Public Guarantees to SME Borrowing. An RDD Evaluation*

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Abstract
The paper evaluates the impact of the guarantees provided by the Italian scheme *Fondo di Garanzia* on the access to credit for the small and medium enterprises. It also assesses to what extent firm performances, in terms of investments and sales, have been affected by the scheme. The study exploits the mechanism that assigns the guarantees, which is based on a scoring system to assess eligibility. By using regression discontinuity techniques, the paper finds that: (i) at the threshold between eligible and non-eligible firms, the program has a positive impact on bank loans to firms; however, the scheme has no impact on the interest rate charged by the banks, while it affects positively the likelihood that a firm is unable to repay its loans. No effect is found for firm investments and only a mixed impact is detected for sales; the guaranteed loans were mostly used to finance working capital; (ii) these findings broadly hold also for infra-marginal (far-from-the-cutoff) firms, at least for a bandwidth of the threshold for which the conditional independence assumption is maintained. For these firms, our results would suggest that: a) a lowering of the eligibility criteria would increase the effectiveness of the program in fostering bank loans; b) the scheme has a favorable impact both on borrowing and interest rates for the firms that easily pass the admission threshold; c) the unfavorable effect on bad loans remains mostly undisputed.
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1. Introduction

Public guarantee schemes (PGSs) aim at supporting firms' access to bank credit by means of providing publicly funded collateral. PGSs are typically targeted to small and medium enterprises (SMEs), which are the type of firms most likely to suffer from credit constraints. These programs, widespread in both developed and developing countries, have experienced a dramatic surge in popularity in the aftermath of the global financial crisis (Beck et al., 2010). Due to the restrictions on the supply of bank credit to firms, PGSs are being considered as a cost-effective public intervention to spur credit creation (OECD, 2013).

PGSs can have desirable effects. They might allow constrained firms to access credit, and risky but-creditworthy firms to get larger financing at a lower cost. PGSs also provide benefits to banks, allowing them to share their credit risk and save on regulation capital.[1] These features of the scheme are very appealing in a situation in which credit risk is very high and the capital requirements for the banks are increasing (Draghi, 2013). Compared to other types of program (such as direct lending, co-funding, interest rate subsidies), PGSs might allow public agencies to increase bank financing to the private sector by using relatively few resources (Action Institute, 2013). In particular, the funding of the scheme has a very high leverage (potentially allowing a great mobilization of private financing) and a revolving nature (when a guaranteed loan is paid back the public resources became available for guaranteeing new credit). However, the desirable effects might fail to materialize. If the firms that receive the guarantee are those that would have been financed anyway, there would be no impact on private sector access to credit. Moreover, the scheme might enhance moral hazard on both bank and firm sides, because of the limited liability mechanism, or other opportunistic behaviors, increasing riskiness. Under these circumstances, a lack of effectiveness of the program would go hand in hand with a very high cost of the scheme for the public finances. All in all, whether the PGSs work is an empirical question. Answers to this question seem to be much needed, as the schemes are gaining attractiveness among policy makers (European commission 2013; European Commission and European Investment Bank, 2013).

This paper evaluates the effectiveness of the Italian PSG scheme, named *Fondo di Garanzia* (FG).[2] The intervention under the FG has been massive: from 2009 to 2012, more than €40 billion loans were guaranteed. The operations of the FG are likely going to increase in the near future, as the scheme was re-financed at the end of the 2013, while a newly appointed government announced, in February 2013, that the program would be a centerpiece of its economic policy.

On more technical grounds, the FG has an eligibility mechanism that allows a credible identification strategy. In particular, the eligibility of the firms interested in the scheme is assessed through a scoring system that is based on balance-sheet observables. By using a fuzzy regression discontinuity design (F-RDD) we are able to estimate the impact of the scheme at the threshold for eligibility. Our results suggest that – when evaluated at the cutoff - the FG has a positive effect on bank loans to firms, but no impact on the interest rate charged by the banks. They also underscore that the scheme affects positively the likelihood that subsidized firms will be unable to repay their loans. Moreover, no effect is found for firm investments while the evidence of the impact on firm

[1] See Regulation EU No 575/2013 of the European Parliament and of the of the Council, 26 June 2013.

[2] See: http://www.fondidigaranzia.it/ and http://www.youtube.com/watch?v=fF5qaI1yIdA.

sales is mixed. Our findings suggest that the extra-finance made available by the scheme has been mostly used to finance working capital, such as inventories and trade credit. We also make use of the Angrist and Rokkanen (2012) conditional independence assumption (CIA) to make inference about the impact of the FG for firms which are away from the admission cutoff. We find that the impacts we estimated at the threshold broadly hold for the firms that display an eligibility score that falls in the bandwidth of the cutoff where the CIA is maintained (which includes 20% of the firms in our sample). The main exception refers to interest rates, for which a favorable impact of the scheme materializes for firms far above the cutoff.

The paper is structured as follows. Sect. 2 describes the previous literature on evaluating PSGs. Sect. 3 provides the relevant institutional details of the FG. Sect. 4 describes our dataset, which includes both balance-sheet data and (confidential) information drawn from the Credit Register. Sect. 5 explains the empirical strategy. In particular, it makes it clear how we deal with our main empirical challenge, the lack of data for non-eligible firms. Then, it provides empirical evidence that substantiates the F-RDD strategy. Sect. 6 present the results we obtain at the eligibility threshold. Sect. 7 describes the findings for the firms far from the cutoff. Sect. 8 concludes, mentioning policy implications, the caveats of the analyses, and some interesting issues for future research.

2. Previous literature

Policies aimed at alleviating firms' financing constraints find their rationale in the possibility of a market failure in the credit market (Stiglitz and Weiss, 1981). In this respect, SMEs show a higher probability of being credit rationed, due to exacerbated problems of asymmetric information (Berger et al., 1992). Minelli and Modica (2009) and Arping et al. (2010) provide theoretical models that compare the respective merits of different policies (such as direct lending, co-funding, interest rate subsidies, and PGSs) in ameliorating credit constraints. Public guarantees amount to a provision of collateral. However, as underscored by Honohan (2010), public collateral is very different from private collateral: the former does not have any role in signaling the creditworthiness of the borrower (Bester, 1985; Besanko and Thakor, 1987). Moreover, public guarantees might increase ex-post moral hazard problems, whereby private collateral typically reduce them (Boot et al., 1991; Boot and Thakor, 1994; Aghion and Bolton, 1997; and Holmstrom and Tirole, 1997). On the bank side, public collateral is very attractive for the virtually risk-free status of the guarantor and the readiness of executions in case of firms' default.

Previous investigations on the effectiveness of PGSs are rather scant. Their results are heterogeneous: while a positive impact on credit flows is documented in the majority of instances, no consensus seems to emerge as regard to the effects of these programs on other credit variables, such as interest rates and riskiness, and firm performances.

Hancock et al. (2007) use state-level US data to estimate the impact of credit guarantees provided by the *Small Business Administration*. They find positive effects of the guarantees on firms' activity, in terms of both output and employment, and a (modest) effect of the program on decreasing firms' risk of default. Using similar data, Craig et al. (2007) provide further evidence on the effectiveness of the scheme, suggesting that the growth of (per capita) income was higher in the states that received a relatively larger amount of guaranteed loans. Riding at al. (2008) use firm-level survey data from a Canadian program (*Canada Small Business Financing*). By relying on a two-step (Heckman) estimation procedure, they highlight that the scheme had a positive impact on loans disbursed by the banks. Kang and Heshmati (2008), who considered two different Korean PGSs, find only weak evidence of an impact on firms' sales, productivity, and employment. They suggest that the guarantees were mainly used to support financially unconstrained firms. Lelarge et al. (2008) use firm-level data from a French PGS (*Sofaris*). They take selection issues into account by exploiting a 1995 change in eligibility rules, which extended the program to new industries, and find that the scheme had positive effects on loans availability, interest rates and firms' performance; however, the program also increased firms' risk taking. Uesugi et al. (2010) use firm-level data from a Japanese program (*SCG*). They adopt a propensity-score matching to deal with selection bias and conclude that the program increased credit availability but it also raised the probability of defaults. As for Italy, Zecchini and Ventura (2009) use data on the Italian *Fondo di Garanzia*, from 2000 to 2005. They employ a difference-in-difference estimation and find a positive, though small, impact on the amount of bank debt and a negative effect on the cost of borrowing (based on firms' balance-sheet interest expenses). More recently, D'Ignazio and Menon (2013) analyze an Italian regional PGS. They tackle selection issues by using an IV regression, which exploits an exogenous event that expanded eligibility to the program to firms previously cut out of it. They find no effect of the scheme on total debt; yet, they document a shift in debt composition in favor to long-term borrowing. Moreover, they find evidence of eased-up financing conditions, in terms of lower interest rates. They also look at treated firms' performance in terms of investments and do not find a significant impact of the policy.

Our paper contributes to the literature on the evaluation of the PGSs. Compared to previous work, our study exploits a highly-credible identification strategy. Moreover, it focuses on a period featured by a credit crunch of unrecorded gravity.

3. The Italian public guarantees scheme

The mission of the *Fondo di Garanzia* is that of promoting funding opportunities for creditworthy but rationed SMEs. The rationale of the scheme is based on the standard market-failure observations: SMEs' access to funding is hampered by the higher costs of small-scale lending, the lack of collateral and the reduced reliability of financial statements, which exacerbates the problem of asymmetric information. As illustrated by OECD (2012), during the financial and economic crisis, Italian SMEs have severely suffered from the credit crunch, experiencing a more significant drop in credit flows and a stronger rise in interest rates with respect to larger firms.

