

The Basel III reform aimed to increase the quality of capital by tightening the definition of the highest quality capital, i.e. CET1. First, CET1 is distinguished from additional Tier 1 capital, the latter being constituted by all unsecured and perpetual non-common share instruments. Second, Under Basel III certain items must be deducted from CET1 capital (intangibles, deferred tax assets, gains from securitization transactions, cross-holdings, and investments in the capital of financial institutions out of the scope of regulatory consolidation). Finally, the new regime requires banks to hold greater Tier 1 buffers against third parties' equity and securitization exposures.[14]

An immediate application of the new definition of high quality capital would have been tough on banks. For example, the EBA (2014) Basel III monitoring exercise reports that the CET1 ratio of large banks (Tier 1 capital greater than € 3 billion and internationally active) would have dropped from 11.9 to 9.1 percent if the new rules were applied altogether. For all the other banks the CET1 ratio would have dropped even more, from 12.4 to 8.8 percent.

To avoid an abrupt drop in the capital ratio's numerator, the Basel Committee (2011) Global Regulatory Framework Report recommended a gradual phase-in of the new capital definition (Section C, paragraphs (c) and beyond).[15] The implementation of the phase-in was left to national regulators. The European Union's CRR Article 478[16] established the deadlines for the implementation of the transitory framework. The Bank

[14] Under Basel II, these exposures qualified for either a 50 percent deduction from Tier 1 and 2 capital or favorable risk weighting. Under Basel III, they require instead further buffer accumulation and are risk-weighted at 1,250 percent. For the full Basel III definition of Tier 1 capital, we refer to Basel Committee (2011, p.15-16) and to Basel Committee (2011, p.21-27) for the complete list of mandated deductions.

[15] Paragraph (d), p.28, suggests a broad time frame for adoption, asking banks to comply with "0% of the required deductions on 1 January 2014, 40% on 1 January 2015, 60% on 1 January 2016, 80% on 1 January 2017, and reach 100% on 1 January 2018".

[16] Available at the European Banking Authority's https://www.eba.Europa.eu/regulation-and-policy/single-rulebook/interactive-single-rulebook/1072.

of Italy's instructions reflected the guidelines and broadly matched the Basel Committee suggestions.[17] We refer to capital ratios computed under these transitory rules as the "transitory" capital ratios.

The Bank of Italy's supervisory reports contain detailed information on the transitory and the fully phased-in capital ratios for Italian banks. We employ this information and measure the hypothetical drop in regulatory capital each bank would face in the absence of the transitory regime as the difference between the two. We provide details on this variable's construction in the next section. We interpret it as the "distance" that each bank would have to go to meet the new minimum requirement and on top of that restore its desired capital buffer.

# III Data and Measurement

We construct our dataset by matching information on loan quantities and interest rates from the Italian Credit Register and the Bank of Italy archive on interest rates (TAXIA). In addition, we source balance sheet data on borrowers from the provider Cerved and balance sheet information on lenders from the Supervisory Files on banks and banking groups.

The Italian Credit Register contains detailed monthly information on all loans issued by banks and other credit intermediaries above the minimum threshold of € $30,000$, irrespective of whether disbursed or not. TAXIA includes information on interest rates on loans to borrowers that have at least € $75,000$ overall granted or disbursed credit, reported by all but the smallest banks. The TAXIA sample is highly representative as the aggregate value of loans of reporting banks is about 80 percent of outstanding credit. Interest rates are the actual rates paid by each borrower on disbursed credit net of commission and fees. Finally, Cerved is a proprietary database containing firms' balance sheet information and a credit score; total credit to Cerved firms covers about three fourths of loans by Italian banks to the nonfinancial corporate sector.

We obtain such information for the years $2013-2014$ to investigate the impact of the reform and for years $2012-2013$ to run placebo tests. We focus on revolving credit

[17] Circular 285 of December 2013 available at the Bank of Italy's https://www.bancaditalia.it/compiti/vigilanza/normativa/archivio-norme/circolari/c285/Circ_285_pub.pdf.

lines' interest rates because, in Italy, these loans are relatively standardized and not collateralized. Moreover, banks can modify the rate on these loans on short notice. Finally, we adjusted our dataset for bank mergers by applying the group structure of 2014 to 2013 and 2013 to 2012. We focus on the top-tier bank holdings for i) capital requirements concern the consolidated entity and ii) group exposure defines SME-SF eligibility.

Our measure of the change in the cost of credit between the pre and the post-SME-SF introduction is the difference between the average rate paid in 2014 and 2013 - winsorized at the upper and lower 2.5 percentile to mitigate the effect of outliers. In addition, we consider a yearly time window as we do not observe when credit lines are re-bargained but only the resulting change in the rates paid. Hence, we want to encompass a period that is long enough to include changes in the cost of the credit line and short enough that it can be reasonable to attribute changes to the implementation of the SME-SF.

## III.1 Defining Eligibility for the SME-SF

The SME-SF applied to exposures below € 1.5 million towards firms with gross sales below € 50 million, excluding any amount collateralized by residential real estate.[18] First, we identify eligible firms employing the data on gross sales from the Cerved database.[19] In a given year, we assess firm size using gross sales in the previous year, which is the latest figure that banks can observe as the current balance sheet will be released several months after the closure of the fiscal year.

We then employ the Credit Register data to identify SMEs' credit relationships below the exposure threshold. Eligible relationships are those for which the total credit disbursed is below 1.5 million, regardless of the amount granted. We assess eligibility at the end of period $t-1$ when analyzing the change in loan rates from $t-1$ to $t$. For example, we assess the total exposure of credit relationships as of December 31, 2013, while in the placebo tests as of December 31, 2012. The eligibility status is thus a proxy for being "treated" with the SME-SF. First, we notice that this is the best we can do,

[18] For example, if a bank grants a €5 million loan and the firm posts residential real estate collateral covering € 4.2 million, the risk weight discount would apply because the exposure net of the collateral is below the threshold.

[19] This criterion is only one of the three that the European Commission follows to define an SME in other contexts; the other two are that an SME must employ less than 250 employees and hold less than € 43 million in assets (see the EU recommendation 2003/361 by the European Commission, available at https://eur-lex.Europa.eu/legal-content/EN/TXT/?uri=CELEX:32003H0361).

as banks do not report treatment status of each credit relationship. Moreover, as long as the correlation between this proxy of treatment assignment and actual treatment assignment is positive and large enough, the effect of mismeasurement will be the attenuation of our estimates. As credit utilization is sticky, and we estimate that lowering capital requirements lowers the cost of treated credit relationships, the above assumption is the most credible.[20]

We take a number of steps to limit the scope of the mismeasurement concern. First of all, we rely on the fact that, according to the regulation, each bank has to verify the eligibility status of its borrowers and report the amount of SME-SF eligible loans to the supervisors on a quarterly basis.[21] This implies that at the end of the first quarter of 2014 banks without policies to track SME-SF eligibility can be distinguished from banks with such policies. Using this information, we drop banks that do not report any SME-SF exposures.

On the firm side, we drop relationships involving firms whose SME status appears uncertain – which is, firms with either low revenues but assigned to the size-class "large" or vice-versa; firms assigned to the size-class "large" which report assets below the 43 million € threshold employed by the EU to define non-SME firms; SMEs that appear to hold a very large number of relationships (more than 11, the last percentile of the number of relationships' distribution).[22] Furthermore, we restrict our attention to good-standing relationships, as the SME-SF only applies to performing borrowers. We also drop collateralized relationships because we cannot distinguish between residential and commercial real estate collateral.[23] Finally, we exclude firms with deeply negative (< -20

[20] Intuitively, if lowering capital requirements leads to a decrease in rates and our proxy of treatment is extremely bad, we could estimate a significant *increase* in the cost of credit for what we consider eligible relationships. For an extreme example of how this could happen, we can think of the case in which all observations below the threshold at the end of December 2013 are not assigned to the SME-SF and vice-versa. As we estimate a significant *drop* in rates, we find at worst a lower bound for the actual effect on rates of lowering capital requirements. There is comparatively little work on measurement error in RDD settings when no additional information regarding treatment status is present. One recent paper systematically addressing the topic is Indarte (2023), to which we refer for a deeper discussion.

[21] The more detailed account we could find about the assessment of eligibility is in the answer to question 2013_417, submitted by an undisclosed bank to the EBA, available at https://eba.Europa.eu/single-rule-book-qa/-/qna/view/publicId/2013_417. The EBA indicates that the requirement must be fulfilled on an ongoing basis, though, the reporting constraint implies that banks must "report to competent authorities every three months their total SME exposures, on the basis of adequate current information".

[22] For example, these may be branches of larger firms and conduits for their parent companies' credit access.

[23] The selection concerns due to this restriction are limited as we focus on revolving loans, which are usually not collateralized.

%) or extremely large EBITDA (> 200 %) or with extremely high (>200 %) or negative leverage. Indeed, extremely high EBITDA may imply deficient assets, while negative leverage indicates extremely negative equity (as we define leverage as debt over debt plus equity). Such firms are likely to either have misreported balance sheet figures – thus, we cannot be sure about their SME status – or are close to default and would add noise to the treatment assignment. Again, banks cannot apply the capital requirement discount to nonperforming borrowers.

## III.2 Explanatory Variables

Our dataset includes information on relationships, borrowers, and banks' characteristics that could influence loan interest rates. We use these variables for three purposes. The first is to verify that there are no discontinuous changes in observable characteristics at the SME-SF eligibility threshold; the second, which we discuss in the Appendix, is to increase the precision of our estimate of the impact of the SME-SF (Angrist and Rokkanen, 2015). Finally, we employ some of these variables to explore the heterogeneity in the SME-SF pass-through across firms or banks.

The first set of variables captures the nature of the relationship between the firm and the bank. It includes the lagged ratio of credit disbursed by bank $b$ to firm $f$ to total credit utilized by firm $f$, which proxies for the importance of the $bf$ relationship to $f$; the lagged ratio of loans utilized to loans granted for each $bf$ relationship, which proxies for the amount of slack that $f$ has in the relationship with $b$; the lagged ratio of revolving credit granted to total credit granted on each $bf$ relationship, which captures the intensity of the relationship, as revolving credit lines generate soft information on the firm (Berlin and Mester, 1999).

Moreover, we include a proxy for the distance between the bank and the firm, using a dummy indicating whether the firm and bank locate their headquarters in the same province. The literature finds that proximity captures the availability of soft information about the firm, which lowers screening and monitoring costs for the bank.[24] We also include the duration of the relationship, a standard proxy for relationship intensity; duration is the number of years we observe the bank-firm pair, and it is truncated at a

[24] For example Degryse and Ongena (2005) and more recently Agarwal and Hauswald (2010) all find that distance is an important factor in determining credit conditions faced by firms.

maximum value of nine because the reports from which we extract our dataset start in 2005.

We collect variables related to credit risk and other firm characteristics, including profitability, leverage, and liquidity, which banks consider when setting interest rates. We measure profitability as gross operating profits, scaled by total assets (EBITDA Ratio); liquidity as liquid assets scaled by total assets; leverage as the ratio of debt to the sum of debt and equity. Furthermore, to capture credit risk we include a score based on the methodology proposed by Altman (1968), computed by Cerved. The score takes values from one to nine, increasing in credit risk. In particular, we focus on a dummy identifying firms with scores above six, considered risky in the Cerved methodology. To proxy for industry and region-specific characteristics, we include industry dummies based on the two-digit Statistical Classification of Economic Activities adopted by the EU,[25] and region dummy variables for the location of the firm's headquarters (North West, North East, Center and South). Finally, we track whether firms own only one credit relationship in good standing or many, to identify captive customers.

Finally, we collect data on banks' characteristics that are likely to influence the cost of loans, particularly funding and capitalization. We construct the following bank variables: the Tier 1 capital ratio, the ratio of liquid assets to total assets, the fraction of assets financed with retail sources, and the fraction of assets financed with wholesale sources, excluding central bank funding. We also include the log of total assets to control for bank size. We use data from the capital section of supervisory reports to compute the difference between banks' transitory CET1 ratios (as of March 2014) and the fully phased-in ones. This measure, whose legal details we explained in Section II.2, will be key in studying bank-level heterogeneity in the impact of the risk-weight discount.

## III.3 Data Description

Matching firms from Cerved and loan data from the Credit Register yields approximately $515,000$ bank-firms pairs for 2014, of which $236,500$ have information on interest rates. Among these, $230,000$ are eligible relationships of eligible firms (most Italian firms are SMEs); approximately $6,500$ observations are instead non-eligible relationships of eligible

[25] See the EuroStat glossary available at https://ec.Europa.eu/Eurostat/statistics-explained/index.php/Glossary:Statistical_classification_of_economic_activities_in_the_European_Community_(NACE) for details.

firms. We also keep data on non-eligible firms to run placebo regressions.

In Figure 2, we show the scatter plot of observations around the two SME-SF assignment thresholds (firms turnover and exposure). The plots show that, although significantly fewer observations refer to large firms with large amounts of disbursed credit (the fourth quadrant in Figure 1), observations' density decreases continuously with size. There are no evident "holes" in our coverage of the treatment space.

We report descriptive statistics regarding SMEs' credit and balance sheet characteristics in Table 1, after the SME-SF implementation (2014, left panel) and before (2013, right panel, a placebo sample). The information on the changes in interest rates refers to average spreads against the quarterly inter-bank rate year-on-year. At the same time, we measure the control variables as of the end of 2013 for the SME-SF implementation sample, and as of the end of 2012 for the placebo sample.

Descriptive statistics for the bank characteristics show that, on average, banks' balance sheets became stronger over time as Liquidity and the CET1 ratio increased, while Retail Funding stayed constant. Although interest rates increased more in 2014 than in 2013, relationship data suggest that only in 2014 the cost of credit increased less on eligible than on non-eligible relationships.[26] All firm characteristics stayed similar in the two periods for both firms with and without eligible credit relationships.

Interestingly, the cost of non-eligible lines increased by 11 basis points more than that of the eligible lines in 2014. In 2013 the rate on non-eligible lines increased by 7 basis points less than on eligible lines. We can derive a rough approximation of the impact of the SME-SF, which turns out to be a reduction of around 18 basis points for treated credit lines, quite close to the result we obtain in Section V. Nevertheless, estimating the effect by regression discontinuity is crucial, as the result of this back-of-the-envelope calculation may well reflect differences between large and small lines that are unrelated to the SME-SF.

For example, we can observe that eligible relationships are younger, have a higher share of revolving loans, and have a lower drawn-to-granted ratio. Such heterogeneity may confound our pass-through estimates if not dealt with appropriately. In the next Section, we describe our approach to estimating the causal effects of the SME-SF based

[26] The quarterly inter-bank rate sharply decreased at the end of 2014, which explains the especially large difference in the change in spreads. This change does not affect any result, as it gets differenced out in any comparison we perform.

on RDD, and discuss evidence supporting its validity.

## IV Empirical Strategy

The ideal experiment to elicit the effect of the SME-SF on the cost of credit would consist of the random assignment of the risk-weight discount to credit relationships of a set of identical firms $f$ borrowing from identical banks $b$. The difference in the cost of credit between treated and untreated relationships would measure the effect of the SME-SF.[27] The design of the SME-SF allows us to approximate this ideal, as we can exploit it for an RDD. The RDD controls for demand confounders affecting different relationships of the same firm and supply confounders affecting different relationships by the same bank. This approach improves on including firm and bank fixed effects and considering all relationships irrespective of their size.[28] For example, the flexibility of the RDD approach allows us to directly address the identification concerns grounded in the firm and bank-level heterogeneity highlighted in Paravisini, Rappoport, and Schnabl (2022).

