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 of a change in capital requirements on the cost of credit. We exploit a reduction in the risk weight applied to small loans to small and medium enterprises, the SME-Supporting Factor (SME-SF). Employing a regression discontinuity design and matched bank-firm data from Italy, we infer an average reduction in interest rates of twenty-six basis points per a two percentage points drop in capital requirements. Thanks to the unique setting, we can measure heterogeneity by considering firms' and banks' characteristics. Firms' switching costs and banks' regulatory capital scarcity drive an approximate fifty and twenty percent effect variation, respectively. Moreover, we find evidence suggesting that the SME-SF in its initial design may have caused the rationing of some marginal customers. Our findings indicate that the distribution of firms' switching costs and regulatory capital scarcity for banks is key to determining who actually benefits from changes in bank capital regulation.

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

JEL Classification: E51, G21, G28.

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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 regarding such cost in a quasi-experimental setting, exploiting a regulatory capital discount targeting a specific category of loans to shield European Small and Medium Enterprises (SMEs) from the adverse effects of Basel III tougher regulation. From the impact of such discount on the spread on SMEs revolving credit facilities, we infer that a 2 percentage point decrease in minimum capital requirements causes a 26 basis points drop in interest rates on bank loans.

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 importance of pass-through heterogeneity. We show that: (i) Borrowers with multiple healthy credit relationships drive the effect; (ii) better borrowers (high EBITDA) get an about 40 percent larger discount; (iii) worse borrowers (high drawn over granted ratio or high risk score) get a 50 to 60 percent smaller discount. Furthermore, banks with less regulatory capital resources should have higher shadow cost of capital, and indeed, we observe a 20 percent larger discontinuity for credit relationships belonging to these banks. Finally, credit to borrowers in worse shape grows less, suggesting that the initial SME-SF's sharp assignment cut-off could engender undesirable effects. Overall, we highlight the role played by the entire distribution of borrowers' and lenders' characteristics in determining the effects of changes in capital requirements.

To obtain our estimates, we collect a rich dataset on bank loans to firms from the Italian Credit Register. We match the credit information with firm and bank characteristics, and exploit a change in regulation called Small and Medium Enterprises Supporting Factor (SME-SF). The SME-SF is a reduction in capital requirements introduced on January 1, 2014, through Article 501(1) of the Capital Requirements Regulation (CRR), for eligible loans to SMEs. Risk weights of eligible exposures are reduced by 23.81 percent; considering a corporate loan to a SME with a risk weight of 100 percent, and a minimum capital requirement of 8 percent of risk weighted assets, the reduction in the minimum capital requirement is approximately 2 percentage points.

The regulation and subsequent guidelines by the European Banking Authority define as eligible SME's credit lines those for which the lending bank's total exposure is below 1.5 million €. Hence, the SME-SF introduces a discontinuous change in the minimum capital requirement for bank-firm pairs involving an SME around the 1.5 million € threshold. We will use this discontinuity to study the pass-through of the reform to the cost of credit for firms, implicitly measuring the cost of capital requirement for banks.

Under the assumption that potential confounding factors do not change discontinuously at the 1.5 million € threshold, we can employ the SME-SF eligibility rule to estimate the effect of capital requirements on lending rates with a 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

[1] The approach, introduced by Thistlethwaite and Campbell (1960), is commonly applied 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).

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 few thousand euros difference in the yearly cost of credit for credit lines that are drawn for more than one million euro. Moreover, we observe that placebos for non-SME relationships and SME relationships before the SME-SF do not bring evidence of spurious effects.[2]

Our baseline analysis shows that, after the SME-SF implementation, loans that benefit from the capital charge discount experience 26 basis points smaller interest rate increases compared with loans that are not eligible. Dividing the estimates above by the 2 percentage point discontinuity in capital requirements implied by the SME-SF, we obtain the average pass-through of 13 basis points per percentage point decrease in the requirements. Nevertheless, this is an estimate of the average impact, which may only partially reflect the benefit coming to the banks from the marginal capital requirements relaxation.

First, if firms are to some extent captive customers for banks, banks can exercise monopoly power and changes in the cost of credit will reflect only a fraction of the surplus. Borrowers are not captive when they have the possibility to access alternative sources of credit, i.e. when they face low switching costs.[3] Second, the supporting factor may be more beneficial to the clients of banks whose regulatory capital is scarcer, as these banks would otherwise apply a greater cost increase than capital abundant banks, after Basel III's tighter capital regulation comes into force.

We exploit our rich dataset to explore firm-level heterogeneity across multiple proxies of switching costs. Furthermore, we access unique supervisory information on banks' regulatory capital gap with respect to what will count as capital under the more restrictive Basel III rules. We use this information to elicit regulatory capital scarcity at the bank

[2] We also show in the Appendix that estimates are stable to the inclusion of additional firm, bank, and relationship control variables, mitigating concerns about the local nature of the results (Angrist and Rokkanen, 2015).

[3] The importance of low switching costs for the dynamic of credit and its cost has been documented, for example, in 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. Indeed, firm fixed effects would identify the pass-through only for firms with multiple relationships, some of which are eligible and some of which are not. These firms should be less captive, and are likely to receive greater discounts (Ioannidou and Ongena, 2010). In the Appendix we demonstrate that this intuition holds true, documenting an increase in the point estimate upon inclusion of fixed effect, and we show that such increase can be fully attributed to sample selection.

level. Indeed, the desired level of equity buffer for each bank is likely to be above the regulatory minimum, with possibly unobservable bank characteristics playing an essential role in determining the distance from the constraint (Repullo and Suarez, 2013). For this reason, looking e.g. at the difference between the CET1 ratio and the regulatory minimum does not necessarily capture how far the bank is from its desired capital ratio. We instead measure capital scarcity in terms of the (adverse) impact of the tighter regulatory capital definition on each bank's capital ratios. The intuition is that the larger the impact, the more significant the future effort by each bank to regain its desired level of buffer, allowing us a window into each bank's shadow cost of regulatory capital.

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 euro of regulatory capital saved. On the other hand, even if we detect no average effect on the amount of credit granted, we observe that banks tend to increase granted credit less on those eligible lines whose utilization is closer to the maximum granted amount. Thus, our exercise stresses the possible danger of unintended effects from risk-weights rules based on sharp cut-offs, supporting the subsequent decision of substituting the SME-SF's cut-off with a smooth tapering of the discount (Lecarpentier et al., 2020).

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.

The assessment of the average pass-through we present is considerably larger than the one suggested by the model in Kisin and Manela (2016), which backs the shadow cost of capital requirements from the extent to which banks exploit a costly loophole in regulation.[4] Our magnitudes are also larger than those presented in the two quasi-

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

experimental studies by Plosser and Santos (2018) and Glancy and Kurtzman (2022). This difference likely stems from focusing on the Basel III framework, as Glancy and Kurtzman (2022). Since Basel III introduced a more significant tightening of capital regulation, the fact that studies focusing on the same period find larger numbers for the cost of bank capital is suggestive of non-linearities in this cost. Moreover, with respect to Glancy and Kurtzman (2022), we focus on European banks, which may face a different cost of financing than US banks.

Uniquely, we highlight how the effect of capital regulation depends jointly 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). On the one hand, we show evidence of how firms' costs of switching lenders can influence the pass-through, suggesting a significant 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] On the other, we document the importance of the individual bank capital scarcity. Indeed, we find substantial variation in the distribution of the shadow cost of capital, with banks that are one standard deviation more capital constrained than average revealing a 20 percent higher shadow cost.