The FC started its activity in 2000. Since then, the volume of bank loans guaranteed gradually increased overtime remaining, however, below €2 billion. The figure boomed with the inception of the economic and financial crises and the increase in the number of Italian SMEs that severely suffered from the credit crunch[3]. From 2009 to 2013 almost €40 billion of loans to SMEs benefited from the public guarantee (Figure 1). The operations of the FG are likely going to increase in the near future, as the scheme was re-financed (under the *Letta* government) at the end of the 2013 and the newly appointed *Renzi* government announced (in February 2014) that the program would be a centerpiece of its economic policy.

[3] See OECD (2012), Bank of Italy (2012, 2013).

[Fig. 1]

The provision of guarantees[4] is limited to SMEs, defined according to EU criteria,[5] of the private sector, which includes manufacturing, construction and services. However, some specific sectors, such as agriculture, automobile and financial services, are not covered because of the limitations imposed by the EU regulation on competition. The public guarantee insures up to 80% of the value of a bank loan. For each firm, however, there is a maximum amount of guarantee, which is equal to € 1,5 million. The FG can guarantee both short-term and long-term loans and there isn't any constraint in terms of the final use of the funding by the borrower.

As other PGSs, the scheme involves three agents, a bank[6], a firm, and the FG. A SME that needs to borrow might ask the bank to apply for a public guarantee.[7] If the bank is interested, it verifies the eligibility of the firm for the scheme through a scoring system (a software) provided by the FG. Enquiring the software is not without costs: while the FC fees are generally very low[8], the labor costs related to the bank official that materially have to collect the information and make use of the software amount to about €600 (as estimated by the officers of the in charge of the program at the Ministry of economic development).

The scoring system takes into account three aspects of the performance of the firm in the two years preceding that of the application. These aspects refer to 1) financial stability; 2) short-term financial burden; 3) cash-flow. The FG guidelines lists the balance-sheet variables, which are intended to measure each single aspect.[9]

For each of the two years preceding that of the application, the software calculates from the values of the balance-sheet variables a single score. The score is discretized in three categories (A=good; B=intermediate, C=bad). Then, the scoring system takes the yearly scores (categories) into account and provides the final outcomes according to the outline described in Table 1[10]. As a result, the applying firms are spitted in three Types (0, 1, and 2). Type-0 firms are not eligible. Type-1 and Type-2 firms are both eligible but do not automatically receive the treatment. They have to go through a further assessment, which is more demanding for the Type-1 firm, as they have worse scores (i.e., poorer lagged balance-sheet observables). [11] The additional assessment concludes with

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[4] We refer to the rules of functioning in place between 2005 and 2010, the period over which our empirical analysis focuses on. The rules have been slightly changed starting from January 2010. See: http://www.fondidigaranzia.it/.

[5] See: http://ec.europa.eu/enterprise/policies/sme/facts-figures-analysis/sme-definition/index_en.htm.

[6] The bank can be both a regular intermediary and a mutual guarantee institution (*Confidi*).

[7] Alternatively, it is the bank that might propose to the firm to apply for the guarantee.

[8] Fees are computed as a fraction of the amount of the guarantee, depending on the type of operation that is financed (risk capital, investments etc.) and the size of the firm. Moreover, operations in favour of "disadvantaged" firms (such as firms located in the South or leaded by a female) are not subjected to a fee. Therefore, fees can vary between 0% and 2% of the guaranteed amount.

[9] Financial stability is measured for the industry (service) sector by the ratios equity and long-term loans/fixed assets (short-term assets/short-term liabilities) and equity/liabilities (short-term assets/sales). Short-term financial burden is proxies by financial expenses/sales. Cash-flow is measured by cash-flow/assets.

[10] As it is clear from the table the scoring system implies that recent scores matter more.

[11] According to the FC guidelines, the additional assessment is referred only to cash-flow requirements for Type-2 firms. As for Type-1 firms, the additional assessment is an in-depth analysis of the economic and financial situation of the firm.

the ultimate approval or rejection. Rejection, however, has been a rare event. Figure 2 shows the numbers of requests received by the FG by year and type of final decision.

[Table 1]

[Fig. 2]

4. The data sources

Thanks to courtesy the Italian Ministry of Economic Development, we have access to the FG dataset. It provides us with detailed information on all the requests of guarantees received by the FC from 2005 to 2012. The dataset does not cover Type-0 firms. This happens because the software that calculates eligibility is run at the bank level. When the bank official finds out that the firm is not eligible (i.e., the firm's lagged balance-sheet observable are poor) the application is not sent to the FC headquarters. Therefore, the firm is not included in the FC dataset. This problem of missing data poses empirical challenges that will be tackled in Sect. 5.

Limited to Type-1 and Type-2 firms the FC dataset includes, among others, the fiscal identifiers for the firms, the date of guarantee approval by the FC and that of the provision of finance by the bank, and the respective amounts of the loan and the guarantee. As for the information on the assignment mechanism, which are crucial to solve the empirical challenges posed by the absence of Type-0 firms in the dataset, we know (for a subset of firms) the categories (A, B, and C) of the yearly score. We are also able to replicate the algorithm that derives the categories from the four balance-sheet variables. We do not have, however, info on the balance-sheet variables used at the bank level to enquire the software (therefore, we need to add balance-sheet information from other sources;see: Sect. 5.1).

We make use of two additional dataset. To collect balance-sheet information we take the CERVED archive. This dataset provides financial accounts for the universe of Italian firms that have the legal structure of limited liability corporations. The use of these data implies that our estimation sample excludes private partnerships and sole proprietorships, which are widespread legal structures for the very small firms.[12]

The second dataset is the Credit Register. This archive, set up for surveillance purposes, is confidential and available only to the staff of the Bank of Italy. The Credit Register collects data at the firm level on financial variables, such as loans, either granted or disbursed by banks, bad loans and interest rates. Only the loans exceeding a threshold of €30,000 are included in the dataset. Thus, the use of these data implies that our estimation sample fails to include the very small firms, which might borrow for amounts below the threshold.

Our estimation sample merges the FG dataset with the CERVED and Credit Register information.

[12] CERVED provides two sets of data. The first refers to classified financial statements; that is, the balance sheets of the firms processed by the CERVED to ensure accounting consistency overtime and across-firms. The second refers to non-classified financial accounts. We use this second source of data, which are in principle more similar than the other to the actual balance sheets used by the bank at the time of the application.

5. The identification strategy and the estimation sample

We first (Sect. 5.1) describe our identification set-up and then (5.2) provide evidence that substantiate the empirical strategy.

5.1 A fuzzy RDD, with two-way noncompliance

Our identification strategy exploits the features of the mechanism that assigns the eligibility explained in Sect. 3. Note that: a) eligibility is awarded through a continuous forcing variable, the score; b) under a certain score, the threshold, no eligibility is awarded (for instance, compare the BB raw with BC one in fig. 2: if a firm with B in year t-2 fails to reach by an arbitrary small amount a value of B in year t-1, it is not eligible); and c) above the threshold, a firm is eligible but not necessarily approved (approval depends on the additional assessment).

It is important to note that if we had in the FG dataset the data on Type-0 firms, then we could identify the impact of the scheme at the threshold with a F-RDD that allows for one-way non-compliance (see: Lee and Lemieux, 2010). Below the threshold, the lack of eligibility implies the unfeasibility of receiving the treatment. Above the threshold, however, the treatment status does not necessarily follow the eligibility status, because some eligible units are not treated (noncompliant units). In this set-up, the probability of treatment does not jump from 0 to 1 when the forcing variable crosses the threshold: the jump is smaller than 1. Moreover, the jump in the relationship between the outcomes and the forcing variable at the threshold is defined as the ITT (intention to treat). The LATE (local average treatment effect) is recovered by dividing the ITT by the fraction of units induced to take-up the treatment at the threshold. It is also essential to note that under these circumstances, the inference will refer to the *applying firms*. That is, to the subsample of SMEs that have shown interest in the FG by applying for the scheme (see: Imbens and Angrist, 1994).

[Fig. 3]

As explained in Sect. 4, this route is precluded: the FG dataset does not collect Type-0 (non-eligible). Note also that we have to face an additional data obstacle: we do not have the actual balance-sheet indicators used by the banks as inputs of the software that calculates eligibility. We have to rely on our external source of data (CERVED non-classified archive; see: Sect. 4) for the financial accounts of the firms.

The solution we have envisaged for the missing data problem is made of three steps.

Step 1. We use the external source of balance sheets to mimic the FC scoring system for Type-1 and Type-2 firms; that is, for the firms guaranteed by the FC. Based on CERVED data, we are able to successfully predict the eligibility status for 99.7% of the firms in the FG dataset. That is, we fail to predict eligibility for 0.3% of them. While this percentage of failures might be related to the fact that the data used by the bank officers might be different from the CERVED data we use in the replication, we cannot rule out some sorts of cheating/manipulation.

Step 2. We augment our dataset by recovering from CERVED the Type-0 firms (non-eligible firms). We take all the firms potentially eligible (SMEs of the sectors covered under the scheme) that in terms of their FC scoring system, as replicated by ourselves, would classify as Type-0 firms. These firms would have not gained eligibility, in the case they had been interested in the public guarantee. It is important to note that at this stage our dataset is unbalanced with respect to the firm willingness to enter the scheme. Type-0 firms, recovered form CERVED, include also firms that would not have applied anyway; on the other hand, Type-1 and Type-2 firms are taken from the FC dataset; that is, they are those that applied for the scheme.

Step 3. We augment our dataset by recovering from CERVED all the eligible (Type-1 and Type-2) non-applying firms. This step solves the unbalancing problem.