Consider a set of banks $b = 1, ..., B$ who lend to firms $f = 1, ..., F$; each firm $f$ can borrow from different banks. There are two periods, before and after the introduction of the SME-SF. For each bank-firm relationship, the bank pricing function is:

$$i_{bft} = f\Big(M_{bf}, D_{bft}, S_{bft}, R_{bft}\Big) \tag{1}$$

where $M_{bf}$ represents all the determinants of the cost of credit that are constant over time and specific to the relationship. $D_{bft}$ collects time-varying credit demand confounders, possibly impacting relationships differently (thus $bft$); $S_{bft}$ denotes time-varying credit supply confounders. Finally, $R_{bft}$ is the regulatory capital charge, i.e., the amount of regulatory capital the bank $b$ has to set aside at time $t$ on a loan granted to firm $f$.

Suppose we try to estimate the effect of a change in $R_{bft}$ on the cost of credit by the following linear regression, specified in changes to mute variation due to unobservable, static components:

[27] The treatment is the SME-SF; treated observations are credit relationships eligible to the SME-SF policy, and vice-versa for the non-treated.

[28] Many papers in empirical banking use firm fixed effects for identification. A seminal work is Khwaja and Mian (2008); other studies are Jiménez et al. (2012), Schnabl (2012), Jiménez et al. (2014), Jiménez et al. (2017).

$$\Delta i_{bf} = \alpha + \beta R_{bf} + \epsilon_{bf} \tag{2}$$

where the effect of the SME-SF would be captured by the coefficient $\beta$ of a dummy $R_{bf}$ equal to 1 if the risk weight applied to the loan to firm $f$ by bank $b$ at time $t = post$ benefits from the SME-SF (the relationship $bf$ is treated), 0 otherwise (the relationship $bf$ is not treated) and $\epsilon_{bf}$ is the residual term. From Equation 1, assuming linear separability in error components, we see that $\epsilon_{bf}$ includes three elements: $\epsilon_{bf} = D^*_{bf} + S^*_{bf} + e_{bf}$, where $e_{bf}$ is the true idiosyncratic error component.

The presence of demand and supply factors in the residual may cause bias for different reasons. First, it may be that $\text{cov}\left(S^*_{bf}, R_{bf}\right) \neq 0$ as, for example, eligible relationships tend to involve small banks. Since small banks typically do not employ internal risk weighting models (Behn, Haselmann, and Wachtel, 2016), they are more affected by the Basel III reform, and their customers should experience a larger increase in interest rates, perhaps offsetting the benefit from the SME-SF. Consequently, $\hat{\beta}$ would be biased downwards by the non-random matching between firms and banks. Moreover, it is likely that $\text{cov}\left(D^*_{bf}, R_{bf}\right) \neq 0$. A firm that borrowed more than 1.5 million before the SME-SF implementation is likely to experience a higher demand for credit than a similar firm that did not. If demand shocks are positively correlated over time, a higher incidence of interest rate increases for firms with non-eligible credit lines will bias upwards $\hat{\beta}$. Finally, even if we focused on firms with multiple relationships, we would include very heterogeneous observations (some very small, some large loans) in the comparison. Banks might be pricing large loans differently, and firms might withdraw more credit from "preferred" relationships in case of demand shocks, while holding some backup credit lines (Detragiache, Garella, and Guiso, 2000; Sette and Gobbi, 2015).

The RDD overcomes the issue of comparability because it is a local approach. It exploits an arbitrary threshold on a continuous variable that defines the treatment status. Suppose that bank-level and firm-level confounding factors do not vary *discontinuously* around the threshold. In that case, we can use the untreated relationships close to the threshold as counterfactual for the treated relationships close to threshold and attribute any discontinuous change in the cost of credit to the SME-SF.

As stated in Section III, eligibility to the SME-SF is based on a bi-dimensional assignment rule (see Figure 1) that takes into account firm's gross sales (turnover) and the credit drawn by the firm from the bank. The turnover threshold is part of the criteria that define an SMEs as per EU law. We thus cannot use it for identification, as other confounding factors vary discontinuously at this threshold. Instead, we focus on SMEs (i.e., firms below such turnover threshold) and implement an RDD around the € 1.5 million threshold.

Conditional on meeting the turnover eligibility criterion, the treatment probability changes sharply at the threshold:

$$x_{bf} = \text{drawn credit}$$

$$\text{Eligibility}_{bf} = R_{bf} = \begin{cases} 1 & \text{if } x_{bf}^{2013} \leq \bar{x} = \text{Euro 1.5 million} \\ 0 & \text{otherwise} \end{cases}$$

The change from 1 to 0 of the treatment probability defines a sharp RDD. The resulting equation is:

$$\begin{aligned} \Delta i_{bf} &= a + \phi(|x_{bf}^{2013} - \bar{x}|) + \beta R_{bf} + \nu_{bf} \\ \text{Estimated on } bf &: \; x_{bf} \in [\bar{x} - h^-, \bar{x} + h^+] \end{aligned} \tag{3}$$

where $h^-, h^+$ delimit the bandwidth of choice; $a$ is a common intercept; $\phi(.)$ summarizes smooth polynomial components in the distance from the threshold; $\beta$ is the parameter of interest, measuring the effect of the treatment; $\nu_{bf}$ is a stochastic error component.

To estimate $\beta$ in Equation 3, we follow Calonico, Cattaneo, and Titiunik (2014); Calonico et al. (2017) and compute $(\hat{\beta}, \hat{\phi}, h^{+/-})$ minimizing the mean square error of a local polynomial regression. We choose a flexible bandwidth, different on the two sides of the threshold, to account for how observations' density decreases with loan size. Automating the bandwidth choice reduces our degrees of freedom, while Calonico et al. (2017)'s routine ensures that we correct our estimates for the bias introduced by bandwidth selection.[29]

## IV.1 Validity of the RDD Design

The validity of RDD depends on the relatively weak assumptions of continuity of all possible confounders at the assignment threshold and of no perfect manipulation of the

[29] In the Appendix, we provide details on the estimation procedure (Section A.1) and show how our choice is conservative, and our results do not depend on it (Section A.2).

assignment variable by treatment takers.[30] Given the richness of our data, we can take several steps to show that concerns about the RDD validity are reasonably limited. We provide two types of evidence to support our assumptions. The first is the direct evidence of the lack of assignment variable manipulation (McCrary, 2008); the second is the evidence of continuity of relevant exogenous variables at the threshold (see, e.g. Lee and Lemieux, 2010). Any evidence of manipulation or discontinuity in covariates would raise the concern of sorting around the threshold, invalidating the design.

**Manipulation**: If the subjects under study knew of the treatment before its introduction and could *perfectly* manipulate drawn credit, they could sort on their preferred side of the threshold. Then, sorting could correlate with unobservables, and these may vary discontinuously at the threshold confounding the policy's effect.

In principle, one could argue that more informed firms anticipated the policy and adjusted their credit demand to stay below the eligibility threshold and benefit from the capital charge discount. If these firms were better managed - they were promptly aware of relevant policy changes - they would also be plausibly able to negotiate lower interest rates for reasons other than the SME-SF. Alternatively, banks that faced a capital shortage might inform their corporate borrowers of the SME-SF, encouraging them to lower their exposure to bring it below 1.5 million.

A first counterargument is that, in practice, the demand for credit of firms is subject to unforeseen shocks that can move marginal credit relationships from one side to the other of the SME-SF eligibility threshold. Exposure (drawn credit) defines eligibility, and the notion of exposure includes contingent liabilities such as guarantees and letters of credit provided by banks. Evidence that firms hold significant amounts of unused credit lines to meet unexpected needs supports the idea that the demand for cash is, to some extent, unpredictable. Indeed, our sample's average ratio of credit disbursed to credit granted is about 60 percent. Perfect manipulation would be difficult and costly. For example, a machine that breaks and needs repairs causes a lumpy need for cash. If the firm must cover the expense with debt, it may not always be able to cut its overall credit demand by the amount that keeps its credit relationship SME-SF compliant. Doing so may imply not executing the repair and delaying production.

[30] Manipulation of the assignment variable would imply that manipulators are on one side of the threshold, violating the continuity assumption. For technical details, see Hahn, Todd, and Van der Klaauw (2001).

A second counterargument is that, even if firms could manage their exposure precisely at all times, perfect manipulation would require ex-ante knowledge of the *exact* eligibility threshold. We note that before the approval of the SME-SF, there was considerable uncertainty about the eligibility rules. Although the discussion on the SME-SF began in 2012, regulators initially considered "a reduction by one third of the risk weight for the retail exposure class and an increase of the threshold for retail from € 1 million to € 5 million for SMEs" (EBA, 2016). The 1.5 million exposure threshold appeared in the final draft, approved on the 26th of June, 2013,[31] but banks were uncertain about the criteria they had to follow until 2014.[32] We can thus conclude that banks were unlikely to be ready to identify eligible exposures early enough to incentivize many marginally ineligible customers to reduce their exposure below the threshold before the change becoming effective.

We test for manipulation following McCrary (2008) to support our case. When the incentive to manipulate goes in one clear direction, a discontinuity in the density of observations around the threshold should be visible. If firms prefer to be eligible and there are enough informed firms, we should observe significantly fewer marginally non-eligible relationships than the marginal eligible ones. A simple density test can highlight a statistically significant drop in the density just above the SME-SF threshold.

We run the test on the drawn credit outstanding density for December 2012, 2013, and 2014. Hence, we search for potential manipulation from the time of the first public mentions of the SME-SF until its implementation *immediate aftermath*. Checking for the aftermath of the policy implementation is important, as borrowers capable of reducing ex post their credit take up to reap the SME-SF benefit would be of arguably greater quality, biasing the treatment effect estimate upwards. The test does not detect any statistically

[31] The SME-SF timeline is: The first official record in a "proposal for a regulation of the European Parliament and of the Council on prudential requirements for credit institutions" dated 12th of June 2012, in which a 2 million limit was discussed (at http://www.Europarl.Europa.eu/sides/getDoc.do?type=REPORT&reference=A7-2012-0171&language=EN#title1); the proposal was assessed by the EBA in September 2012 (EBA, 2012), which focused on the possibility of increasing the retail threshold to € 2 million for banks calculating their capital requirement with the Standard Approach, and to € 5 million for banks calculating their capital requirement with the Internal Ratings Based Approach; the Commission proposal was then brought to final debate in the European institutions during spring 2013; the reform was finally approved in June 2013.

[32] As in Section III, we refer to the EBA Q&A, which included questions submitted until the 27th of November 2013, and to which answers were provided well into the 2nd quarter of 2014 (see the EBA's Q&A at https://eba.Europa.eu/single-rule-book-qa/-/qna/view/publicId/2013_565 and https://eba.Europa.eu/single-rule-book-qa/-/qna/view/publicId/2013_417).

significant discontinuity in the density of observations at the threshold, as shown in the different panels of Figure 3 and by test statistics reported in Table 2.

Such lack of manipulation is not surprising in light of the empirical findings we present in the next Sections. Our estimates suggest that with an average of 26 basis points drop in the cost of credit (see Table 3, Section V), the saving on a credit line manipulated to fall below the € 1,5 million threshold would stand around € 3,900 a year.[33] Even a firm at the top of the effects' distribution, say with a previous EBITDA over asset ratio a standard deviation above average and dealing with a bank with a high shadow cost of regulatory capital (see Table 4, Section V, implying a 40 basis points discount for such a relationship), would get a mere € 6,000 discount on the yearly cost of revolving credit.

We can see that such a number is small by computing the average yearly cost of credit for firms with at least one revolving credit line with drawn credit between the SME-SF threshold and € 1.6 million as of December 2013. For Italian standards, these are medium-sized firms with multiple credit lines, and their total credit drawn at the firm level is approximately € 6 million. The average yearly revolving rate for such firms is 8%, and thus the annual average cost of revolving lines hovers around € 500 thousand. Even in the best-case scenario, a manipulating firm would only save 1.2% of its average yearly cost for revolving lines. Therefore, we are not surprised by the lack of manipulation in the year immediately after the SME-SF implementation.

**Discontinuity of covariates**: Even in the absence of evidence of manipulation, it could be possible that relationships, firms, or banks with specific characteristics are more likely to appear on one side of the threshold than the other. We estimate a version of Equation 3, replacing the dependent variable with each of the relationship, firm, or bank variables described in Section III, to dispel this doubt. In Figure 4, we plot the discontinuity estimates (black diamonds) and confidence intervals (gray shaded areas) for linear RD specifications targeting different bank, firm, and relationship-level characteristics.[34] The results do not support the existence of discontinuities at the SME-SF threshold for any of the variables considered.

[33] This number equals the delta rates we estimate times a € 1,49 million credit line utilization.

[34] We report point estimates in Table A1, including results from a simple comparison of means and a second-degree polynomial, showing that continuity is specification-robust.

# V Results

We inspect the behavior of interest rate changes around the SME-SF eligibility threshold. To do so, we show in Figure 5 fit and confidence intervals from local kernel regressions of changes in interest rates on past credit utilization at the relationship-level, in a neighborhood of the SME-SF threshold.[35]

The plots show that in 2014 interest rates increased on average, most likely because the implementation of Basel III increased the overall cost of credit. More importantly, only in 2014 and for the SMEs sample is there evidence of a discontinuity in the interest rate changes at the policy threshold. In 2014, the cost of credit appears to grow more for relationship not eligible to the SME-SF than for their eligible counterparts. Local kernel regressions on 2013 data, or at placebo thresholds inspected at the same time as the SME-SF implementation, or for non-SME, do not show comparable and statistically significant “jumps” in rates.

This evidence suggests that the policy change had an effect. Still, to get a precise idea of the significance and magnitude of the effect we need to correct our discontinuity estimates and confidence intervals for bandwidth selection bias (Calonico, Cattaneo, and Titiunik, 2014). We display results in the first row of Table 3, using a simple comparison of means (degree 0 polynomial), local linear, and quadratic polynomials.[36] Our estimates show a sharp difference in the change in interest rates between eligible and non-eligible relationships for SMEs. The magnitude of the difference is between 20 and 27 basis points and is statistically significant.[37]

**Placebos.** Thanks to the rich SME-SF’ treatment space, we can check whether we detect any threshold effect where we expect none. The first placebo addresses the possibility that other policies already in place may be affecting relationships below and above the threshold of the SME-SF differently. Several policy interventions have supported access to credit for Italian SMEs. There are two main programs for this purpose, the *Nuovo*

[35] We select such a neighborhood employing the mean square error minimization method studied in Calonico, Cattaneo, and Titiunik (2014). We perform the necessary computations in Stata, employing the most recent update of the rdrobust package. We constrain the width of the eligible and non-eligible intervals to be equal for the clarity of the graphical presentation.

[36] For arguments in favor of focusing on the results of low degree (first and second) local polynomial specifications, see Andrew and Imbens (2017).

[37] In the first subsection of Appendix A.2, Tables A2 and A4, we show respectively that (i) the inclusion of covariates does not affect our result, as expected given the continuity shown in Figure 4 and Table A1, and (ii) the result is preserved and larger in magnitude for very small and hand-picked bandwidths.

*Plafond PMI Investimenti* and the *Fondo Centrale di Garanzia*. None of such programs, to the best of our knowledge, impinges on the same exposure threshold as the SME-SF.[38]

As both programs were already active as of December 2013, we check that no other discontinuity at the SME-SF threshold was present for $\Delta i_{bf}$ in 2012-2013 by repeating the estimation of Equation (3) on the pre-treatment period. In the second line of Table 3, we see that none of the specifications exhibit a statistically significant discontinuity.