Our approach to measuring the shadow cost, similarly to the one in Plosser and Santos (2018), has the advantage of not depending on a difference-in-difference plus fixed effects strategy. The latter approach has been recently subject of methodological revisions (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 analysis sheds light on using risk weights as a policy instrument. Targeted changes in risk weights are being employed more and more within the framework of

[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., 2015; 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.

macro-prudential policy (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) on the macro-prudential side, and 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 and showing evidence suggestive of an unintended effect on credit access around the implementation threshold. Indeed, we see a two to four percent smaller growth in credit granted around threshold for eligible exposures that were riskier, belonged to firms with only one credit relationship, or presented a one standard deviation higher than average utilization rate. In this sense, our findings are closely linked to Becker, Opp, and Saidi (2021) which, in an alternative discontinuity setting and for insurance companies, documents how sharp changes in the risk-weights assignment can 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 strategy; Section V illustrates and interprets the results. Section VI concludes.

II Institutional Background

Bank capital requirements are based on three main ingredients: 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 new standards were adopted in the European Union in June 2013, and came into force on January 1, 2014;[7] some of the measures were applicable immediately while others were subject to a gradual phase in.[8]

[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.

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 bank capital regulatory buffers for loss absorption.

Considering that under the previous framework (Basel II) banks were required to hold 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. First, the Small and Medium Enterprise Supporting Factor (SME-SF), which will help us identify the effects of capital regulation changes. Second, the transitory regime for the adoption of the more stringent definition of capital, which will help us trace out bank-level heterogeneity.

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[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 Common Equity Tier 1; 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 Common Equity Tier 1 capital [...] and Regulatory adjustments applied in the calculation of Common Equity Tier 1" (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.

[11] For a more detailed comparison between the Basel II and Basel III regimes, we refer 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 athttps://eur-lex.europa.eu/legal-content/EN/TXT/?uri=celex%3A32013R0575.

II.1 The SME Supporting Factor

The SME-SF is a discount of 23.81 percent on the risk weight that applies to loans granted to firms with turnover below euro 50 million, provided that the total exposure of the lender to each eligible firm is below euro 1.5 million. The magnitude of the SME-SF was set to exactly counteract 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. As a consequence, 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 euro $128,000$. Instead, the minimum requirement on a 1.4 million SME exposure would amount to $85,000$ euro, taking the SME-SF into account. Such stark change in minimum capital requirements at the SME-SF eligibility threshold provides ground to 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:

[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.

[13] One of the largest Italian banking groups.

> 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, which is 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 that the SME-SF provides a promising testing ground to improve our understanding of the effects of minimum capital requirement regulation. In particular, the effect of the SME-SF on 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

One of the key aims of the Basel III reform is to increase the quality of capital by tightening the definition of the highest quality capital (Common Equity Tier 1 - CET1). CET1 must now be distinguished from additional Tier 1 capital, the latter being constituted by all unsecured and perpetual non-common shares instruments. Moreover, the Basel Committee demands deductions from CET1 capital for 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, some third parties' equity and securitization exposures that under Basel II used to require 50 percent deductions from Tier 1 capital can no longer be deducted.[14]

An immediate application of these tighter definition of equity 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. The drop would depend on a 16.4 and 21.8 percent wipe out of common equity resources for large and other banks, respectively, and on a 10 percent increase in risk-weighted assets common to all banks.

In the previous subsection we described one measure targeting the capital ratio denominator through risk weights to mitigate the impact of the new rules. Other measures were adopted to avoid an abrupt drop in the capital ratio's numerator. The Basel Committee (2011) Global Regulatory Framework Report contains not only recommendations for the phase-in of the increase in capital ratios (Section C, paragraphs (a) and (b)), but also on the phase-in of the capital quality increase (paragraphs (c) and beyond). These last recommendations design a gradual path to distribute the equity resources' wipe-out over time. 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".[15] Capital ratios computed under these laxer rules, before 2018, are "Transitory" capital ratios, which we compare with the full phase-in ones.

Detailed information on the transitory and the full phase-in capital ratios for Italian banks was collected by the Bank of Italy's supervisory reports. We employ this information to measure the overall capital wipe-out each bank would face if immediate full phase-in was enacted, instead of the transitory regime. We interpret this measure as capturing 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.

[14] 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] The implementation of the phase-in is left to supra-national and national regulators. The European Union's CRR Article 478[16] establishes the deadlines for the implementation of the transitory framework for deductions. These are reflected in the Bank of Italy's instructions[17] and broadly match the Basel Committee suggestions.

III Data and Measurement

We construct our dataset by matching information on loan quantities and interest rates from the Italian Credit Register and from the Bank of Italy archive on interest rates (TAXIA) with balance sheet information on borrowers from Cerved dataset, 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 $30,000$ euro, irrespective of whether disbursed or not. TAXIA includes information on interest rates on loans to borrowers that have at least euro $75,000$ overall granted or disbursed credit, reported by all but the smallest banks. The TAXIA sample is still highly representative, as the aggregate value of loans of reporting banks is about 80 percent of credit outstanding. 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 years $2013 - 2014$ to investigate the impact of the reform, and years $2012 - 2013$ to run placebo tests, and we focus on revolving credit lines. We focus on interest rates on revolving credit lines as, in Italy, these loans are relatively standardized and not collateralized, with a rate that is adjustable on short notice. We adjust our dataset for banks' mergers applying the group structure of 2014 to 2013 relationships and, of 2013 to 2012 relationships. We also aggregate credit relationships at the top tier bank holding company level, because capital requirements are set for the consolidated entity and eligibility for the SME-SF is based on group exposure.

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. We make the choice to consider such yearly time window as we do not observe when credit lines are re-bargained, but only the resulting change in rates. Hence, we want to encompass a period of time that is long enough to include changes in the cost of the line, and short enough such that it can be reasonable to attribute changes to the implementation of the SME-SF.

III.1 Defining Eligibility for the SME-SF

To perform our analysis we need to identify relationships that are eligible according to the regulation. The SME-SF is applicable to exposures below euro 1.5 million towards firms with gross sales below euro 50 million, excluding any amount that is 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 resort to the Credit Register data to identify SMEs' credit relationships that are below the exposure threshold. Eligible relationships are those for which total credit disbursed is below 1.5 million, regardless of the amount granted. We assess eligibility as of the end of period t-1 when analyzing the change in loan rates from t-1 to t. This means that 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 we recover is thus a proxy for being "treated" with the SME-SF. First of all, we notice that this is the best that can be done, as banks do not report treatment status of each credit relationship, only aggregate exposure to SME-SF eligible loans. 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

[18] For example, if a bank grants a euro 5 million loan and the firm posts residential real estate collateral covering euro 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 euro 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).

[20] Intuitively, if lowering of 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 threshold at the end of December 2013 end up not being assigned to the SME-SF, and vice-versa. As we estimate a significant *decrease* in rates, we can conclude that we find at worst a lower bound for the actual effect on rates of lowering capital requirements. To the best of our knowledge, 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 (2021), to which we refer for a deeper discussion of the matter.

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 that do not have policies in place to track SME-SF eligibility can be distinguished from banks that are active in exploiting the SME-SF. We have access to this bank-level information on whether credit relationships eligible to the SME-SF have been recorded and reported, and we use it to drop banks that do not report any SME-SF exposures.

Moreover, we drop relationships involving firms whose SME status appears uncertain – which is, firms with either a low revenue, but assigned to the size-class "large" and vice-versa, or firms assigned to the size-class "large" which report assets below the 43 million Euro threshold employed by the EU to define non-SME firms, or SMEs that appear to hold an extremely large number of relationships (more than 11, which is the last percentile of the number of relationships' distribution).[22] Furthermore, as we cannot distinguish between residential and commercial real estate collateral, we focus on relationships that are not collateralized and drop the others. As we focus with revolving loans, which are usually not collateralized, the selection concerns due to this restriction are limited. Moreover, we restrict our attention to good-standing relationships, as the SME-SF only applies to performing borrowers. Finally, we exclude firms with deeply negative (< -20 %) 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 extreme 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 they are close to default and add noise to the treatment assignment. Again, banks cannot apply the capital requirement discount to nonperforming borrowers.

[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] These may be for example branches of larger firms, managing the large firm credit access.