As a result of the above steps, we have three sources of non-compliance in our RDD set-up. First, there are eligible and applying firms, rejected by the FG following the additional assessment. Second, there are eligible non-applying firms, which are recovered from CERVED (Step 3, above). Third, there are non-eligible firms that applied and were treated (Step 1 and Step 2, above) as we fail to replicate eligibility for 100% of the cases. Under these circumstances, both the ITT and the ATT are identified with a F-RDD set-up, which allows for two-way noncompliance. Our inference will refer to the *universe* of Italy's SMEs covered under the scheme, either interested or not interested in the FC.

5.2 Sample details and balancing properties.

We start from the merged FG-CERVED-Credit Register sample (described in Sect. 4) and augment it (as explained in Sect. 5.1) with non-eligible and eligible non-applying firms. The estimation sample focuses on the functioning of the FG during the period 2005-2010. We make use of pre-intervention data, to assess the suitability of our comparison groups, and post-intervention (2010-2012) data, to have a time-window that allows the impact to materialize. Observations are collapsed by the year in which the guarantee has been received. The time structure of the control units replicates that of applying firms. Appendix 1 describes the details of the sample construction. The estimation sample includes about 84,000 manufacturing and service SMEs. Appendix 2 provides the description of the variables used throughout the paper.

Figure 3 describes the fraction of firms in our sample that receive the treatment. It illustrates well the two-way non-compliance that we have in our set-up. Below the cutoff the fraction is small, but it is not zero. At the threshold, there is a sizable jump, which however is smaller than one. Note also that the fraction of treated units first increases monotonically moving further away from the cutoff. For a sufficiently high score, however, the fraction starts to decrease. This finding is explained by the fact that firms with high scores have very good lagged balance-sheet observables; therefore, they are unlikely to be rationed. Since there are non-negligible application costs (see Sect. 3), for these firms the FC guarantee does not pay out.

Table 2 illustrates the composition of our estimation sample with respect to the FG types and the applying/non-applying status. Below the threshold, our sample includes 4,779 SMEs; 41 of them have applied for the scheme. Above the threshold, we have 21,251 and 57,563 firms, for Type-1 and Type-2 respectively. The fraction of applying firms is 12% and 17% for the two groups, respectively. We have 12,252 treated firms in our sample. Note that for these firms the share of the loan guaranteed with the FC collateral is on average equal to 55%, with a small standard deviation (15%).[13]

Because the sample is collapsed overtime, treated firms might have received the guarantees during either pre-crisis years (2005-2007) or after the crisis broke out, in 2008. As the FG operations boomed after the inception of the crisis, our sample reflects predominantly firms that received the public collateral starting from 2008 (about 65% of the treated in the estimation sample).

[Table 2]

As it is well known, the RDD is deemed preferable to other non-experimental methods because if the units of the analysis (in our case the Italian SMEs) are unable to manipulate precisely the forcing variable (the distance from the border), the variation around the border (changes in the eligibility score) is randomized as though the firms had been randomly drawn on just one or other side of the boundary (Lee, 2008). One implication of the local randomized result is that the empirical validity of the RDD can be tested. If the variation in the eligibility near the edge is approximately randomized, it follows that all "baseline covariates" – those variables determined prior to the start of the policy – should have about the same distribution on the two sides of the border.

Table 3 presents a test for the absence of discontinuity in baseline characteristics around the boundaries that substantiates the empirical strategy. We run RDD regressions (of the type of those used to estimate the impact of the scheme on the outcomes, which are described in the next Sect.) using as dependent variables those factors that we suspect could be driving the results. If no effect is detected then that variable can be considered as controlled for in the RDD exercise. We focus on a large number of characteristics that should capture most of the firm heterogeneity, using both parametric (Panel A) and non-parametric (Panel B) estimation methods. The table shows the estimates for both the ITT and the LATE. Overall, we find good balancing properties for the baseline covariates. Both parametric and non-parametric estimates suggest that no jump occurs at the threshold for recent pre-treatment (2-year) trends of bank debt (both granted and disbursed) and probability of bad loans. Similarly, no discontinuity is observed for firm size (proxies by sales) and for the variables that capture the strength of the bank-firm relationships (such as the share of the main bank in total loans and the Hehrfindahl index). A less favorable evidence is found for the pre-treatment trend of investments: both parametric and non-parametric estimations suggest that eligible firms invested less than non-eligible ones in the two years ahead of the request. Note that, as explained by Lee and Lemieux (2010), some of the differences in covariates across the cutoff might be statistically significant by random chance. To check for this possibility, we combine the multiple tests into a single test statistic (a stacked test) that measures whether data are broadly consistent with the random treatment hypothesis around the border. A χ^2 test for discontinuity gaps in all the equations equal to zero is always supported by data.

[13] The reduced variability across firms of the percentage of coverage reassures that our estimates are not driven by relevant non-linearity.

[Table 3]

6. The results at the eligibility threshold

In this Sect. we document the estimates of the ITT and the LATE for a number of outcomes measured at firm level over the two years after the extension of the FG guarantee. Our main findings refer to: bank loans (both disbursed and granted), interest rates, bad loans, investments and sales. It is important to note that, since we are interesting on the total effectiveness of the scheme, our measures for credit availability, interest rates and bad loans reflect the firm position *vis a vis* the banking system as a whole. Therefore, they include the credit relations that a firm might have with banks different from the one that provided the guaranteed loan. For instance, if these banks provide additional (non-guaranteed) loans because they are happy that the firm successfully applied for the FG, these loans will be computed as part of the treatment.

Results come from two different estimation methods. Parametric estimates reflect a polynomial specification with the degree determined by the AIC test. Non-parametric results are calculated by using the optimal bandwidth procedure suggested by Imbens and Kalyanaraman (2009), with a rectangular kernel. Figures 4-9 illustrate the canonical RDD graph for each outcome. In the figures, the jump at the threshold corresponds to the ITT. Each graph depicts both the non-parametric estimates (dashed line), with the corresponding 95% confidence interval, and the parametric estimates (solid line).

[Figg. 4-9]

The econometric results are displayed in Table 4. We find (Panel A and Panel B) that - when evaluated at the eligibility threshold - the guarantee provided by the FC has a significant impact on the availability of credit for the universe of Italy's SMEs. The parametrically estimated ITT is equal to 5% of the (two-year cumulative) growth rates in credit flows, for both loans disbursed and granted. When estimated with non-parametric methods the ITT lowers to 3%, remaining highly significant. Parametric estimates of the LATE suggest that for the treated firms the two-year cumulative growth rate in loans (both disbursed and granted) increases of about 50%. Non-parametric estimates signal that the impact of the scheme on the treated might be somehow lower, however close to 40%. The first-stage F-tests reassure on the role of a weak-instrument problem. Note also that the impact estimated for granted loans is very similar with that measured for disbursed loans. This is consistent with the idea that during the credit crunch all the financing made available by the banks was drained by the firms.

Panel C describes the results we obtain by using as outcome the two-year variations in the interest rates charged by the banks to the SMEs. At the cutoff our estimates suggest that the scheme does not have an impact on the cost of credit.

Panel D turns to riskiness. As mentioned in Sect. 1, PGSs might have unwanted consequences. The guarantee provided by the public sector increases the share of the loan for which neither banks nor firms are liable. This moral hazard problem, which might affect firm effort and bank monitoring, might increase the likelihood of bad loans. Moreover, there could be opportunistic behaviors: for instance, banks might be more inclined to signal as non-performing credits the loans for which it is easier to collect the collateral. Panel D shows that the probability (calculated over two years) that a firm enters the bad loans significantly increases because of the FG (of about 15% parametrically, much more when estimated non-parametrically). The estimated ITT, which represents the increase of firms with bad loans for the universe of SMEs attributable to the scheme is estimated to be equal to 1.5% and 3.5%, respectively.

Finally, we check whether the guarantees have effects on some measures of firm performance. We consider investments (Panel E), which should be positively affected if they are financially constrained, and sales (Panel F), which should rise if business growth is limited by the availability of short-term capital. In the first case, we fail to find any impact. In the second case, we find a positive effect of the scheme when parametric methods are used, which is not confirmed by non-parametric analysis.[14]

[Table 4]

Table 5 provides the estimates of the impact of the program on the main outcomes for a sample that includes only manufacturing firms (about 30,000 of them). For this sample, unobserved firm heterogeneity should be reduced, because both the greater similarity in production and the fact that tradables are less affected by the idiosyncratic conditions of the local markets. For instance, in the estimation time-window (2005-2010) the economic crises was mainly export-led: therefore, manufacturing firms have likely been more homogeneously hit. Overall, the estimates at the threshold we obtain with this sample are very similar to those described in Table 4. As for the credit flows, the ITT is estimated to be about 6% (very stable across estimation methods). The absence of impact for the interest rates is confirmed. The ITT impact on bad loans is estimated to be around 4%. No effect is detected as for investments and sales.

[Table 5]

Our estimates so far point to a positive impact on loans that does not percolate to measures of firm "real" activity, as proxied by investment and sales. In a situation in which, as a result of the crisis, firms were cutting investment plans and struggling with short-term financing needs, it is likely that the extra-finance made available by the FG was devoted to tackle liquidity difficulties. To shed light on these aspects, Table 6 documents the impact of the FG on some additional balance-sheet outcomes. Our results suggest that the extra-credit was mainly used to finance inventory accumulation and to extend trade credit to customers (Panel A). We fail to find an impact on short-term liquid assets (Panel B) and on commercial debt (Panel C). These latter findings seem reasonable in the context of a liquidity squeeze, in which short-term finance is a scarce resource.

