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PUBLIC GUARANTEES ON LOANS TO SMES: AN RDD EVALUATION[*]

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Abstract

The paper evaluates the impact of the guarantees provided by the Italian *Fondo di Garanzia* scheme on access to credit for small and medium enterprises. 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 on 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 banks, and has a positive effect on the likelihood that a firm will become unable to repay its loans. The guaranteed loans were mostly used to finance working capital. Finally, the paper provides inference for firms far from the threshold.

Keywords: credit guarantees, public guarantees, access to credit, SMEs financing.
JEL Classification: L25, O12, G28.

Contents

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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 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 regulatory 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. 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 low initial outlays (Action Institute, 2013). However, these 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 adverse selection and moral hazard because of the limited liability mechanism, increasing the likelihood of bad loans. Under these unfortunate 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 are effective in supporting firms' access to credit is an empirical question. Answers to this question seem to be much needed, as the schemes are gaining attractiveness among policy makers since the start of the crisis (European commission 2013; European Commission and European Investment Bank, 2013).

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

This paper evaluates the effectiveness of the Italian PGS, named *Fondo di Garanzia* (FG).[2] During the crisis, the intervention under the FG has been massive: from 2009 to 2014, €54 billion loans were guaranteed. Our aim is to assess the impact of the public guarantees on SMEs access to credit, in terms of both the amount of loans obtained by firms and the level of interest rates charged by banks. Moreover, we analyze the effects of the program on the firms' probability of default. Finally, we study "second round" effects of the guarantees in terms of spurring investments, growth of sales or financing working capital; these effects depend on how the beneficiaries SMEs make use of the financial resources obtained with the support of FG.

The FG has an eligibility mechanism that allows a credible identification strategy. Since the eligibility of the firms to FG guarantee is assessed through a scoring system based on balance-sheet observables, we are able to estimate the impact of the scheme at the threshold for eligibility by using a regression discontinuity design (RDD). 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 become unable to repay their loans. Moreover, no effect is found for firm investments and sales. 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 (2015) conditional independence assumption (CIA) to make inference about the impact of the FG for firms that are distant 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 within 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

[2] See: http://www.fondidigaranzia.it/.

includes both balance-sheet data and (confidential) information drawn from the Credit Register. Sect. 5 explains the empirical strategy and provides empirical evidence that substantiates the 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 the policy implications and some interesting issues for future research.

2. Related literature

Policies aimed at alleviating firms' financing constraints find their rationale in the possibility of a 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 and Udell, 1992). Among the set of instruments aimed to facilitate SMEs access to credit, PGSs gained increasing popularity among policymakers, especially in the aftermath of the financial crisis.[3] In the eyes of a policymaker, these schemes have a very appealing feature: due to their very high multiplier, they induce a great mobilization of private financing compared to the public funds involved.

Economic reasoning, however, would suggest a more mixed picture. On the bright side, if firms are unable to raise adequate collateral, credit guarantees can improve their access to credit, both in terms of quantity and costs; credit guarantees can also lead to a learning process, where banks might reshape their risk perception of beneficiary firms (Meyer and Nagarajan, 1996). However, the benefits of the intervention might fall below expectations to the extent that the firms that receive the extra-collateral are those that would have been financed anyway. Not only: distortive effects might also make their appearance. As suggested by Saito and Tsuruta (2014), PGSs might enhance both adverse selection - since banks are insured against incurring losses from default, they

[3] Minelli and Modica (2009) provide a theoretical model that compares the respective merits of different policies in ameliorating credit constraints. They find that interest rate subsidies and loan guarantees are preferred to investment subsidies and direct provision of collateral. Similarly, Arping et al. (2010) argue that credit guarantees should be preferred to firm subsidies.

are enticed to ask seemingly risky borrowers to apply for credit guarantees - and moral hazard - small businesses with guaranteed loans are more likely to default, as banks have lower incentives in exerting both accurate screening and monitoring of the borrowers. Finally, public collateral is very attractive for the banks, due to the virtually risk-free status of the guarantor and the readiness of executions in case of firms' default.

