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

Emilia Bonaccorsi di Patti, Mirko Moscatelli and Stefano Pietrosanti*

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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. Employing a regression discontinuity design and Italian matched bank-firm data, we infer a reduction in interest rates of 9.5 basis points per percentage point drop in capital requirements. Moreover, the estimate of the effect is larger for borrowers with low costs of switching between banks. This suggests that the effectiveness of changes in banks' capital buffers as a policy tool depends on banks ability to exercise monopoly power.

Keywords: Capital requirements, SME, Cost of credit, Credit access, Market power.
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* Emilia Bonaccorsi di Patti: Bank of Italy, emilia.bonaccorsidipatti@bancaditalia.it; Mirko Moscatelli: Bank of Italy, mirko.moscatelli@bancaditalia.it; Stefano Pietrosanti, Bank of Italy, stefano.pietrosanti@esterni.bancaditalia.it. We thank Mitchell Berlin, Marcello Bofondi, Nicola Branzoli, Pablo D'Erasmo, Roberto Felici, Giorgio Gobbi, Edison Yu, Francesco Palazzo, Michael Schwert, Enrico Sette, Holger Sieg, André F. Silva and seminar participants at the Bank of Italy, University of Pennsylvania, Wharton, 4th ECB Macroprudential Policy and Research Conference – especially Diane Pierret for her helpful discussion, Bank of Denmark, Federal Reserve Board, Bank of England, LUISS, Università di Bologna, Federal Reserve Bank of Boston. The authors wish to express their special thanks to Francesco Manaresi, for insightful comments and suggestions. Stefano Pietrosanti wishes to express his special thanks to Guillermo Ordóñez, Michael Roberts, Alessandro Dovis, Marc Flandreau and Petra Todd for providing invaluable guidance throughout the preparation of this work. The opinions expressed do not necessarily reflect those of the Bank of Italy, the Eurosystem or their staffs. All errors are our own.

I Introduction

Bank regulators have employed minimum capital requirements to ensure bank solvency since the introduction of the Basel framework in the 1980s. More recently, minimum capital requirements have become part of the macro-prudential policy toolkit, which includes countercyclical changes in mandatory capital buffers to moderate lending booms in good times and mitigate lending busts in bad times.[1]

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,[2] 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. We do so by investigating the effect of a discount in capital requirements targeted to a specific category of loans. This was introduced 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 spreads of SMEs revolving credit facilities, we infer that a 1 percentage point decrease in minimum capital requirements causes a 9.5 basis points drop in interest rates on bank loans. Moreover, we observe that estimates obtained from a restricted sample of firms with low switching costs result in a larger effect, i.e. a 12.5 to 15.5 basis points drop.

[1] For a detailed overview of macro-prudential policy and its tools, we refer to Claessens (2015).
[2] For theoretical arguments regarding why this may be the case, see Diamond and Rajan (2000).

We interpret the increase in the magnitude of the estimates for firms with low switching costs as a result of lower banks' monopoly power. This is important, as it is evidence that banks' capacity to exert monopoly power on borrowers drives the pass-through of changes in minimum capital requirements to firms, thereby influencing the effectiveness of such policy tool.

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 eligibility in terms of two criteria: (i) the borrower must have a turnover (gross sales) of less than euro 50 million, and (ii) the bank's total exposure to that borrower has to be below euro 1.5 million. Hence, the SME-SF introduces a discontinuous change in the minimum capital requirement for bank-firm pairs around the two regulatory thresholds for otherwise similar credit relationships.

Under the assumption that potential confounding factors do not change discontinuously at the eligibility threshold, we can employ the SME-SF eligibility rule to estimate the effect of capital requirements on lending rates with a Regression Discontinuity Design (RDD).[3] 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

[3] 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) and Rodano, Serrano-Velarde, and Tarantino (2018). We use the RDD to disentangle the effect of the SME-SF from demand and supply confounders. In this, and similarly to Rodano, Serrano-Velarde, and Tarantino (2018), we use the RDD as an alternative to the firm fixed effect estimation strategy (Khwaja and Mian (2008)). With respect to Rodano, Serrano-Velarde, and Tarantino (2018), our discontinuity is at the relationship, not at the firm's balance sheet level.

no bunching of credit relationships immediately below the threshold before the policy implementation. Moreover, we observe that placebos for non-SME relationships and SME relationships before the SME-SF do not bring evidence of spurious effects. Finally, we show that estimates are stable to the inclusion of additional firm, bank, and relationship control variables, mitigating concerns about the local nature of the estimates (Angrist and Rokkanen (2015)).

Our baseline analysis shows that, after the SME-SF implementation, loans that benefit from the capital charge discount experience an average interest rate reduction of about 19 basis points 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 9.5 basis points per percentage point decrease in the requirements. This is an average impact estimate, which may only partially reflect the benefit coming to the banks from the marginal capital requirements relaxation. Banks market power on borrowers can limit the extent of the pass-through, creating a wedge between the average effect on the cost of credit, and the marginal value of the relaxation to each bank.

To investigate the matter further, we combine our RDD analysis with the "within" identification strategy proposed by Khwaja and Mian (2008).[4] This strategy uses firm fixed effects and identifies the impact of the SME-SF exploiting firms with both eligible and non-eligible relationships around the threshold.[5] These relationships are *ex ante* virtually identical as they refer to the same borrower and are close to the threshold before the SME-SF, but become *ex post* different as a result of the policy. The fixed-effect estimator addresses the matter of bank market power in two ways. First, by including firm fixed effects we restrict our estimates to a sub-sample of firms which already have easily accessible outside options, thus arguably low switching costs.[6] If banks did not lower the cost of the eligible relationships, these firms would be indifferent between using their eligible and non-eligible credit lines, and can thus credibly walk away. Second, firm fixed

[4] The within-firm design is the standard in the most recent literature identifying the effects of bank shocks on credit (e.g. Jiménez et al. (2012), Jiménez et al. (2014), Paravisini et al. (2014)).

[5] Our sample includes about $7,500$ such relationships around the SME-SF assignment threshold, belonging to approximately $3,100$ firms.

[6] Evidence of the decreasing relationship between number of relationships and switching costs has been found, for example, by Ioannidou and Ongena (2010) and Barone, Felici, and Pagnini (2011), which uses our same data sources. 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).

effects absorb all firm-level unobservable sources of disturbance, including differences in bargaining power against lenders which cannot be easily accounted for.[7]

The estimates obtained under the discontinuity plus firm fixed effects design are larger, highlighting interest rate reduction is between 25 and 31 basis points. We take a number of steps to verify our interpretation of such increase in magnitudes, i.e. that it is a result of lower banks' monopoly power. First, if this is the case, sample selection should play a role per se. Firms with credible outside options should get larger discounts. Estimating the model again, on the same subsample, but omitting the fixed effects, we show that sample selection is enough to see an increase in point estimates. Second, we notice that a larger threshold effect may also come from a larger increase in rates for non-eligible relationships in the fixed effects subsample. This, clearly, would be at odds with our interpretation. We compare changes in the cost of credit for non-eligible relationships between the fixed effects subsample and the overall sample. We observe that, if anything, the cost of the first grows less than the cost of the second, backing our interpretation. Third and last, we document how firms that have observably less bargaining power against their lenders (highly leveraged firms, or firms with low profitability) do get smaller discounts, coherently with our interpretation.

Under the assumption that banks transfer the entire benefit of the capital discount to borrowers that enjoy lower switching costs, we find that banks would be happy to pay up to 12.5 - 15.5 cents per one euro of reduction in the capital requirements. Thus, the upper bound of the benefit from a 1 percentage point decrease in capital requirements for a firm with a 1 million euro loan would be between 1.25 and 1.5 thousand euro less in terms of interest payments.

Related literature: Our assessment of the price effect of capital requirements contributes to the literature on the impact of minimum capital requirements on the supply of credit (Aiyar, Calomiris, and Wieladek (2016), Behn, Haselmann, and Wachtel (2016), Jiménez et al. (2017), Mayordomo and Rodríguez-Moreno (2018)). In particular, we use tools from the above empirical literature to contribute to the efforts to quantify the costs of capital regulation (e.g. Kashyap, Stein, and Hanson (2010), Miles, Yang, and Marcheggiano (2012)). The magnitude of our results is approximately in line with the

[7] We stress that such disturbances are very likely to only increase noise in our setting. Firm-level unobservables bias the RDD estimates only if discontinuous at the SME-SF assignment threshold. As all loan, firm, and bank-level characteristics we observe appear smooth at such threshold, the scope for concern is limited.

long-run cost literature (see Dagher et al. (2016)), and suggests that transition costs of capital regulation may be smaller than extrapolations from the impact of adverse shocks to banks' capital would imply, but still not negligible.

Our work is closely related to recent efforts to directly quantify the cost of bank capital regulation, i.e. Kisin and Manela (2016), Glancy and Kurtzman (2018), Plosser and Santos (2018). Our assessment of the average pass-through 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.[8] Our magnitudes are instead closer to the ones suggested by the two works exploiting quasi-experiments, Glancy and Kurtzman (2018), which exploits variation coming from the increase in capital requirements applied to risky commercial real estate loans, and Plosser and Santos (2018), which exploits changes in capital requirements on long-term vs short-term commitments throughout Basel I and II implementation. With respect to Plosser and Santos (2018) and Glancy and Kurtzman (2018), we add evidence highlighting how the degree of competition between banks can influence such estimates to a large extent, suggesting an important 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), Hale and Santos (2009), Santos and Winton (2019)).[9]

Finally, our analysis sheds light on the use of risk weights as a policy instrument. Targeted changes in risk weights are being employed more and more within the framework of macro-prudential policy (see 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. (2019) for the SME-SF in particular. While Mayordomo and Rodríguez-Moreno (2018) and Lecarpentier et al. (2019) study the effect of the SME-

[8] 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.

[9] For what regards the effects of capital regulation, the only important exception we are aware of is Corbae and D'Erasmo (2019), 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) and Wang et al. (2018)).

SF on credit access, we focus on the cost of credit dimension, and add on the importance of firms' relationships portfolio in driving the pass-through.

The paper proceeds as follows: Section II provides background information on the SME-SF and discusses how it should affect the cost of credit, and Section III describes our data; Section IV explains our identification strategy, and Section V illustrates 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 onset of 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;[10] some of the measures were applicable immediately while others were subject to a gradual phase in.[11]

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),[12] 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.[13]

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

[11] 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.

[12] CET1 mostly includes retained earning and common shares; additional Tier1 includes other types of shares; Tier2 capital includes some subordinated debt instruments. For a detailed account on capital definitions, see "Basel III: A global regulatory framework for more resilient banks and banking systems" at https://www.bis.org/publ/bcbs189.pdf by the Basel Committee's.

[13] 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.

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.[14]

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 a Small and Medium Enterprise Supporting Factor (SME-SF). 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.[15]

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[16] response to the Call for Evidence on the SME-SF by the European Banking Authority (EBA):

[14] 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.

[15] 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.