[14] We also investigated (results not reported) the extent to which our results could be affected by: (i) the circumstance that in some cases the loan was intermediated by mutual guarantee institutions, instead that regular intermediaries (Sect. 3); (ii) the differential exposure of the firms in the sample to the crisis, as some of them received the guarantee before the Lehman collapse (Sect. 5.2); (iii) the unbalancing of pre-treatment investments (Sect. 5.2). To this aim, in the parametric specifications we inserted: (i) a dummy for the firms that received the extra-collateral trough a mutual guarantee institutions; (ii) a dummy for the firms that received the treatment after October 2008; and (iii) a control for the pre-treatment investment flows. In all the cases results remain undisputed.

Finally, there is no impact on the leverage of the firm (Panel D), which in turn might have had an effect on bad loans, irrespective of moral hazard.

[Table 6]

7. Inference far from the threshold

As it is well known, the estimates of what happens at the threshold might be considered as only partially informative. The impact of the treatment on infra-marginal firms is also of interest, but the regression discontinuity framework is less suitable to provide such estimates (see, Campbell and Stanley, 1963). In our case, identification away of the cutoff is particularly interesting: policy makers might want to know what might have happened if firms with eligibility scores below the threshold would have gained access to the scheme; by the same token, they might wonder whether the public money spent for the firms that easily pass the admission threshold carry with it deadweight losses.

In this Sect. we make use of the Angrist and Rokkanen (2012) conditional independence assumption (CIA) to gain some insights about the impact of the program on infra-marginal (away-from-the cutoff) firms. The idea of the CIA is to break the relationship between treatment status and outcomes by means of a vector of covariates such that, conditional on it, outcomes are mean independent of the running variable. The vector of covariates is then used to identify counterfactual values for the outcome variables of interest.

To ensure that the relationship between the running variable and the outcomes has been removed, we document the results from CIA tests. Table 7 focuses on three outcomes (disbursed loans, interest rates, and probability of bad loans).[15] It shows the results from three estimation windows of various width: 0.3, 0.6 and 0.9 normalized scores on the two sides of the eligibility cutoff. CIA tests come from models that control for balance-sheet variables measured in the year before those used to calculate eligibility (t-3, in terms of the Table 1), along with sector and location dummies. The results offer only little evidence of CIA violations (we obtain only one rejection referring to bad loans, above the threshold, with the width=0.3). Note also that the bandwidth of [-0.9,0.9] is the largest one for which the CIA is satisfied. Therefore, we are unable to provide far-from-the-threshold inference for firms with an eligibility score outside the [-0.9,0.9] interval. Notice also that the interval for which the CIA assumption is maintained is not negligible: 20% of the firms in our sample fall into it.

[Table 7]

Figures 10-12 illustrates CIA-based estimates by plotting linear reweighting (Kline, 2011) estimates of the ITT for all values of the eligibility score in the [-0.9,0.9] interval. For each outcome, the figures depict both the fitted values for observed outcomes and the CIA-based extrapolations. The estimated impact of the scheme for infra-marginal firms is illustrated by the vertical difference between the two series.

[15] However, applying the CIA strategy to the other outcomes we obtain the estimates at the threshold also hold for infra-marginal firms (within the bandwidth of the cutoff for which the CIA is maintained).

As for disbursed loans (Figure 10) we find a remarkably stable increase in the ITT away from the cutoff. These findings amount to say that a lowering of the eligibility criteria (Panel A) would increase the effectiveness of the program in fostering bank loans; at the same time, they highlight that the scheme has a positive impact on borrowing also for firms that easily pass the admission threshold (Panel B). Regarding the interest rates (Figure 11), our results confirm that below the threshold the effect remains undistinguishable from zero (Panel A); however, they suggest that above the threshold (Panel B) the impact of the scheme could be more beneficial for the firms, as the cost of credit decreases. Finally, the positive impact of the FC on bad loans (Figure 12) remains constant within a certain range of the [-0.9,0.9] interval. Towards the end of the interval, on both sides (Panels A and B), the impact tends to vanish. These results suggest that the effect on non-performing loans induced by the scheme is not a relevant problem for very good firms (which want to avoid to be signaled as bad borrower) and very bad firms (which may have repayment problems irrespective of the public guarantees).

[Figg. 10-12]

8. Conclusions

By exploiting regression discontinuity techniques, this paper evaluates the impact of the Italian scheme *Fondo di Garanzia* on a number of firm-level outcomes, referring to the credit and the good market. The analysis highlights that the scheme has been quite effective in enhancing credit flows. The expected impact of the scheme on the cost of credit, however, seems to materialize only for the firms that easily pass the admission cutoff. The program increases the likelihood that a firm is unable to pay back its loans. Our results suggest that the impact of the public guarantees on investments and sales is scant: the extra-finance made available by the scheme has been mostly used to finance working capital, such as inventories and trade credit.

As for the policy implications, our study recommends that having a less severe award scheme might be a step in the right direction, insofar maximizing private financing to SMEs is the main goal of the policy makers.[16] At the same time, the impact of the scheme on the probability of entering the bad loans is an important finding that should be taken into account in assessing the fiscal cost of the scheme (which is normally measured with reference to the probability of default prevailing on average in the population of eligible firms; therefore, without considering the possibility that the likelihood of default increases because of the treatment).

An important caveat applies to our research. The study exploits a severe episode of crisis. The contraction in economic activity and credit flows during the period 2005-2010 was the greatest since the Second World War (see: Jenkins et al., 2011). As our findings are derived for a period of exceptional circumstances, they cannot be easily extrapolated for less extreme economic conditions.

We have measured the aggregate impact of the scheme on both the treated firms and the population of SMEs. We have not investigated what happened within the bank-firm relationship because of the availability of the scheme. For instance, behind the unfavorable impact on bad-loans

[16] To some extent this policy suggestion has already been taken, as the admission criteria for the FG were relaxed in March 2014.

there could be more than one story (moral hazard, opportunistic behavior etc.). Also, the bargaining position of the firm in the credit market might be affected. For instance, the bank that assists the firm *vis a vis* the FG might gain informational advantages that ensures a longer relationship (capture). At the same time, the firm that has been assessed from the FG might use the good signal to find easier access to credit elsewhere. These aspects, and others, are interesting topic for future research, which we will soon tackle.

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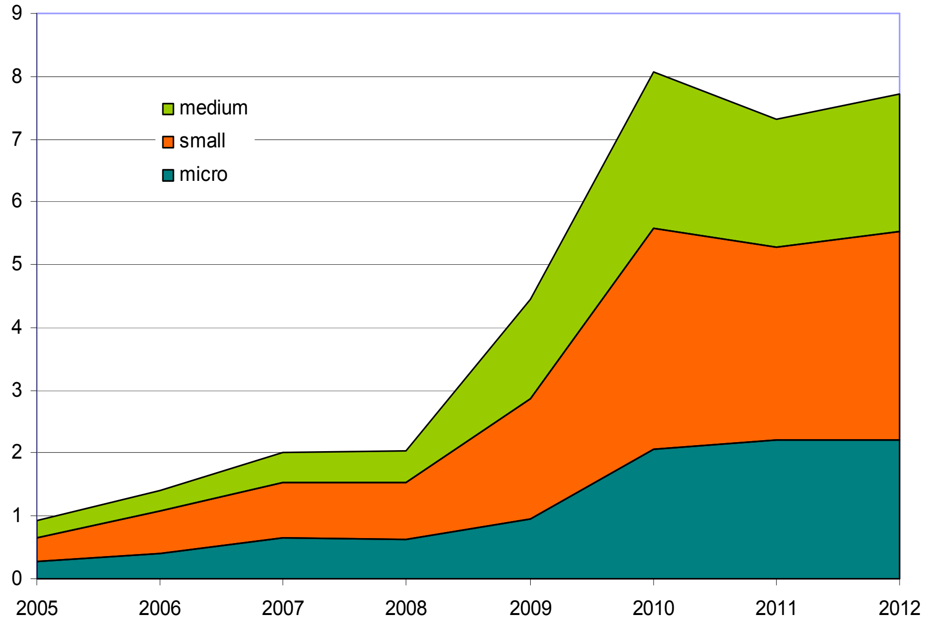
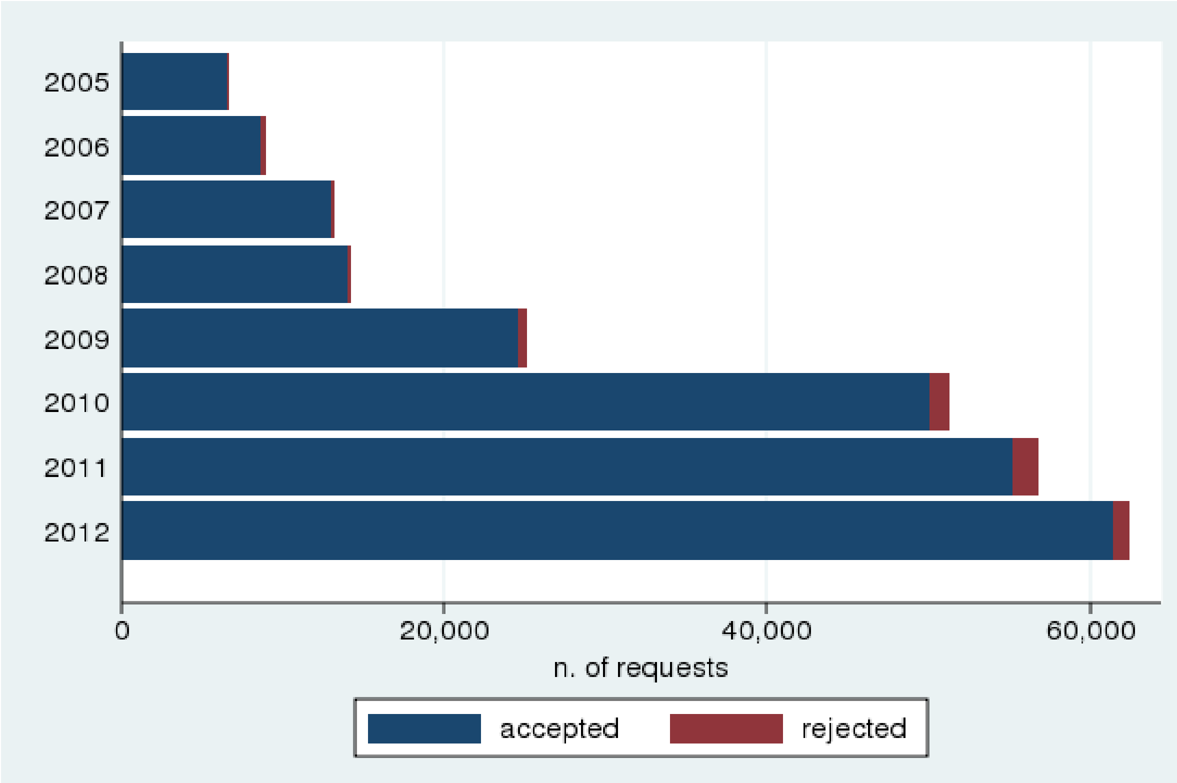
Figure 1. Bank loans to SMEs guaranteed by the FC