The second placebo addresses the concern that there could be some alternative driver of our result concerning small credit relationships. It is unlikely for these relationships to be cheaper or subject to a smaller price increase, as the incidence of fixed costs is greater for smaller loans. Yet we may entertain the possibility that capital-constrained banks see them as less capital-consuming, regardless of the SME-SF. If enough banks treat the € 1.5 million in terms of past exposure as a rule of thumb for classifying relationships as small, we could have a spurious driver of our results.

If this were the case, though, we should also find a discontinuity at the threshold for firms that are not SMEs according to the specific definition that applies to the SME-SF. Therefore, we run a placebo test estimating Equation (3) on firms with turnover above € 50 million. We display the results in the third line of Table 3, showing no such discontinuity, no matter the specification.

**Bandwidth robustness.** We run a further check on the behavior of estimates under different bandwidths. In Figure 6, we show that if we drop the optimal bandwidth selection (and thus bias-correction) and we force small but progressively increasing bandwidths, we converge to the estimate reported in Table 3. Going from a symmetric bandwidth of € 25,000 to one of € 205,000, we see a discontinuity estimate gradually approaching -25 basis points in 2014 and 0 in 2013, with progressively smaller errors. This orderly behavior mitigates the concern that our result is spurious and only due to including odd observations through sample selection.[39]

## V.1 Heterogeneity: Switching Costs and Capital Scarcity

The baseline results capture the average pass-through from the SME-SF capital discount to the cost of credit. This effect might not reflect the full extent of the benefit of a capital

[38] We refer to Infelise (2014) for details on such programs.
[39] We provide details on the point estimates in Appendix Table A4.

discount for banks. First, the pass-through may be smaller for firms that have difficulties finding alternative sources of credit. Second, the shadow cost of regulatory capital will likely differ across banks, depending on how binding the regulatory constraint is. If our $\hat{\beta}$ captures the shadow cost of regulatory capital, we expect estimates to be greater for banks with a higher shadow cost. As our setting does not require any fixed effects for identification, we are in a unique position to explore such heterogeneity.

In this subsection, we first introduce our proxies of switching costs and regulatory capital scarcity. Then, we demonstrate that both dimensions drive heterogeneity in the SME-SF pass-through to the cost of credit. Moreover, we investigate the effect of the SME-SF on credit growth and find no average local effect from the SME-SF on the growth of credit granted.[40] Nevertheless, the absence of a quantity effect at the threshold may conceal pass-through heterogeneity. We show that the evidence points in this direction, suggesting that banks reduce granted credit to subsets of eligible borrowers that can be identified as weaker/more bank dependent. These include riskier eligible relationships, relationships belonging to firms either characterized with high past utilization rates or endowed with only one credit line, possibly increasing supply to the rest.

We start from **firm-level heterogeneity** as mapped by switching costs proxies. The importance of bargaining power in bank-firm credit relationships is a classic result (e.g. Rajan, 1992), with abundant empirical evidence in support of its relevance as a driver of credit access and cost (e.g. Detragiache, Garella, and Guiso, 2000; Ioannidou and Ongena, 2010; Santos and Winton, 2019). Moreover, recent evidence shows that the same channel is an important mediator for monetary policy pass-through (Agarwal et al., 2023; Benetton and Fantino, 2021; Scharfstein and Sunderam, 2016). Our results suggest that the same holds for the pass-through of capital regulation, consistently with studies finding that the borrower's capacity to switch to other credit providers limits the exploitation of bank market power in credit relationships (see Ioannidou and Ongena, 2010; Barone, Felici, and Pagnini, 2011; Allen, Clark, and Houde, 2019).

For a firm, the ability to switch does not simply amount to having two or more credit relationships. For example, a firm on the verge of default endowed with many credit relationships may be more captive than one with one relationship but a healthy balance sheet. For this reason, we consider not only the number of bank credit lines but also each

[40] We document this in Appendix Table A5.

relationship's past utilization rate, the firm's capacity to generate earnings (EBITDA over assets), and the firm's credit risk rating.

In greater detail, the multiple relationships indicator is a dummy taking value one if a firm has multiple credit relationships in good standing. We track the degree of utilization of credit lines and firms' earning capacity using the standardized lag drawn over granted ratio and the standardized EBITDA over assets ratio. We standardize by subtracting to a variable its population mean and dividing the resulting difference by the original standard deviation. Thus, we can interpret results as the effect of one standard deviation changes around the mean in the variable of interest on the pass-through. Finally, we identify risky firms by a dummy variable equal to one if the risk rating is in the top four notches of the nine-value scale of the rating.

Regarding **bank-level heterogeneity**, we consider a measure of capital scarcity based on the discussion in Section II.2. We construct our variable as the standardized difference between transitory and fully phased-in capital ratios, using the first available information (March 2014):[41]

$$\text{Basel III Gap}_{bt} = \text{Transitory Ratio}_{bt} - \text{Full Basel III Phase-In Ratio}_{bt}$$

we assume that the greater the adverse impact of the new definition, the greater the shadow value of an additional euroof regulatory capital to that specific bank.

The Basel III Gap based on supervisory reports is a better measure than the level of the CET1 ratio, as we are interested in capturing the distance from the desired level of equity for each bank. Indeed, each bank's capital ratio is typically higher than the regulatory minimum. As discussed in Repullo and Suarez (2013) and Corbae and D'Erasmo (2021), a bank with a high regulatory capital ratio may be willing to accumulate even more equity and thus have a very high shadow value of capital, and vice versa. As long as the transitory ratio is closer to the bank's desired target than the fully phased-in one, a positive value of the gap indicates that the bank will need to increase regulatory capital (and/or shrink risk-weighted assets) to revert to its desired buffer by the year 2018. Conversely, a negative value indicates that a bank will likely hold too much capital under the new regime.

[41] We use the risk-weighted asset as of March 2014 as denominators to obtain the capital ratios employed for the results in Table 4. In the Appendix Tables A6 and A7, we show the robustness of our heterogeneity results to different definitions of the gap variable.

Our implicit assumptions are that the new regime causes a shock, the size of which depends on the initial composition of the bank's capital, and that this shock is independent of each relationship's SME-SF eligibility status. The continuity tests displayed in Figure 4 and Appendix Table A1 (last line) support this last assumption. The difference between the transitory and the fully phased-in CET1 ratios is continuous at the SME-SF assignment threshold.

To obtain a meaningful comparison of parameters and confidence intervals, we estimate the interaction effects in the following local parametric specification:[42]

$$\begin{aligned}&\Delta y_{bf} = \alpha + \beta_M R_{bf} + \gamma_F \cdot \text{Switching Costs Proxy}_{bft} \cdot R_{bf} + \\ &\gamma_B \cdot \text{Basel III Gap}_b \cdot R_{bf} + \Omega X_{bf} + \phi(|x_{bf}^{2013} - \bar{x}|) + \epsilon_{bf} \\ &\text{Estimated on } bf: \ x_{bf} \in [\bar{x} - h^-, \bar{x} + h^+]\end{aligned} \tag{4}$$

where the Switching Costs Proxy is one of the four variables previously introduced; $\phi$ is a linear polynomial estimated independently on the threshold's two sides; $X_{bf}$ collects controls, which we always include here to mitigate the concern of picking up spurious variations with our interaction coefficients; $\epsilon_{bf}$ is an error term clustered simultaneously over firms and banks. Finally, the $\Delta y_{bf}$ dependent variable stands alternatively for the change in interest rates on the relationship between bank $b$ and firm $f$ and the log change in credit granted by bank $b$ to firm $f$.

We report in Table 4 the results of estimating Equation 4 for each of the different proxies and with either the price change or the change in the quantity of credit granted as the outcome variable. In the first four columns, we document interest rate change results, and in the last four, those for the difference in the log of credit granted. The first column of each set of regressions reports for reference the estimate of the treatment effect without the inclusion of interaction terms; the second column includes the interaction between the SME-SF and Basel III gap; the third column displays the results obtained by adding the interaction between the SME-SF and the switching costs proxies. Finally, the fourth column jointly shows all interactions. We present four alternative panels, one

[42] To perform the estimation, we select the bandwidth using Calonico et al. (2017), construct triangular kernel weights based on such bandwidths, and finally estimate a locally weighted regression employing the Correia (2016) reghdfe package. Of course, the cost of doing so is not correcting point estimates and standard errors for bias as when using Calonico et al. (2017). However, as such correction has a low impact on our main results (see Table A3, which omits the correction), we argue that the scope for concern is limited.

for each switching costs proxy.

We find economically significant pass-through heterogeneity across the first four columns. The interaction coefficients range from 50 to almost 100 percent of the estimates reported in Table 3. Both demand and supply heterogeneity are relevant mediators of the pass-through to interest rates. For example, in the fourth row of the first panel, we note that a standard deviation increase in the drawn over granted ratio almost halves the discount from 28 to 15. A similar result holds for firms with high risk scores, as seen in the last panel's last row.

Conversely, the central two panels show that firms with better outside options drive the pass-through. The last row of the third panel shows that one standard deviation higher EBITDA implies an 11 basis points higher discount. In the second panel, we can see that firms with more than one credit line in good standing drive the SME-SF effect. Finally, in each panel's third row, we show the estimates of the Basel III Gap interaction effect. Across all specifications, banks with one standard deviation larger gap decrease rates to eligible relationships by about six basis points more.

In the second four columns, we study the effect of the SME-SF on credit allocation, measured by the change in the log of credit granted. In the first column of this block, we cannot see any significant average effect on credit granted, i.e., banks do not increase loans to eligible firms more than to other firms. On the contrary, eligible relationships whose utilization rate is higher start being rationed after the introduction of the SME-SF. The effect is economically significant: Credit declines by two percent if the past drawn over granted ratio is one standard deviation larger. A similar result holds for high-risk firms (last panel). In contrast, the second panel shows that the insignificant average effect is probably due to the composition of a decrease in granted credit to firms with only one credit line in good standing with an increase to firms with more than one credit line. Finally, a high Basel III Gap does not influence the pass-through of the SME-SF to quantities.

Overall, our findings on quantities yield support for the policy change introduced in the new EU capital requirement regulation (CRR II), which smoothed the sharp eligibility threshold.[43] Indeed, evidence suggests that the sharp discontinuity may have discouraged the extension of credit to creditworthy albeit more fragile customers.

Our results are consistent with the insights from recent theoretical studies showing that firm and bank-level heterogeneity influences the bank lending channel (Ambrocio and Jokivuolle, 2017; Bahaj and Malherbe, 2020; Harris, Opp, and Opp, 2020). In particular, our findings align with Harris, Opp, and Opp (2020), suggesting that adjustments to risk weights disproportionately affect the access to credit of marginal (more credit-constrained) customers.

## V.2 What We Learn on the Cost of Capital Regulation to Banks

Our estimates easily convert into a measure of the impact on the cost of credit to the firms from a one percent decrease in the minimum capital requirement. From this impact estimate, under some assumptions, we can learn about the benefit to banks.

Below, how regulators set the minimum capital ratio requirement:

$$\Omega_{bA} = \underbrace{\Theta}_{\text{Minimum Fraction}} * \underbrace{\omega_A}_{\text{Risk Weight}} * A_b$$

here $\Omega_{bA}$ is the mandated minimum equity amount bank $b$ must set aside given it finances asset $A$ for a sum of € $A_b$.[44] $\Omega_{bA}$ is a $\Theta$ fraction of the $A_b$ amount weighted by the $\omega_A$ risk weight on assets of type $A$.

Changes in the risk weights cause a change in $\Omega_{bA}$. The eligibility to the SME-SF implies an approximately 2 percentage points saving on the capital required *vis-a-vis* the same exposure without the SME-SF:[45]

[43] The new rules foresee an increase in the SME-SF eligibility threshold from € 1.5 million to 2.5 million. Moreover, if the exposure exceeds the threshold, the credit relation will still be eligible for benefits. The fraction below € 2.5 million will still receive the original 76.9 percent support factor. The exceeding portion will enjoy a reduced support factor of 85 percent. See https://eur-lex.Europa.eu/legal-content/EN/TXT/?uri=CELEX%3A32019R0876.

[44] In practice, banks hold more than the minimum buffer for prudential reasons, i.e., there exists a $\Theta_b > \Theta$ for each bank $b$. For a theoretical explanation of such a behavior, see Repullo and Suarez (2013).

[45] We use 100 percent as the reference number for the baseline (without SME-SF) risk weight and eight percent as the baseline minimum capital ratio as they are the same employed in the design of the SME-SF itself (see, e.g. EBA, 2016, p.43). They correspond to the one faced by a corporate exposure for a bank that relies on external risk weights (Standard Approach) and does not use an internal risk weighting system for that exposure.

$$\Delta \frac{\Omega_{bA}}{A} = \underbrace{\Theta}_{\text{Minimum Fraction}} * \underbrace{\Delta \omega_A}_{\text{Risk Weight}} \approx -8\% * 24\% = -0.02$$

where 24 percent is the approximate decrease in the risk weight on eligible exposures.

The previous sections show that the estimated average impact $\hat{\beta}$ is around 26 basis points. Then, a simple calculation yields the value of the impact on the cost of credit for 1 percentage point change in the minimum capital ratio:

$$\frac{\hat{\beta}}{\Delta \frac{\Omega_{bA}}{A}} = \frac{-26}{-2 \text{ (percentage points)}} =$$

$$13\, bp \text{ per percentage point change in the capital requirement}$$

given the heterogeneity results, we stress that this average number is indeed just an average, and the pass-through to better customers should capture the benefit to banks more precisely.

Similarly to Plosser and Santos (2018), we apply the above back-of-the-envelope calculation to a 1 euro loan. The minimum capital requirement on this loan would decrease by 2 cents after the SME-SF implementation. Assuming that the drop in rates for a firm with one standard deviation higher EBITDA thoroughly reflects the initial benefit to the bank from the reform, we sum the first and the last line of column (4) in Table 4's EBITDA panel, and divide the resulting average discount of about 32 basis point by the 2 cent decrease in the requirement per unit of credit. Thus, we derive the shadow cost of 1 additional euro of mandated minimum capital buffer for banks as approximately 16 €-cents.

Then, interactions with our measure of capital scarcity suggest that the marginal benefit varies with the extent to which each bank is constrained, reaching about 19 €-cents for banks with one standard deviation greater shortfall in regulatory capital resources from an immediate Basel III phase-in (19 is the result of adding the 3 cents greater benefits per euro of requirements' reduction to these banks).

Under the assumption that banks optimally choose their balance-sheet structure, that they are using to the full possible extent every alternative to equity they have, and that they will keep a fixed buffer on top of the minimum requirement – so that 1 euro less in the minimum requirement would imply 1 euro less of equity to hold for the bank – we can read this number as an approximation of the increase in bank profit for holding 1 euro less in equity to finance the loan.

# VI Conclusion

We evaluate by an RDD the impact of the 2 percent discount in capital requirements implied by the SME-SF, which applies to SMEs' loans below € 1.5 million, and find that the cost of eligible loans decreases by approximately 26 basis points. Moreover, we document that the estimated effect is larger for firms more likely to switch to other banks and for banks whose shadow cost of regulatory capital is higher.

Under the assumption of a complete pass-through of the benefit from a lower capital requirement to these low switching costs borrowers, we quantify an approximate 16 bps relief to banks' cost of funding from decreasing the minimum capital buffer by 1 percent, with sizable bank heterogeneity driven by our proxy for regulatory capital scarcity.