[16] One of the largest Italian banking groups.

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

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

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

Even so, the initial effort by the EBA (EBA (2016)) to evaluate the effect of the SME-SF on lending has returned no strong evidence in favor of an effect. However, the EBA's analysis is based on survey data and, for this reason, it cannot fully disentangle supply from demand, or clearly separate the effect of the SME-SF from 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. (2019), 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.

III Data and Measurement

We construct our dataset by matching information on loan quantities and interest rates from the Italian Credit Register and the from 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.[17] First, we identify eligible firms employing the data on gross sales from the Cerved database.[18] 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.[19]

We take a number of steps to limit the scope of the mismeasurement concern. First

[17] 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.

[18] 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).

[19] 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 (2019), 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.[20] 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. Furthermore, as we cannot distinguish between residential and commercial real estate collateral, we focus on relationships that are not collateralized and drop the others. Most short term loans, especially revolving loans, are not collateralized, which limits the selection concerns coming from this restriction. Finally, we also restrict our attention to relationships in good standing and exclude firms with deeply negative EBITDA (below negative 20 percent), because banks cannot apply the capital requirement discount to borrowers that are nonperforming.

III.2 Control variables

Our dataset includes information on relationship, borrower and bank characteristics that could influence the interest rate on loans. We use these variables for two 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).

The first set of controls proxies for 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

[20] 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".

for the amount of slack that f has in the relationship with b; the 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,[21] 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 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. Finally, 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,[22] and region dummy variables for the location of the firms' headquarters (North West, North East, Center and South).

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

[21] 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.

[22] 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.

with wholesale funding excluding central bank funding. We also include the log of total assets to control for bank size.

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 $253,000$ have information on interest rates. Among these, $235,000$ are eligible relationships of eligible firms (most Italian firms are SMEs); approximately $7,000$ observations are instead non-eligible relationships of eligible firms. We also keep data on non-eligible firms to run placebo regressions; $7,000$ such observations refer to relationships by non-eligible firms that were below the eligibility threshold at the end of December 2013, while $4,000$ are relationships by non-eligible firms that were above the eligibility threshold at the same date.

In Figures 2 and 3 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' relationships characteristics in treated (2014-2013) and placebo (2013-2012) samples in Tables 1 and 3 respectively; for non-SME firms (used as further placebo), we report statistics in Tables 2 and 4; moreover we report firms' characteristics in Tables 5 and 6; banks' in Tables 7 and 8. The information on the changes in interest rates pertain to the SME-SF implementation window (2014-2013), and to the placebo window of (2013-2012). The control variables are instead measured as of the end of 2013 for the implementation time window, and as of the end of 2012 for the placebo time window.

The descriptive statistics on banks' balance sheets (Tables 7 and 8) show that, on average, banks' balance sheets became stronger over time as Retail Funding, Liquidity and the CET1 ratio slightly increased. Although interest rates increased more in 2014 than 2013 (Tables 1 and 2 for 2014, *vs* Tables 3 and 4 for 2013), the relationship data suggest that only for SMEs and only in the 2014-2013 time window the cost of credit increased less on eligible than on non-eligible relationships (Table 1, and Table 3 for SMEs, and Tables 2 and 4 for non-SMEs). All firms' characteristics but leverage, instead, were similar in the two periods (Table 5 for 2014, and Table 6 for 2013).

Interestingly, as non-eligible lines of non-SMEs have grown in cost by 10 basis points less than non-SMEs eligible lines, while for SMEs the opposite is true, we can derive a rough approximation of the SME-SF's impact. This is around 20 basis point lower cost for treated credit lines, which is basically the same result we obtain through our Regression Discontinuity Design. 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, as shown in Tables 1 and 3, eligible relationships are younger relationships, have a higher share of revolving loans, and have a lower drawn to granted ratio. Such heterogeneity suggests that, across all the sample, demand and supply shocks unrelated to the SME-SF are likely to affect such relationship heterogenously and may well account for different patterns of change in the interest rate payed by firms.[23] In the next section, we describe the details of our approach to estimate the causal effects of the SME-SF based on Regression Discontinuity Design, and discuss evidence supporting its validity.

IV Empirical strategy

As the SME-SF eligibility threshold impinges on credit utilization, it is difficult to derive the causal effect of the capital requirement discount on credit supply through changes in credit quantities. On the one hand, after the implementation of the policy, SMEs with marginally eligible credit lines might have an incentive to keep their credit utilization below the eligibility threshold; on the other hand, firms with marginally non-eligible loans might have an incentive to reduce their credit utilization to benefit from a lower cost on their overall credit exposure. We thus focus on the change in the interest rate, as, at difference with credit drawn, it is not related to the eligibility for the SME-SF, and cannot be directly manipulated by the borrower.

[23] For extensive discussion of how relationship level heterogeneity can interact with demand and supply shocks and mediate their effects, we refer to Paravisini et al. (2014), and Paravisini, Rappoport, and Schnabl (2017).

IV.1 Identification challenges

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. The cost of credit by bank b to firm f at time t, i_{bft}, is a function of a number of factors that may change over time for reasons other than the SME-SF:

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

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 borrower characteristics, including unobservable changes in the risk of the borrower and in the demand for credit. For example, f may experience a negative shock that induces the banks that grant credit to f to consider it riskier and raise the interest rate that f pays. A shock to firm demand might produce similar results on prices.[24] S_{bft} denotes time-varying bank characteristics, including changes in unobservable factors affecting credit supply, such as shocks to the cost of funding or to capital, which would cause a rise in i_{bft}, and/or a reduction in quantity of credit supplied.[25] 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 the loan granted to firm f.

Our goal is to estimate the effect of a change in R_{bft} on the cost of credit, exploiting the change induced by the SME-SF on the subset of eligible relationships. To clarify which identification challenges we need to address, we consider estimating this effect with the following linear regression model:

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

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*

[24] Here we use the bft notation instead of the ft since we are not assuming that such demand-side shocks affect equally all the relationships of the same firm f.

[25] The overall effect of the Basel III reform on bank credit supply conditions would be captured by the S_{bft} term. Again, we are using the bft notation, and not bt, as we are not starting from the assumption that supply-side disturbances affect the different relationships of the same bank equally.

benefits from the SME-SF (the relationship bf is treated), 0 otherwise (the relationship bf is not treated). The treatment status of the bf relationship depends on the joint condition of the firm being an SME and the exposure of b to f being below the eligibility threshold. Finally, ϵ_{bf} is the residual term, and the regression is specified in terms of changes Δi_{bf}, so that all the variation stemming from observable and unobservable sources of heterogeneity that are fixed over time are removed.

Dropping the t subscript and focusing on the linear framework, we can see that the residual ϵ_{bf} includes three main components: $\epsilon_{bf} = D^*_{bf} + S^*_{bf} + e_{bf}$. D^*_{bf} and S^*_{bf} capture any variation due to demand and supply factors not explained by the regression variables, while e_{bf} is an idiosyncratic error component. The estimate of β from Equation 2 would be a biased estimate of the effect of the SME-SF on the cost of credit if any of the confounding factors included in ϵ_{bf} are correlated with R_{bf}. This could occur for different reasons.

First, $\text{cov}\left(S^*_{bf}, R_{bf}\right) \neq 0$ if eligible relationships $(R_{bf} = 1)$ are systematically affected by supply shocks that are different from those affecting non-eligible relationships. For example, SMEs tend to borrow more frequently from small banks; if the Basel III rules implied a larger increase in capital requirements for small banks, the borrowers of these banks might experience an increase in interest rates that would compensate part or all of the benefit from the SME-SF, and the $\hat{\beta}$ would be biased downwards by the non-random matching between firms and banks.[26]

The parameter $\hat{\beta}$ would be biased also if $\text{cov}\left(D^*_{bf}, R_{bf}\right) \neq 0$, which could occur if firms with eligible exposures face demand shocks that differ from those of firms with non-eligible exposures. We cannot rule out this hypothesis because eligibility is based not only on firm size but also on the volume of outstanding credit. A firm that borrows more than 1.5 million before the SME-SF implementation is likely to have a higher demand for credit than a similar firm that utilizes a significantly smaller amount of credit. If demand shocks are positively correlated over time, the first firm would be more likely to increase its demand for credit and, consequently, to face an increase in the interest rate. A systematically higher incidence of interest rates increases for firms with non-eligible credit lines would bias upwards $\hat{\beta}$, without any causal relationship with the SME-SF.

[26] This is possible because typically small banks do not use internal risk weighting models, but rely on the standardized approach to calculate risk weighted assets and capital requirements (Behn, Haselmann, and Wachtel (2016)).

The recent literature on the impact of bank-level shocks on credit supply usually follows an identification strategy based on a difference-in-difference plus fixed effects approach. Identification is achieved using observations referring to firms borrowing from more than one bank, some "shocked" and some not, and controlling for demand-side confounding factors with firm fixed effects.[27] This strategy requires the assumption that any unobserved confounding factor affects equally all the credit relationships of the same firm.[28]

In the case of the SME-SF, it is not plausible to assume that all eligible and non-eligible relationships are affected by the same demand and supply shocks even for the same firm. If we compared all non-eligible relationship with eligible ones within firms with multiple banks, we would be including in the comparison relationships that are very heterogeneous (some very small, some very large). Banks might be pricing large loans differently from small loans, and firms might withdraw more credit from "preferred" relationships in case of unexpected needs (demand shocks) but hold some backup credit lines that are not often utilized (Sette and Gobbi (2015)).

We address the issue of comparability using a local approach based on Regression Discontinuity Design (RDD), exploiting the fact that the treatment status is defined by an arbitrary threshold on 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 counterfactuals 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.

Given the richness of our data we can take a number of steps to show that concerns about RDD validity are reasonably limited. First, there is no evidence of bunching of relationships at the SME-SF threshold, suggesting that no manipulation of the assignment variable was performed; moreover, we can show that bank, firm, and relationship level characteristics vary smoothly at the threshold, confirming that no evident sign of sorting is detectable (see Section IV.3 for details).

[27] One of the first examples of such approach 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).

[28] If $\text{cov}(D^*_{bf}, R_{bf}) \neq 0$ and our objective was to identify the effect of a bank shock accounting for unobservable and correlated demand disturbances, firm fixed effects would capture such disturbances if and only if $\text{cov}(D^*_{bf}, R_{bf}) = \eta_f$ for every f. Conversely, if we were concerned about supply-side unobservable confounders $\text{cov}(S^*_{bf}, R_{bf}) \neq 0$, then bank fixed effects would remove them if and only if $\text{cov}(S^*_{bf}, R_{bf}) = \eta_b$ for every b.

An advantage of RDD with respect to the diff-in-diff plus fixed effects approach is that we do not need to focus only on firms borrowing from multiple banks - which would force us to drop smaller firms that are the target of the SME-SF. The cost of using RDD is that the estimate will apply only locally to credit relationships near the threshold. We therefore perform multiple robustness tests to examine whether the estimates are robust, or depend on local observations characteristics. We provide this evidence in Section V.