III.2 Explanatory Variables

Our dataset includes information on relationship, borrower and bank characteristics that could influence the interest rate on loans. We use these variables for three purposes. The first purpose is to verify that there are no discontinuous changes in observable characteristics at the SME-SF eligibility threshold; the second is to increase the precision of our estimate of the impact of the SME-SF, as suggested by Angrist and Rokkanen (2015). Finally, we use some of these variables to explore the heterogeneity in the SME-SF pass-through across firms or across banks.

The first set of variables is meant to capture 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 of 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 for 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 is located in the same province as the one where the bank is headquartered or not. The literature finds that proximity captures availability of soft information about the firm,[23] which lowers screening and monitoring costs for the bank. 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 maximum value of 9, because the reports from which we extract our dataset start in the year 2005.

The second set of controls proxies for credit risk and other firm characteristics, including profitability, leverage and liquidity, which banks take into account when they set interest rates. We measure profitability as gross operating profits, scaled by total assets (EBITDA Ratio); liquidity is measured as liquid assets scaled by total assets; leverage is computed as the ratio of debt to the sum of debt and equity. Furthermore, to capture credit risk on top and above leverage, we include a score based on the methodology

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

proposed by Altman (1968), computed by Cerved. The score takes values from 1 to 9, increasing in credit risk. To exploit such information in our regressions, we include a dummy identifying firms with scores above 6, considered risky in the Cerved methodology. To proxy for industry and regional specific characteristics, we include industry dummy variables based on the two-digit Statistical Classification of Economic Activities adopted by the EU,[24] and region dummy variables for the location of the firms' headquarters (North West, North East, Center and South). Finally, we track whether firms have access to multiple or only one credit relationships in good standing to identify captive customers.

The third and last set of controls is meant to capture banks' characteristics that are likely to influence the cost of loans, particularly funding and capitalization. We collect the following bank variables: Tier 1 capital ratio, the ratio of liquid assets to total assets, the fraction of assets funded with retail funding sources, the fraction of assets funded with wholesale funding excluding central bank funding. We also include the log of total assets to control for bank size.

Finally, we collect from the capital section of supervisory reports the CET1 ratio of banks immediately after the Basel three reforms entered into force (transitory CET1 ratio as of March 2014) and the fully phased-in CET1 ratio, i.e. what that same ratios would be if the Basel III rules concerning capital quality were applied altogether. As Basel III increases the quantity and the quality of banks' capital, the difference between the actual CET1 ratio and the fully phased-in measures the scarcity of capital induced by the reform. We standardize this difference in units of standard deviation to rank banks in terms of relative regulatory capital scarcity. 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

[24] 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.

SMEs); approximately $6,500$ observations are instead non-eligible relationships of eligible 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 there are significantly fewer observations referring to large firms with large amounts of disbursed credit (fourth quadrant in Figure 1), observations' density decreases continuously with size and 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 for the placebo (2013, right panel) samples. The information on the changes in interest rates refers to average spreads against the quarterly inter-bank rate year on year, while the control variables are instead measured 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 the 2014-2013 time window the cost of credit increased less on eligible than on non-eligible relationships.[25] All firm characteristics, instead, were similar in the two periods, for both firms with and without eligible credit relationships.

Interestingly, the cost of non-eligible lines increases by 11 basis points more than the one of eligible lines in 2014, while in 2013 the rate on non-eligible lines increases 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 point for treated credit lines, quite close to the result we obtain through our Regression Discontinuity Design (RDD). The estimation of the effect by regression discontinuity is though crucial, as the result of this back of the envelope calculation may well reflect differences between large and small lines that are not related 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

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

may confound our pass-through estimates if not dealt with appropriately. In the next section we describe our approach to estimate the causal effects of the SME-SF based 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.[26] The design of the SME-SF allows us to approximate this ideal, as we can exploit it for a regression discontinuity design (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 is an improvement with respect to including firm and bank fixed effects and considering all relationships irrespective of their size.[27] 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, there is a pricing function chosen by the bank:

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

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 that the bank b has to set aside at time t on loan granted to firm f.

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

[27] Such approach is widely employed in the empirical banking literature. One of the first examples in a banking context is Khwaja and Mian (2008); other studies are Jiménez et al. (2012), Schnabl (2012), Jiménez et al. (2014), Jiménez et al. (2017).

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:

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

where the effect of the SME-SF would be captured by the coefficient β 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 ϵ_{bf} is the residual term. From Equation 1, we see that ϵ_{bf} includes three main components: $\epsilon_{bf} = D^*_{bf} + S^*_{bf} + e_{bf}$ 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 more frequently. Since these 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. As a consequence, $\hat{\beta}$ 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 would bias upwards $\hat{\beta}$. Finally, even if we focused on firms with multiple relationships, we would include in the comparison very heterogeneous observations (some very small, some very large loans). 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 (Sette and Gobbi, 2015).

The RDD overcomes the issue of comparability because it is a local approach. It exploits the fact that the treatment status is defined by an arbitrary threshold for a continuous variable. If bank-level and firm-level confounding factors do not vary *discontinuously* around the threshold, we can use the untreated relationships that are close to the threshold as counterfactual for the treated relationships that are close to threshold, and attribute any discontinuous change in the cost of credit (Δi_{bf}) between the two types of relationships to the SME-SF policy.

Eligibility for the SME-SF is based on a bi-dimensional assignment rule (see Figure 1) that takes into account firm gross sales (turnover) and the credit drawn by the firm *vis a vis* the bank. As stated in Section III, the turnover threshold is part of the criteria that define an SME for other policies in the EU. We thus cannot use it for identification, as it is highly likely there are other confounding factors that vary discontinuously at this threshold. Instead, we focus on SMEs (i.e., firms below such turnover threshold), and implement a RDD around the euro 1.5 million exposure threshold.

Conditional on meeting the turnover eligibility criterion, the treatment probability changes sharply at the exposure 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 threshold, meant to control for disturbances that vary continuously with credit utilization; β is the parameter of interest, measuring the effect of the treatment; ν_{bf} is a stochastic error component.

To estimate β 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 keep into account the decrease in observation density that occurs with relationship size. Automating the bandwidth choice reduces our degrees of freedom, while Calonico et al. (2017)'s routine ensures that our estimates are corrected for the bias introduced by bandwidth selection.[28]

IV.1 Validity of the RDD Design

The validity of RDD depends on the relatively weak assumptions that all possible confounders are continuous at the threshold defining the treatment assignment rule, and that there is no manipulation of the threshold by treatment takers.[29] Given the richness of our data we can take a number of steps to show that concerns about the RDD validity are reasonably limited. We provide two types of evidence to support the assumption of continuity. The first type is direct evidence of absence of manipulation of the assignment variable (McCrary, 2008); the second, is evidence of the absence of a discontinuity at the threshold for relevant exogenous variables (see, e.g. Lee and Lemieux, 2010). Any evidence of manipulation or discontinuity in covariates would raise the concern of sorting around the threshold, which would invalidate the design. Below we show that there is no evidence of manipulation or discontinuity in relevant covariates in our data.

Manipulation: If the subjects under study were aware of the treatment before its introduction, and could *perfectly* manipulate drawn credit (the forcing variable), they would be able to sort on their preferred side of the threshold. Sorting could correlate with some unobservable characteristic of subjects, implying that such unobservable characteristic varied discontinuously at the threshold, invalidating the continuity assumption.

In principle, one could argue that firms that are more informed anticipate the policy and adjust their relationship level credit demand to stay below the eligibility threshold and benefit from the capital charge discount. If these firms were the better managed ones - they were aware of relevant policy changes - they would also plausibly be able to negotiate lower interest rates for reasons other than the SME-SF. Alternatively, one could think that banks that are facing 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, for example by posting additional collateral.

A first counterargument is that, in practice, the demand for credit of firms is subject to unforeseen shocks that can move marginal credit relationships on the two sides of

[28] 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).