The empirical research on the effectiveness of PGSs is rather scant. 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. (2008) 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. (2007) use firm-level survey data from a Canadian program (*Canada Small Business Financing*), finding 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. (2010) 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. Similarly, Saito and Tsuruta (2014) focus on Japanese credit guarantee programs. Using bank-level data, they find a positive correlation between the amount of guaranteed loans and the rate of firm default. Moreover, the ratio of default is larger in the case of fully guaranteed loans.

As for Italy, Zecchini and Ventura (2009) use data on the Italian *Fondo di garanzia*, from 2000 to 2005. They employ a difference-in-differences 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).[4] 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 guaranteed 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: the RDD set-up. Moreover, it focuses on the years of the financial crisis, 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 FG started its activity in 2000. Initially the volume of bank loans guaranteed remained quite small, totaling €11 billion until 2008 (Figure 1). The figure boomed with the inception of the crises. From 2009 to 2014, €54 billion of loans to SMEs benefited from the public guarantee. The growth in volumes reflects the desire of the Italian authorities to counterbalance the effect of the credit crunch, particularly severe for SMEs, which, in an

[4] A follow-up study, by Boschi et al. (2014), extends the analysis by looking at the asymmetries in effectiveness following different coverage ratios (guarantees over guaranteed loans), finding that low coverage are associated with a lack of additionality.

environment of increased credit risk, experienced a more significant drop in credit flows and a stronger rise in interest rates with respect to larger firms.[5]

The provision of guarantees[6] is limited to SMEs, defined according to EU criteria,[7] of the private sector, which includes manufacturing, construction and services. However, some specific sectors, such as agriculture, automobile and financial services, are not covered by the scheme 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 are no constraints in terms of the final use of the funding by the borrower. It is important to notice that in case of default the financing institution can immediately enforce the FG to meet its obligation ("first demand guarantee").

As other PGSs, the scheme involves three agents: a bank, a firm, and the FG.[8] A SME that needs to borrow might ask the bank to apply for a public guarantee. Alternatively, it is the bank that might propose to the firm to apply for the guarantee. The bank has to verify 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 FG fees are generally low, the labor costs related to the bank official that materially have to collect the information and make use of the software are not negligible.[9] The scoring system takes into account four indicators (they are slightly different according to the firm economic sector) of the firm financial condition in the two years preceding that of the

[5] See Albareto and Finaldi Russo (2012), Bank of Italy (2012, 2013), OECD (2012, 2013).

[6] 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/.

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

[8] In case of counter-guarantees the scheme could also involve a mutual guarantee institution (so called *Confidi*). We analyze the role of *Confidi* for our results in Sect. 6.3.

[9] The costs would amount to about €600, as estimated by the officers of the in charge of the program at the Ministry of economic development.

application; the FG guidelines list the balance-sheet variables to be used for computing the indicators.[10]

For each of the two years preceding that of the application, the software calculates from the values of the balance-sheet variables a single continuous "partial score". The partial score is discretized in three categories (A=good; B=intermediate, C=bad), as described in Table 1. The combination of the two partial scores, one for each year (with higher weights envisaged for more recent scores), allows the assignment of the final score. According to the final score, the applying firms are splitted 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 the ultimate approval or rejection. Rejection, however, has been a rare event. Figure 2 shows, for the period 2005-12,[12] the numbers of requests received by the FG by year and type of final decision.

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 FG 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 FG Therefore, the firm is not included in the FG dataset.

[10] The soundness of firms' financial structure is measured for the industry (service) sector by the ratios of equity and long-term loans on fixed assets (short-term assets on short-term liabilities) and equity on total liabilities (short-term assets on sales). Short-term financial burden is measured by the ratio of financial expenses on sales. Cash-flow is measured by the ratio of cash-flow on total assets.

[11] According to the FG 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.

[12] This is the period for the which FG micro data have been made available to us (see Sect. 4).

Limited to Type-1 and Type-2 firms the FG dataset includes, among others, the fiscal identifiers for the firms, the date of guarantee approval by the FG and that of the provision of finance by the bank, and the respective amounts of the loan and the guarantee. Crucially, we know the algorithm used to calculate the FG final score (eligibility) from the firm balance-sheet observables of the two years prior that of the request. As explained in Sect. 5.1, we calculate the final score for the SMEs not included in the FG dataset (therefore, non-eligible firms and eligible non-applying firms).