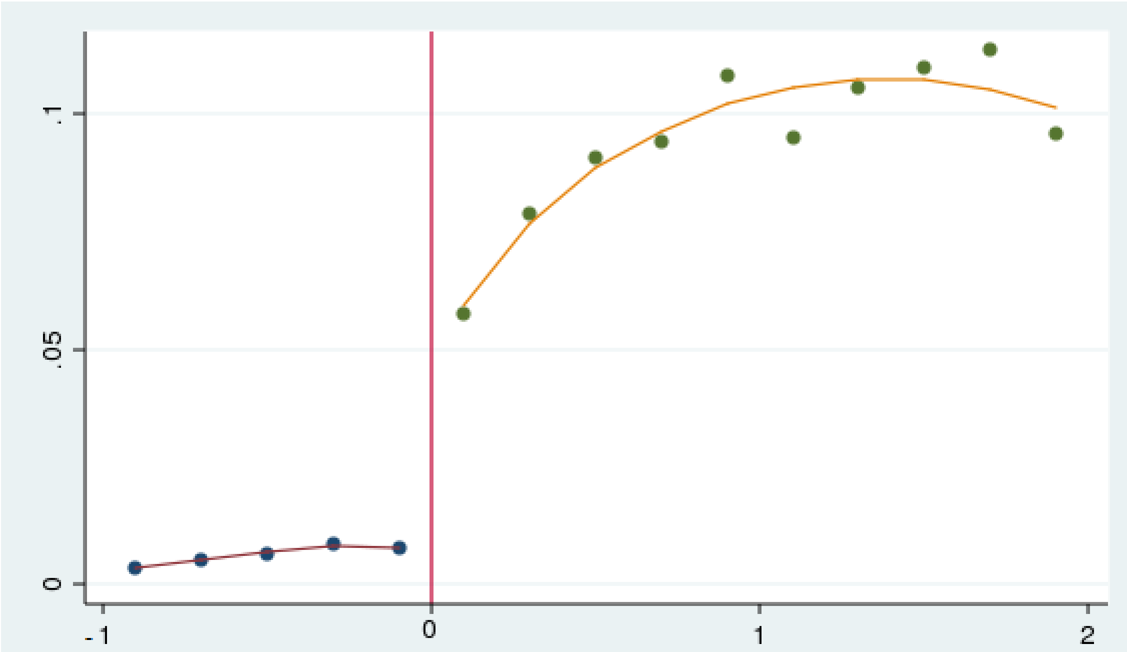
Notes: € billion, outstanding amounts. Source: FC dataset.

Figure 2. Requests approved and rejected, by year



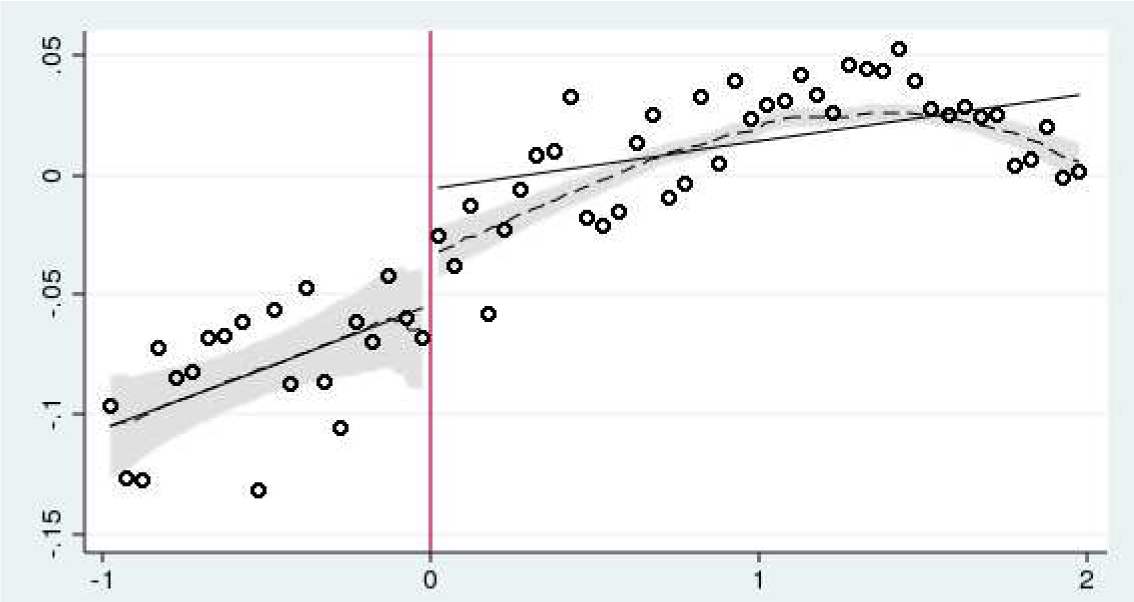
Notes: number of applications received by the FC. Source: FC dataset.

Figure 3. Probability of receiving the treatment



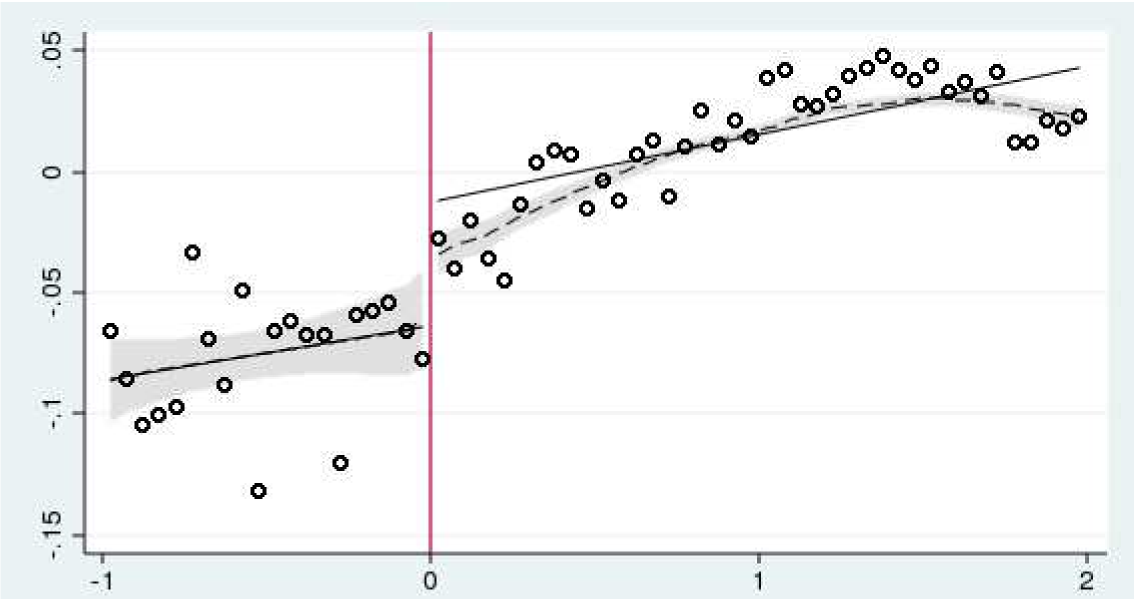
Notes: The threshold is normalized at the value of 0. Source: our own calculations.