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

the SME-SF eligibility threshold. The policy is based on the notion of exposure (drawn credit), which includes also exposure generated by contingent liabilities such as guarantees and letters of credit provided by banks. The unpredictability of liquidity demand is supported by evidence that firms hold significant amounts of unused credit lines to meet unexpected needs. In our sample, the average ratio of credit disbursed to credit granted is about 60 percent. Perfect manipulation would be difficult.

A second counterargument is that, even if firms could manage exactly their exposure at all times, manipulation would require ex-ante knowledge of the *exact* eligibility threshold. We note that before the approval of the SME-SF regulation there was considerable uncertainty about the eligibility threshold that would have been applied, and on how exposure had to be computed. 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 euro 1 million to euro 5 million for SMEs" (EBA, 2016). The 1.5 million exposure threshold appeared in the final draft that was approved the 26th of June 2013,[30] but banks were uncertain about the criteria they had to follow to compute the eligible exposure until 2014.[31] We can thus conclude that banks were unlikely to be ready to identify eligible exposures sufficiently in advance, and to incentivise marginally ineligible customers to reduce their exposure below the threshold before the change was effective.

To support our case, we test for manipulation following McCrary (2008). When the incentive to manipulate goes in one clear direction, a discontinuity in the density of observations around the threshold should be observable. If firms prefer to be eligible, and there are enough firms that are informed, we should observe significantly less marginally non-eligible relationships than the marginal eligible ones. A simple density test can

[30] The SME-SF timeline is: first official record in a "proposal for a regulation of the European Parliament and of the Council on prudential requirements for credit institutions" dated 12 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 euro 2 million for banks calculating their capital requirement with the Standard Approach, and to euro 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 is finally approved in June 2013.

[31] 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).

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 till the *immediate aftermath* of its implementation. 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, possibly biasing the treatment effect estimate upwards. Coherently with the logic above, the test does not detect any statistically 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 the light of the empirical findings we present in the next Sections. Our estimate 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.[32] Even a firm at the top of the effect 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 (proxied with the extent of CET1 ratio wiped out by the full phase-in Basel III rules, see Table 4, Section V), thus getting about a 40 basis points discount, would get a mere 6,000 € discount on the yearly cost of revolving credit.

We can see that such numbers are economically 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 €.[33] The average yearly revolving rate for such firms is 8%, and thus the average yearly cost of revolving lines overs around 500 thousand €. Consequently, even in the best case scenario, a manipulating firm would 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

[32] This number is equal to the delta rates we estimate times a 1,49 million euro credit line utilization.

[33] The fact that firms establish a multiplicity of credit relationships is common in Italy and has been often documented, e.g., Detragiache, Garella, and Guiso (2000); Sette and Gobbi (2015).

more likely to appear on one side of the threshold than the other. We estimate the exact specification as Equation 3, replacing the dependent variable with each of the relationship, firm, or bank variables described in Section III, to detect any significant discontinuity at the eligibility threshold. In Figure 4 we plot the discontinuity estimates (black diamonds) and confidence intervals (grey 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 characteristics considered.

V Results

We start inspecting the behavior of interest rates changes around the SME-SF eligibility threshold. In order to do so, we show in Figure 5 fit and confidence intervals from local kernel regressions of changes in interest rates (dependent variable) on past credit utilization of firm f from bank b, 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, there is a evidence of a discontinuity in the interest rate changes at the policy threshold. Which is, the price of credit relationships that were not eligible to the SME-SF discount in December 2013 appears to grow more than the price of their eligible counterparts. Local kernel regressions for the 2012-2013 sample, or at placebo thresholds inspected at the same time as the SME-SF implementation, or for non-SME (Figure 5 top right and bottom panels), do not show comparable and statistically significant "jumps" in the behavior of rates.

This evidence is suggestive of an effect of the policy, but in order to get a precise idea of the significance and magnitude of the effect we need to compute discontinuity point estimates and confidence intervals corrected for the bias coming from bandwidth

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

[35] Such neighborhood is selected employing the mean square error minimization method studied in Calonico, Cattaneo, and Titiunik (2014). In particular, we perform the necessary computations in Stata, employing the most recent update of the rdrobust package, and constrain the width of the eligible and non-eligible intervals to be equal for clarity of graphical presentation. As our data continuously decrease in density with the increase in the dimension of the credit lines, this choice is not the most conservative, and we remove such restriction when we compute the discontinuity point estimates in order to quantify the average effect.

selection (Calonico, Cattaneo, and Titiunik, 2014). The results are displayed 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 that there is a statistically significant 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.[37]

Placebos. We complement our main result with two key placebos, thanks to the rich treatment space implied by the SME-SF. The first one addresses the possibility that other policies already in place may be affecting differently relationships below and above the threshold of the SME-SF. Access to credit for Italian SMEs has been supported by different policy interventions. There are two main programs at this purpose, the *Nuovo 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 of these programs were already active as of December 2013, we check that no other discontinuity at the SME-SF threshold was present for Δi_{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 detect a statistically significant discontinuity in the change in interest rates between 2012-2013 for relationships with credit drawn above and below euro 1.5 million at the end of 2012.

The second placebo addresses the concern that there could be some alternative driver of our result, having to do with small credit relationships. It is unlikely for small credit relationship to be less pricey or less subject to price increase, as fixed costs hit them more heavily, but we may entertain the possibility that capital constrained banks see them as less capital consuming in general, no matter the SME-SF. If enough banks would treat the euro 1.5 million in terms of past exposure as a rule of thumb to classify small credit lines, we may have a spurious driver of our results.

If this were the case, though, we should find a discontinuity at the threshold also for firms that are not SMEs according to the definition of the SME-SF. We run a placebo test estimating Equation (3) on firms with turnover above euro 50 million. We display

[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 **??**, 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 actually larger in magnitude for very small and hand-picked bandwidths.

[38] For details on such programs, we refer to Infelise (2014).

the results in the third line of Table 3, showing how there is no such discontinuity, no matter the specification.

V.1 Heterogeneity: Switching Costs and Capital Scarcity

Even if the baseline results capture the average pass-through from the SME-SF to the cost of credit, this might not reflect the full extent of the benefit of a capital discount for banks. First, as SMEs often depend on banks for their funding, it is reasonable to suspect that the pass-through may differ for firms that have difficulties finding alternative sources of credit. Second, the shadow cost of regulatory capital is likely to differ across banks, depending on how binding each regulatory constraint is. If our $\hat{\beta}$ indeed captures the shadow cost of regulatory capital, we expect estimates to be greater for banks with higher shadow cost. As our setting does not require fixed effects of any sort 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 prices *and credit granted*. Within our data, we find no average local effect from the SME-SF on the growth of credit granted.[39] Nevertheless, the absence of quantity effects at the threshold may conceal pass-through heterogeneity. Here we show that the evidence points in this direction, suggesting that banks reduce granted credit to 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, there is recent incremental evidence that the same channel is an important mediator for monetary policy pass-through (Agarwal et al., 2015; Scharfstein and Sunderam, 2016; Benetton and Fantino, 2021). 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 credit providers limits the exploitation

[39] We document this in Appendix Table A5.

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, on top and above a simple indicator of the presence of multiple credit lines, we also track the overall past utilization rate; the firm capacity to generate earnings, as proxied by EBITDA over assets; the firm's 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. By standardized we mean a continuous variable taking value one if relationship bf's figure exceeds the population average by at least one standard deviation. Finally, we identify risky firms by a dummy variable equal to one if the risk rating is in the top 4 notches of the nine value scale of the rating.

As regards **bank-level heterogeneity** we consider the measure of capital scarcity grounded in the transitory regime established for the implementation of Basel III, and we refer the reader to Section II.2 for the regulatory details underlying our measures. Starting with the first available dates, i.e., March 2014, we track the difference between reported transitory capital ratios, and what the capital ratios would be under full phase-in of the new capital buffer definition:

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

we assume that the greater the adverse impact of the fully phased-in definition, the greater the shadow value of an additional euro of regulatory capital to that specific bank.