IV.2 Estimating the SME-SF effect - a Regression Discontinuity Design

Ideally, to elicit the effect of the SME-SF on the cost of credit, we would need to observe identical credit relationships of the same firm f with the same bank b, some of which are randomly "treated" with the SME-SF and some of which are not.[29] The difference in the cost of credit between treated and untreated relationships would measure the effect of the SME-SF. The design of the SME-SF allows us to get close to this ideal, because the discontinuity in assignment can be exploited to achieve identification through an RDD.

RDD does not require perfect randomization in the treatment assignment, but only a sharp change in the probability of treatment induced by the existence of a threshold on a continuous assignment variable. Sufficient condition for obtaining a valid causal estimate of the effect of a treatment is the validity of the relatively weak assumption that all possible confounders are continuous at the threshold defining the treatment assignment rule, plus lack of manipulation of the threshold by treatment takers.[30]

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 disboursed by the bank to the firm. 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.

[29] From now on, the treatment is the SME-SF, treated observations are credit relationships eligible to the SME-SF policy, and vice-versa for the non-treated.

[30] Manipulation of the assignment variable would imply that all the 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). A review of recent applications of RDD can be found, among others, in Lee and Lemieux (2010) and Calonico et al. (2019).

The SME-SF treatment consists in the reduction in the risk weight, and the assignment variable is the total amount of credit disbursed by bank b to firm f before policy implementation, while the outcome variable is the change in the cost of credit by bank b to firm f after the SME-SF implementation. Using changes is consistent with the difference-in-difference within approach of Khwaja and Mian (2008) that we also implement to examine robustness in Section V below.

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

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

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

The change from 1 to 0 of the treatment probability defines a sharp RDD.

Our main estimating equation is:

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

where h^{-}, h^{+} delimit the bandwidth of choice, selected using data-driven methods, as explained in Section V. Here, a is a common intercept; $\phi^{+,-}(.)$ are smooth polynomial component in the distance from threshold (+ on the right, − on the left), meant to control for disturbances that vary continuously with credit utilization; β is the bias corrected (bias due to bandwidth choice) parameter of interest, measuring the effect of the treatment; ν_{bf} is a stochastic error component.

IV.2.1 Identification by RDD and the continuity assumption

An RDD strategy achieves identification of the treatment of interest, β, when

$$\lim_{x_{bf} \to \bar{x}^{-}} E[\Delta i_{bf}(1)|x_{bf}] - \lim_{x_{bf} \to \bar{x}^{+}} E[\Delta i_{bf}(0)|x_{bf}] = \beta \tag{4}$$

In the vicinity of the threshold, the "jump" between the expected interest rate change below threshold, $E[\Delta i_{bf}(R_{bf} = 1)|x_{bf}]$, and above threshold $E[\Delta i_{bf}(R_{bf} = 0)|x_{bf}]$ isolates the impact of the SME-SF. Identification is achieved if both the conditional expected outcome functions are continuous at the assignment threshold, i.e. if and only if the following holds:

> Continuity Assumption: for every value of $R_{bf} \in \{0, 1\}$, $E[\Delta i_{bf}(R_{bf})|x_{bf}]$ is continuous in x_{bf} at $\bar{x}$.

From Equation 2 and using the notation in Section IV.1 on demand and supply confounders, we can write:

$$E[\Delta i_{bf}(1)|x_{bf}] = a + E\left[D^*_{bf} + S^*_{bf}|R_{bf} = 1, x_{bf}\right] + \beta$$
$$E[\Delta i_{bf}(0)|x_{bf}] = a + E\left[D^*_{bf} + S^*_{bf}|R_{bf} = 0, x_{bf}\right]$$

The continuity assumption in our context means that $E\left[D^*_{bf} + S^*_{bf}|R_{bf} = 0, x_{bf}\right] = \phi^+(|x_{bf} - \bar{x}|)$ and $E\left[D^*_{bf} + S^*_{bf}|R_{bf} = 1, x_{bf}\right] = \phi^-(|x_{bf} - \bar{x}|)$, for some smooth functions $\phi^{+,-}$ whose limits at $\bar{x}$ exist and coincide. If this is true, then:

$$\lim_{x_{bf} \to \bar{x}^-} E[\Delta i_{bf}(1)|x_{bf}] - \lim_{x_{bf} \to \bar{x}^+} E[\Delta i_{bf}(0)|x_{bf}] = \phi^-(0) - \phi^+(0) + \beta = \beta$$

i.e. 4 is verified. Violation of the continuity assumption, instead, would invalidate the design because we would not be able to claim that the discontinuity depends solely on the treatment.

It is important to notice that the assumption of continuity *at threshold* above is less restrictive than the assumption of a bias unrelated to any relationship characteristic, required by the fixed effect diff-in-diff strategy widely employed in the recent literature. Although we cannot directly test the validity of the continuity assumption, there are tests that can be performed to mitigate concerns that Equation 4 is not a correct description of reality.

IV.3 Evidence supporting continuity

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.

IV.3.1 Manipulation

If the subjects under study were aware of the treatment before its introduction, and could *perfectly* manipulate 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 marginal credit relationships below the eligibility threshold to 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 the SME-SF eligibility threshold. The policy is based on the notion of exposure, 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 53 percent (see Table 1). Furthermore, fluctuations of real collateral values also affect the value of exposure that counts towards eligibility. Perfect manipulation would be difficult.

A second counterargument is that, even if firms could manage exactly their exposure at all times, manipulating 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,[31] but banks were uncertain about the criteria they had to follow to compute the eligible exposure until after the beginning of 2014.[32] We can thus conclude that banks were unlikely to be ready to identify eligible exposures with sufficient advance, and to have incentivised marginally ineligible customers to reduce their exposure below the threshold. It is also unlikely that firms were able to target their exposure before the introduction of the SME-SF.

To support our case, we also 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 highlight a statistically significant drop in the density just above the SME-SF threshold.

We run the test on the density with respect to the amount of credit outstanding both in the treatment ($2013-2014$) and placebo samples ($2012-2013$). We do so to reject manipulation since the beginning of the discussion on the SME-SF in 2012. The test does not detect any statistically significant discontinuity in the density of observations at the threshold in either sample, as shown in Figures 4, 5 and in Table 9.

[31] 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=ENtitle1); 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.

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

IV.3.2 Discontinuity of covariates

Even in the absence of evidence of manipulation, it could be possible that relationships, firms, or banks with specific characteristics were more likely to appear on one side of the threshold than the other. We estimate the same 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. We show a simple comparison of means (polynomial of order 0), and regressions with polynomials in the forcing variable of orders 1 and 2. The results, shown in Tables 10, 11, and 12, do not support the existence of discontinuities at the SME-SF threshold for any of the covariates.

V Results

We start inspecting the behavior of interest rates changes around the SME-SF eligibility threshold. In order to do so, we show 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.[33]

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, and the increase is larger than the ones occurred in 2013. 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. This, only for SME firms at the SME-SF threshold (Figure 6). Local fits for the 2012-2013 sample, or at placebo thresholds inspected at the same time as the SME-SF implementation, or for non-SME (Figure 6 and 7), do not show comparable "jumps" in the behavior of rates.

This evidence is suggestive of an effect of the policy, but in order to get a precise

[33] Such neighborhood is selected employing the mean square error minimization method documented in Calonico et al. (2019). In particular, we perform the necessary computations in Stata, employing the last update of the 2016 version of the rdrobust package, described in Calonico et al. (2017), 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.

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 selection (Calonico et al. (2019)). To do so, we estimate Equation 3 as a local polynomial regression, using the Stata routine based on Calonico et al. (2019) and described in Calonico et al. (2017).

In order to drive a clear link between our estimation procedure and the framework introduced in Equation 3, for expositionary purpose we focus on the local linear specification case (as in Hahn, Todd, and Van der Klaauw (2001)). The expected value of interest rate changes conditional on eligibility for the SME-SF can be expressed as $E[\Delta i_{bf}(1)|x_{bf}] = a^- + b^-(|x_{bf}^{2013} - \bar{x}|)$ for observations whose past exposure x_{bf}^{2013} was below the threshold; $E[\Delta i_{bf}(0)|x_{bf}] = a^+ + b^+(|x_{bf}^{2013} - \bar{x}|)$ for observations whose past exposure x_{bf}^{2013} was above the threshold. We want then to estimate $a^{+/-}$ and $b^{+/-}$, and then use the difference $\hat{a^-} - \hat{a^+}$ to infer β. To do so, we choose $(\hat{a}^{+/-}, \hat{b}^{+/-})$ minimizing

$$\sum_{bf} \left(\Delta i_{bf} - a^- - b^-(|x_{bf}^{2013} - \bar{x}|)\right)^2 K\left(\frac{|x_{bf}^{2013} - \bar{x}|}{h^-}\right) \text{ below threshold} \tag{5}$$

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

where $h^{+/-}$ are again data-driven bandwidth limits, allowed to be different on the two sides of the threshold,[34] and K is a triangular kernel weight function.

The results are displayed in the first row of Table 13 using local linear and quadratic polynomials;[35] for the main result table we also report, as further robustness, the estimates obtained from a simple weighted comparison of means (degree 0 polynomial specification). Our estimates show that there is a statistically significant sharp difference in the change in interest rates between eligible and non eligible relationships, only for SMEs, and only at the moment of SME-SF implementation. The magnitude of the difference is approximately 19 basis points, with very little difference between the first and second degree specification.

[34] As our sample density continuously decreases with relationship's size, we allow the bandwidth to be different on either side of the threshold for all our reported point estimates of the discontinuity. Such choice is the most conservative, and results are robust (and larger) if the alternative option of constraining the bandwidth to be equal on the two sides is chosen.

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

Robustness and Placebos. 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.3), their inclusion can increase precision and also provide information on the effect of heterogeneity in observable characteristic on our coefficient of interest. As suggested by Angrist and Rokkanen (2015), including control variables can mitigate concerns of lack of external validity of RDD estimates.

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, every result in the robustness section is computed clustering the standard errors at the firm level. We thus obtain the results displayed in Table 14. The estimated effects are very similar to our baseline results without the inclusion of covariates - a discontinuity of approximately 18.5 basis points - while statistical significance remains unchanged.

Another test 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,[36] 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.

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 (3) on the pre-treatment period. As shown in Tables 13 and 16, 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.

Finally, 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,

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

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. The placebo regressions shows that there is no such discontinuity, no matter the specification (see Tables 13 and and 16).

V.1 How competition affects the pass-through

Even if the main result captures the average pass-through from the SME-SF to the cost of credit, this does not mean that it reflects the full extent of the benefit to banks from such capital requirement discount. Indeed, as firms are small with respect to banks, and SMEs in particular have difficulties replacing banks' credit, it is reasonable to suspect that the average pass-through does not fully reflect the implicit benefit to banks from the SME-SF. In order to further investigate how the degree of borrower lock-in affects our result we modify our main specification by adding firm fixed effects.