We make use of two additional datasets. 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 very small firms. The CERVED archive is used both to collect outcomes and covariates (see Sect. 6) and to estimate calculate the FG final score for the firms not included in the FG dataset.[13] The second dataset is the Credit Register (CR). 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 register.[14] 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.

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[13] 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. Note, however, that we cannot be sure that banks used exactly these data to apply for the FG guarantee: it is possible that they use provisional financial statements. As matter of fact, as documented in Sect. 5.1) we fail to predict eligibility for a very small percentage of firms receiving the guarantee.

[14] Only bad loans are included in the Credit Register independently of their amount. For further details, see: http://www.bancaditalia.it/servizi-cittadino/servizi/accesso-cr/index.html.

5. The identification strategy and the estimation sample

We first (Sect. 5.1) describe our identification set-up and then (5.2) provide the sample details and the 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. As explained in Sect. 3, eligibility is awarded through a multi-stage scoring system, which determines a continuous forcing variable: the final score. Under a certain score, the threshold, no eligibility is awarded. Above the threshold, a firm is eligible but not necessarily approved (approval depends on the additional assessment).

For the SMEs belonging to the sectors covered under the scheme (see Sect. 3) we calculate the final score, by replicating the FG routine on CERVED data. Equipped with the FG measure for eligibility, we are able to estimate the ITT (*intention to treat*) parameter, which is an interesting quantity for the policy makers, as it measures the effect of the program over the wide population of firms targeted under the scheme, which includes both treated and untreated firms. Then, we enrich the data with information on the firms that actually received the guarantees. This allows us to estimate the LATE (*local average treatment effect*), by using a fuzzy RDD (F-RDD) (see: Lee and Lemieux, 2010 and Imbens and Angrist, 1994).

Notice that in our set-up compliance is imperfect, both below and above the threshold (*two-way noncompliance*). Above the cutoff, we have eligible non-applying firms and eligible and applying firms, rejected by the FG following the additional assessment. Below the cutoff, we also have noncompliant units, because by replicating the FG routine with CERVED data we fail to predict successfully the eligibility status for 0.3% of the firms that were treated under the program. This occurrence is likely related to the fact that the data used by the bank officers might be different from the CERVED data we use in the replication.

5.2 Sample details and balancing properties.

We use the merged FG-CERVED-Credit Register sample focusing on the functioning of the FG during the period 2005-2010. This time span permits us having enough balance-sheet and credit register data to analyze the effects of FG guarantees in the two years following each operation, a time window that allows the impacts to materialize and aims at highlighting not temporary, medium-term effects of the intervention.

Observations are collapsed by the year in which the guarantee has been received, and we follow the treated firms for the first two years following the treatment. The time structure of the control units replicates that of applying firms.[15] Therefore, macroeconomic changes occurred in the six years of our analysis are differentiated away. 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). In any case, we also analyze the extent to which pre-crisis impacts differ from those referring to post-2008 (see Sect. 6.3). We consider several outcome variables, in order to assess both direct (first round) and indirect (second round) effects of the public guarantee: disbursed bank loans; granted bank loans; interest rate; probability of bad loans; sales; investments; commercial debts; working capital. We also focus on a set of additional covariates, which we mainly use to test the balancing properties of our sample (Appendix 2 provides the description of the variables used throughout the paper).

The sample includes about 84,000 manufacturing and service SMEs. Figure 3 describes the fraction of firms in our sample that receive the treatment. On the x-axis it depicts the running variable; that is, the final score, which is normalized to display the value of zero at the threshold between Type-0 and Type-1 firms (the score that splits Type-1 from Type-2 firms is approximately at the value of 1

[15] Appendix 1 provides additional details of the sample construction.

of the forcing variable). The figure illustrates 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 FG guarantee does not pay out.

Table 2 illustrates the composition of our 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 FG collateral is on average equal to 55%, with a small standard deviation (15%).[16] The results presented below are calculated by using an estimation sample for which it is required that a firm has both in CERVED and CR at least one pre-intervention and two post-intervention observations. This requirement helps to ensure that the findings derived with two different datasets remain comparable. However, our results are confirmed (see: Sect. 6.3) when using a larger sample, in which we lift this requirement (and therefore use all the available information in each dataset, irrespective that for a given firm we might fail to find information in the other dataset).