Again, we standardize the variable for interpretation ease. A one unit variation in the Basel III Gap_{bt} implies that bank b funding relationship f at time t faces one standard deviation larger gap from Basel III new rules than the average bank. We focus on the difference between the transitory and fully phased-in CET1 ratios computed using the risk-weighted asset as of March 2014 as denominator.[40]

The Gap based on supervisory reports adds key insights on top and beyond simply looking at 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. Instead, a 1.4% difference between the transitory and the fully phased-in CET1 ratio (one standard deviation) is a better indication of capital scarcity. 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 resources to revert to its desired buffer by year 2018.[41] Conversely, a negative value indicates that a bank is likely to hold too much capital under the new regime.

We thus assume that the new regulation causes a shock that 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 transitory and full phase-in CET1 ratios is indeed continuous at the SME-SF assignment threshold.

In order to obtain a meaningful comparison of parameters and confidence intervals, we estimate 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; ϕ is

[40] In the Appendix Tables A6 and A7, we show that our results are robust to using as first term of the above difference either Tier 1 capital at the numerator, or the CET1 ratio at the end of 2013 as the numerator and the 2013 risk-weighted assets as the denominator.

[41] With gradual steps that will start in 2015, as the phase-in starts gradually by that date. See p.11 for more details.

[42] To perform the estimation, we select the bandwidth using Calonico et al. (2017), then 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 can be considered limited.

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. ϵ_{bf} is an error term clustered simultaneously over firms and banks. Finally, the Δy_{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.

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

We can see that across all estimates in the first four columns block, there is relevant variation in the pass-through to interest rates. The interaction coefficients range from 50 to almost 100 percent of the average discount estimates reported in Table 3. Both demand and supply side sources of heterogeneity are significant mediators of the pass-through to interest rates. 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, decreasing it by about 12 basis points. A similar result holds for firms with high risk scores, as we can see in the last row of the last panel. 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 a one standard deviation higher EBITDA implies a 11 basis points higher discount. In the second panel, we can instead see how the effect of the SME-SF is driven by firms with more than one credit line in good standing. In the third row of each panel, we collect estimates of the Basel III Gap interaction effect. Across all specifications, banks with a one standard deviation larger gap decrease rates to eligible relationships by about 6 basis points more than the baseline pass-through.

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 can see that there is no significant average effect on credit granted. Interestingly, eligible relationships whose utilization rate is higher start being rationed, with a 2 percent decrease for a one standard deviation increase in the past drawn over granted ratio. A similar result holds for high-risk firms (last panel), while the second panel shows that the insignificant average result is probably due to the composition of a decrease in granted credit to firms with only one credit line in good standing compensated by a slightly larger increase to firms with more than one credit lines. Finally, a high Basel III Gap does not appear to influence the pass-through of the SME-SF to quantities.

Overall, our findings on quantities yield support to the change of the policy that was introduced in 2015 (Lecarpentier et al., 2020), which smoothed the stark eligibility threshold. Indeed, we find evidence suggestive that the sharp discontinuity may have discouraged the extension of credit to creditworthy albeit more fragile customers. Moreover, 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 are in line with Harris, Opp, and Opp (2020) suggesting that adjustments to risk weights may affect disproportionately 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 of a 1 percentage point decrease in the minimum capital requirement on the cost of credit to the firms, from which, under some assumptions, we can learn about the benefit of the same change for banks.

Below is how regulators set the minimum capital ratio requirement in the following expression:

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

here Ω_{bA} is the mandated minimum equity amount bank b must set aside given it finances asset A for a sum of euro A_b.[43] Ω_{bA} is a Θ fraction of the whole A_b amount, on the basis of the ω_A risk weight on assets of type A.

Changes in the risk weights cause a change in Ω_{bA}. The eligibility to the SME-SF implies a saving on the capital required of approximately 2 percentage points *vis-a-vis* the same exposure without the SME-SF:[44]

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

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

In the previous sections we have shown that the estimated average impact $\hat{\beta}$ is close to 26 basis points. Then, a simple calculation yields us the value of the impact on the cost of credit per 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 though stress that this average number is indeed just an average, and the marginal benefit to banks may be better reflected by the pass-through to better customers.

Similarly to Plosser and Santos (2018), we apply the above back of the envelope calculation to a loan of 1 euro. 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 a one standard deviation higher EBITDA thoroughly reflects the initial benefit to the bank from the reform, we sum the first and the the last line of column (4) in Table 4's EBITDA panel, and divide the resulting average discount of about 32 basis

[43] In practice banks hold more than the minimum buffer for prudential reason, i.e. there exists a $\Theta_b > \Theta$ for each bank b. For a theoretical explanation of such behavior see Repullo and Suarez (2013) . This does not matter for our analysis, as our reform affects the risk weights (and the relative cost of lending) directly, whatever the buffer desired by the bank.

[44] We use 100 percent as the reference numbers for the baseline (without SME-SF) risk weight, and 8 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 roughly 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.

point by the 2 cent decrease in the requirement per unit of credit. Thus, we obtain that the shadow cost of 1 more euro of mandated minimum capital buffer for the banks is approximately 16 €-cent.

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 €-cent for banks with a one standard deviation greater shortfall in regulatory capital resources from immediate Basel III phase-in (19 is the result of adding the 3 cents greater benefits per euro of requirements' reduction to these banks).

Our calculation is based on a different assumption than the one by Plosser and Santos (2018). The focus of Plosser and Santos (2018) is on the difference between the interest rate charged by banks on new syndicated credit commitments with a maturity of less than 364 days and the interest rate charged on longer term identical commitments. The assumption behind their calculation of the cost of capital regulation is that the market for the short term commitments is not saturated. Only in this case a bank can satisfy more demand for such facilities by decreasing their price; banks will reduce the price up to the extent to which the loss in profits is compensated by the saving on costly regulatory capital resources. This may in part explain why our estimates, even if in the same ball-park, are larger than the 5 *bp*s per percentage point difference in requirements found in their work.

Finally, if we believe that banks are optimally choosing their balance-sheet structures, 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 and above the minimum requirements – so that one euro less minimum requirement would imply one euro less equity to hold for the bank – we can read this number as an approximation of the increase in bank profit for holding one euro less in equity to finance the loan.

VI Conclusion

We evaluate by a Regression Discontinuity Design the impact of the discount in the capital requirement implied by SME-SF, which favors exposures to SMEs below 1.5 million, and find that the cost of eligible loans decreases by approximately 26 basis points relative to non-eligible loans to SMEs. Normalizing this estimate by the 2 percentage points drop in regulatory capital required implied by the SME-SF, we obtain that lowering the capital requirement by 1 percentage point causes, on average, a reduction in the cost of credit of 13 basis points.

The estimated effect is larger for firms that are more likely to switch to other banks and for banks whose shadow cost of regulatory capital is higher. Under the assumption of a full pass-through of the benefit from a lower capital requirement to these low switching costs borrowers, we derive an approximation of the relief to banks from decreasing minimum capital buffer by 1 percentage point close to 16 bps, with sizable bank heterogeneity driven by our proxy for regulatory capital scarcity.

Overall, the considerable variation in the estimated effect of the capital discount underscores the importance of considering the entire distribution of firms and banks when determining who gains or loses the most 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					
Δi_{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					
Δi_{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 whose balance sheet data are available through CERVED. The first three columns of the table report information on mean, dispersion and count for the year 2014, and all variables with the exception of the change in the interest rate are measured as of the end of year 2013. The second three columns of the table report information on mean, dispersion and count for the year 2013, and all variables with the exception of the change in the interest rate are measured as of the end of year 2012. Δi_{bf} measures changes in yearly revolving rates and is reported in basis points, while all ratios are reported in percentage points. The CET1 gap variable is the difference between the transitory CET1 ratio reported by banks and the ones applying fully phased-in Basel III criteria. Relationship-level information reported regards only relationships with non-missing 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, with number of observation considered in the density estimation at the left and right of the cutoff reported in the first row. In all cases, the null hypothesis is that there is no discontinuity in the density. The bandwidths are optimally selected minimizing the MSE of the density estimates, independently on the two sides of the threshold, and are reported in thousands of € below the number of observations.