Overall, the considerable variation in our estimate of the SME-SF's effect across firms and banks stresses the importance of considering the entire distribution of banks and firms' characteristics to understand who gains or loses from changes in bank capital regulation.

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Table 1: **Descriptives**

| | 2014 | | | 2013 | | |
|---|---|---|---|---|---|---|
| | Mean | Std Dev. | Count | Mean | Std Dev. | Count |
| | | | **Non-Eligible Credit Relationships** | | | |
| $\Delta i_{bf}$ | 37.02 | 200.76 | 6,510 | -3.293 | 221.18 | 8,901 |
| Drawn | 3,069.94 | 3,697.87 | 6,510 | 3,032.69 | 3,674.05 | 8,901 |
| Granted | 3,931.78 | 4,479.97 | 6,510 | 3,510.72 | 4,204.72 | 8,901 |
| Drawn/Granted | 81.151 | 17.695 | 6,509 | 82.428 | 17.489 | 8,615 |
| Revolving F. | 17.9 | 28.8 | 6,509 | 17.2 | 28.3 | 8,615 |
| Age Rel. | 7.202 | 2.534 | 6,510 | 6.369 | 2.352 | 8,901 |
| | | | **Firms With Only Non-Eligible Relationships** | | | |
| Sales | 3,809.64 | 7,917.63 | 6,985 | 3,860.6 | 7,925.29 | 8,656 |
| Leverage | 73.206 | 31.670 | 6,983 | 72.797 | 32.01 | 8,653 |
| EBITDA | 3.413 | 6.575 | 6,985 | 3.489 | 6.356 | 8,656 |
| Risk Score | 5.498 | 1.673 | 6,587 | 5.467 | 1.686 | 8,190 |
| N. Relations | 1.900 | 1.383 | 6,985 | 1.895 | 1.351 | 8,656 |
| | | | **Eligible Credit Relationships** | | | |
| $\Delta i_{bf}$ | 26.22 | 180.12 | 229,871 | 4.61 | 186.6 | 252,586 |
| Drawn | 216.65 | 253.12 | 229,871 | 226.9 | 261.2 | 252,586 |
| Granted | 390.66 | 473.64 | 229,871 | 386.7 | 506.96 | 252,586 |
| Drawn/Granted | 58.795 | 32.045 | 229,667 | 59.709 | 32.101 | 252,586 |
| Revolving F. | 32.3 | 33.3 | 229,667 | 0.322 | 0.333 | 252,586 |
| Age Rel. | 5.432 | 3.123 | 229,871 | 4.988 | 2.759 | 252,586 |
| | | | **Firms With At Least One Eligible Relationship** | | | |
| Sales | 2,710.57 | 5,143.67 | 170,283 | 2,688.78 | 5,089.24 | 185,697 |
| Leverage | 57.721 | 33.937 | 170,230 | 58.676 | 34.126 | 185,639 |
| EBITDA | 6.795 | 8.993 | 170,278 | 6.547 | 9.085 | 185,697 |
| Risk Score | 5.192 | 1.734 | 162,704 | 5.265 | 1.732 | 177,944 |
| N. Relations | 2.666 | 1.723 | 170,283 | 2.680 | 1.727 | 185,697 |
| | | | **Banks** | | | |
| CET1 Ratio | 12 | 3.6 | 90 | 11.7 | 3.5 | 94 |
| Retail Funding Ratio | 62.2 | 15.6 | 90 | 62.2 | 16.3 | 94 |
| Liquidity Ratio | 20.8 | 8.6 | 90 | 16.1 | 7.4 | 94 |
| Basel III CET1 Gap | 0.1 | 1.4 | 61 | | | |

*Note*: A "relationship" is a bank-firm pair, reporting the total exposure firm $f$ has toward bank $b$. The loan-level data comprise all performing loans from Italian banks in good standing (for which we have complete balance sheet information), to Italian firms with available CERVED balance sheet data. The first three columns of the Table report information on mean, dispersion and count for the year 2014, and we measure all variables as of the end of year 2013, except for the change in the interest rate, which averages 2014 quarterly changes. The second three columns of the table report information on mean, dispersion and count for the year 2013, and we measure all variables as of the end of year 2012, except for the change in the interest rate, which averages 2013 quarterly changes. $\Delta i_{bf}$ measures the changes in yearly revolving rates in basis points; we report all ratios in percentage points. The CET1 gap variable is the difference between the transitory and full-Basel III-phase-in CET1 ratios. We only report information for the sub-sample of observations for which we can observe interest rates.

Table 2: **McCrary's Density Test for outstanding exposure**

| | SME End of 2014 | SME 2014 | Non-SME 2014 | SME 2013 |
|---|---|---|---|---|
| Observations (l - r) | $822 - 492$ | $746 - 463$ | $42 - 74$ | $689 - 595$ |
| Optimal Bandwidths | 98.25 - 58.57 | 90.921 - 58.42 | 34.06 - 34.34 | 69.24 - 56.26 |
| t-Statistics | $-0.051$ | 0.708 | 1.49 | 0.82 |
| p-Values | 0.959 | 0.479 | 0.136 | 0.411 |

*Note:* The Table presents the t-statistics and p-values of the McCrary's density test. We report in the first row the number of observations we consider for density estimation at the left and right of the cutoff. In all cases, the null hypothesis is that there is no discontinuity in the density. We optimally select bandwidths minimizing the MSE of the density estimates, and do so independently on the two sides of the threshold. We report bandwidth limits in thousand of Euros.

Table 3: **Dependent Variable** Interest Rate Change in *bp*; **Method** Simple RD

| | RD, Pol(0) | RD, Pol(1) | RD, Pol(2) |
|---|---|---|---|
| $\hat{\beta}$ 2014 | $-19.785^{***}$ | $-25.949^{***}$ | $-26.991^{***}$ |
| | (6.458) | (8.202) | (10.138) |
| Obs. (left; right) | 4,816; 3,181 | 9,450; 5,675 | 17,948; 6,232 |
| MSE-Optimal Bdw. | 447.62 - 695.61 | 659.72 - 2,811.44 | 981.32 - 5,649.42 |
| $\hat{\beta}$ 2013 | 0.812 | 1.595 | 1.599 |
| | (5.2) | (6.602) | (7.959) |
| Obs. (left; right) | 10,326; 5,021 | 24,511; 7,469 | 37,293; 8,204 |
| MSE-Optimal Bdw. | 618.58 - 967.23 | 930.34 - 2,883.47 | 1,063.78 - 5,551.15 |
| $\hat{\beta}$ 2014 (Non-SME) | $-6.87$ | $-6.562$ | 1.204 |
| | (16.642) | (20.285) | (29.56) |
| Obs. (left; right) | 328; 2,774 | 701; 3,825 | 686; 4,126 |
| MSE-Optimal Bdw. | 237.15 - 3,657.1 | 496.66 - 10,802.43 | 487.75 - 20,095.03 |

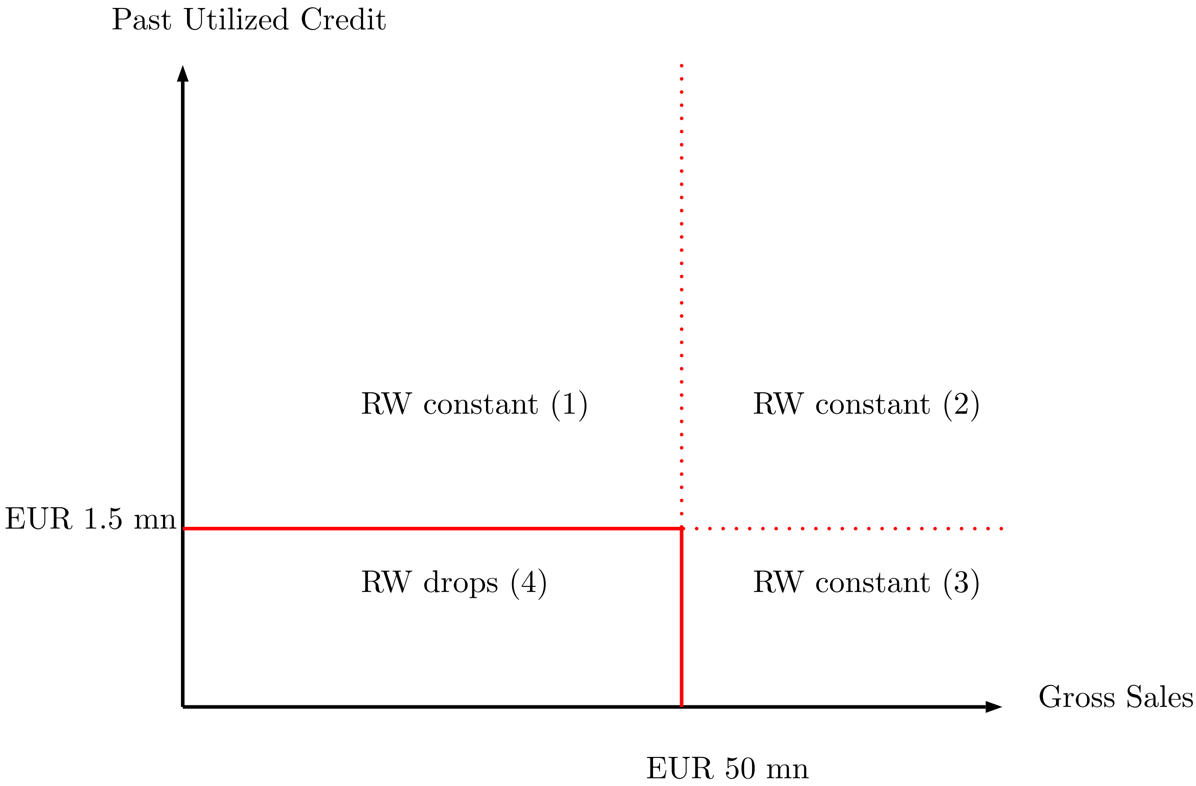
*Note:* This Table presents the results of discontinuity tests run *via* local estimation of $\hat{\beta}$ in Equation 3: $\Delta i_{bf} = \alpha + \beta R_{bf} + \phi(|x_{bf}^{2013} - \bar{x}|) + \nu_{bf}$, where $\Delta i$ is the interest rate change in basis points, $x_{bf}$ the past drawn credit, $\bar{x}$ the € 1.5 million threshold, $\phi$ the $x_{bf}$ polynomial independently estimated on the two sides of the threshold, and the null hypothesis of each test is $\beta = 0$. The different columns report increasing polynomial specifications. We obtain estimates for the SMEs 2014 sample, for the SMEs 2013 and non-SMEs 2014 placebo samples. Estimates reported employ triangular kernel weights, with robust standard errors displayed in parentheses. We report observations left and right of the cutoff and the corresponding optimal bandwidths (in thousands of Euros)below each estimates' block.

## Table 4: **The Role of Bank Capital and Credit Scarcity**

| | *Dep. Variable:* | Rates Change, bps | | | | Granted, Log Change | | | |
|---|---|---|---|---|---|---|---|---|---|
| | | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **Drawn/ Granted Panel** | SME-SF | -20.91*** | -18.92** | -29.06*** | -27.81*** | .0015 | .0022 | .0145 | .0168 |
| | | (-3.10) | (-2.66) | (-3.87) | (-3.46) | (0.15) | (0.21) | (1.05) | (1.12) |
| | Basel III Gap | | -20.6*** | | -20.62*** | | -.0088*** | | -.0087*** |
| | | | (-5.40) | | (-5.43) | | (-3.15) | | (-3.09) |
| | Basel III Gap * SF | | -5.601*** | | -5.646*** | | -.0014 | | -.0016 |
| | | | (-3.24) | | (-3.24) | | (-0.44) | | (-0.53) |
| | Drawn/Granted | | | 8.154 | 8.946 | | | .0215** | .0231** |
| | | | | (1.17) | (1.23) | | | (2.28) | (2.35) |
| | Drawn/Granted * SF | | | 11.73** | 12.76** | | | -.0179** | -.02** |
| | | | | (2.14) | (2.41) | | | (-2.09) | (-2.22) |
| **Relationships Panel** | SME-SF | | | -.8016 | .9183 | | | -.0391** | -.0472** |
| | | | | (-0.06) | (0.07) | | | (-2.24) | (-2.57) |
| | Basel III Gap | | | | -20.68*** | | | | -.0087*** |
| | | | | | (-5.40) | | | | (-3.11) |
| | Basel III Gap * SF | | | | -5.434*** | | | | -.0018 |
| | | | | | (-3.19) | | | | (-0.59) |
| | Multi Rel. | | | 3.807 | 2.824 | | | -.0089 | -.01 |
| | | | | (0.39) | (0.24) | | | (-0.91) | (-0.97) |
| | Multi Rel. * SF | | | -21.16** | -20.84* | | | .0443*** | .0537*** |
| | | | | (-2.09) | (-1.75) | | | (3.29) | (3.90) |
| **EBITDA Panel** | SME-SF | | | -23.41*** | -21.69*** | | | .0024 | .0036 |
| | | | | (-3.53) | (-3.05) | | | (0.24) | (0.34) |
| | Basel III Gap | | | | -20.39*** | | | | -.0089*** |
| | | | | | (-5.38) | | | | (-3.19) |
| | Basel III Gap * SF | | | | -5.905*** | | | | -0011 |
| | | | | | (-3.37) | | | | (-0.37) |
| | EBITDA | | | -3.55 | -3.488 | | | .0221*** | .0216*** |
| | | | | (-1.11) | (-1.08) | | | (3.42) | (3.26) |
| | EBITDA * SF | | | -9.819** | -10.98** | | | .0049 | .0078 |
| | | | | (-2.24) | (-2.49) | | | (0.88) | (1.41) |
| **Risk Panel** | SME-SF | | | -25.97*** | -24.73*** | | | .0162 | .0185 |
| | | | | (-3.88) | (-3.49) | | | (1.46) | (1.55) |
| | Basel III Gap | | | | -20.54*** | | | | -.009*** |
| | | | | | (-5.40) | | | | (-3.27) |
| | Basel III Gap * SF | | | | -5.886*** | | | | -1.8e-04 |
| | | | | | (-3.30) | | | | (-0.06) |
| | Log Risk Score | | | 17.63 | 18.95 | | | .0041 | .0047 |
| | | | | (1.58) | (1.61) | | | (0.34) | (0.37) |
| | High Risk * SF | | | 12.51*** | 14.51*** | | | -.0426*** | -.0477*** |
| | | | | (4.72) | (5.46) | | | (-4.83) | (-5.10) |
| | Linear | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| | Rel., Firm, Bank Controls | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| | Observations | 14,644 | 13,817 | 14,644 | 13,817 | 18,212 | 17,059 | 18,212 | 17,059 |

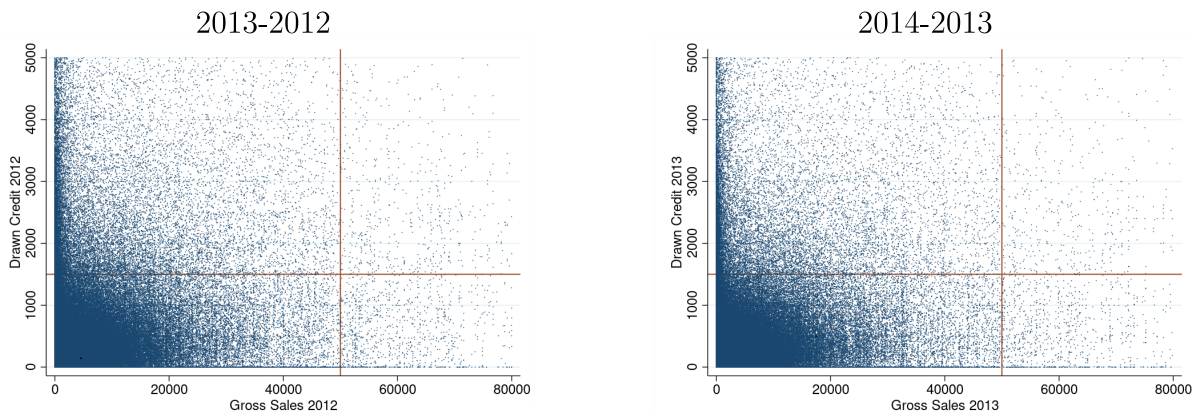
*Note:* This Table explores how credit scarcity, proxied by different relationship or firm-level characteristics, and bank capital scarcity, proxied by the difference between bank's CET1 under the transitional and full Basel III phase-in definitions (Basel III Gap), interact with the SME-SF pass-through. The first four columns report effects on rates; first, the baseline effect of a local linear specification, for reference; second, the interaction between SME-SF Eligibility and bank capital scarcity; third, the interaction with credit scarcity; fourth, the model including both interaction terms. The second four columns report effects on granted credit. The four different panels use alternative proxies for credit scarcity. We estimate all specifications locally with triangular kernel weights, over bandwidths chosen to minimize MSE. t-Statistics, from errors clustered at the firm and bank level, are in parentheses. *Controls*: **Relationship-level**: Lags of share of total drawn credit, revolving granted/total granted, utilized/granted, firm's and bank's hq in same province, log(relationship age). **Firm-level**: Lags of liquidity ratio, leverage, log(assets), log risk score (Altman z-score), EBITDA/assets, industry dummies, regional dummies, dummy for the presence of multiple relationships, investment ratio. **Bank-level**: A dummy singling out cooperative banks; the lag of CET1, liquidity, retail and wholesale funding ratios; the log of lag assets. We report robustness to alternative proxies of capital scarcity in Tables A6 and A7.