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

[16] The reduced variability across firms of the percentage of coverage reassures that our estimates are not driven by relevant non-linearity in the ratio between guarantees and guaranteed loans.

side of the boundary (Lee, 2008). A commonly used test to assess the absence of manipulation across the threshold is the McCrary test (McCrary, 2008). Unfortunately this test is not applicable to our data, as the continuous forcing variable is devised starting from a discrete score of firms (types 0, 1, and 2). In particular, types 0 firms will fall in the interval (-1,0); types 1 firms in the interval (0,1); types 2 firms in the (1,2) interval (see Appendix). As the number of firms of type 0 (not eligible) is larger than that of type 1 (eligible) the density function of the forcing variables jumps by construction at the threshold. The absence of manipulation, however, is supported by the following considerations. Firstly, we replicated the scoring mechanism using firm balance sheet variables taken from Cerved and not those passed by the firms to the banks and used by the latter to verify the eligibility of the firm (through the scoring system (software) provided by the FG): only a very small fraction of firms (0.3%) that we classified as not eligible was actually admitted to the guarantee program. Secondly, the screening procedure involved both the commercial banks and the FG, making cheating more difficult.

One implication of the local randomized result is that the empirical validity of the RDD can be tested, at least for a large set of observables. 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 threshold 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 default.[17] 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 Herfindahl index), as well as pre-intervention riskiness, working capital, and commercial debts. 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.[18] 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.

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.

6.1 Access to credit and bad loans.

Since we are interested in the total effectiveness of the scheme, our measures for credit availability, interest rates and bad loans reflect the firm position *vis-à-vis* the banking system as a whole. Therefore, they include the credit relations that a firm might have with banks different from the

[17] Throughout the paper, firms' probability of default is measured referring to being classified by the banks among the bad debts.

[18] To account for this unbalancing, in Sect. 6.3 we explicitly control for lagged investments in our specifications.

one that provided the guaranteed loan. For instance, if these banks provide additional (non-guaranteed) loans because of a positive signaling effect stemming from the fact 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 third degree polynomial specification (see, however, Sect. 6.3 for alternative specifications). Non-parametric results are calculated by using the optimal bandwidth procedure suggested by Imbens and Kalyanaraman (2012), with a triangular kernel (again, Sect. 6.3 explains that this choice is not crucial for the findings). Figures 4 illustrate the canonical RDD graph when the outcome is taken to be disbursed loans. In the figure, 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).

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 FG has a significant impact on the availability of credit for the universe of Italy's SMEs. The parametrically estimated ITT is equal to 4.9% and 3.8% of the (two-year cumulative) growth rates in credit flows, respectively for loans disbursed and granted. When estimated with non-parametric methods the ITT is equal to 3.0% and 3.8% respectively, 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%. 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. We consider the average interest rate on short-term loans. This variable is computed with respect to the whole set of banks issuing loans to the firm, consistently with our measures for credit flows.[19] At the cutoff our estimates suggest that the scheme does not have an impact on the cost of credit.

Panel D turns to riskiness. It shows that the probability (calculated over two years) that a firm enters the bad loans significantly increases because of the FG (of about 50% both parametrically and 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 3.2% and 3.0%, respectively. This result confirms that a scheme like the FG might have unwanted consequences: limited liability might enhance adverse selection and moral hazard problems, which reflect themselves into the likelihood that the credit turns into a bad loan.

6.2 Second round effects.

Next, we check whether the guarantees have effects on some aspects of firms' economic and financial conditions. This set of results could give interesting hints on "second round" effects of the policy and suggest how firms used the additional financing provided by the FG guarantees documented in Sect. 6.1.

In Table 5, Panel A we consider investments, which should be positively affected if firms are financially constrained, and sales (Panel B), which should rise if business growth is limited by the availability of external financing. For both outcomes, we fail to find any impact. 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 5 also documents the impact of the FG on some additional balance-sheet outcomes. Our results suggest that the extra-credit was mainly

[19] Focusing on short-term loans is standard when access to credit is at stake. We also estimate the impact on long-term interest rate, finding no effect.

used to finance inventory accumulation and to extend trade credit to customers (Panel C). We fail to find a positive impact on liquid assets (Panel D) or any sign of substitution effect with trade debt (Panel E). These latter findings seem reasonable in the context of a liquidity squeeze, in which short-term finance is a scarce resource.