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

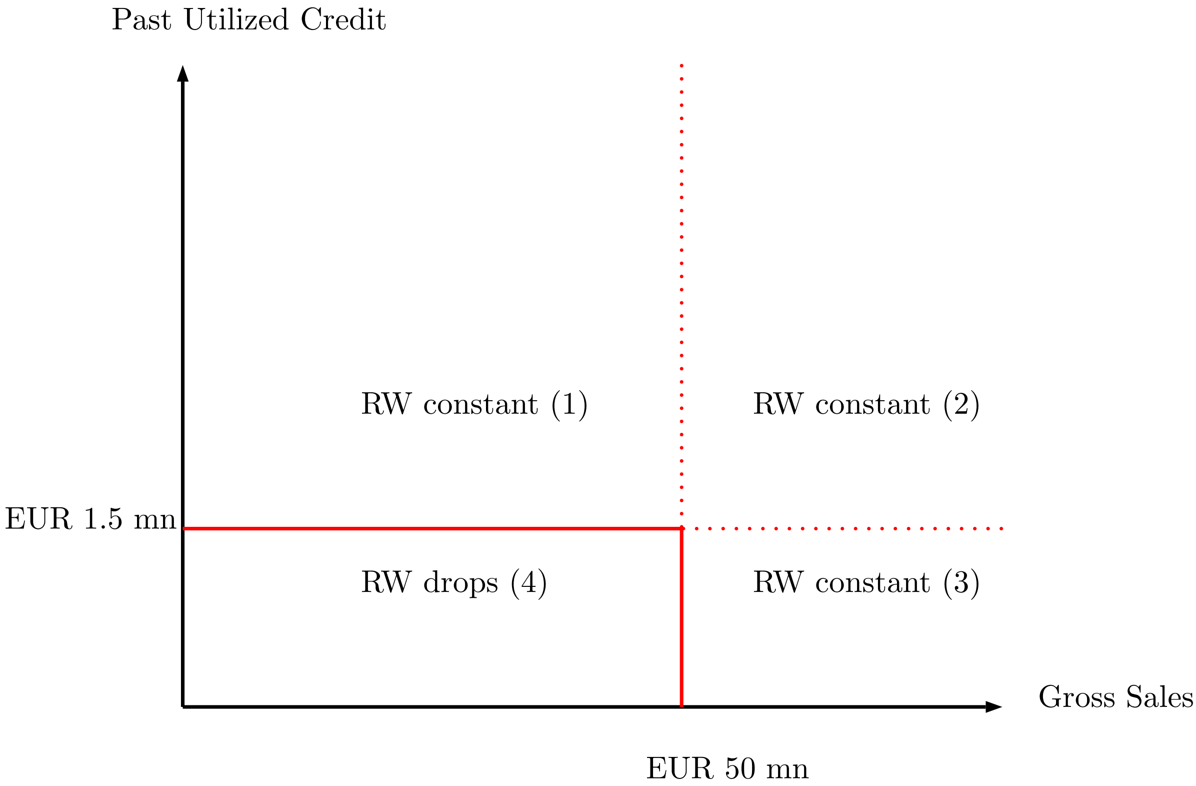
Note: This Table presents the results of discontinuity tests run *via* local estimation of $\hat{\beta}$ in Equation 3: $\Delta i_{bf} = \alpha + \beta R_{bf} + \phi(|x_{bf}^{2013} - \bar{x}|) + \epsilon_{bf}$, where Δi is the interest rate change in basis points, x_{bf} the past drawn credit, $\bar{x}$ the euro 1.5 million threshold, ϕ 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. Estimates are computed for the SMEs 2014 sample, and for the SMEs 2013 and non-SMEs 2014 samples for placebo purposes. Estimates reported employ triangular kernel weights, with robust standard errors displayed in parentheses. Observations left and right of the cutoff and the corresponding optimal bandwidths (in thousands of €) are reported 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

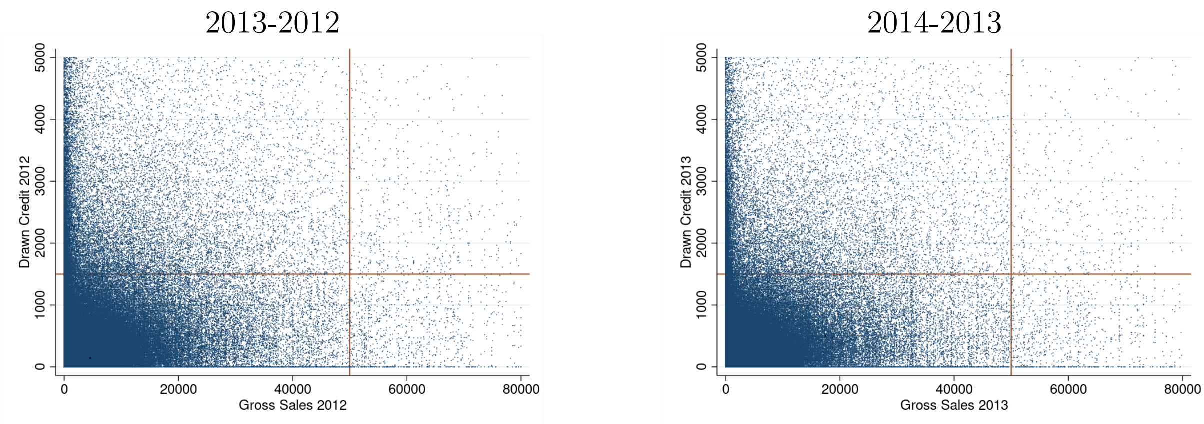
Note: This Table explores how credit scarcity, proxied by different relationship or firm-level characteristics, and bank capital scarcity, proxied by the difference between bank's CET1 under the transitional and full Basel III phase-in definitions (Basel III Gap), interact with the SME-SF pass-through. The first four columns report effects on rates; first, the baseline effect of a 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. All specifications are estimated 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. Robustness to alternative proxies of capital scarcity are reported in Tables A6 and A7.