We argue that this limits the extent of the problem in two ways. First, by demeaning observations at the firm level, we absorb all observable sources of heterogeneity that can affect our results, included differences in firms bargaining power. Furthermore, by restricting our identification to firms that have multiple relationships, some eligible and some not, *in the vicinity of the SME-SF threshold*, we are restricting our estimation to a subsample of firms that have a very easy way in substituting credit. In this Section, we show how doing so increases the magnitude of the result, and we show multiple pieces of evidence in favor of the interpretation that lower monopoly power by banks on such borrowers is the driver of this increase in magnitudes.

The implementation of 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. (2019), then construct triangular kernel weights on the basis of such bandwidths, and finally estimate a weighted fixed effect regression using the routine described in Correia (2016), useful to handle high dimensional fixed models.[37] In Table 16, we can observe how the magnitude of the point estimates increase to values ranging between 25 and 31 basis points, while statistical significance increases.

There are two possible explanations for such increase in the point estimates. The first is that the firm fixed effects are absorbing some attenuation bias due to unobservables, which may or may not be due to borrower lock-in with the creditor; the second, that the sample selection imposed by the fixed effect estimator is focusing our attention on a subsample for which the SME-SF pass-through is larger.

In order to understand which of the two is the actual driver, we estimate again the model on the subset of observations for which the fixed effect estimator of the SME-SF treatment is identified, but omitting the fixed effects. We run local regressions using observations belonging to firms that have 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. Results of such estimation are shown in Table 17, and highlight even larger effects, suggesting that sample selection, and not the control of attenuation bias through fixed effects, drives the increase in the point estimates.

Still, the fact that the estimates are larger for the fixed effects subsample may be caused by two possible reasons. 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 grow more. We thus check that the increase in the estimated SME-SF impact is not due to higher increase in the rates of non-eligible credit relationships of the firms in the fixed effects subsample. In Table 18 we show the result of a comparison in rate changes for non-eligible credit relationships of firms in and out the fixed effects subsample. Across different specifications, we can see that firms in the subsample experience changes in rates that are in line with other firms' (or smaller). We can thus conclude that the SME-SF effect on eligible relationships appears to be stronger in the firm fixed effects subsample.

The fact that sample selection from the firm fixed effect strategy is enough to see

[37] We make this choice as the rdrobust Stata routine (Calonico et al. (2019)) does not provide a way to directly handle high dimensional fixed effects. This would imply that, to keep working within 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. The cost of doing so are point estimates and standard errors that are not corrected for bias as when using Calonico et al. (2019). As such correction has very low impact on our main results (see Table 15, which omits the correction), we argue that the scope for concern can be considered limited.

an increase in the result, substantiates the interpretation of the increase in magnitude of point estimates as coming from higher bargaining power of firms in the subsample where the fixed-effect estimator is identified. 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. The pass-through would instead be larger for firms that can switch between existing relationships, which limits banks capacity to extract rents.[38]

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

V.1.1 Heterogeneity driven by firm observables

As a further check of our intuition that competition between lenders determines the pass-through, we look also to how firm-level observables drive heterogeneity in the estimates. If it is true that borrower-capture by banks is attenuating the extent of the pass-through, and the increase in magnitude of the pass-through we observe under the fixed effect specification is linked to this phenomenon, it should also be true that firms that are more credit constrained, or in general are less attractive for a lender, get less of the discount.

In order to verify the above, we select two proxies of lack of outside options at the firm level. First, high leverage, second, low profitability. In order to do so, we create two dummies per variable, each equal to one if the credit relationship belongs to a firm showing low/high levels of the proxy chosen. We show results for thresholds of below 45 percent leverage for the low leverage dummy, and above 90 percent leverage for the high leverage dummy; below 3 percent return on assets for the low profitability dummy, and above 10 percent return on assets for the high profitability dummy.[39]

We estimate a local parametric interaction specification, in the following form

[38] For theoretical work arguing in this sense, see Rajan (1992). Evidence coherent with such theoretical work has been provided, for example, in Detragiache, Garella, and Guiso (2000) and Ioannidou and Ongena (2010).

[39] Thresholds are chosen on the base of the lowest and highest quartile of every characteristic. Profitability is proxied by EBITDA.

$$
\begin{aligned}
\Delta i_{bf} &= \alpha + \beta_M R_{bf} + \omega_L Low_f * R_{bf} + \omega_H High_f * R_{bf} \\
&+ \Gamma X_{bf} + \phi_-(|x_{bf}^{2013} - \bar{x}|) + \phi_+(|x_{bf}^{2013} - \bar{x}|) + \epsilon_{bf} \\
&\text{Estimated on } bf:\ x_{bf} \in [\bar{x} - h^-, \bar{x} + h^+]
\end{aligned} \tag{7}
$$

where $Low_f/High_f$ are the dummies that take value one if the firm is in the low/high level of the characteristic group, ϕ functions are linear or second order polynomial components, X_bf collects controls and ϵ_{bf} is an error term which allows for clustering simultaneously at the firm and bank level. The coefficient of the $Low_f/High_f$ interaction with the threshold term R_{bf} can be interpreted as the extent to which being in the low/high group changes the pass-through of the policy for relationships close to the threshold, while the β_M coefficient-estimate tracks the effect for firms with values of the characteristic in the middle of the distribution. In every specification, we include independently the level of the characteristics as a control, and we progressively saturate the regression with relationships, firm, and bank controls.

We report the results of such estimation in Table 19. We can see that across all specifications, we consistently find that being in the less "mobile" group – respectively the firms with low profitability and the firms with high leverage – results in lower pass-through of the discount. In particular, firms with leverage above 90 percent get almost no discount after policy implementation, as we would expect if firm bargaining power against the bank played a role. Furthermore, we can appreciate how β_M is both stable across specifications and close to the baseline non-parametric estimate of the average pass-through. This is a further robustness, and ensures us that our quantification of the average pass-through is not driven by extreme observations.

V.2 What we learn on the cost of capital regulation to banks

Our estimates easily convert into a measure of the impact of 1 percentage point decrease in the minimum capital ratio 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.

To see how, we start summarizing 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.[40] 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 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:[41]

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

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

In the previous Sections we have shown that the estimated impact $\hat{\beta}$ is close to 19 basis points. Then, a simple fraction yields us a value of the impact on the cost of credit per percentage point change in the minimum capital ratio:

$$\frac{\hat{\beta}}{\Delta\frac{\Omega_{bA}}{A}} = \frac{-19}{-2\ \text{(percentage points)}} =$$

$$9.5\, bp \text{ per percentage point change in the capital requirement}$$

It is interesting to note that Glancy and Kurtzman (2018), studying an *increase* in capital requirement for real estate loans, find approximately the same effect on the cost of credit; what we add, though, is a deeper investigation of the extent of the pass-through from the bank to the firm, which suggests that average impact estimates may

[40] 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 harm for our analysis, as our reform affects the risk weights (and the relative cost of lending) directly, whatever the buffer desired by the bank.

[41] 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.42). 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.

underestimate the total effects from such changes in capital regulation. Indeed, if we focus on our fixed effect estimate we obtain a pass through between 12.5 to 15.5 basis points per percentage point change in the capital requirement.

Under the assumption that banks transfer the benefit of the SME-SF capital discount to firms, we can employ our estimates to infer the implicit cost of raising the minimum capital requirements for banks. We argue that the estimates obtained with the sample of multiple bank borrowers is more appropriate to perform the calculation, since the pass-through would be larger and hence the value given up by the banks closer to the banks' implicit evaluation of the discount.

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 value of the reform to the banks is reflected in the drop in rates estimated through the fixed effects specification, we divide the range $[25\, bp, 31\, bp]$ by the 2 cent decrease in the requirement per unit of credit, and obtain that the shadow cost of 1 more euro of mandated minimum capital buffer for the banks is in the range of $[12.5 \text{ € cent}, 15.5 \text{ € cent}]$.

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 19 basis points relative to non-eligible loans to SMEs. Normalizing this estimate by the 2 percentage points drop in 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 9.5 basis points.

The estimated effect is larger upon the inclusion of firm fixed effects in the RDD. In such case the estimation is performed on the subsample of firms with multiple relationships, some eligible and some not, in the neighborhood of the SME-SF threshold. Given that these firms are likely to have lower switching costs, we interpret this finding as evidence that competition plays an important role in the pass through of capital requirements to the cost of credit. Under the assumption of a full pass-through of the benefit from a lower capital requirement to these low switching cost borrowers, we derive an approximation of the relief to banks from decreasing minimum capital buffer by 1 percentage point ranging between 12.5 and 15.5 bps.

Such figures imply that the potential benefit to firms from such measures is quite small, at least looking at the cost of credit. For example, a firm with low switching costs and a 1 million euro loan outstanding can at most gain 1.5 thousand euro less in interest rate payments from a 1 percentage point decrease in minimum capital requirements. Furthermore, the evidence that low switching cost firms get larger discounts suggests that the effectiveness of minimum requirements relaxations – as implied by macro-prudential regulations – may be hindered by lack of competition between lenders.

Table 1: **Descriptive statistics - SMEs' relationships - SME-SF time window**

	Mean	Std Deviation	P(10)	P(50)	P(90)	Count
Non-Eligible Relationships						
Rate Change_{2014}	36.842	201.474	-160.051	23.327	261.341	7,072
$\frac{\text{Drawn}}{\text{Total Drawn}}$	0.435	0.279	0.138	0.355	0.954	7,072
$\frac{\text{Revolving}}{\text{Granted}}$	0.172	0.281	0.006	0.050	0.632	7,072
$\frac{\text{Drawn}}{\text{Granted}}$	0.812	0.175	0.558	0.851	0.997	7,072
Years of Relationship	7.248	2.512	3	9	9	7,072
Close Bank	0.158	0.365	0	0	1	6,948
Eligible Relationships						
Rate Change_{2014}	26.58	179.476	-154.288	21.074	205.203	235,584
$\frac{\text{Drawn}}{\text{Total Drawn}}$	0.366	0.327	0.034	0.251	1	231,156
$\frac{\text{Revolving}}{\text{Granted}}$	0.320	0.333	0.030	0.176	1	235,584
$\frac{\text{Drawn}}{\text{Granted}}$	0.589	0.320	0.076	0.634	0.982	235,584
Years of Relationship	5.446	3.120	1	5	9	235,584
Close Bank	0.149	0.356	0	0	1	232,605
All the Relationships						
Rate Change_{2014}	26.879	180.163	-154.39	21.136	206.892	242,656
$\frac{\text{Drawn}}{\text{Total Drawn}}$	0.368	0.326	0.036	0.255	1	238,228
$\frac{\text{Revolving}}{\text{Granted}}$	0.316	0.332	0.028	0.169	1	242,656
$\frac{\text{Drawn}}{\text{Granted}}$	0.596	0.319	0.083	0.644	0.983	242,656
Years of Relationship	5.498	3.118	1	6	9	242,656
Close Bank	0.149	0.357	0	0	1	239,553

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. All variables with the exception of the change in the interest rate are measured as of the end of year 2013. Information reported regard only relationships for which the interest rate is reported.