Figure 1: SME-SF Discount Assignment



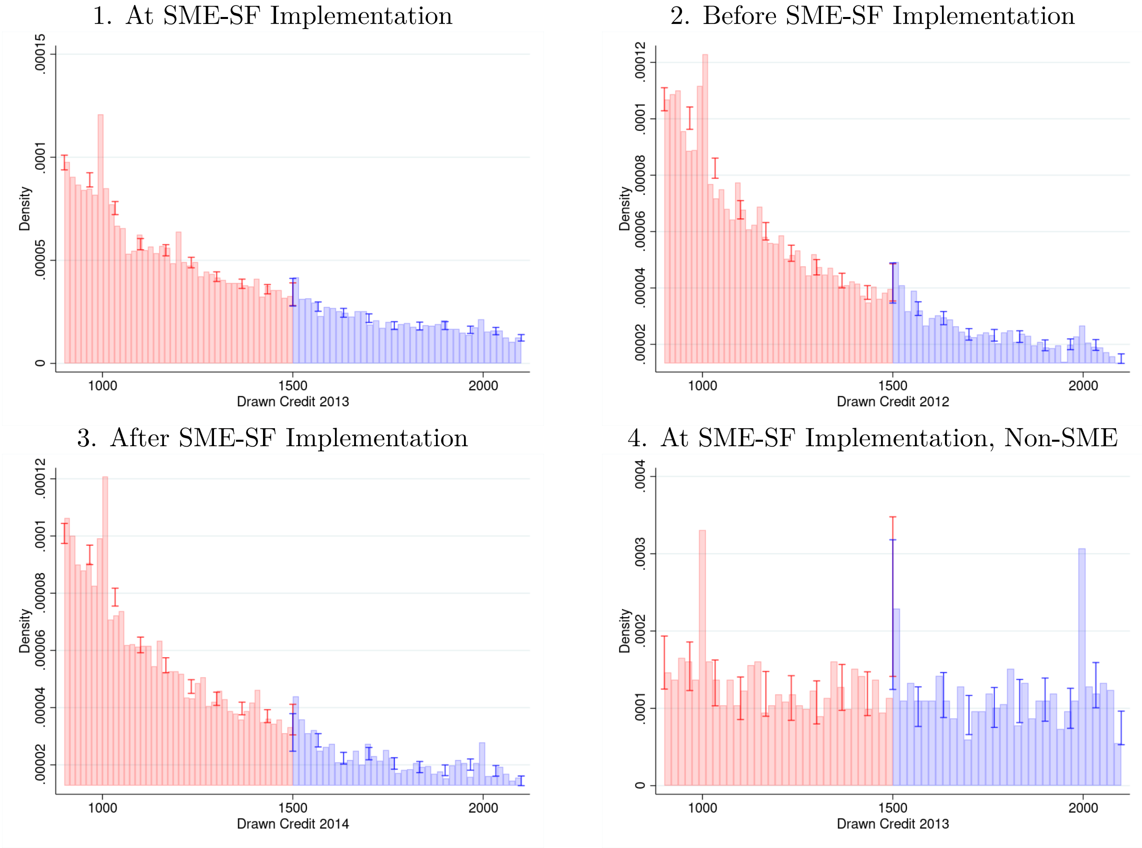

*Note:* The Figure presents the assignment space defined by the SME-SF eligibility rules.

Figure 2: Observations in the Treatment Space



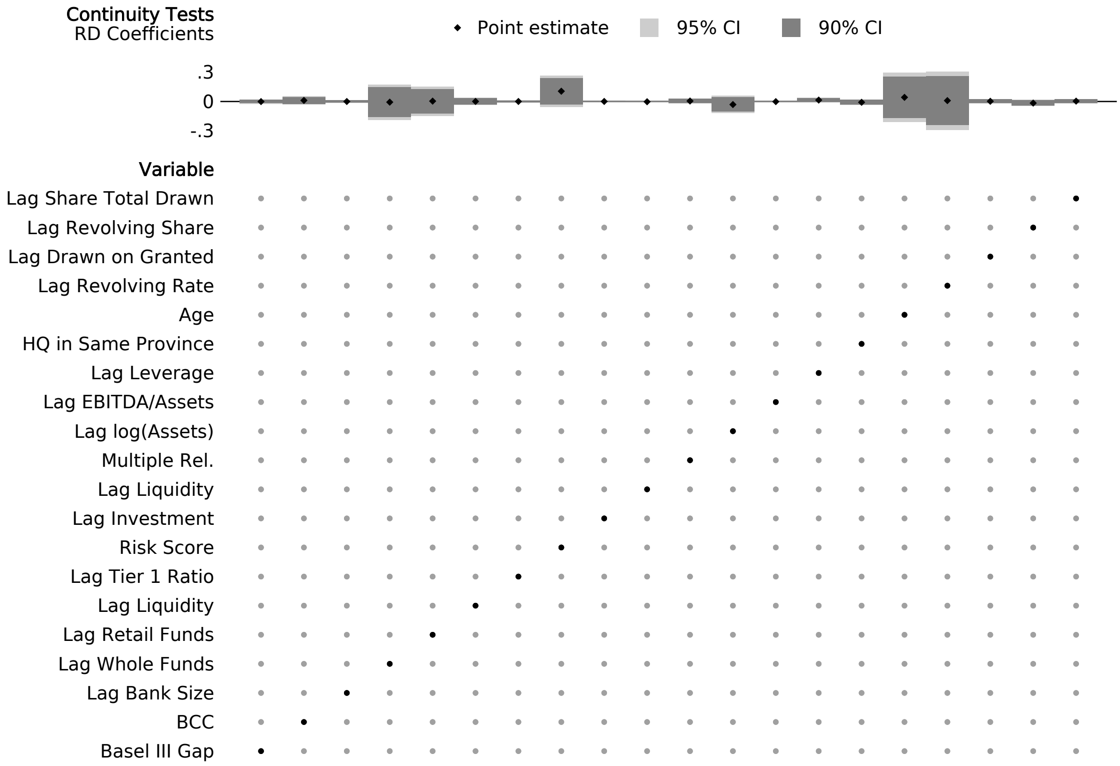

*Note*: The Figures present the distribution of bank-firm relationships over the treatment space.

Figure 3: Tests for the Discontinuity in Observation Density



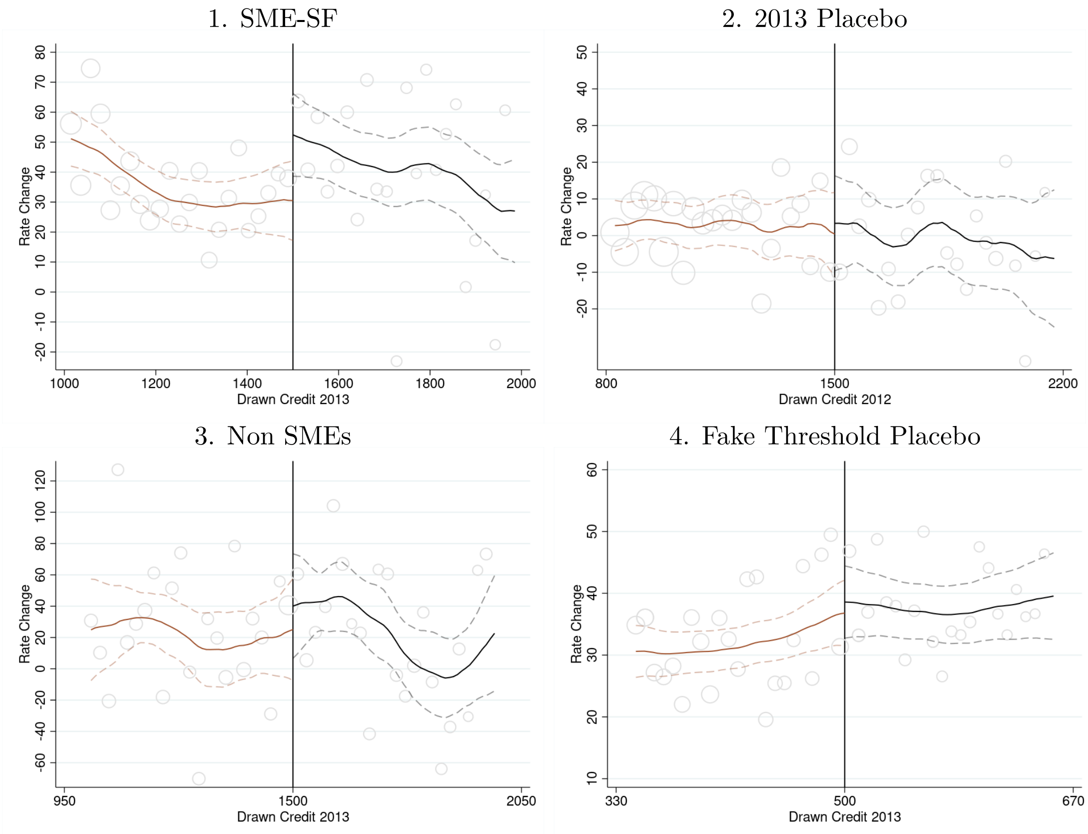

*Note:* The Figures present the graphical outputs of testing for discontinuity in the density of observations on the left and the right of the cutoff. The error bars plot the 95 percent confidence interval around the density point estimates. The first panel (top-left) reports the test for SMEs, using credit drawn at the end of 2013 as a running variable; the second (top-right) reports the test for SMEs, using credit drawn at the end of 2012 as a running variable; the third (bottom-left) reports the test for SMEs, using credit drawn at the end of 2014 as a running variable; the fourth (bottom-right) reports the test for Non-SMEs, using credit drawn at the end of 2013 as a running variable.

Figure 4: SME-SF Discount Assignment



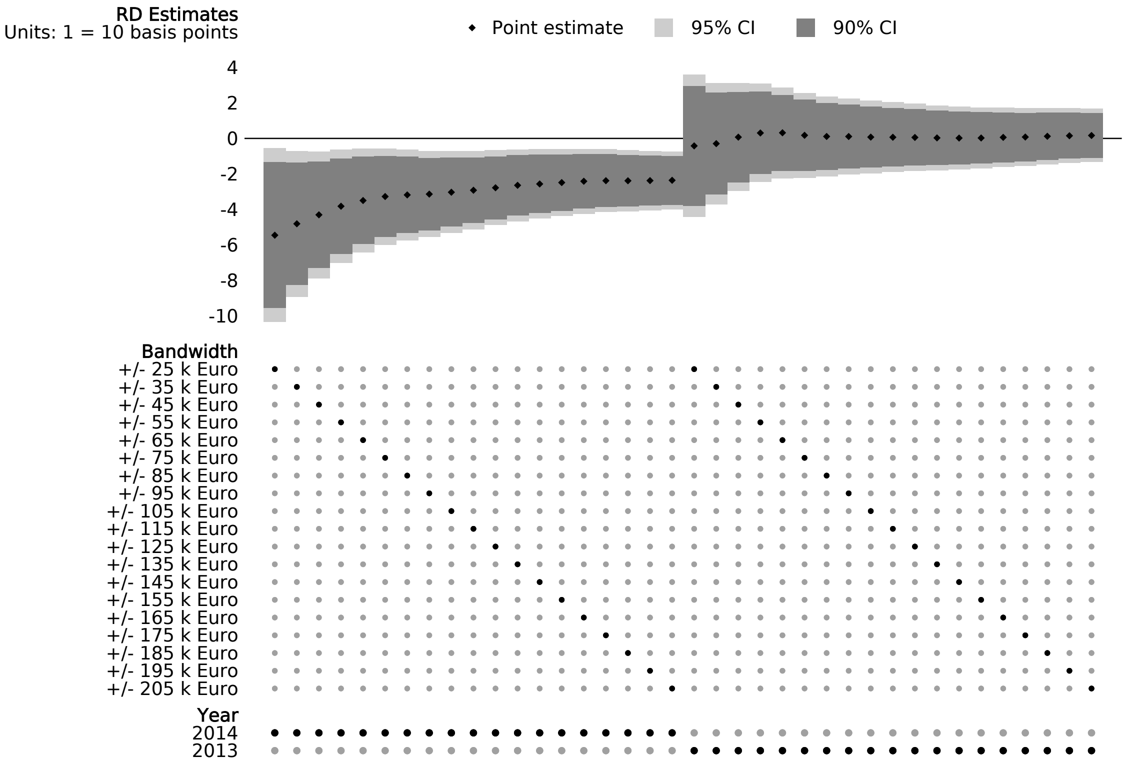

*Note:* The Figure reports the point estimates (black diamonds) and confidence interval (gray shaded areas) for discontinuities in relationship, firm and bank-level characteristics. We estimate the following specification $\text{covariate}_{bf2013} = b_0 + b_1 R_{bf} + \phi(|x_{bf}^{2013} - \bar{x}|) + e_{bf}$ within optimally chosen bandwidths, with a triangular kernel. Here $x_{bf}$ is drawn credit, $\bar{x}$ the € 1.5 million threshold, $\phi$ the linear $x_{bf}$ polynomial independently estimated on the two sides of the threshold, and $b_1$ the parameter of interest we estimate and plot. Below the graph, under the label "Variable", each line marks which variable we test for discontinuity. A black dot to the right of the corresponding line marks that the above estimate and confidence interval concern that specific covariate.