Figure 1: SME-SF Discount Assignment



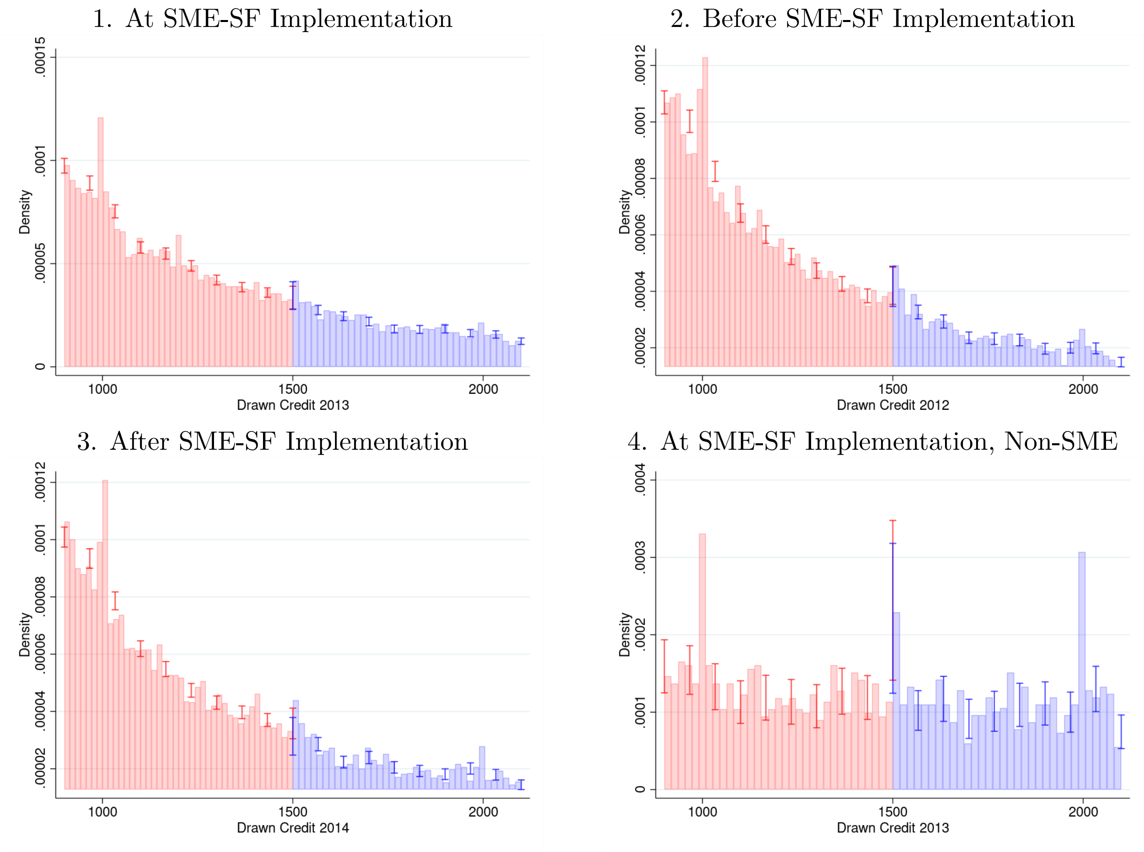
Note: The figures present the assignment space defined by the SME-SF eligibility rules.

Figure 2: Observations in the Treatment Space



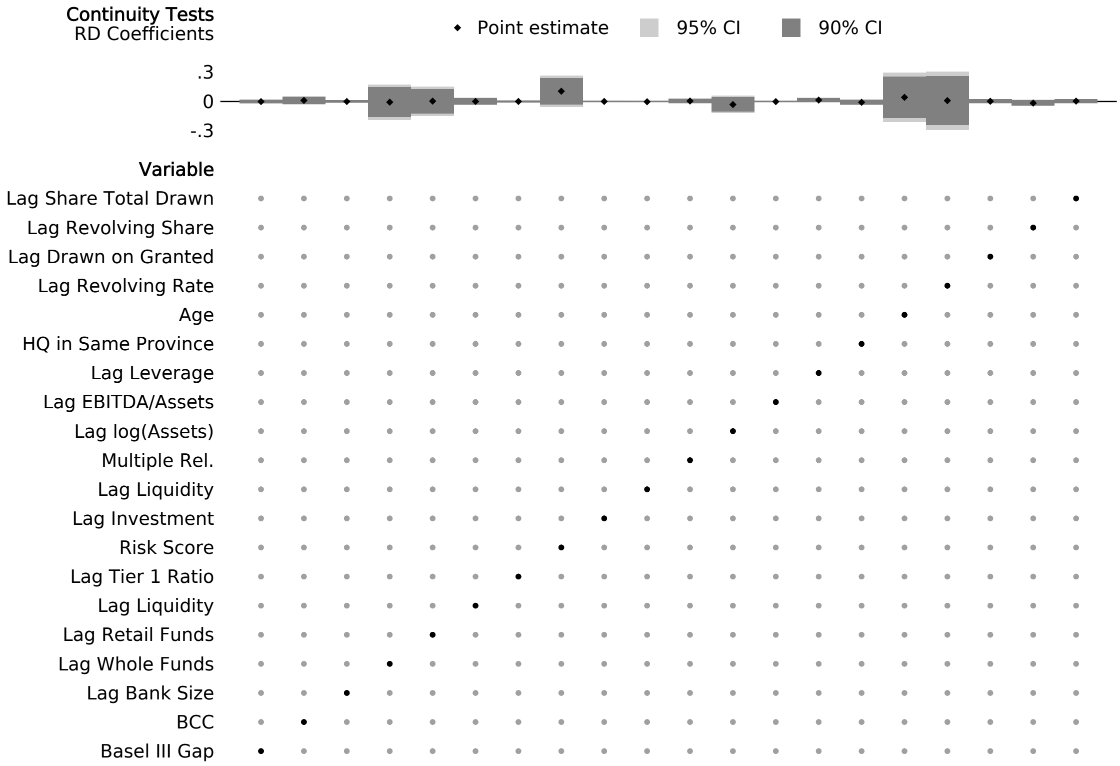
Note: The figure presents the distribution of relationships in the vicinity of the assignment to the SME-SF threshold.

Figure 3: Tests for the Discontinuity in Observation Density



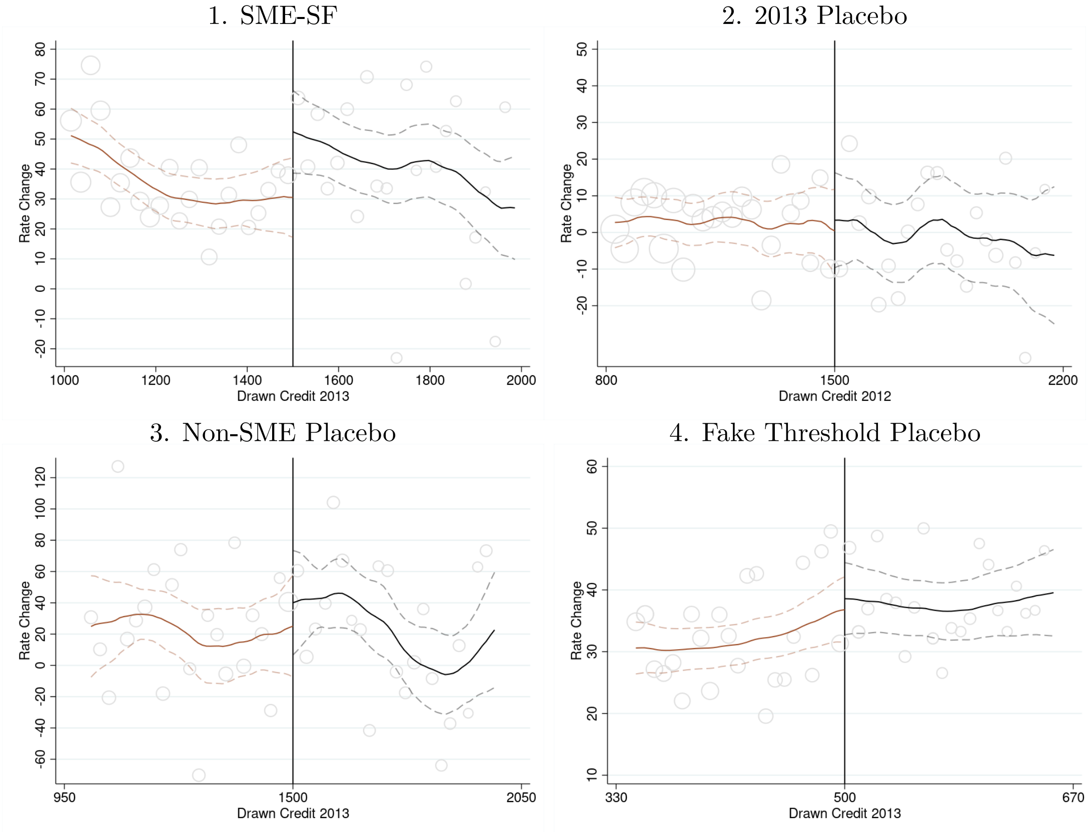
Note: The figures present the graphical outputs of testing discontinuity in the density of observations on the left and the right of the cutoff. The error bars plot the 95 percent confidence interval around the density point estimates. The first panel (top-left) reports the test for SMEs, using credit 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 estimate (black diamond) and confidence interval (grey shaded areas) for discontinuities in relationship, firm and bank-level characteristics. Discontinuities are obtained estimating the following specification 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 euro 1.5 million threshold, ϕ the linear x_{bf} polynomial independently estimated on the two sides of the threshold, and the null hypothesis is $b_1 = 0$. Below the plot, under "Variable", each line marks which variable is tested for discontinuity in the local estimation. 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 euro 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. We present binned averages of the data as gray balls, whose dimension reflects the number of observations in each equally spaced bin, and local polynomial smoothing estimates (smoothing bandwidths dividing the space between the x-axis extreme and the policy threshold in three) of the change in rates with respect to past drawn amounts, together with their 95% confidence intervals.