6.3 Robustness.

The above findings have been checked trough a full-fledged robustness analysis. Results are documented in the Appendix. For the sake of brevity, we present only results from parametric specifications (non-parametric results, available upon requests, provide the same evidence).

Table A1 provides, in columns (1) - (3), the estimates of the impact of the program on (some of the) outcomes for a sample that includes only manufacturing firms (about 20,000 of them). The firms in this sample are likely characterized by greater similarity in production; moreover, the sales of tradable goods should be less affected by the idiosyncratic conditions of the local markets.[20] Overall, the estimates at the threshold we obtain with this sample are very similar to those described in Tables 4 and 5. As for the credit flows, the ITT is estimated to be about 5% while the LATE is always larger than 40%. 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 for sales and investments.

Table A1 depicts in columns (4) - (6) the results we obtain by using a larger estimation sample, in which we do not require that a single firm has both CERVED and CR information (see Sect. 5.2). Results mirror those previously obtained. The probability that a firm enters the bad loans is slightly higher with respect to the baseline estimates.

[20] For instance, the recession in 2008-09 – which is included in the estimation time-window – was mainly export-led: therefore, manufacturing firms have likely been more homogeneously hit.

Table A2 analyses the extent to which the results could be affected by the circumstance that in some cases the guarantee is extended through a mutual guarantee institution (counter-guarantee; see Section 3). We considered a restricted definition of treatment, selecting only firms that received the extra collateral through a mutual guarantee institution only. Results, displayed in the first two columns, replicate those obtained on the entire sample. We also study the differential exposure of the firms to the crisis, as some of them received the guarantee before the Lehman collapse (Section 5.2). We added a dummy for the firms that received the treatment after October 2008. Results, shown in columns (c), (d) and (e) of Table A2 confirm our previous findings. They also highlight that the period of crisis resulted into a lower amount of loans, an increase of bad loans and a drop of sales for both treated and untreated firms. Finally, the last three columns of table A2 tackle the issue of the unbalancing of investments in the two years before the FG intervention (Sect. 5.2). We consider the parametric specification and include as additional control the pre-treatment investment flows. Results are once again confirmed.

Finally, a number of additional checks on the methodological side were also performed.[21] We estimated the non-parametric model using an alternative bandwidth (larger +/-25% with respect to Imbens and Kalyanaraman, 2012), finding the same results as those of the baseline estimates (see Table A3). Finally, we perform another battery of robustness checks. We estimate the non-parametric model using also the Calonico-Cattaneo-Titiunik optimal bandwidth; we also used the rectangular, rather than the triangular Kernel weights. We select the order of the parametric model by the AIC criterion. For all these checks the results were nicely confirmed.

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

[21] These results are not reported; they are, however, available from the authors.

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 (2015) 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 6 focuses on three outcomes (disbursed loans, interest rates, and probability of bad loans).[22] It shows the results from four estimation windows of various width: 0.3, 0.6, 0.9, and 1.2 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 bandwidth of [-0.9,0.9] is the largest one for which the CIA is satisfied (bounded CIA): within that interval 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). Therefore, we are unable to provide far-from-the-threshold inference for firms with an eligibility score outside the [-0.9,0.9]

[22] 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), with very minor modifications.

interval.[23] 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.

Figures 5-7 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, while Table 7 reports CIA estimates. For each outcome, the figures depict both the fitted values for observed outcomes and the bounded CIA-based extrapolations. The estimated impact of the scheme for infra-marginal firms is illustrated by the vertical difference between the two series (i.e., the fitted values for observed outcomes and the extrapolations).

As for disbursed loans (Figure 5) 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). Therefore, as access to credit is concerned, there seems to be no support for deadweight losses. Regarding the interest rates (Figure 6), 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 FG on bad loans (Figure 7) 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 might want to avoid to be signaled as bad borrower) and very bad firms (which may have repayment problems irrespective of the public guarantees).