Figure 5: Discontinuity Plots, Reform and Placebos




*Note:* From the top left, we report the reform effect at the eligibility threshold for SME credit lines in 2013-2014; the placebo for SME credit lines at the SME-SF threshold in 2012-2013; the placebo discontinuity employing non-SME credit lines in 2013-2014; the placebo for the fictitious € 500 thousand of past utilization threshold, for SMEs in 2013-2014. The figure plots on the y-axis the delta in yearly rates before and after SME-SF implementation (and for the 2012-2013 window in subfigure (b)); on the x-axis, we plot the lag of credit drawn, in thousands of €. The overall limits of the x-axis shown are selected minimizing the MSE of the discontinuity point estimate, under the constraint of equal spans on the two sides of the threshold for presentation clarity. Gray balls represent binned averages of the data, with ball dimension increasing in the number of observations in each equally spaced bin. Dark (right-of-threshold) and ocra (left-of-threshold) solid lines are smooted polynomial estimates of the relationship between past drawn amounts and rate changes, while dotted lines are the 95% confidence intervals.

Figure 6: Bandwidth Robustness




*Note:* The Figure reports the point estimates (black diamonds) and confidence intervals (gray shaded areas) for discontinuities at the SME-SF assignment threshold obtained under increasingly larger bandwidths. We estimate the following specification $\Delta i_{bf} = a + \beta R_{bf} + \phi(|x_{bf}^{2013} - \bar{x}|) + \nu_{bf}$, where $x_{bf}$ is drawn credit, $\bar{x}$ the € 1.5 million threshold, $\phi$ the linear $x_{bf}$ polynomial independently estimated on the two sides of the threshold, and $\beta$ the parameter of interest we estimate and plot. Below the graph, under the label "Bandwidth", each line marks the furthest observation to the right and left of the threshold we include in the local estimation. Below the "Year", each line marks which sample we employ, whether concerning the year of treatment (2014), or the year before. A black dot to the right of the corresponding line marks that we computed the estimate above using that specific bandwidth and sample. We report details on the point estimates up to bandwidth € 105 thousand in Table A4.

# A Appendix

## A.1 Estimation Details

To estimate $\beta$ in Equation 3, we model the expected value of interest rate changes separately below and above the SME-SF threshold, as:

$$
\begin{aligned}
E[\Delta i_{bf}(1)|x_{bf}] &= a^- + \phi^-(|x_{bf}^{2013} - \bar{x}|) \;\; 1,500 - h^- \leq x_{bf}^{2013} < 1,500 \\
E[\Delta i_{bf}(0)|x_{bf}] &= a^+ + \phi^+(|x_{bf}^{2013} - \bar{x}|) \;\; 1,500 \leq x_{bf}^{2013} \leq 1,500 + h^+
\end{aligned}
\tag{5}
$$

Then, using the Stata routine based on Calonico, Cattaneo, and Titiunik (2014) and described in Calonico et al. (2017), we choose $(\hat{a}^{+/-}, \hat{\phi}^{+/-}, h^{+/-})$ minimizing

$$
\begin{aligned}
&\sum_{bf} \left(\Delta i_{bf} - a^- - \phi^-(|x_{bf}^{2013} - \bar{x}|)\right)^2 K\left(\frac{|x_{bf}^{2013} - \bar{x}|}{h^-}\right) \text{ below threshold} \\
&\sum_{bf} \left(\Delta i_{bf} - a^+ - \phi^+(|x_{bf}^{2013} - \bar{x}|)\right)^2 K\left(\frac{|x_{bf}^{2013} - \bar{x}|}{h^+}\right) \text{ above threshold}
\end{aligned}
\tag{6}
$$

where $K$ is a triangular kernel weight function. Consequently:

$$\hat{\beta} = \hat{a}^- - \hat{a}^+ \tag{7}$$

## A.2 Robustness of the Results

**Covariates' continuity is robust to degree 0 and 2 polynomials specifications**: In Table A1, we show that the absence of statistically detectable discontinuities in other covariates, shown in Figure 4, does not depend on the polynomial degree of choice. Moreover, we report in this Table the exact estimates for reference.

**Controls inclusion**: We repeat the estimation, including bank, firm, and relationship characteristics as further independent variables for robustness. Even though these controls do not vary discontinuously at the eligibility threshold (as we have shown in Section IV.1), their inclusion can increase precision and also provide information on the effect of heterogeneity in observable characteristics on our *average* pass-through effect. As suggested by Angrist and Rokkanen (2015), including control variables mitigates concerns of lack of external validity of RDD estimates and further limits the bias due to the inclusion of observations far from the threshold when the bandwidth is wide. Including covariates brings far-off observations to a more equal footing.

Furthermore, our relationships are stratified at the firm and bank levels. The main correlation concern is at the firm level, as it is reasonable to think that the same team makes decisions on the pricing of loans of the same firm based on the same set of information (e.g., leverage, profitability, credit score). For this reason, we cluster the standard errors at the firm level. We thus obtain the results displayed in Table A2. The estimated effects are similar to our baseline results without including covariates - a discontinuity of 23 to 25 basis points - while statistical significance remains unchanged.

**The role of bias adjustment**: Then, as the interaction results in display play an essential role in our argument, and as we employ the same bandwidths but cannot correct for the bandwidth selection bias in that case, we must make sure that such adjustment does not play an important role in our main result magnitudes. For this purpose, we report results omitting bias correction in Table A3, where we can see that the results change but for a few basis points.

**Hand-picked bandwidths**: We show the broad robustness of our result to simple mean comparisons in extremely restricted neighborhoods of the threshold. To do so, we drop the optimal bandwidth selection algorithm (and thus bias-correction) and force alternative bandwidths moving from a symmetric distance of € 25,000 from the threshold to a € 105,000 distance at increments of € 10,000. We can see in Table A4 that the results are larger in magnitude, still significant albeit expectedly noisier, and consistent with a discount caused by the SME-SF, while the 2013 placebo is still small and insignificant.[46]

**Relationship between the estimate and the threshold**: In Figure A1, we check how the SME-SF's estimate changes if **Relationship between the estimate and the threshold**: In Figure A1, we check how the SME-SF's estimate changes if we use treatment assignment cutoffs progressively further from the true one. In RDD settings, spurious results may arise from a non-linear relationship between the assignment and the dependent variables. The sharp discontinuity estimator may pick such continuous non-linear relationships up and "read" them as discontinuities. If such a problem drives our result, we would expect the significance and magnitude of the estimate not to peak at the assignment threshold. We check for this behavior in the same spirit as the permutation test proposed in Ganong and Jäger (2018) for regression kink designs. In the Figure,

[46] We lack sufficient density to estimate the Non-SMEs placebo on such restricted bandwidths reliably.

we see that the estimate's magnitude and significance peaks at € 1.5 million, turning non-significant and smaller as we progressively use assignment thresholds further from the true one.

**Lack of an average SME-SF impact on granted credit**: In Table A5 we show that estimating Equation 6 using changes in lag granted credit as the left-hand side variable delivers a null result.

**Robustness of the heterogeneity result to different Basel Gap measures**: In Tables A6 and A7, we show that our heterogeneity study is robust to measuring heterogeneity in the banks' shadow cost of capital with alternative proxies of the regulatory capital shortfall induced by Basel III. First, Table A6 displays results employing Tier 1 capital instead of CET1 capital. Tier 1 capital is a broader class encompassing CET1 and other equity-like instruments. A bank that experiences a large Tier 1 wipeout should also have a high shadow cost of regulatory capital. Indeed, results are not affected.

In Table A7, we instead explore what happens with a different measure of CET1 shortfall. The measure we employ in the main body of the paper looks at the difference between transitory and full phase-in CET1 ratios in March 2014, the first date banks started providing such information. The denominator of both ratios is the risk-weighted assets as of March 2014. Employing the same denominator for both ratios, we effectively mute the impact of changes in risk-weighting introduced by Basel III (including the SME-SF). Here, we construct a measure that encompasses the effect of changing risk weights on the target ratios. Instead of using the transitory CET1 ratio as of March 2014, we employ the reconstructed CET1 ratio in December 2013, whose denominator is the 2013 risk weights under the pre-Basel III rules. Then, we subtract the fully phased-in CET1 ratio using March 2014 risk-weighted assets as the denominator. In the Table, we can see that this has no material effect on our results.

## A.3 On The Shortcomings of Within Identification

We show the large extent of heterogeneity in the effect of the SME-SF, likely led by switching costs and bargaining power in credit relationships. Moreover, we see that the availability of a backup source for the firm's credit demand - i.e., multiple relationships and a low utilization ratio - is one key factor determining the magnitude of the pass-through. This Appendix further explores this aspect, simultaneously showing a possible pitfall of using the classic Khwaja and Mian (2008) fixed-effect strategy in this and similar contexts.

Using firm fixed effects implies identifying the treatment effect using the within estimator considering the sample of firms with multiple credit relationships. Unfortunately, this procedure can significantly alter the estimated coefficient, even without demand bias, through sample selection. In our case, a firm-fixed effects identification strategy would select a sub-sample of firms with outside options, i.e., firms that can better capture a larger share of the surplus generated by the SME-SF.

Implementing a within RD estimation with high dimensional fixed effects requires some adjustment to the estimation procedure. To perform the within RD, we select the bandwidth using Calonico et al. (2017), construct triangular weights based on such bandwidths, and finally estimate a weighted fixed effect regression using the routine described in Correia (2016), useful to handle high dimensional fixed models.[47] In Table A8, we can observe how the magnitude of the point estimates increases to values ranging between 35 and 39 basis points, still highly significant.

As we have extensively shown no discontinuous change in a host of observables at the discontinuity threshold, the most likely reason for the difference in the point estimates is the sample selection imposed by the fixed effect estimator. To be sure that this is why we observe increased point estimates, we estimate the model using the same subset of observations exploited by the fixed effect estimator but omitting the fixed effects. We run local regressions using observations belonging to firms with at least two relationships, one eligible for the SME-SF and one not, in the neighborhood of the eligibility threshold selected through the data-driven algorithm. We present the results of such estimation in the first panel of Table A9. The estimated effect is larger than our main result and similar to the one obtained with the fixed effects model, confirming our hypothesis.

Two possible reasons may cause the larger magnitude of the estimated effect for the fixed effects sub-sample. On the one hand, it may be the case that the rates on eligible credit lines of such firms grow less; on the other hand, it may also be true that the rates on non-eligible relationships of such firms rise more. We thus check that the increase in

[47] We make this choice as the rdrobust Stata routine (Calonico et al., 2017) does not provide a way to handle high dimensional fixed effects directly. To keep working within the rdrobust framework, one should create thousands of firm identifier dummies and feed them to the model, manually dropping local singleton observations for clustered error cases (Correia, 2015). As the reghdfe performs all such steps automatically, it is the least ad-hoc option at our disposal.

the estimated SME-SF impact is not due to a higher increase in the rates of non-eligible credit relationships in the fixed effects sub-sample. In the second panel of Table A9, we show the result of comparison in rate changes for non-eligible credit relationships of firms in and out of the fixed effects sub-sample. Across different specifications, we can see that firms in the sub-sample experience rate changes in line with other firms. We can thus conclude that the SME-SF effect on eligible relationships appears to be stronger in the firm fixed effects sub-sample *independently* of the inclusion of fixed effects.

The fact that sample selection from the fixed-effects strategy is enough to see an increase in the result substantiates our interpretation of the increase in the magnitude of point estimates as coming from the higher bargaining power of firms in the fixed-effects subsample. If firms borrowing from a single bank were locked in a monopolistic relationship with their lender, the latter would not necessarily transfer the benefit from the SME-SF to the firm. Instead, the pass-through would be more significant for firms that can switch between existing relationships, limiting banks' capacity to extract rents.[48]

The sub-sample on which we identify the local fixed effect estimator of the treatment effect is composed of firms with multiple *similar* relationships, at least one of which is eligible and one not. Hence, they are precisely the firms that are less likely to be captured by a relationship lender, as they have other credit relationships that are close substitutes.

[48] Again, as argued in Rajan (1992), Detragiache, Garella, and Guiso (2000) and Ioannidou and Ongena (2010).

Table A1: **Continuity of Covariates**

| Control Variable | Test, Pol(0) | Test, Pol(1) | Test, Pol(2) |
|---|---|---|---|
| Lag Share of Total Drawn | −0.002 | −0.004 | −0.004 |
| | (0.012) | (0.011) | (0.015) |
| Lag Revolving Fraction | 0.014 | 0.016 | 0.024 |
| | (0.014) | (0.016) | (0.016) |
| Lag Drawn on Granted | −0.002 | −0.003 | 0.001 |
| | (0.01) | (0.012) | (0.013) |
| Lag Revolving Rate | 0.012 | 0.009 | 0.07 |
| | (0.143) | (0.154) | (0.197) |
| Age | −0.008 | −0.043 | 0.048 |
| | (0.119) | (0.13) | (0.129) |
| Hq in Same Province | 0.004 | 0.008 | 0.013 |
| | (0.013) | (0.014) | (0.02) |
| Lag Leverage | −1.128 | −1.497 | −1.13 |
| | (1.016) | (1.119) | (1.372) |
| Lag Ebitda/Assets | 0.07 | 0.098 | 0.145 |
| | (0.266) | (0.31) | (0.34) |
| Lag log(Assets) | 0.025 | 0.031 | 0.024 |
| | (0.05) | (0.045) | (0.045) |
| Multi. Rel. | −0.004 | −0.005 | −0.011 |
| | (0.012) | (0.013) | (0.013) |
| Lag Liquidity | 0.002 | 0.002 | 0.003 |
| | (0.002) | (0.002) | (0.003) |
| Lag Investment | −0.001 | −0.001 | −0.001 |
| | (0.002) | (0.003) | (0.004) |
| Risk Score | −0.085 | −0.106 | −0.094 |
| | (0.059) | (0.082) | (0.097) |
| Lag CET1 Ratio | −0.0005 | −0.001 | −0.001 |
| | (0.005) | (0.005) | (0.005) |
| Lag Liquidity | 0.0007 | −0.0009 | −0.002 |
| | (0.02) | (0.02) | (0.02) |
| Lag Retail Fund. | −0.004 | −0.004 | −0.003 |
| | (0.076) | (0.077) | (0.075) |
| Lag Whole Fund. | −0.013 | 0.006 | 0.008 |
| | (0.091) | (0.093) | (0.092) |
| Lag Bank Size | 0.0002 | −0.00002 | 0.0001 |
| | (0.007) | (0.007) | (0.007) |
| BCC Dummy | −0.002 | −0.011 | −0.009 |
| | (0.02) | (0.021) | (0.02) |
| Basel III CET1 Gap | 0.001 | 0.001 | 0.002 |
| | (0.01) | (0.011) | (0.011) |

Robust std. errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

*Note:* The Table reports the statistical significance and coefficients' values for discontinuities in each of the covariates included in the covariates augmented version of Equation 3. This means the following specification: $\text{covariate}_{bf} = b_0 + b_1 R_{bf} + \phi(|x_{bf}^{2013} - \bar{x}|) + e_{bf}$, estimated locally, with a triangular kernel. Here $x_{bf}$ is drawn credit, $\bar{x}$ the € 1.5 million threshold, $\phi$ the $x_{bf}$ polynomial independently estimated on the two sides of the threshold, and the null hypothesis is $b_1 = 0$.