Figure 4. Disbursed loans



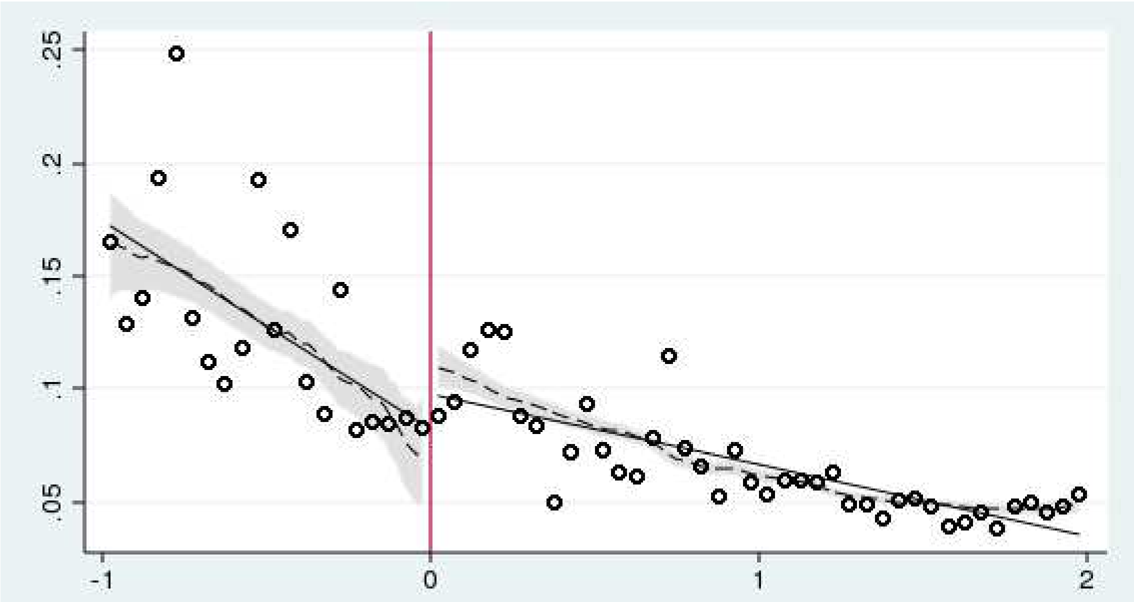
Notes: graphical representation of RDD. Circles stand for averages of the outcome (two-years variation in logarithm of disbursed loans) computed at 0.05 bins (for a total of 60 bins). Solid line (dashed line) stands for linear regression (local linear regression). The local linear regression is computed with the Imbens-Kalyanaraman (2012) optimal bandwidth with rectangular Kernel. The shaded area represents the 95% confidence interval for the local linear regression.

Figure 5. Granted loans



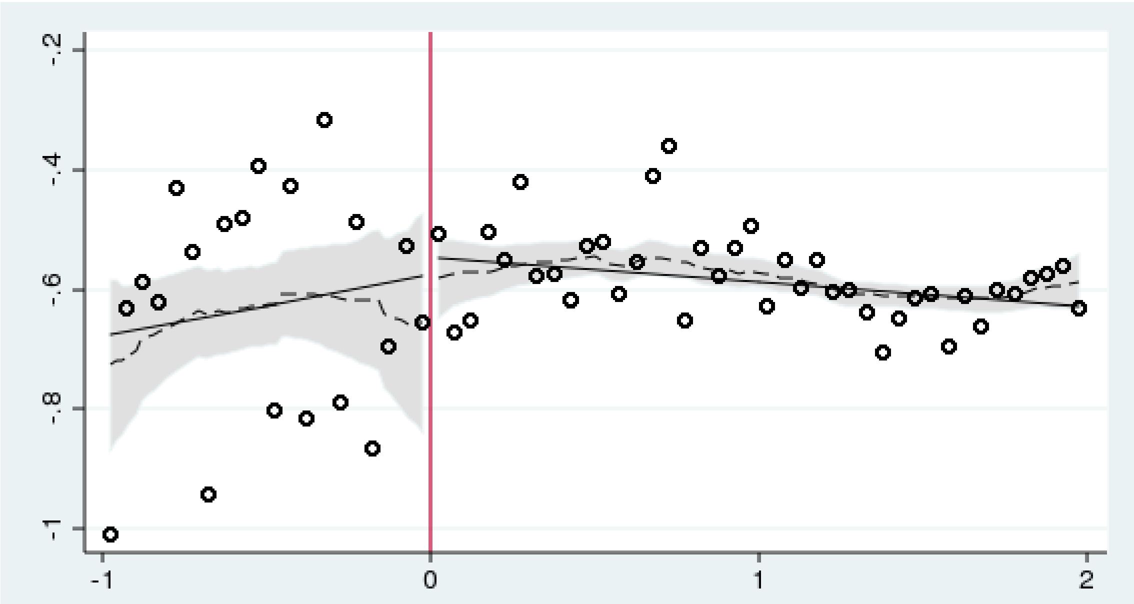
Notes: graphical representation of RDD. Circles stand for averages of the outcome (two-years variation in logarithm of granted loans) computed at 0.05 bins (for a total of 60 bins). Solid line (dashed line) stands for linear regression (local linear regression). The local linear regression is computed with the Imbens-Kalyanaraman (2012) optimal bandwidth with rectangular Kernel. The shaded area represents the 95% confidence interval for the local linear regression.

Figure 6. Probability of bad loans



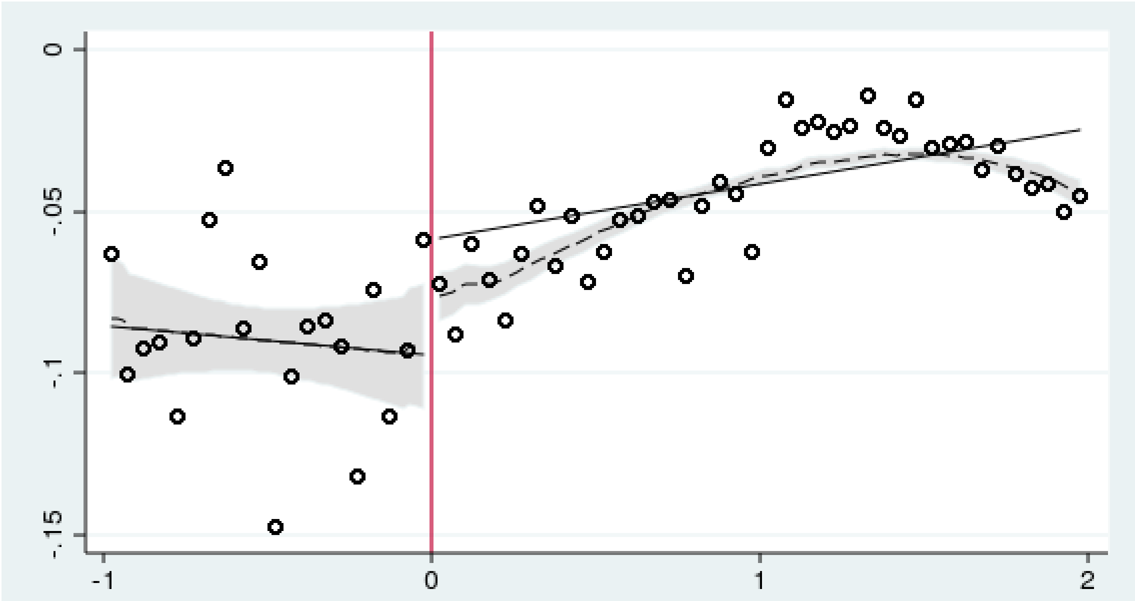
Notes: graphical representation of RDD. Circles stand for averages of the outcome (two-years variation in probability of having at least one bad loan) computed at 0.05 bins (for a total of 60 bins). Solid line (dashed line) stands for linear regression (local linear regression). The local linear regression is computed with the Imbens-Kalyanaraman (2012) optimal bandwidth with rectangular Kernel. The shaded area represents the 95% confidence interval for the local linear regression.

Figure 7. Interest rates



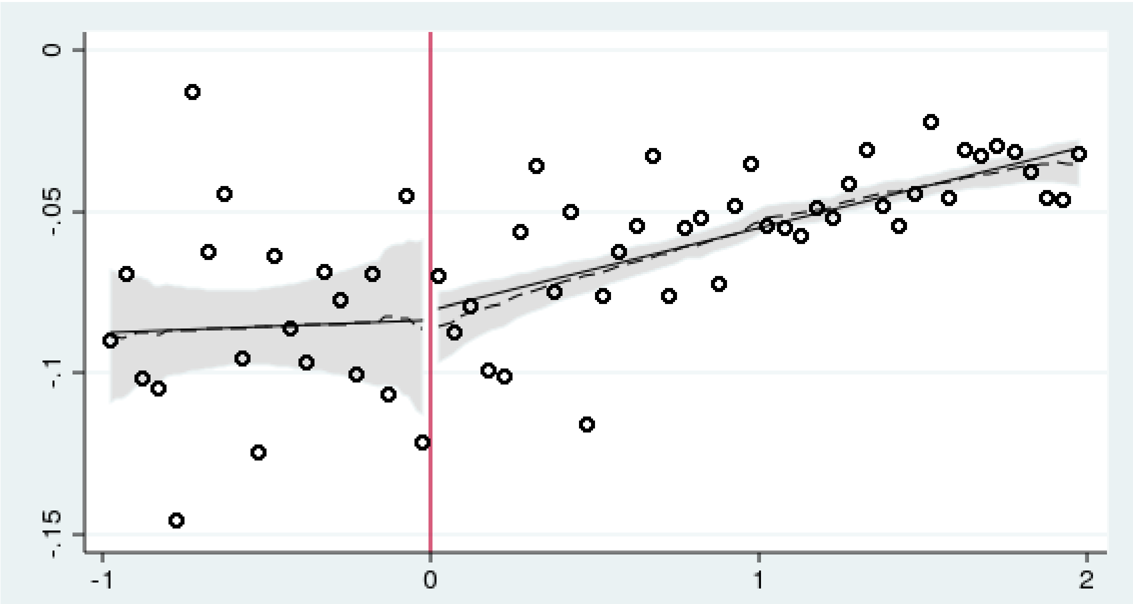
Notes: graphical representation of RDD. Circles stand for averages of the outcome (two-years variation in average interest rate) computed at 0.05 bins (for a total of 60 bins). Solid line (dashed line) stands for linear regression (local linear regression). The local linear regression is computed with the Imbens-Kalyanaraman (2012) optimal bandwidth with rectangular Kernel. The shaded area represents the 95% confidence interval for the local linear regression.

Figure 8. Sales



Notes: graphical representation of RDD. Circles stand for averages of the outcome (two-years variation in logarithm of total sales) computed at 0.05 bins (for a total of 60 bins). Solid line (dashed line) stands for linear regression (local linear regression). The local linear regression is computed with the Imbens-Kalyanaraman (2012) optimal bandwidth with rectangular Kernel. The shaded area represents the 95% confidence interval for the local linear regression.