Table 2: **Descriptive statistics - Non-SMEs' relationships - SME-SF time window**

	Mean	Std Deviation	P(10)	P(50)	P(90)	Count
Non-Eligible Relationships						
Rate Change$_{2014}$	18.781	206.389	-199.243	19.554	224.015	4,499
$\frac{\text{Drawn}}{\text{Total Drawn}}$	0.193	0.189	0.044	0.133	0.406	4,499
$\frac{\text{Revolving}}{\text{Granted}}$	0.161	0.254	0.005	0.050	0.504	4,499
$\frac{\text{Drawn}}{\text{Granted}}$	0.704	0.231	0.364	0.741	0.976	4,499
Years of Relationship	6.720	2.861	2	9	9	4,499
Close Bank	0.121	0.326	0	0	1	4,384
Eligible Relationships						
Rate Change$_{2014}$	28.034	192.591	-166.932	21.098	222.006	7,121
$\frac{\text{Drawn}}{\text{Total Drawn}}$	0.300	0.364	0	0.102	1	6,995
$\frac{\text{Revolving}}{\text{Granted}}$	0.355	0.367	0.019	0.185	1	7,121
$\frac{\text{Drawn}}{\text{Granted}}$	0.499	0.376	0	0.511	0.998	7,121
Years of Relationship	4.844	3.320	0	5	9	7,121
Close Bank	0.135	0.342	0	0	1	7,014
All the Relationships						
Rate Change$_{2014}$	24.451	198.09	-178.23	20.535	223.559	11,620
$\frac{\text{Drawn}}{\text{Total Drawn}}$	0.258	0.312	0.002	0.123	0.981	11,494
$\frac{\text{Revolving}}{\text{Granted}}$	0.279	0.341	0.01	0.111	1	11,620
$\frac{\text{Drawn}}{\text{Granted}}$	0.578	0.343	0.001	0.641	0.987	11,620
Years of Relationship	5.570	3.280	1	6	9	11,620
Close Bank	0.130	0.336	0	0	1	11,398

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. All variables with the exception of the change in the interest rate are measured as of the end of year 2013.

Table 3: **Descriptive statistics - SMEs' relationships - placebo time window**

	Mean	Std Deviation	P(10)	P(50)	P(90)	Count
Non-Eligible Relationships						
Rate Change$_{2013}$	12.013	222.464	-235.795	16.141	243.825	20,955
$\frac{\text{Drawn}}{\text{Total Drawn}}$	0.609	0.313	0.185	0.597	1	20,955
$\frac{\text{Revolving}}{\text{Granted}}$	0.167	0.28	0	0.046	0.613	20,955
$\frac{\text{Drawn}}{\text{Granted}}$	0.892	0.152	0.669	0.954	1.005	20,955
Years of Relationship	6.330	2.173	3	8	8	20,955
Close Bank	0.186	0.389	0	0	1	20,466
Eligible Relationships						
Rate Change$_{2013}$	8.351	193.4	-203.538	16.004	193.451	312,852
$\frac{\text{Drawn}}{\text{Total Drawn}}$	0.424	0.348	0.045	0.312	1	308,448
$\frac{\text{Revolving}}{\text{Granted}}$	0.304	0.329	0.018	0.167	1	312,852
$\frac{\text{Drawn}}{\text{Granted}}$	0.650	0.319	0.135	0.720	1	312,852
Years of Relationship	5.094	2.705	1	5	8	312,852
Close Bank	0.160	0.366	0	0	1	307,882
All the Relationships						
Rate Change$_{2013}$	8.58	195.353	-205.125	16.012	195.878	333,807
$\frac{\text{Drawn}}{\text{Total Drawn}}$	0.436	0.349	0.048	0.329	1	329,403
$\frac{\text{Revolving}}{\text{Granted}}$	0.296	0.328	0.013	0.158	1	333,807
$\frac{\text{Drawn}}{\text{Granted}}$	0.665	0.317	0.153	0.743	1	333,807
Years of Relationship	5.171	2.692	1	6	8	333,807
Close Bank	0.161	0.368	0	0	1	328,348

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. All variables with the exception of the change in the interest rate are measured as of the end of year 2012.

Table 4: **Descriptive statistics - Non-SMEs' relationships - placebo time window**

	Mean	Std Deviation	P(10)	P(50)	P(90)	Count
Non-Eligible Relationships						
Rate Change_{2013}	0.222	228.361	-270.588	7.567	222.926	5,283
$\frac{\text{Drawn}}{\text{Total Drawn}}$	0.231	0.234	0.046	0.147	0.553	5,283
$\frac{\text{Revolving}}{\text{Granted}}$	0.158	0.255	0.003	0.046	0.5	5,283
$\frac{\text{Drawn}}{\text{Granted}}$	0.742	0.219	0.413	0.789	0.987	5,283
Years of Relationship	6.090	2.488	2	8	8	5,283
Close Bank	0.121	0.326	0	0	1	5,152
Eligible Relationships						
Rate Change_{2013}	16.003	205.426	-205.805	18.62	221.038	10,109
$\frac{\text{Drawn}}{\text{Total Drawn}}$	0.390	0.383	0.001	0.239	1	9,981
$\frac{\text{Revolving}}{\text{Granted}}$	0.316	0.346	0	0.167	1	10,109
$\frac{\text{Drawn}}{\text{Granted}}$	0.605	0.372	0	0.691	1.007	10,109
Years of Relationship	4.750	2.882	1	5	8	10,109
Close Bank	0.153	0.360	0	0	1	9,970
All the Relationships						
Rate Change_{2013}	10.586	213.7	-227.134	14.514	221.39	15,392
$\frac{\text{Drawn}}{\text{Total Drawn}}$	0.335	0.347	0.011	0.183	1	15,264
$\frac{\text{Revolving}}{\text{Granted}}$	0.261	0.326	0.002	0.111	1	15,392
$\frac{\text{Drawn}}{\text{Granted}}$	0.652	0.334	0.058	0.742	1	15,392
Years of Relationship	5.210	2.825	1	6	8	15,392
Close Bank	0.142	0.349	0	0	1	15,122

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. All variables with the exception of the change in the interest rate are measured as of the end of year 2012.

Table 5: **Descriptive statistics - firms - SME-SF time window**

	Mean	Std Deviation	P(10)	P(50)	P(90)	Count
Non-SMEs						
Sales	252,721.715	1,124,447.220	55,225	92,967	401,653	2,937
Leverage	53.993	93.821	11.4	55.8	83.5	2,937
EBITDA Ratio	6.981	7.546	-0.166	5.996	15.213	2,937
Risky Firm	0.176	0.381	0	0	1	2,937
log(Assets)	11.42	1.143	10.159	11.253	12.923	2,937
Number of Relationships	7.162	3.957	2	7	12	2,937
Liquidity Ratio	0.054	0.079	0.002	0.026	0.141	2,937
Investment Ratio	0.032	0.049	0.002	0.018	0.072	2,937
SMEs						
Sales	2,900.305	5,677.063	86	1,000	7,043	181,783
Leverage	59.462	184.651	0	66.3	96.3	181,726
EBITDA Ratio	6.606	10.069	-1.733	5.561	16.207	181,773
Risky Firm	0.305	0.461	0	0	1	181,783
log(Assets)	7.425	1.247	5.908	7.313	9.113	181,773
Number of Relationships	2.679	1.824	1	2	5	181,783
Liquidity Ratio	0.041	0.077	0	0.011	0.118	181,773
Investment Ratio	0.035	0.083	0	0.007	0.091	181,773
All Firms						
Sales	6,872.401	145,275.121	89	1030	8,103.5	184,720
Leverage	59.375	183.560	0	66	96.2	184,663
EBITDA Ratio	6.612	10.034	-1.71	5.570	16.194	184,710
Risky Firm	0.303	0.460	0	0	1	184,720
log(Assets)	7.488	1.342	5.919	7.340	9.246	184,710
Number of Relationships	2.751	1.959	1	2	5	184,720
Liquidity Ratio	0.042	0.077	0	0.011	0.118	184,710
Investment Ratio	0.035	0.082	0	0.007	0.091	184,710

Note: The firm-level information reported concern all Italian firms with available balance sheet information in the CERVED firms' dataset. All variables refer to year 2013 balance sheets.

Table 6: **Descriptive statistics - firms - placebo time window**

	Mean	Std Deviation	P(10)	P(50)	P(90)	Count
Non-SMEs						
Sales	255,246.193	1,161,372.144	55,038	93,303	403,917	3,154
Leverage	48.331	235.141	12.7	57	85	3,154
EBITDA Ratio	6.673	7.820	-0.537	5.654	15.480	3,154
Risky Firm	0.194	0.396	0	0	1	3,154
log(Assets)	11.449	1.132	10.214	11.292	12.916	3,154
Number of Relationships	7.014	3.789	2	7	12	3,154
Liquidity Ratio	0.048	0.072	0.001	0.022	0.126	3,154
Investment Ratio	0.035	0.059	0.002	0.019	0.074	3,154
SMEs						
Sales	2,874.788	5,607.778	86	1,000	6,971	199,337
Leverage	60.829	226.946	0	67.8	96.8	199,271
EBITDA Ratio	6.339	10.510	-2.089	5.246	16.025	199,332
Risky Firm	0.318	0.466	0	0	1	199,337
log(Assets)	7.435	1.249	5.916	7.327	9.126	199,332
Number of Relationships	2.686	1.817	1	2	5	199,337
Liquidity Ratio	0.039	0.075	0	0.01	0.112	199,332
Investment Ratio	0.038	0.179	0	0.008	0.098	199,332
All Firms						
Sales	6,805.725	148,356.628	89	1,028	7,972	202,491
Leverage	60.634	227.080	0	67.6	96.8	202,425
EBITDA Ratio	6.344	10.473	-2.068	5.253	16.01	202,486
Risky Firm	0.316	0.465	0	0	1	202,491
log(Assets)	7.498	1.343	5.924	7.351	9.255	202,486
Number of Relationships	2.753	1.939	1	2	5	202,491
Liquidity Ratio	0.040	0.075	0	0.01	0.113	202,486
Investment Ratio	0.038	0.177	0	0.008	0.098	202,486

Note: The firm-level information reported concern all Italian firms with available balance sheet information in the CERVED firms' dataset. All variables refer to year 2012 balance sheets.

Table 7: **Descriptive statistics - banks - SME-SF time window**

	Mean	Std Deviation	P(10)	P(50)	P(90)	Count
CET1 Ratio	0.132	0.038	0.095	0.126	0.187	90
Liquidity Ratio	0.229	0.105	0.097	0.228	0.359	90
Retail Funding	0.636	0.154	0.480	0.680	0.775	90
Wholesale Funding	0.261	0.196	0.093	0.202	0.455	90
log(Assets)	22.122	1.543	20.672	21.639	24.434	90

Note: The bank-level data comprise information on Italian and are collected from the Supervisory Reports. All variables refer to year 2013 balance sheets.