Table A2: **Dependent Variable** Interest Rate Change in *bp*; **Method** Simple RD

| | RD, Pol(0) Firm Clustered Errors | RD, Pol(1) Firm Clustered Errors | RD, Pol(2) Firm Clustered Errors |
|---|---|---|---|
| $\hat{\beta}$ 2014 | $-22.936^{***}$ | $-22.644^{***}$ | $-24.887^{***}$ |
| | (7.618) | (7.89) | (9.495) |
| Obs. (left; right) | 2,853; 2,079 | 12,018; 5,112 | 23,874; 5,921 |
| MSE Optimal Bdw. | 305.73-389.93 | 753.16-2,116.434 | 991.995-4,685.959 |
| $\hat{\beta}$ 2013 | 2.261 | 1.923 | $-0.09$ |
| | (5.863) | (6.752) | (8.849) |
| Obs. (left; right) | 5,410 ; 4,714 | 17,675; 7,269 | 16,482;7,905 |
| MSE Optimal Bdw. | 424.72-913.37 | 823.26-2,916.233 | 798.46-5,087.99 |
| $\hat{\beta}$ 2014 (Non-SMEs) | $-8.055$ | $-6.681$ | 18.706 |
| | (14.119) | (17.177) | (29.268) |
| Obs. (left; right) | 358; 2,536 | 766; 3,515 | 555; 3,842 |
| MSE Optimal Bdw. | 297.152-3,544.12 | 579.06-10,443.26 | 454.09-21,636.88 |
| Controls | ✓ | ✓ | ✓ |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

*Note:* This Table presents the results of discontinuity tests run *via* local estimation of $\hat{\beta}$ in Equation 3: $\Delta i_{bf} = \alpha + \beta R_{bf} + \phi(|x_{bf}^{2013} - \bar{x}|) + \Omega C_{bf} + \nu_{bf}$, where $\Delta i$ is the interest rate change in basis points, $x_{bf}$ the past drawn credit, $\bar{x}$ the € 1.5 million threshold, $\phi$ the $x_{bf}$ polynomial independently estimated on the two sides of the threshold, $C_{bf}$ is a matrix of controls, and the null hypothesis of each test is $\beta = 0$. The different columns report increasing polynomial specifications. We compute estimates for the SMEs 2014 sample, the SMEs 2013 and non-SMEs 2014 placebo samples. We employ triangular kernel weights, and display robust standard errors clustered at the firm level in parentheses.
*Controls*: **Relationship level**: Lags of share of total drawn credit, revolving granted/total granted, utilized/granted, firm's and bank's hq in same province, log(relationship age). **Firm level**: Lags of liquidity ratio, leverage, log(assets), log risk score (Altman z-score), EBITDA/assets, industry dummies, regional dummies, dummy for the presence of multiple relationships, investment ratio. **Bank level**: Lags of tier 1 capital ratio, liquidity, retail funding ratio, wholesale funding ratio, log(assets), a BCC dummy.

Table A3: **Dependent Variable** Interest Rate Change in *bp*; No correction

| | RD, Pol(0) | RD, Pol(1) | RD, Pol(2) |
|---|---|---|---|
| $\hat{\beta}$ 2014 | $-15.587^{***}$ | $-22.244^{***}$ | $-26.017^{***}$ |
| | (5.15) | (6.917) | (8.402) |
| Obs. (left; right) | 4,816; 3,181 | 9,450; 5,675 | 17,948; 6,232 |
| MSE-Optimal Bdw. | 447.62 - 695.61 | 659.72 - 2,811.44 | 981.32 - 5,649.42 |
| $\hat{\beta}$ 2013 | 3.25 | 1.505 | 5.599 |
| | (4.12) | (5.511) | (6.918) |
| Obs. (left; right) | 10,326; 5,021 | 24,511; 7,469 | 37,293; 8,204 |
| MSE-Optimal Bdw. | 618.58 - 967.23 | 930.34 - 2,883.47 | 1,063.78 - 5,551.15 |
| $\hat{\beta}$ 2014 (Non-SME) | $-3.813$ | $-7.851$ | 1.204 |
| | (13.044) | (17.03) | (29.56) |
| Obs. (left; right) | 328; 2,774 | 701; 3,825 | 686; 4,126 |
| MSE-Optimal Bdw. | 237.15 - 3,657.1 | 496.66 - 10,802.43 | 487.75 - 20,095.03 |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

*Note:* This Table presents the results of discontinuity tests run *via* local estimation of $\hat{\beta}$ in Equation 3: $\Delta i_{bf} = \alpha + \beta R_{bf} + \phi(|x_{bf}^{2013} - \bar{x}|) + \nu_{bf}$, where $\Delta i$ is the interest rate change in basis points, $x_{bf}$ the past drawn credit, $\bar{x}$ the € 1.5 million threshold, $\phi$ the $x_{bf}$ polynomial independently estimated on the two sides of the threshold, and the null hypothesis of each test is $\beta = 0$. *Point estimates and errors are, in this case, not corrected for bandwidth selection.* The different columns report increasing polynomial specifications. We compute estimates for the SMEs 2014 sample, the SMEs 2013 and non-SMEs 2014 placebo samples. We employ triangular kernel weights, and display robust standard errors clustered at the firm level in parentheses.

Table A4: **Dependent Variable** Interest Rate Change in *bps*; Hand-picked Bandwidth

| Bandwidth | 25-25 | 35-35 | 45-45 | 55-55 | 65-65 | 75-75 | 85-85 | 95-95 | 105-105 |
|---|---|---|---|---|---|---|---|---|---|
| $\hat{\beta}$ 2014 | $-54.54^{**}$ | $-48.093^{**}$ | $-43.043^{**}$ | $-38.195^{**}$ | $-34.968^{**}$ | $-32.736^{**}$ | $-31.813^{**}$ | $-31.368^{**}$ | $-30.307^{**}$ |
| | (24.98) | (20.944) | (18.193) | (16.306) | (14.909) | (13.849) | (13.04) | (12.348) | (11.741) |
| Obs. (left; right) | 184; 234 | 268; 301 | 352; 375 | 442; 446 | 528; 515 | 613; 571 | 690; 633 | 776; 693 | 870; 762 |
| $\hat{\beta}$ 2013 | $-4.187$ | $-2.916$ | 0.733 | 3.1 | 3.194 | 1.775 | 1.109 | 1.101 | $-0.755$ |
| | (20.473) | (17.423) | (15.468) | (14.118) | (13.017) | (12.163) | (11.479) | (10.911) | (10.424) |
| Obs. (left; right) | 262; 296 | 340; 401 | 448; 487 | 556; 580 | 654; 684 | 739; 765 | 836; 841 | 935; 907 | 1,059; 981 |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

*Note:* This Table presents the results of discontinuity tests run *via* local estimation of $\hat{\beta}$ in a simplified version of Equation 3: $\Delta i_{bf} = \alpha + \beta R_{bf} + \nu_{bf}$, where $\Delta i$ is the interest rate change in basis points, $x_{bf}$ the past drawn credit, $\bar{x}$ the € 1.5 million threshold, and the null hypothesis of each test is $\beta = 0$. The different columns report increasingly larger hand-picked bandwidths for a linear polynomial specification of $\phi$, with bandwidth size in thousands of Euros reported in the column header. We compute estimates for the SMEs 2014 sample, the SMEs 2013 and non-SMEs 2014 placebo samples. We employ triangular kernel weights, and display robust standard errors clustered at the firm level in parentheses.

Table A5: **Dependent Variable** Log changes in granted credit; **Method** Simple RD

| | RD, Pol(0) | RD, Pol(1) | RD, Pol(2) |
|---|---|---|---|
| $\hat{\beta}$ 2014 | 0.002 | 0.006 | 0.014 |
| | (0.006) | (0.012) | (0.014) |
| Obs. (left; right) | 5,676; 6,327 | 9,051; 9,796 | 11,997; 10,401 |
| MSE-Optimal Bdw. | 371.47 - 1,040.96 | 505.83 - 4,282.74 | 601.45 - 8,824.57 |
| $\hat{\beta}$ 2013 | $-0.006$ | $-0.006$ | $-0.006$ |
| | (0.009) | (0.012) | (0.013) |
| Obs. (left; right) | 4,080; 9,881 | 8,096; 12,815 | 13,702; 13,526 |
| MSE-Optimal Bdw. | 250.05 - 1,678.1 | 423.35 - 4,844.43 | 581.7 - 9,343.82 |
| $\hat{\beta}$ 2014 (Non-SME) | $-0.03$ | $-0.033$ | $-0.038$ |
| | (0.025) | (0.032) | (0.042) |
| Obs. (left; right) | 570; 3,773 | 1,152; 6,227 | 1554; 6,682 |
| MSE-Optimal Bdw. | 248.22 - 2,624.72 | 487.92 - 13,292.23 | 594.88 - 28,113.90 |

*Note:* This Table presents the results of discontinuity tests run *via* local estimation of $\hat{\beta}$ in an alternative to Equation 3: $\Delta\log(\text{granted})_{bf} = \alpha + \beta R_{bf} + \phi(|x_{bf}^{2013} - \bar{x}|) + \nu_{bf}$, where $\Delta\log(\text{granted})$ is the log change in granted credit, $x_{bf}$ the past drawn credit, $\bar{x}$ the€ 1.5 million threshold, $\phi$ the $x_{bf}$ polynomial independently estimated on the two sides of the threshold, and the null hypothesis of each test is $\beta = 0$. The different columns report increasing polynomial specifications. We compute estimates for the SMEs 2014 sample, the SMEs 2013 and non-SMEs 2014 placebo samples. We employ triangular kernel weights, and display robust standard errors clustered at the firm level in parentheses. We report observations left and right of the cutoff and the corresponding optimal bandwidths (in thousands of Euros) below each estimates' block.

## Table A6: **SME-SF's Effect Heterogeneity, Tier 1 Capital Robustness**

| | *Dep. Variable:* | Rates Change, bps | | | | Granted, Log Change | | | |
|---|---|---|---|---|---|---|---|---|---|
| | | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **Drawn/ Granted Panel** | SME-SF | -20.91*** | -19.58*** | -29.06*** | -28.44*** | .0015 | .0024 | .0145 | .0174 |
| | | (-3.10) | (-2.90) | (-3.87) | (-3.63) | (0.15) | (0.24) | (1.05) | (1.21) |
| | Basel III Gap | | -20.61*** | | -20.63*** | | -.0081*** | | -.008*** |
| | | | (-4.82) | | (-4.84) | | (-2.76) | | (-2.69) |
| | Basel III Gap * SF | | -5.062*** | | -5.099*** | | -.0029 | | -.0032 |
| | | | (-3.32) | | (-3.26) | | (-1.08) | | (-1.22) |
| | Drawn/Granted | | | 8.154 | 7.895 | | | .0215** | .0219** |
| | | | | (1.17) | (1.11) | | | (2.28) | (2.29) |
| | Drawn/Granted * SF | | | 11.73** | 12.77** | | | -.0179** | -.0207** |
| | | | | (2.14) | (2.41) | | | (-2.09) | (-2.32) |
| **Relationships Panel** | SME-SF | | | -.8016 | -.6387 | | | -.0391** | -.0425** |
| | | | | (-0.06) | (-0.05) | | | (-2.24) | (-2.36) |
| | Basel III Gap | | | | -20.7*** | | | | -.008*** |
| | | | | | (-4.81) | | | | (-2.72) |
| | Basel III Gap * SF | | | | -4.863*** | | | | -.0034 |
| | | | | | (-3.14) | | | | (-1.28) |
| | Multi Rel. | | | 3.807 | 2.08 | | | -.0089 | -.0101 |
| | | | | (0.39) | (0.19) | | | (-0.91) | (-1.01) |
| | Multi Rel. * SF | | | -21.16** | -19.94* | | | .0443*** | .0488*** |
| | | | | (-2.09) | (-1.81) | | | (3.29) | (3.50) |
| **EBITDA Panel** | SME-SF | | | -23.41*** | -21.98*** | | | .0024 | .0035 |
| | | | | (-3.53) | (-3.26) | | | (0.24) | (0.34) |
| | Basel III Gap | | | | -20.43*** | | | | -.0082*** |
| | | | | | (-4.80) | | | | (-2.8) |
| | Basel III Gap * SF | | | | -5.294*** | | | | -.0027 |
| | | | | | (-3.41) | | | | (-0.04) |
| | EBITDA | | | -3.55 | -4.064 | | | .0221*** | .0213*** |
| | | | | (-1.11) | (-1.28) | | | (3.42) | (3.23) |
| | EBITDA * SF | | | -9.679** | -9.285** | | | .0049 | .0061 |
| | | | | (-2.21) | (-2.04) | | | (0.87) | (1.10) |
| **Risk Panel** | SME-SF | | | -25.97*** | -25.28*** | | | .0162 | .0177 |
| | | | | (-3.88) | (-3.74) | | | (1.46) | (1.54) |
| | Basel III Gap | | | | -20.54*** | | | | -.0083*** |
| | | | | | (-4.82) | | | | (-2.85) |
| | Basel III Gap * SF | | | | -5.886*** | | | | -.0019 |
| | | | | | (-3.30) | | | | (-0.77) |
| | Log Risk Score | | | 17.63 | 19.08* | | | .0041 | .0049 |
| | | | | (1.58) | (1.67) | | | (0.34) | (0.40) |
| | High Risk * SF | | | 12.51*** | 14.23*** | | | -.0426*** | -.0444*** |
| | | | | (4.72) | (5.57) | | | (-4.83) | (-4.94) |
| | Linear | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| | Rel., Firm, Bank Controls | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| | Observations | 14,644 | 14,367 | 14,644 | 14,367 | 18,212 | 17,726 | 18,212 | 17,726 |

*Note:* This Table explores how credit scarcity, proxied by different relationship or firm-level characteristics, and bank capital scarcity, proxied by the difference between bank's Tier 1 under the transitional and full Basel III phase-in definitions (Basel III Gap), interact with the SME-SF pass-through. The first four columns report effects on rates; first, the baseline effect of a linear local specification, for reference; second, the interaction between SME-SF Eligibility and bank capital scarcity; third, the interaction with credit scarcity; fourth, the model including both interaction terms. The second four columns report effects on granted credit. The four different panels use alternative proxies for credit scarcity. We estimate all specifications locally with triangular kernel weights, over bandwidths chosen to minimize MSE. t-Statistics, from errors clustered at the firm and bank level, are in parentheses. *Controls*: **Relationship-level**: Lags of share of total drawn credit, revolving granted/total granted, utilized/granted, firm's and bank's hq in same province, log(relationship age). **Firm-level**: Lags of liquidity ratio, leverage, log(assets), log risk score (Altman z-score), EBITDA/assets, industry dummies, regional dummies, dummy for the presence of multiple relationships, investment ratio. **Bank-level**: A dummy singling out cooperative banks; the lag of CET1, liquidity, retail and wholesale funding ratios; the log of lag assets.