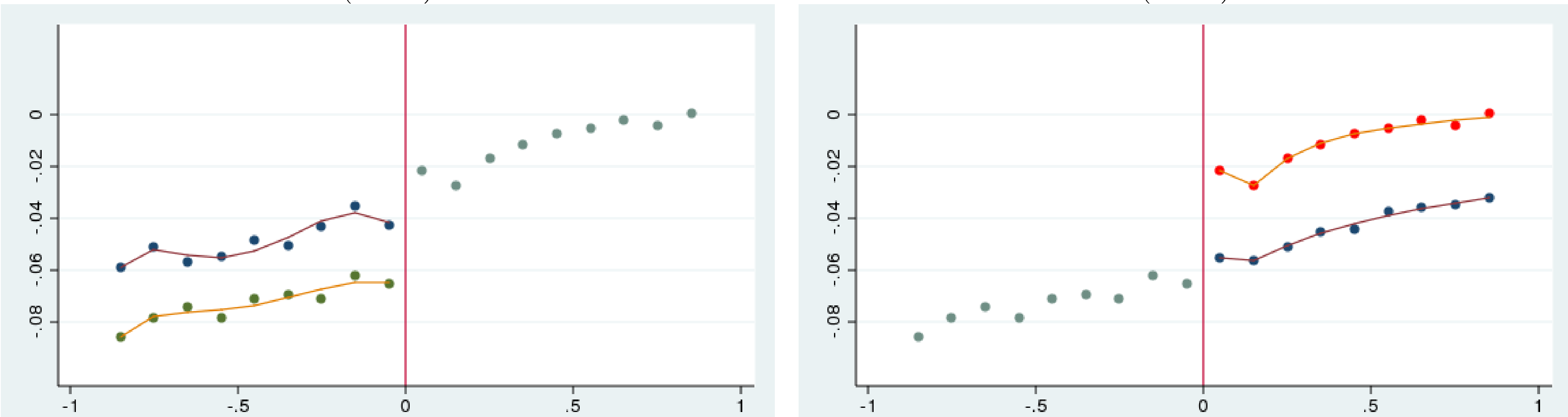
Figure 9. Investments



Notes: graphical representation of RDD. Circles stand for averages of the outcome (two-years variation in investments) computed at 0.05 bins (for a total of 60 bins). Solid line (dashed line) stands for linear regression (local linear regression). The local linear regression is computed with the Imbens-Kalyanaraman (2012) optimal bandwidth with rectangular Kernel. The shaded area represents the 95% confidence interval for the local linear regression.

Figure 10. CIA-based estimates, Disbursed loans

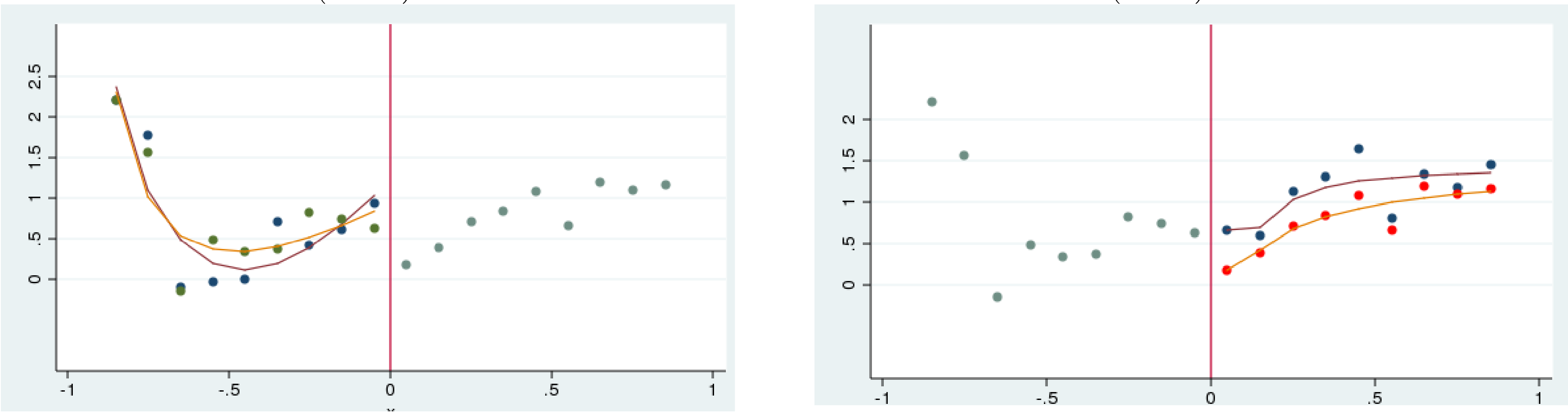
(Panel A) (Panel B)



Notes: graphical representation of CIA-based estimates (Angrist and Rokkanen, 2012). The extrapolations are computed through Kline's linear reweighting procedure (Kline, 2011). In Panel A, to the left of the cutoff blue dots represent the CIA-based extrapolations while the green dots represent the fitted values for observed outcomes. In Panel B, to right of the cutoff blue dots represent the CIA-based extrapolations while red dots are the fitted values for observed outcomes.

Figure 11. CIA-based estimates, Interest rate

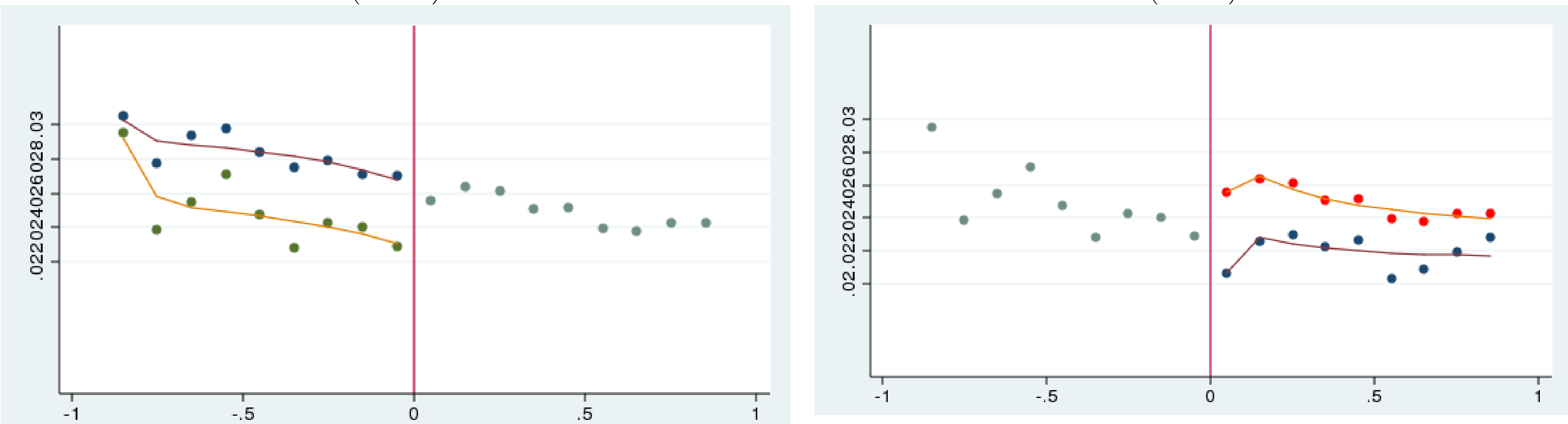
(Panel A) (Panel B)



Notes: graphical representation of CIA-based estimates (Angrist and Rokkanen, 2012). The extrapolations are computed through Kline's linear reweighting procedure (Kline, 2011). In Panel A, to the left of the cutoff blue dots represent the CIA-based extrapolations while the green dots represent the fitted values for observed outcomes. In Panel B, to right of the cutoff blue dots represent the CIA-based extrapolations while red dots are the fitted values for observed outcomes.

Figure 12. CIA-based estimates, Probability of bad loans

(Panel A) (Panel B)



Notes: graphical representation of CIA-based estimates (Angrist and Rokkanen, 2012). The extrapolations are computed through Kline's linear reweighting procedure (Kline, 2011). In Panel A, to the left of the cutoff blue dots represent the CIA-based extrapolations while the green dots represent the fitted values for observed outcomes. In Panel B, to right of the cutoff blue dots represent the CIA-based extrapolations while red dots are the fitted values for observed outcomes.

Table 1. The FC scoring system: yearly categories and types

Year t-2 Category	Year t-1 Category	Types
A	A	Type-1
B	A	
A	B	Type-2
B	B	
C	B	
C	A	
A	C	
B	C	Type-3
C	C	

Source: FC official guidelines.

Table 2. Composition of the estimation sample

Type	Non Applying		Applying		Total
0	4,738	99.1%	41	0.9%	4,779
1	18,781	88.4%	2,470	11.6%	21,251
2	47,822	83.1%	9,741	16.9%	57563
Total	71,341	85.3%	12,252	14.7%	83,593

Notes: the details of the sample construction are provided in the text. Source: our own calculations.