Table 8: **Descriptive statistics - banks - placebo time window**

	Mean	Std Deviation	P(10)	P(50)	P(90)	Count
CET1 Ratio	0.119	0.036	0.073	0.113	0.169	95
Liquidity Ratio	0.208	0.084	0.1	0.212	0.321	95
Retail Funding	0.612	0.160	0.417	0.660	0.745	95
Wholesale Funding	0.273	0.230	0.086	0.193	0.584	95
log(Assets)	22.099	1.502	20.592	21.671	24.336	95

Note: The bank-level data comprise information on Italian and are collected from the Supervisory Reports. All variables refer to year 2013 balance sheets.

Table 9: **McCrary's Density Test for outstanding exposure**

	2014	2013
Observations (l - r)	$2761 - 2075$	$3301 - 2670$
T-Stat	0.43	0.46
P-Value	0.66	0.64

Note: The table presents the t-statistics and p-values of the McCrary's density test, with number of observation considered in density estimation at the left and right of the cutoff reported in the second row. In both cases, the null hypothesis is that there is no discontinuity in the density. The bandwidth is handpicked so to fit an interval of $+/-$ euro $500,000$ around the threshold.

Table 10: **Continuity of Relationship Covariates**

Control Variable	Test, Pol(0)	Test, Pol(1)	Test, Pol(2)
Lag Share of Total Drawn	0.002	0.001	0.003
	(0.01)	(0.011)	(0.013)
Lag Revolving Rate	0.049	0.055	0.116
	(0.178)	(0.195)	(0.221)
Lag Revolving Fraction	0.009	0.011	0.014
	(0.01)	(0.012)	(0.013)
Lag Drawn on Granted	0.002	0.001	0.003
	(0.01)	(0.011)	(0.013)
log(Age)	-0.005	-0.009	-0.012
	(0.02)	(0.025)	(0.028)
$\mathbb{1}$(Hq in Same Province)$_{bf}$	0.003	0.006	0.014
	(0.013)	(0.014)	(0.019)

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 relationship level covariates included in the covariates augmented version of Equation 3. This means the following specification: covariate$_{bf2013} = b_0 + b_1 R_{bf} + \phi_-(|x_{bf}^{2013} - \bar{x}|) + \phi_+(|x_{bf}^{2013} - \bar{x}|) + e_{bf}$ locally, estimated locally, with a triangular kernel. Here x_{bf} is drawn credit, $\bar{x}$ the euro 1.5 million threshold, $\phi_{+/-}$ the right/left polynomial in the x_{bf} centered at the threshold, and the null hypothesis is $b_1 = 0$.

Table 11: **Continuity of Firm Covariates**

Control Variable	Test, Pol(0)	Test, Pol(1)	Test, Pol(2)
Lag Leverage	3.179	3.323	5.224
	(3.69)	(3.682)	(4.511)
Lag Ebitda/Assets	0.164	0.193	0.231
	(0.252)	(0.297)	(0.327)
Lag D. Risky	−0.012	−0.015	−0.015
	(0.015)	(0.02)	(0.024)
Lag log(Assets)	−0.019	−0.022	−0.015
	(0.049)	(0.044)	(0.044)
Lag Liquidity	0.002	0.002	0.002
	(0.002)	(0.002)	(0.003)
Lag Investment	−0.002	−0.002	−0.001
	(0.002)	(0.003)	(0.003)
Lag N. Rel.	−0.209	−0.237	−0.279*
	(0.143)	(0.148)	(0.168)

Robust std. errors, clustered at the firm level, in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The Table reports the statistical significance and coefficients' values (up to third digit) for discontinuities in each of the firm level covariates included in the covariates augmented version of Equation 3. This means the following specification: $\text{covariate}_{bf2013} = b_0 + b_1 R_{bf} + \phi_-(|x_{bf}^{2013} - \bar{x}|) + \phi_+(|x_{bf}^{2013} - \bar{x}|) + e_{bf}$, estimated locally with a triangular kernel. Here x_{bf} is drawn credit, $\bar{x}$ the euro 1.5 million threshold, $\phi_{+/-}$ the right/left polynomial in the x_{bf} centered at the threshold, and the null hypothesis is $b_1 = 0$.

Table 12: **Continuity of Bank Covariates**

Control Variable	Test, Pol(0)	Test, Pol(1)	Test, Pol(2)
CET1 Ratio	−0.001	−0.001	−0.001
	(0.008)	(0.009)	(0.009)
Total Capital Ratio	−0.002	−0.001	−0.002
	(0.01)	(0.01)	(0.01)
Lag Liquidity	0.003	−0.002	−0.004
	(0.026)	(0.025)	(0.025)
Lag Retail Fund.	0.006	0.02	0.03
	(0.076)	(0.076)	(0.073)
Lag Whole Fund.	−0.014	0.008	0.007
	(0.103)	(0.105)	(0.104)
Lag Bank Size	0.007	−0.003	0.009
	(0.703)	(0.705)	(0.705)

Robust standard errors, clustered at the bank level, in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The Table reports the statistical significance and coefficients' values (up to third digit) for discontinuities in each of the firm level covariates included in the covariates augmented version of Equation 3. This means the following specification: $\text{covariate}_{bf2013} = b_0+b_1R_{bf}+\phi_-(|x_{bf}^{2013}-\bar{x}|)+\phi_+(|x_{bf}^{2013}-\bar{x}|)+e_{bf}$, estimated locally with a triangular kernel. Here x_{bf} is drawn credit, $\bar{x}$ the euro 1.5 million threshold, $\phi_{+/-}$ the right/left polynomial in the x_{bf} centered at the threshold, and the null hypothesis is $b_1 = 0$.

Table 13: **Dependent Variable** Interest Rate Change in *bp*; **Method** Simple RD

	RD, Pol(0)	RD, Pol(1)	RD, Pol(2)
$\hat{\beta}$ 2014	-11.754^{**}	-19.135^{**}	-19.541^{**}
	(5.833)	(7.828)	(9.362)
Obs. (left; right)	6,844; 3,797	9,378; 6,284	19,195; 6,854
$\hat{\beta}$ 2013	-2.087	-1.594	1.943
	(3.953)	(5.092)	(6.592)
Obs. (left; right)	11,852; 12,917	27,481; 17,965	25,625; 19,860
$\hat{\beta}$ 2014 (Non-SME)	$-6,745$	-3.354	11.761
	(14.445)	(18.454)	(28.294)
Obs. (left; right)	402; 2,821	833; 3,875	671; 4,181

Note: This Table presents the results of discontinuity tests run *via* local estimation of $\hat{\beta}$ in Equation 3: $\Delta i_{bf} = \alpha + \beta R_{bf} + \phi_{-}(|x_{bf}^{2013} - \bar{x}|) + \phi_{+}(|x_{bf}^{2013} - \bar{x}|) + \epsilon_{bf}$, where Δi is the interest rate change in basis points, x_{bf} the past drawn credit, $\bar{x}$ the euro 1.5 million threshold, $\phi_{+/-}$ the right/left polynomial in the x_{bf} centered at 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 on the SMEs 2013 and non-SMEs 2014 samples for placebo purposes. Estimates reported employ triangular kernel weights, with robust standard errors.

Table 14: **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	-15.461^{**}	-18.841^{**}	-18.611^{**}
	(7.201)	(8.058)	(8.743)
Obs. (left; right)	2,609; 3,191	8,047; 5,919	26,803; 6,540
$\hat{\beta}$ 2013	-1.694	-1.293	3.394
	(4.306)	(5.187)	(6.987)
Obs. (left; right)	8,208; 11,523	24,780; 17,147	18,566; 18,990
$\hat{\beta}$ 2014 (Non-SMEs)	-8.656	-11.896	23.767
	(14.014)	(16.603)	(30.341)
Obs. (left; right)	389; 2,533	786; 3,672	554; 3,936
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: $\Delta i_{bf} = \alpha + \beta R_{bf} + \phi_{-}(|x_{bf}^{2013} - \bar{x}|) + \phi_{+}(|x_{bf}^{2013} - \bar{x}|) + \Gamma C_{bf} + \epsilon_{bf}$, where Δi is the interest rate change in basis points, x_{bf} the past drawn credit, $\bar{x}$ the euro 1.5 million threshold, $\phi_{+/-}$ the right/left polynomial in the x_{bf} centered at 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.

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), risk dummy (low Altman z-score), EBITDA/assets, industry dummies, regional dummies, number of credit relationships, investment ratio; **bank level**: Tier 1 capital ratio, liquidity, fraction of retail funding, fraction of wholesale funding, log(assets).

Table 15: **Dependent Variable** Interest Rate Change in *bp*; No correction

	RD, Pol(0) Firm Clustered Errors	RD, Pol(1) Firm Clustered Errors	RD, Pol(2) Firm Clustered Errors
$\hat{\beta}$ 2014	-11.591^{**}	-16.05^{**}	-16.728^{**}
	(5.917)	(6.931)	(7.486)
Obs. (left; right)	2,609; 3,191	8,047; 5,919	26,803; 6,540
$\hat{\beta}$ 2013	-1.409	-1.450	1.728
	(3.462)	(4.335)	(6.263)
Obs. (left; right)	8,208; 11,523	24,780; 17,147	18,566; 18,990
$\hat{\beta}$ 2014 (Non-SMEs)	-4.837	-9.462	18.181
	(11.171)	(14.193)	(26.793)
Obs. (left; right)	389; 2,533	786; 3,672	554; 3,936
Controls	✓	✓	✓

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: $\Delta i_{bf} = \alpha + \beta R_{bf} + \phi_-(|x_{bf}^{2013} - \bar{x}|) + \phi_+(|x_{bf}^{2013} - \bar{x}|) + \Gamma C_{bf} + \epsilon_{bf}$, where Δi is the interest rate change in basis points, x_{bf} the past drawn credit, $\bar{x}$ the euro 1.5 million threshold, $\phi_{+/-}$ the right/left polynomial in the x_{bf} centered at the threshold, C_{bf} is a matrix of controls, 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 robust standard errors clustered at the firm level.
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), risk dummy (low Altman z-score), EBITDA/assets, industry dummies, regional dummies, number of credit relationships, investment ratio; **bank level**: Tier 1 capital ratio, liquidity, fraction of retail funding, fraction of wholesale funding, log(assets).