[23] Hence, we are not able to extend our inference to the Type-2 (outstanding) firms.

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.[24] 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).

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 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-à-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

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

good signal to find easier access to credit elsewhere. These aspects are interesting topics for future research.

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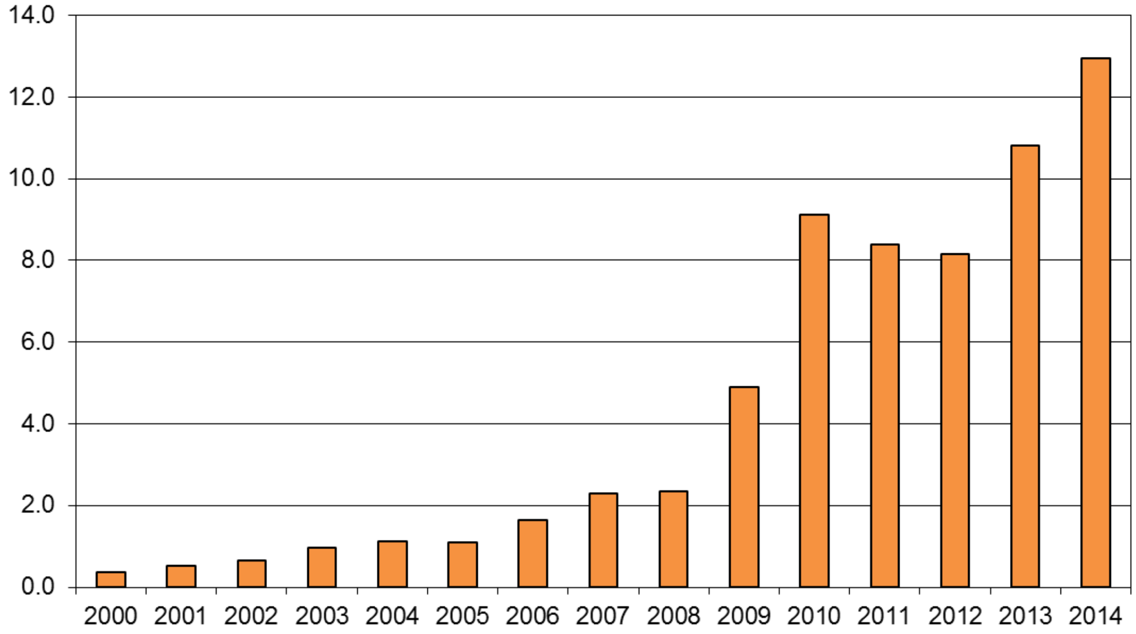
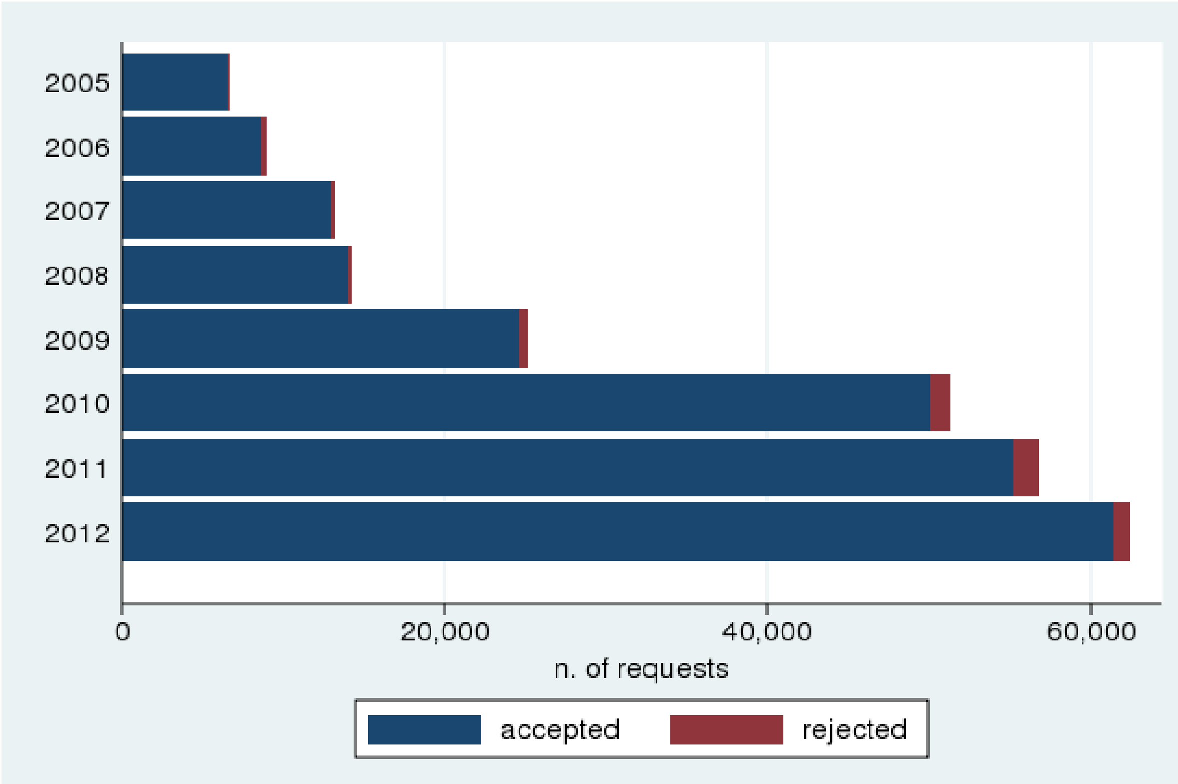
Figure 1. Bank loans to SMEs guaranteed by the FG