Table 3. Balancing properties

Baseline covariate:	ITT	LATE
	A. Parametric analysis	
Δ Sales	0.0262***	0.289**
	(0.00996)	(0.116)
Δ Investments	-0.0657***	-0.702***
	(0.0172)	(0.213)
Δ Disbursed loans	-0.0172	-0.188
	(0.0172)	(0.193)
Δ Granted loans	0.0125	0.138
	(0.0129)	(0.141)
Δ Probability of bad loan	0.00917	0.118
	(0.00747)	(0.0994)
Herfindahl index	0.396	3.728
	(0.863)	(8.231)
Bank share	-0.0385	-0.359
	(0.824)	(7.679)
Sales	0.0367	0.745
	(0.0376)	(0.745)
	B. Non-parametric analysis	
Δ Sales	-0.00600	-0.156
	(0.0110)	(0.287)
Δ Investments	-0.0572***	-1.181***
	(0.0160)	(0.344)
Δ Disbursed loans	-0.0176	-0.303
	(0.0205)	(0.354)
Δ Granted loans	-0.0311	-0.577
	(0.0215)	(0.412)
Δ Probability of bad loan	0.00545	0.113
	(0.0106)	(0.221)
Herfindahl index	0.311	4.867
	(0.916)	(14.40)
Bank share	-1.223	-21.07
	-1.059	(18.20)
Sales	-0.0376	-1.148
	(0.0455)	(1.417)

Notes: Parametric estimates with polynomial degree determined by AIC test. Optimal bandwidth for non-parametric estimates: Imbens and Kalyanaraman (2009) procedure, rectangular Kernel. Δ = 2 years variation computed in the pre-treatment period. Standard errors in brackets.

Table 4. The impact of FC on the main outcomes

Parametric analysis			Non-parametric analysis	
ITT (1)	LATE (2)	F-test (3)	ITT (4)	LATE (5)
A. Granted loans				
0.052***	0.513***	60.88	0.027**	0.387**
(0.011)	(0.124)		(0.019)	(0.172)
N=72300				
AIC recommended polynomial degree: 1				
B. Disbursed loans				
0.049***	0.497***	62.44	0.0278*	0.407*
(0.015)	(0.160)		(0.0154)	(0.227)
N=68109				
AIC recommended polynomial degree: 1				
C. Interest rate				
-0.009	-0.082	59.17	0.078	1.106
(0.076)	(0.672)		(0.110)	(1.569)
N=61752				
AIC recommended polynomial degree: 1				
D. Probability of bad loans				
0.015*	0.167*	116.8	0.035**	0.544**
(0.008)	(0.088)		(0.017)	(0.266)
N=83593				
AIC recommended polynomial degree: 1				
E. Investments				
0.008	0.008	55.12	0.018	0.018
(0.162)	(0.162)		(0.324)	(0.324)
N= 63813				
AIC recommended polynomial degree: 1				
F. Sales				
0.040***	0.421***	48.22	0.0157	0.330
(0.011)	(0.128)		(0.0114)	(0.242)
N= 64233				
AIC recommended polynomial degree: 1				

Notes: columns (1) to (3) report parametric estimates with polynomial degree determined by AIC test. Columns (4) and (5) report non-parametric estimates. The optimal bandwidth for non-parametric estimates has been retrieved by Imbens and Kalyanaraman (2009) procedure with rectangular Kernel. Outliers below 5 or above 95 percentile were dropped. Standard errors in brackets.

Table 5. The impact of FC on the main outcomes, manufacturing firms

Parametric analysis			Non-parametric analysis	
ITT (1)	LATE (2)	F-test (3)	ITT (4)	LATE (5)
A. Granted loans				
0.0573**	0.254**	88.84	0.006*	0.564*
(0.0269)	(0.119)		(0.0355)	(0.340)
N=25420				
AIC recommended polynomial degree: 1				
B. Disbursed loans				
0.0538***	0.227***	103.7	0.0538**	0.533**
(0.0170)	(0.0732)		(0.0247)	(0.257)
N=22718				
AIC recommended polynomial degree: 1				
C. Interest rate				
0.0278	0.0972	79.51	0.288	1.918
(0.146)	(0.510)		(0.235)	(1.591)
N=16540				
AIC recommended polynomial degree: 1				
D. Probability of bad loans				
0.0343***	0.155***	116.8	0.0412*	0.485*
(0.0128)	(0.0593)		(0.0185)	(0.224)
N=29990				
AIC recommended polynomial degree: 1				
E. Investments				
-0.0075	-0.0377	98.29	0.0156	0.229
(0.0223)	(0.112)		(0.027)	(0.399)
N= 29532				
AIC recommended polynomial degree: 1				
F. Sales				
0.007	0.03	88.9	0.001	0.006
(0.017)	(0.087)		(0.018)	(0.237)
N= 29891				
AIC recommended polynomial degree: 1				

Notes: columns (1) to (3) report parametric estimates with polynomial degree determined by AIC test. Columns (4) and (5) report non-parametric estimates. The optimal bandwidth for non-parametric estimates has been retrieved by Imbens and Kalyanaraman (2009) procedure with rectangular Kernel. Outliers below 5 or above 95 percentile were dropped. Standard errors in brackets.

Table 6. The impact of FC on additional outcomes

Parametric analysis			Non-parametric analysis	
ITT (1)	LATE (2)	F-test (3)	ITT (4)	LATE (5)
A. Inventories and accounts receivable				
0.0745***	1.058***	44.06	0.0426*	0.842**
(0.0134)	(0.244)		(0.0169)	(0.343)
N=94381				
AIC recommended polynomial degree: 1				
B. Cash and marketable securities				
0.001	0.0142	36.18	-0.0631	-1.369
(0.0362)	(0.571)		(0.0441)	(0.967)
N=92173				
AIC recommended polynomial degree: 1				
C. Commercial debts				
0.0086	0.131	36.69	-0.0229	-0.525
(0.0177)	(0.269)		(0.0241)	(0.556)
N=90518				
AIC recommended polynomial degree: 1				
D. Leverage				
0.00533	0.0554	51.08	0.00530	0.0844
(0.0071)	(0.0736)		(0.0077)	(0.123)
N=69222				
AIC recommended polynomial degree: 1				

Notes: columns (1) to (3) report parametric estimates with polynomial degree determined by AIC test. Columns (4) and (5) report non-parametric estimates. The optimal bandwidth for non-parametric estimates has been retrieved by Imbens and Kalyanaraman (2009) procedure with rectangular Kernel. Outliers below 5 or above 95 percentile were dropped. Standard errors in brackets.

Table 7. Conditional indipendence test

	Loans disbursed		Probability of bad loans		Interest rate	
Window	*below the threshold*	*above the threshold*	*below the threshold*	*above the threshold*	*below the threshold*	*above the threshold*
0.9	-0.0122	0.0213	-0.00408	-0.00577	-17.01	10.15
	(0.0285)	(0.0154)	(0.0128)	(0.00461)	(46.70)	(8.415)
Obs	2,949	13,326	2,156	10,925	1,974	8,725
0.6	-0.0270	0.0178	-0.000745	-0.00933	-56.22	5.405
	(0.0563)	(0.0265)	(0.0231)	(0.00777)	(91.83)	(5.301)
Obs	1,772	9,612	1,322	7,713	1,175	6,265
0.3	-0.0125	0.0280	-0.0605	0.0406*	-543.2974	16.90
	(0.1613)	(0.0730)	(0.0696)	-0.0237	(402,1)	(13.75)
Obs	5,708	5,708	690	4,458	611	3,718

Notes: Regression based tests of the conditional independence assumption. The table reports the estimated coefficient of the running variable in a regression of each output variable (indicated in columns) controlling also for balance-sheet variables, sector dummies and location dummies. Estimates use only observations below or above the threshold and were computed in the forcing variable window indicated in the first column.

Appendix 1 – Sample construction

Treated firms

The FG dataset reports information on 238.825 requests of guarantees, at the bank-firm-loan level, evaluated between 2005 and 2012 (firms could request the guarantees for more than one loan, to the same bank or different banks, in different years). We focus on the subset of requests that reached the Fund before the 10th of January 2010, because requests channelled after that date follow a different eligibility rule. This leaves us with about 74,000 observations. We exclude roughly 4,000 observations referring to construction, which might have trends of economic activity barely comparable with those of manufacturing and services, and the very few observations in the FG dataset referring to energy, real estate, and agriculture.

After merging such data with CERVED balance-sheet data we end up with about 34,000 observations, at the bank-firm-loan level. Then, the data are collapsed at the firm level. Firms that have been treated in more than one year are excluded. Our final dataset includes about 12,000 observations.

Control firms

As explained in the text, control units have been recovered from the CERVED dataset. We exclude firms belonging to the sectors not covered under the scheme (see: Sect. 3) and the firms belonging to the sectors excluded in the treated group (see above). Since the time dimension of the sample of applying firms has been collapsed, we replicate the same time distribution of the treated for the controls. Therefore, for each year in the interval (2005-2010) the percentage of control firms is the same of that of treated units in the sample. The control sample recovered from CERVED includes about 71,000 firms.

The sample derived by the merging FG data with CERVED data is then further merged with the Credit Register data.

Appendix 2 – List of the variables

- Sales: firm total sales (CERVED)
- Investments: firm fixed assets (CERVED)
- Inventories and accounts receivable: raw materials, work-in-process goods and completely finished goods; owed to a company by a customer for products and services provided on credit (CERVED)
- Cash and marketable securities: cash and very liquid securities (CERVED)
- Commercial debts: long term and short term debts with suppliers (CERVED)
- Leverage: ratio of financial debts over the sum of financial debts and equity (CERVED)
- Disbursed loans: sum of all the loans disbursed in the year to the firm by all the banks with whom it has a financial relationship (Credit Register)

- Granted loans: sum of all the loans that have been granted to the firm in the year by all the banks with whom it has a financial relationship (Credit Register)
- Probability of bad loan: dummy that takes value of 1 if one or more loans to the firm are signalled as non-performing loans by a bank during the year
- Herfindahl index: sum of the squared shares of each bank in terms of disbursed loans to the firm during the year (Credit Register)
- Interest rate: weighted average of the interest rates applied by the banks to the firm, based on disbursed loans (Credit Register)
- Bank share: share of main bank in terms of disbursed loans (Credit Register)