Table A7: **SME-SF's Effect Heterogeneity, 2013 Capital Gap Robustness**

| | *Dep. Variable:* | Rates Change, bps | | | | Granted, Log Change | | | |
|---|---|---|---|---|---|---|---|---|---|
| | | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **Drawn/ Granted Panel** | SME-SF | -20.91$^{***}$ | -18.91$^{***}$ | -29.06$^{***}$ | -27.79$^{***}$ | .0015 | .0023 | .0145 | .0168 |
| | | (-3.10) | (-2.70) | (-3.87) | (-3.55) | (0.15) | (0.22) | (1.05) | (1.13) |
| | Basel III Gap | | -21.79$^{***}$ | | -21.77$^{***}$ | | -.0088$^{***}$ | | -.0088$^{***}$ |
| | | | (-7.10) | | (-7.14) | | (-2.65) | | (-2.60) |
| | Basel III Gap * SF | | -6.524$^{***}$ | | -6.623$^{***}$ | | .0011 | | .001 |
| | | | (-2.87) | | (-2.89) | | (0.22) | | (0.21) |
| | Drawn/Granted | | | 8.154 | 9.553 | | | .0215$^{**}$ | .0234$^{**}$ |
| | | | | (1.17) | (1.31) | | | (2.28) | (2.38) |
| | Drawn/Granted * SF | | | 11.73$^{**}$ | 12.74$^{**}$ | | | -.0179$^{**}$ | -.0199$^{**}$ |
| | | | | (2.14) | (2.41) | | | (-2.09) | (-2.22) |
| **Relationships Panel** | SME-SF | | | -.8016 | .9065 | | | -.0391$^{**}$ | -.047$^{**}$ |
| | | | | (-0.06) | (0.07) | | | (-2.24) | (-2.55) |
| | Basel III Gap | | | | -21.89$^{***}$ | | | | -.0087$^{***}$ |
| | | | | | (-7.14) | | | | (-2.60) |
| | Basel III Gap * SF | | | | -6.312$^{***}$ | | | | -.6.9e-04 |
| | | | | | (-2.83) | | | | (-0.53) |
| | Multi Rel. | | | 3.807 | 2.518 | | | -.0088 | -.01 |
| | | | | (0.39) | (0.22) | | | (-0.91) | (-0.98) |
| | Multi Rel. * SF | | | -21.16$^{**}$ | -20.81$^{*}$ | | | .0443$^{***}$ | .0535$^{***}$ |
| | | | | (-2.09) | (-1.74) | | | (3.29) | (3.90) |
| **EBITDA Panel** | SME-SF | | | -23.41$^{***}$ | -21.67$^{***}$ | | | .0024 | .0037 |
| | | | | (-3.53) | (-3.11) | | | (0.24) | (0.35) |
| | Basel III Gap | | | | -21.61$^{***}$ | | | | -.0089$^{***}$ |
| | | | | | (-7.10) | | | | (-2.67) |
| | Basel III Gap * SF | | | | -6.763$^{***}$ | | | | .0013 |
| | | | | | (-2.91) | | | | (0.26) |
| | EBITDA | | | -3.55 | -3.582 | | | .0221$^{***}$ | .0216$^{***}$ |
| | | | | (-1.11) | (-1.10) | | | (3.42) | (3.27) |
| | EBITDA * SF | | | -9.819$^{**}$ | -10.91$^{**}$ | | | .0049 | .0079 |
| | | | | (-2.24) | (-2.47) | | | (0.88) | (1.42) |
| **Risk Panel** | SME-SF | | | -25.97$^{***}$ | -24.56$^{***}$ | | | .0162 | .0186 |
| | | | | (-3.88) | (-3.52) | | | (1.46) | (1.56) |
| | Basel III Gap | | | | -21.79$^{***}$ | | | | -.0089$^{***}$ |
| | | | | | (-7.15) | | | | (-2.63) |
| | Basel III Gap * SF | | | | -5.886$^{***}$ | | | | .0016 |
| | | | | | (-3.30) | | | | (0.36) |
| | Log Risk Score | | | 17.63 | 18.95 | | | .0041 | .0042 |
| | | | | (1.58) | (1.61) | | | (0.34) | (0.34) |
| | High Risk * SF | | | 12.51$^{***}$ | 14.08$^{***}$ | | | -.0426$^{***}$ | -.0479$^{***}$ |
| | | | | (4.72) | (5.70) | | | (-4.83) | (-5.12) |
| | Linear | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| | Rel., Firm, Bank Controls | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| | Observations | 14,644 | 13,817 | 14,644 | 13,817 | 18,212 | 17,059 | 18,212 | 17,059 |

*Note:* This Table explores how credit scarcity, proxied by different relationship or firm-level characteristics, and bank capital scarcity, proxied by the difference between bank's CET1 as reported at December 2013 and full Basel III phase-in definitions (Basel III Gap), interact with the SME-SF pass-through. The first four columns report effects on rates; first, the baseline effect of a linear local specification, for reference; second, the interaction between SME-SF Eligibility and bank capital scarcity; third, the interaction with credit scarcity; fourth, the model including both interaction terms. The second four columns report effects on granted credit. The four different panels use alternative proxies for credit scarcity. We estimate all specifications locally with triangular kernel weights, over bandwidths chosen to minimize MSE. t-Statistics, from errors clustered at the firm and bank level, are in parentheses. *Controls*: **Relationship-level**: Lags of share of total drawn credit, revolving granted/total granted, utilized/granted, firm's and bank's hq in same province, log(relationship age). **Firm-level**: Lags of liquidity ratio, leverage, log(assets), log risk score (Altman z-score), EBITDA/assets, industry dummies, regional dummies, dummy for the presence of multiple relationships, investment ratio. **Bank-level**: A dummy singling out cooperative banks; the lag of CET1, liquidity, retail and wholesale funding ratios; the log of lag assets.

Table A8: **Dependent Variable** Rate Change in *bp*; **Method** Fixed Effects RD

| | WRD, Pol(1) Double Clustered Errors | WRD, Pol(2) Double Clustered Errors | WRD, Pol(1) Double Clustered Errors | WRD, Pol(2) Double Clustered Errors |
|---|---|---|---|---|
| $\hat{\beta}$ 2014 | $-35.374^{***}$ | $-35.111^{***}$ | $-38.648^{***}$ | $-37.628^{***}$ |
| Eligible firms | (10.321) | (11.57) | (10.795) | (11.812) |
| Clusters | 2,931 (Firms), 93 (Banks) | 4,849 (Firms), 94 (Banks) | 2,852 (Firms), 90 (Banks) | 4,717 (Firms), 91 (Banks) |
| N. Observations | 7,566 | 13,081 | 7,340 | 12,695 |
| $\hat{\beta}$ 2013 | 7.034 | 2.506 | 6.807 | 1.56 |
| Eligible firms | (8.441) | (10.594) | (7.941) | (10.806) |
| Clusters | 6,484 (Firms), 96 (Banks) | 9,576 (Firms), 96 (Banks) | 6,313 (Firms), 96 (Banks) | 9,329 (Firms), 96 (Banks) |
| N. Observations | 17,368 | 26,346 | 16,935 | 25,690 |
| $\hat{\beta}$ 2014 | $-1.941$ | 15.126 | 2.293 | 23.180 |
| Non-Eligible firms | (20.360) | (32.773) | (21.234) | (35.1) |
| Clusters | 1,047 (Firms), 77 (Banks) | 1,089 (Firms), 78 (Banks) | 1,013 (Firms), 75 (Banks) | 1,051 (Firms), 76 (Banks) |
| N. Observations | 4,006 | 4,285 | 3,847 | 4,112 |
| Rel. Controls | | | ✓ | ✓ |
| Bank. Controls | | | ✓ | ✓ |
| Firm FE | ✓ | ✓ | ✓ | ✓ |

*Note:* This Table presents the results of discontinuity tests run *via* local estimation of $\hat{\beta}$ in Equation 3, augmented with fixed effects: $\Delta i_{bf} = \alpha + \beta R_{bf} + \phi(|x_{bf}^{2013} - \bar{x}|) + f + \nu_{bf}$, where $\Delta i$ is the interest rate change in basis points, $x_{bf}$ the past drawn credit, $\bar{x}$ the € 1.5 million threshold, $\phi$ the $x_{bf}$ polynomial independently estimated on the two sides of the threshold, $f$ firm fixed effect, and the null hypothesis of each test is $\beta = 0$. The different columns report increasing polynomial specifications, and - final columns - the estimates of the linear polynomial specification adjusted for covariates insertion.We compute estimates for the SMEs 2014 sample, the SMEs 2013 and non-SMEs 2014 placebo samples. We employ triangular kernel weights, and display in parentheses robust standard errors, double-clustered at the bank and firm level. The acronym WRD stands for "within RD".

*Controls*: **Relationship level**: Lags of share of total drawn credit, revolving granted/total granted, utilized/granted, firm's and bank's hq in same province, log(relationship age). **Firm level**: Lags of liquidity ratio, leverage, log(assets), log risk score (Altman z-score), EBITDA/assets, industry dummies, regional dummies, dummy for the presence of multiple relationships, investment ratio. **Bank level**: Lags of tier 1 capital ratio, liquidity, retail funding ratio, wholesale funding ratio, log(assets), a BCC dummy.

Table A9: **Dependent Variable** Interest Rate Change in *bp*; FE-Sample estimates

| **Simple RDD on Firm-FE Sample** | | | |
|---|---|---|---|
| $\hat{\beta}$ 2014 Linear | -41.39*** | -39.60*** | -45.97*** |
| | (-3.94) | (-4.31) | (-4.84) |
| Observations | 5,087 | 5,036 | 4,959 |
| $\hat{\beta}$ 2014 Quadratic | -43.50*** | -38.12*** | -43.61*** |
| | (-4.29) | (-3.87) | (-4.76) |
| Observations | 7,686 | 5,842 | 5,753 |
| **Non-Eligible Lines, Firm-FE vs Full Samples** | | | |
| $\hat{\gamma}$ 2014 Linear | 2.011 | 0.358 | -1.902 |
| | (0.36) | (0.06) | (-0.29) |
| Observations | 5,654 | 5,580 | 5,478 |
| $\hat{\gamma}$ 2014 Quadratic | 2.672 | -0.118 | -2.988 |
| | (0.52) | (-0.02) | (-0.42) |
| Observations | 6,202 | 6,117 | 6,007 |
| Firm Controls | ✓ | ✓ | ✓ |
| Bank Controls | | ✓ | ✓ |
| Relationship Controls | | | ✓ |

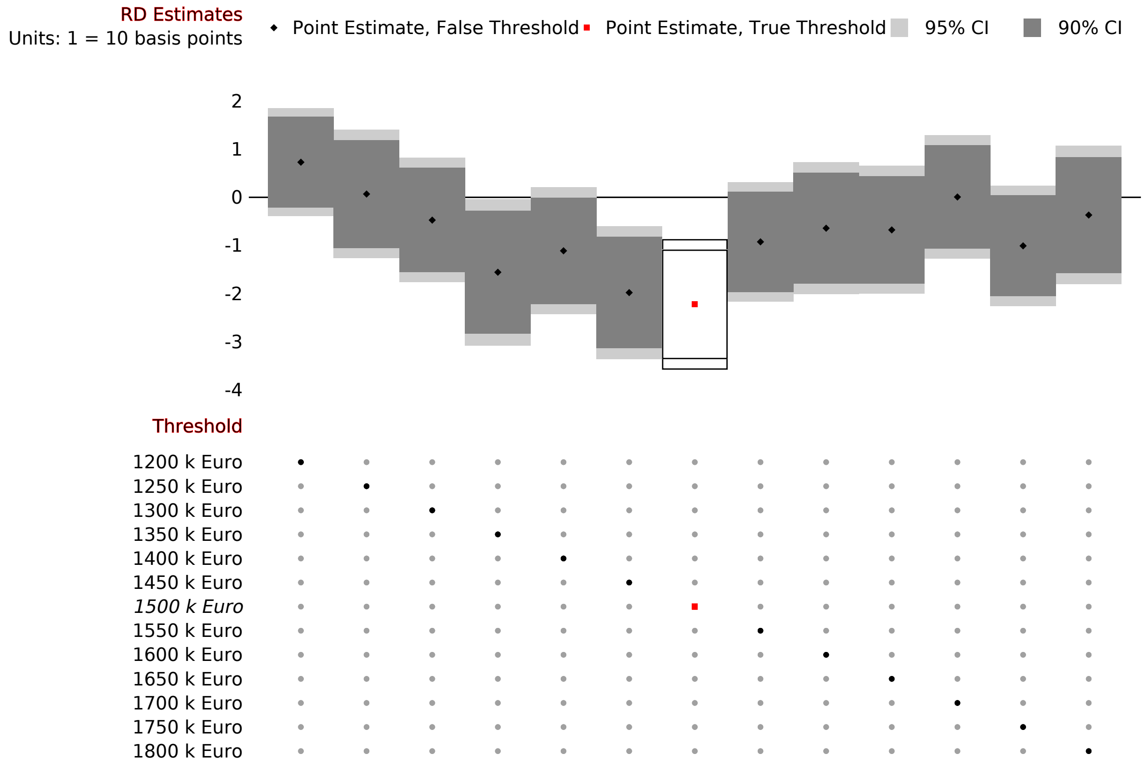
*t* statistics in parentheses

*** p<0.01, ** p<0.05, * p<0.1

*Note:* This Table presents, in the first two panels, the results of discontinuity tests run *via* local estimation of $\hat{\beta}$ in Equation 3: $\Delta i_{bf} = \alpha + \beta R_{bf} + \phi(|x_{bf}^{2013} - \bar{x}|) + \nu_{bf}$, on the sub-sample of observations on which the fixed effect estimator of $\beta$ is identified. Such sub-sample consists of all the observations belonging to firms that have at least one eligible and one non-eligible observation within the bandwidth selected by minimizing the MSE. $\Delta i$ is the interest rate change in basis points, $x_{bf}$ the past drawn credit, $\bar{x}$ the € 1.5 million threshold, $\phi$ the $x_{bf}$ polynomial independently estimated on the two sides of the threshold, and the null hypothesis of each test is $\beta = 0$. The different columns report specification including different controls. In the second two panels, the Table reports the results of the comparison between the pre-post reform change in the rates of non-eligible relationships within the firm fixed effects sample, and within the overall sample. Each time, we select the relationships within the right side of the data-driven bandwidth of the respective (1st or 2nd order) specification, and run the following test: $\Delta i_{bf} = \eta + \gamma S_{bf} + \phi_{+}(|x_{bf}^{2013} - \bar{x}|) + \Omega C_{bf} + \epsilon_{bf}$. $\Delta i_{bf}$ is the interest rate change in basis points, $S_{bf}$ is a dummy equal to one if the observation falls in the local sub-sample for which the firm fixed effect is identified, $x_{bf}$ the past drawn credit, $\bar{x}$ the € 1.5 million threshold, $\phi_{+}$ the right$x_{bf}$ polynomial independently estimated on the two sides of the threshold, $C_{bf}$ includes other covariates, $\nu_{bf}$ is the stochastic error term, for which we allow clustering at the bank and firm level, and the null hypothesis of each test is $\gamma = 0$. The different columns report specifications including different controls.

*Controls*: **Relationship level**: Lags of share of total drawn credit, revolving granted/total granted, utilized/granted, firm's and bank's hq in same province, log(relationship age). **Firm level**: Lags of liquidity ratio, leverage, log(assets), log risk score (Altman z-score), EBITDA/assets, industry dummies, regional dummies, dummy for the presence of multiple relationships, investment ratio. **Bank level**: Lags of tier 1 capital ratio, liquidity, retail funding ratio, wholesale funding ratio, log(assets), a BCC dummy.

Figure A1: Permutation Test




*Note:* The Figure reports the point estimates and confidence intervals (gray shaded areas) for discontinuities in the cost of credit dynamics at alternative thresholds, progressively more distant from the true SME-SF assignment cut-off. Black diamonds represent point estimates obtained at the displaced thresholds, while the red square marks the estimate at the real SME-SF assignment threshold. We estimate the following specification $\Delta i_{bf} = a + \beta R_{bf} + \phi(|x_{bf}^{2013} - \bar{x}_p|) + \nu_{bf}$ over MSE-minimizing bandwidths, different at the two sides of the threshold, and employing a triangular kernel. Here $x_{bf}$ is past drawn credit in thousand of Euros, $\bar{x}_p$ the assignment threshold we choose, $\phi$ the linear $x_{bf}$ polynomial independently estimated on the two sides. Below the plot, under the label "Threshold", each line marks for which $\bar{x}_p$ we estimated the model. A black dot to the right of the corresponding line marks that the estimate above and its confidence interval concern that specific threshold $\bar{x}_p$.