Table 16: **Dependent Variable** Interest 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	-27.507^{***}	-27.106^{**}	-30.545^{***}	-25.034^{**}
Eligible firms	(9.895)	(10.46)	(10.596)	(10.314)
Clusters	3,109 (Firms), 93 (Banks)	5,293 (Firms), 94 (Banks)	2,778 (Firms), 87 (Banks)	6,763 (Firms), 89 (Banks)
N. Observations	8,198	14,769	7,198	19,146
$\hat{\beta}$ 2013	4.996	12.179	2.447	17.360
Eligible firms	(8.225)	(8.983)	(7.932)	(10.621)
Clusters	7,157 (Firms), 95 (Banks)	7,193 (Firms), 96 (Banks)	6,719 (Firms), 92 (Banks)	5,860 (Firms), 92 (Banks)
N. Observations	18,662	18,856	17,430	15,096
$\hat{\beta}$ 2014	−5.134	18.418	−8.371	28.845
Non-Eligible firms	(18.005)	(33.444)	(14.922)	(37.378)
Clusters	1,086 (Firms), 77 (Banks)	1,105 (Firms), 77 (Banks)	1,043 (Firms), 76 (Banks)	1,044 (Firms), 73 (Banks)
N. Observations	4,174	4,314	4,099	4,062
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: $\Delta i_{bf} = \alpha + \beta R_{bf} + \phi_{-}(|x_{bf}^{2013} - \bar{x}|) + \phi_{+}(|x_{bf}^{2013} - \bar{x}|) + f + \epsilon_{bf}$, where Δi is the interest rate change in basis points, x_{bf} the past drawn credit, $\bar{x}$ the euro 1.5 million threshold, $\phi_{+/-}$ the right/left polynomial in the x_{bf} centered at 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 (when included): **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; **bank level**: Tier 1 capital ratio, liquidity, fraction of retail funding, fraction of wholesale funding, log(assets).

Table 17: **Dependent Variable** Interest Rate Change in *bp*; FE-Sample estimates

$\hat{\beta}$ 2014	-29.162^{***}	-29.295^{***}	-24.087^{***}
1st Order Specification	(10.463)	(10.789)	(8.504)
Observations	5,665	5,665	5,601
$\hat{\beta}$ 2014	-30.645^{***}	-28.565^{***}	-23.743^{***}
2nd Order Specification	(9.485)	(7.612)	(7.911)
Observations	8,917	6,645	6,566
Firm Controls		✓	✓
Bank Controls			✓
$\hat{\beta}$ 2014	-34.931^{***}	-33.108^{***}	-28.976^{***}
1st Order Specification	(10.991)	(10.714)	(8.551)
Observations	5,200	5,200	5,145
$\hat{\beta}$ 2014	-33.688^{***}	-31.205^{***}	-27.875^{***}
2nd Order Specification	(9.215)	(6.951)	(6.966)
Observations	10,138	6,893	6,812
Relationship Controls	✓	✓	✓
Firm Controls		✓	✓
Bank Controls			✓

t statistics 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: $\Delta i_{bf} = \alpha + \beta R_{bf} + \phi_{-}(|x_{bf}^{2013} - \bar{x}|) + \phi_{+}(|x_{bf}^{2013} - \bar{x}|) + \epsilon_{bf}$, on the subsample of observations on which the fixed effect estimator of β is identified. Such subsample 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, $\phi_{+/-}$ the right/left polynomial in the x_{bf} centered at the threshold, and the null hypothesis of each test is $\beta = 0$. 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), risk dummy (low Altman z-score), EBITDA/assets, industry dummies, regional dummies, number of credit relationships, investment ratio; **bank level**: Tier 1 capital ratio, liquidity, fraction of retail funding, fraction of wholesale funding, log(assets).

Table 18: **Dependent Variable** Interest Rate Change in *bp*; Large lines, FE and Local sample

γ 2014	3.364	1.284	−2.860
1st Order Specification	(6.753)	(7.027)	(7.148)
Observations	6,214	6,118	6,012
Firm Controls	✓	✓	✓
Bank Controls		✓	✓
Relationship Controls			✓
γ 2014	−1.244	−4.597	−9.364**
2nd Order Specification	(4.387)	(4.392)	(4.358)
Observations	6,840	6,732	6,616
Firm Controls	✓	✓	✓
Bank Controls		✓	✓
Relationship Controls			✓

t statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: This Table presents 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 subsample for which the firm fixed effect is identified, x_{bf} the past drawn credit, $\bar{x}$ the euro 1.5 million threshold, ϕ_+ the right polynomial in the x_{bf} centered at 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), risk dummy (low Altman z-score), EBITDA/assets, industry dummies, regional dummies, number of credit relationships, investment ratio; **bank level**: Tier 1 capital ratio, liquidity, fraction of retail funding, fraction of wholesale funding, log(assets).

Table 19: **Dependent Variable** Interest Rate Change in *bp*; high, medium, low splits

	Pol(1)	Pol(1)	Pol(1)	Pol(2)	Pol(2)	Pol(2)
Profitability						
$\hat{\omega_H}$	-1.531	-1.893	-1.145	-3.427	-3.558	-2.613
	(5.592)	(5.774)	(5.537)	(6.01)	(5.796)	(5.009)
$\hat{\beta_M}$	-18.732^{***}	-18.496^{***}	-17.861^{***}	-18.807^{***}	-18.794^{***}	-18.204^{***}
	(4.824)	(4.716)	(5.359)	(6.028)	(5.887)	(6.618)
$\hat{\omega_L}$	9.364^{**}	8.337^{*}	7.08^{*}	7.402^{***}	6.94^{***}	5.914^{***}
	(4.231)	(4.413)	(4.118)	(1.881)	(2.039)	(1.806)
Observations	14,257	14,257	14,056	33,254	33,252	32,753
Leverage						
$\hat{\omega_H}$	17.960^{***}	10.222^{*}	11.223^{*}	17.119^{***}	10.735^{**}	11.317^{**}
	(6.611)	(6.156)	(6.612)	(4.552)	(4.920)	(5.65)
$\hat{\beta_M}$	-17.744^{***}	-16.323^{***}	-15.659^{**}	-18.995^{***}	-17.78^{***}	-17.023^{**}
	(5.599)	(5.507)	(6.143)	(6.377)	(6.319)	(7.063)
$\hat{\omega_L}$	-4.315	-7.821	-9.446	-2.358	-5.729	-7.312
	(8.035)	(8.119)	(7.615)	(7.823)	(7.822)	(7.849)
Observations	14,257	14,257	14,056	33,254	33,252	32,753
Rel. Controls	✓	✓	✓	✓	✓	✓
Firm Controls		✓	✓		✓	✓
Bank Controls			✓			✓

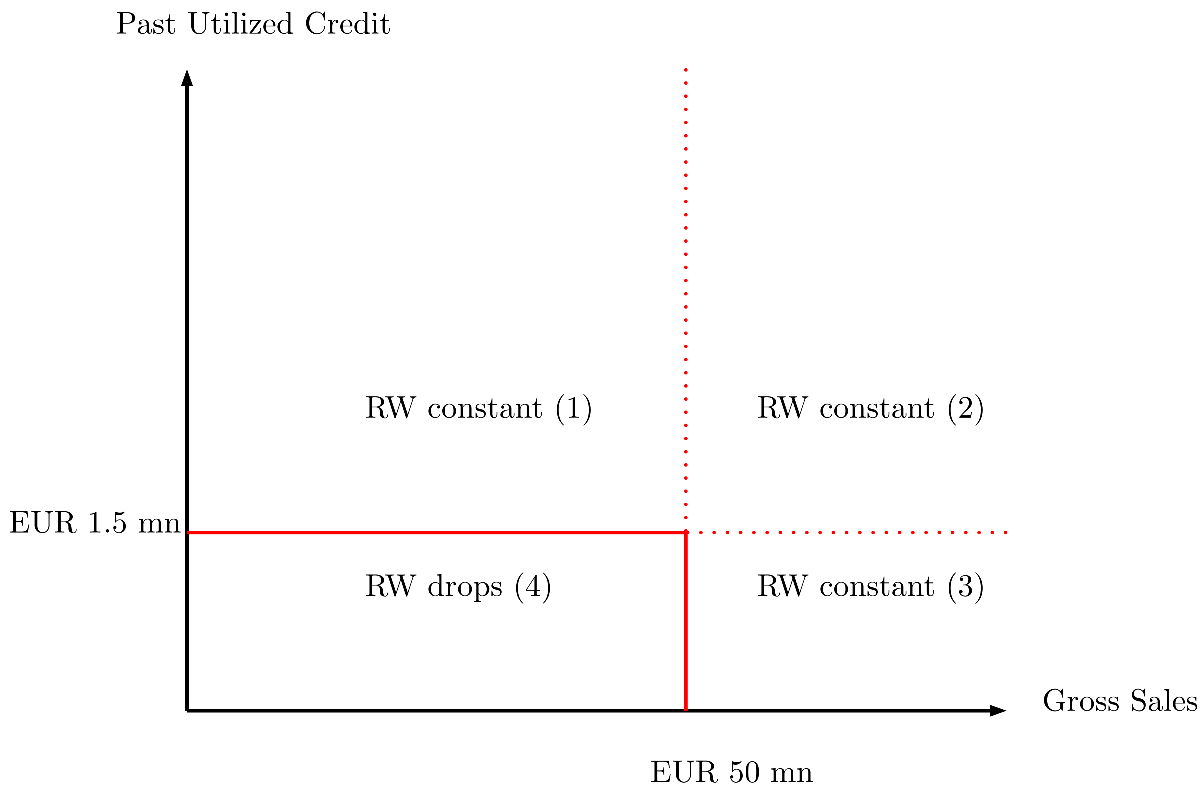
t statistics in parentheses

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

Note: This Table presents tests of differential pass-through for low and high profitability, and low/high leverage firms. Specifications are as follows $\hat{\beta}$ in Equation 7: $\Delta i_{bf} = \alpha + \beta_M R_{bf} + \omega_L Low_f * R_{bf} + \omega_H High_f * R_{bf} + \Gamma X_{bf} + \phi_-(|x_{bf}^{2013} - \bar{x}|) + \phi_+(|x_{bf}^{2013} - \bar{x}|) + \epsilon_{bf}$. They are estimated locally. Δi is the interest rate change in basis points, x_{bf} the past drawn credit, $\bar{x}$ the euro 1.5 million threshold, $\phi_{+/-}$ the right/left polynomial in the x_{bf} centered at the threshold, and the null hypothesis of each test is that the true value of the parameter is equal zero. $High_f$ and Low_f are dummies that equal one if the firm falls in the group with high and low characteristic (profitability/leverage). X_{bf} collects all controls. 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), risk dummy (low Altman z-score), EBITDA/assets, industry dummies, regional dummies, number of credit relationships, investment ratio; **bank level**: Tier 1 capital ratio, liquidity, fraction of retail funding, fraction of wholesale funding, log(assets).