A Appendix

A.1 Estimation Details

To estimate β 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: For robustness purposes we repeat the estimation including bank, firm and relationship characteristics as further independent variables. 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 characteristic 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 on more equal footing.

Furthermore, we observe that our relationships are stratified at the firm and bank level. The main correlation concern is at the firm level, as it is reasonable to think that decisions on the pricing of loans of the same firm are taken by the same team 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 very similar to our baseline results without the inclusion of covariates - a discontinuity of 23 to 25 basis points - while statistical significance remains unchanged.

The role of bias adjustment: Then, as the interaction results we display play an important 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 we force alternative bandwidths moving from a symmetric distance of 25 thousand €s from the threshold to a 105 thousand €s distance at increments of 10 thousand €s. We can see in Table A4 that the results are actually 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.[45]

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 assessment of how the SME-SF impact is driven by different measures of firms and bank heterogeneity are 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

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

instead of CET1 capital. Tier 1 capital is a broader class of capital encompassing CET1 and other equity like instruments. A bank that experiences a large Tier 1 wipe out should have high shadow cost of regulatory capital too. 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 from which banks start 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 instead construct a measure that encompasses the impact 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. From this, we subtract the full-phase in CET1 ratio using March 2014 risk weighted assets as denominator. In the Table, we can see that this has, again, 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 back-up source for the firm's credit demand - i.e. the existence of multiple relationships and a low utilization ratio - is one key factor determining the magnitude of the pass-through. In this Appendix we further explore this aspect, at the same time 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 alter the estimated coefficient significantly even in the absence of demand bias through sample selection. In our case, we can show that a firm-fixed effects identification strategy would select a sub-sample of firms with outside options, i.e. firms that are better able to 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), then 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.[46] 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 that there is 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 indeed 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 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 a comparison in rate changes for non-eligible credit relationships of firms in and out the fixed effects sub-sample. Across different specifications, we can see that firms in the sub-sample experience changes in rates 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

[46] 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, we consider it to be the least ad-hoc option at our disposal.

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 stemming 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.[47]

The sub-sample on which we identify the local fixed effect estimator of the treatment effect is composed of such firms that have 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.

[47] 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 euro 1.5 million threshold, ϕ 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: $\Delta i_{bf} = \alpha + \beta R_{bf} + \phi(|x_{bf}^{2013} - \bar{x}|) + \Omega C_{bf} + \epsilon_{bf}$, where Δi is the interest rate change in basis points, x_{bf} the past drawn credit, $\bar{x}$ the euro 1.5 million threshold, ϕ 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. Estimates are computed for the SMEs 2014 sample, and on the SMEs 2013 and non-SMEs 2014 samples for placebo purposes. Estimates reported employ triangular kernel weights, with robust standard errors clustered at the firm level displayed 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: $\Delta i_{bf} = \alpha + \beta R_{bf} + \phi(|x_{bf}^{2013} - \bar{x}|) + \epsilon_{bf}$, where Δi is the interest rate change in basis points, x_{bf} the past drawn credit, $\bar{x}$ the euro 1.5 million threshold, ϕ 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. Estimates are computed for the SMEs 2014 sample, and on the SMEs 2013 and non-SMEs 2014 samples for placebo purposes. Estimates reported employ triangular kernel weights, with standard errors displayed in parentheses.

Table A4: **Dependent Variable** Interest Rate Change in *bp*s; 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: $\Delta i_{bf} = \alpha + \beta R_{bf} + \epsilon_{bf}$, where Δi is the interest rate change in basis points, x_{bf} the past drawn credit, $\bar{x}$ the euro 1.5 million threshold, and the null hypothesis of each test is $\beta = 0$. The different columns report increasingly large hand-picked bandwidths for a linear polynomial specification of ϕ, with bandwidth size in thousands of € reported in the column header. Estimates are computed for the SMEs 2014 sample and the SMEs 2013 sample for placebo purpose and, in this case, are not corrected for bandwidth selection. Estimates reported employ triangular kernel weights, with standard errors displayed 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: $\Delta\log(\text{granted})_{bf} = \alpha + \beta R_{bf} + \phi(|x_{bf}^{2013} - \bar{x}|) + \epsilon_{bf}$, where $\Delta\log(\text{granted})$ is the log change in granted credit, x_{bf} the past drawn credit, $\bar{x}$ the euro 1.5 million threshold, ϕ 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. Estimates are computed for the SMEs 2014 sample, and for the SMEs 2013 and non-SMEs 2014 samples for placebo purposes. Estimates reported employ triangular kernel weights, with robust standard errors displayed in parentheses. Observations left and right of the cutoff and the corresponding optimal bandwidths (in thousands of €) are reported 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)
	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***
Drawn/			(-4.82)		(-4.84)		(-2.76)		(-2.69)
Granted	Basel III Gap * SF		-5.062***		-5.099***		-.0029		-.0032
Panel			(-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)
	SME-SF			-.8016	-.6387			-.0391**	-.0425**
				(-0.06)	(-0.05)			(-2.24)	(-2.36)
	Basel III Gap				-20.7***				-.008***
Relationships					(-4.81)				(-2.72)
Panel	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)
	SME-SF			-23.41***	-21.98***			.0024	.0035
				(-3.53)	(-3.26)			(0.24)	(0.34)
	Basel III Gap				-20.43***				-.0082***
EBITDA					(-4.80)				(-2.8)
Panel	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)
	SME-SF			-25.97***	-25.28***			.0162	.0177
				(-3.88)	(-3.74)			(1.46)	(1.54)
	Basel III Gap				-20.54***				-.0083***
Risk					(-4.82)				(-2.85)
Panel	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. All specifications are estimated 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. All specifications are estimated 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: $\Delta i_{bf} = \alpha + \beta R_{bf} + \phi(|x_{bf}^{2013} - \bar{x}|) + f + \epsilon_{bf}$, where Δi is the interest rate change in basis points, x_{bf} the past drawn credit, $\bar{x}$ the euro 1.5 million threshold, ϕ 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. Estimates are computed for the SMEs 2014 sample, and on the SMEs 2013 and non-SMEs 2014 samples for placebo purposes. Estimates reported employ triangular kernel weights, with 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: $\Delta i_{bf} = \alpha + \beta R_{bf} + \phi(|x_{bf}^{2013} - \bar{x}|) + \epsilon_{bf}$, on the sub-sample of observations on which the fixed effect estimator of β is identified. Such sub-sample is composed by all the observation belonging to firms that have at least one eligible and one non-eligible observation within the bandwidth selected by minimizing the MSE. Δi is the interest rate change in basis points, x_{bf} the past drawn credit, $\bar{x}$ the euro 1.5 million threshold, ϕ 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: $\Delta i_{bf} = \eta + \gamma S_{bf} + \phi_+(|x_{bf}^{2013} - \bar{x}|) + \Omega C_{bf} + \epsilon_{bf}$. Δi_{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 euro 1.5 million threshold, ϕ_+ the rightx_{bf} polynomial independently estimated on the two sides of the threshold, C_{bf} includes other covariates, ϵ_{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 specification 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.