Figure 1: SME-SF Discount Assignment



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

Figure 2: 2013-2012

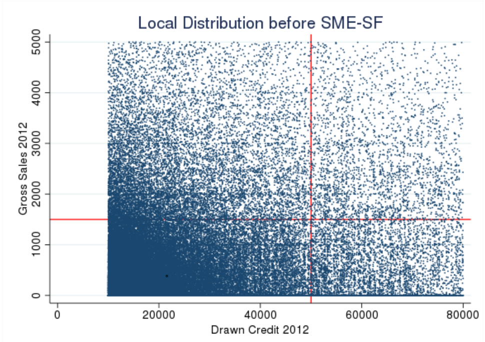
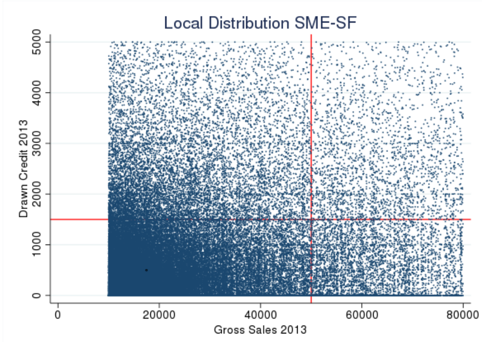


Figure 3: 2014-2013



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

Figure 4: Density Plot, 2014

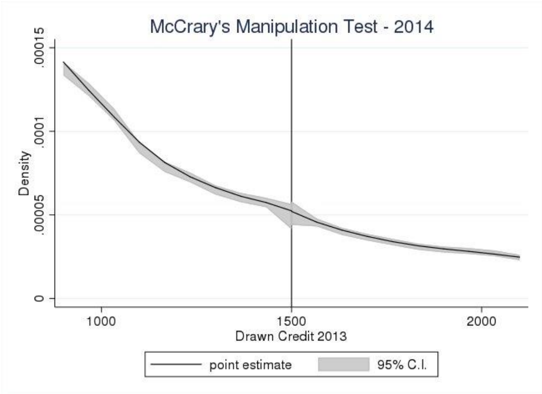
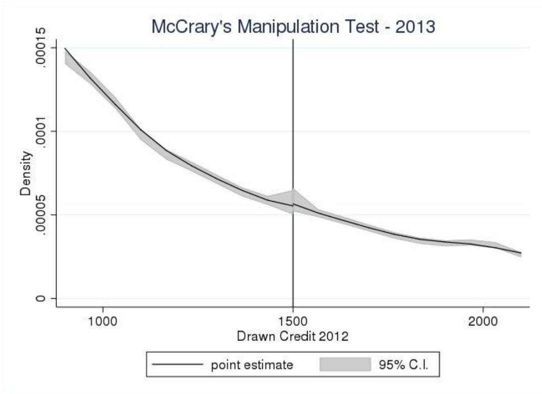


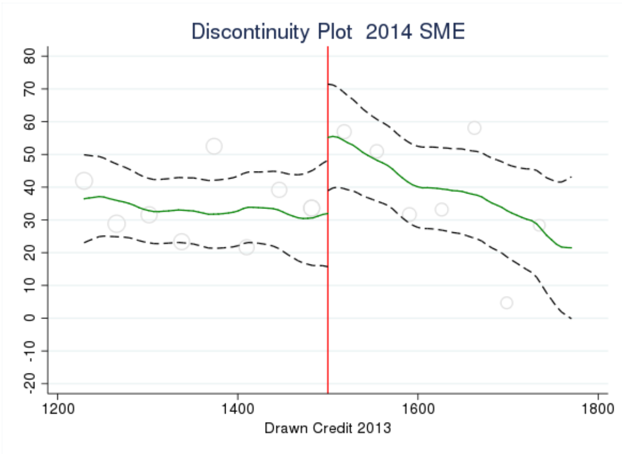
Figure 5: Density Plot, 2013



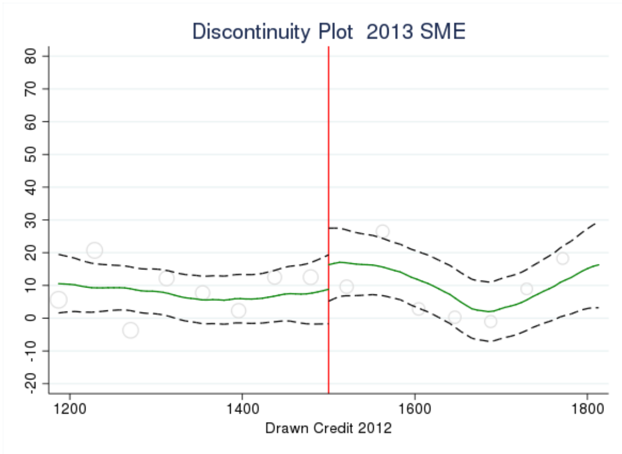
Note: The figures present the graphical outputs of testing discontinuity in the density of observations on the left and the right of the cutoff. The shaded area plots the 95 percent confidence interval around the smoothed density backed from the data.

Figure 6: Discontinuity Plots, Reform and Placebos

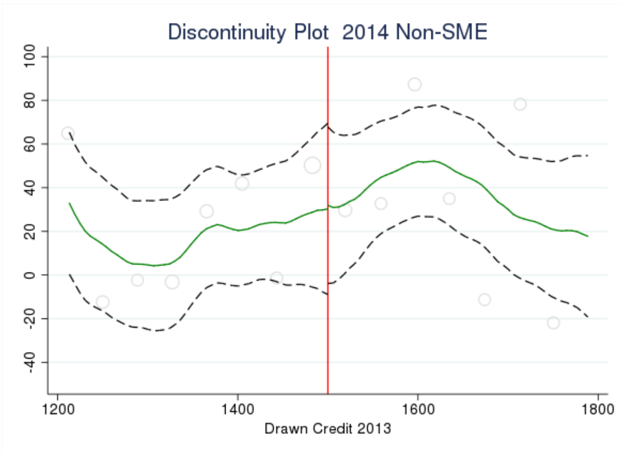
(a)



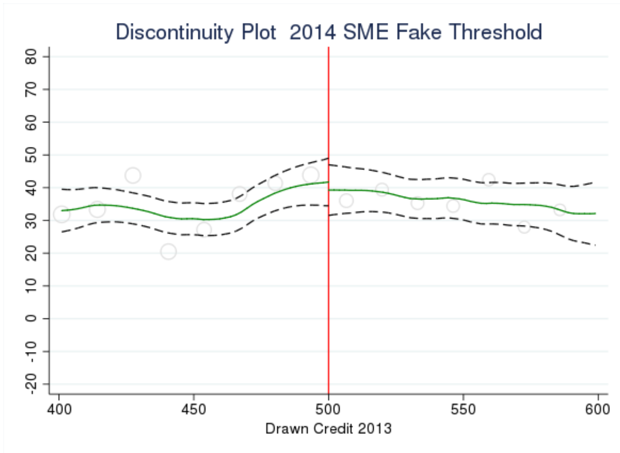
(b)



(c)



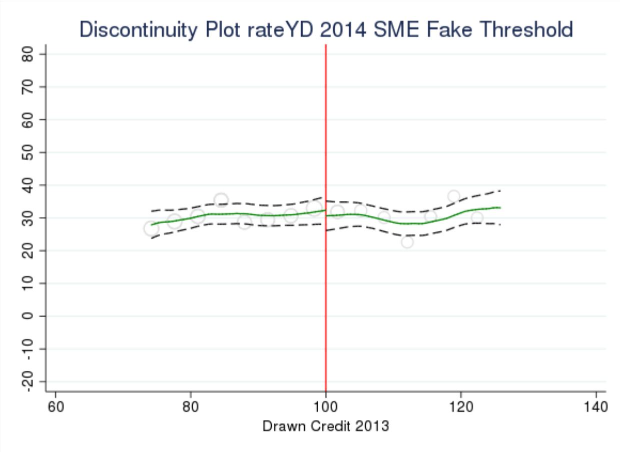
(d)



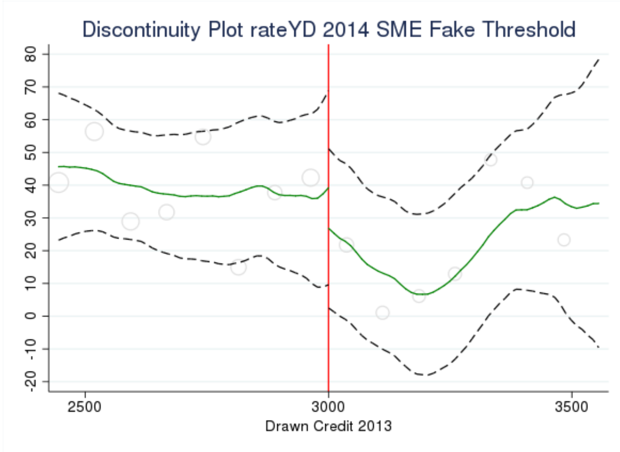
Note: From the top left, we report the reform effect at the eligibility threshold for SME credit lines in 2013-2014; the placebo for SME credit lines at the SME-SF threshold in 2012-2013; the placebo discontinuity employing non-SME credit lines in 2013-2014; the placebo for the fictitious 500 thousand 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 as large as the bin size) of the change in rates with respect to past drawn amounts, together with their 95% confidence intervals.

Figure 7: Discontinuity Plots, Reform and Placebos

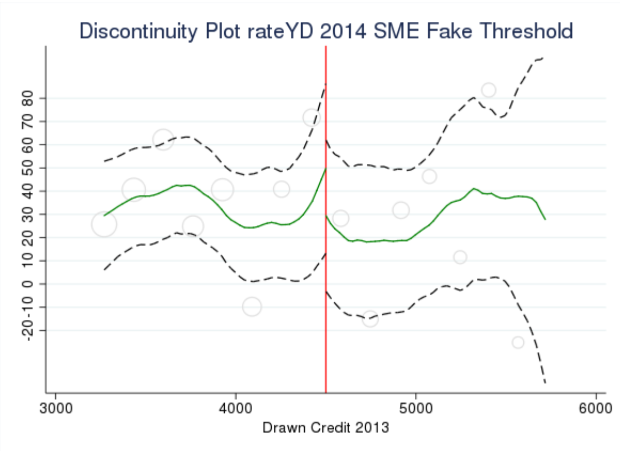
(a)



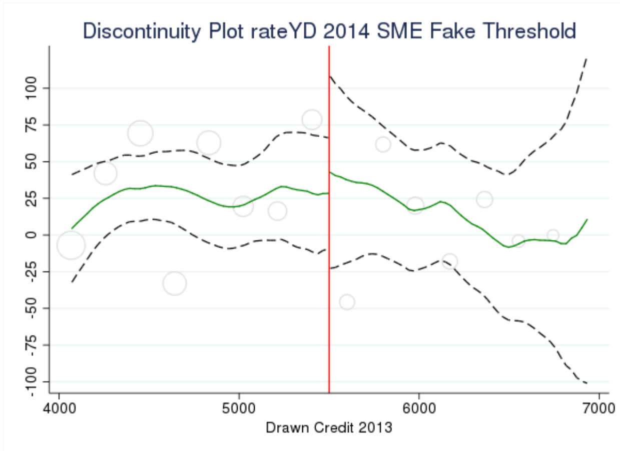
(b)



(c)



(d)



Note: From the top left, we report alternative placebo thresholds we inspect to run placebo graphical tests. The figure plots on the y-axis the delta in yearly rates before and after SME-SF implementation; 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 as large as the bin size) of the change in rates with respect to past drawn amounts, together with their 95% confidence intervals.

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