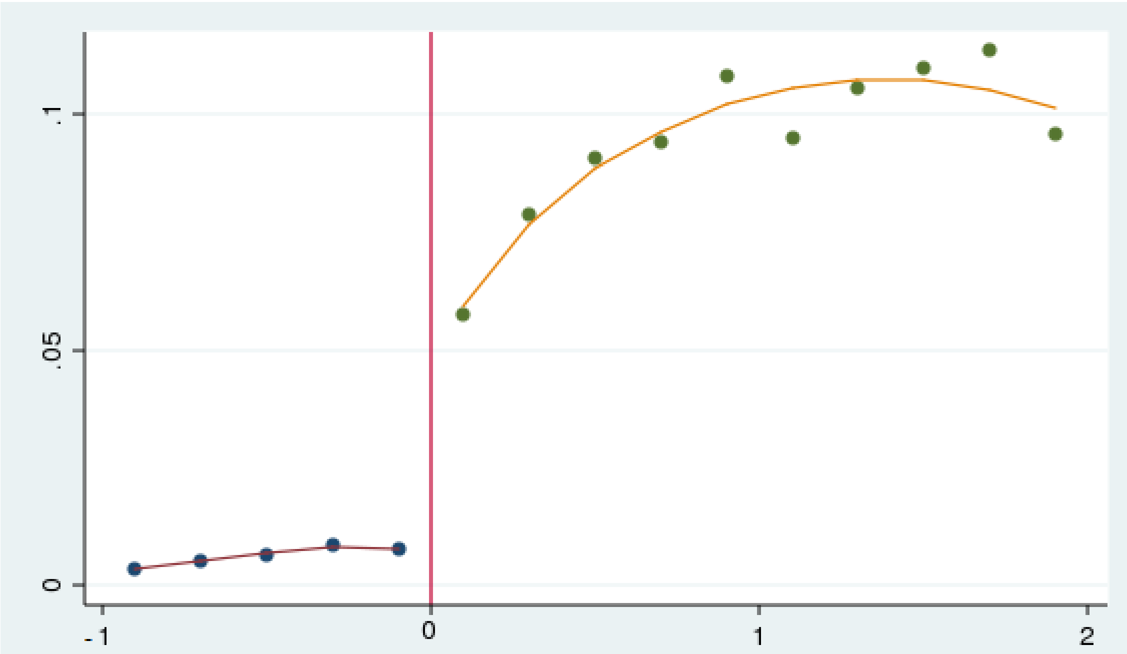
Notes: € billion, outstanding amounts. Source: *Fondo di Garanzia*.

Figure 2. Requests approved and rejected, by year



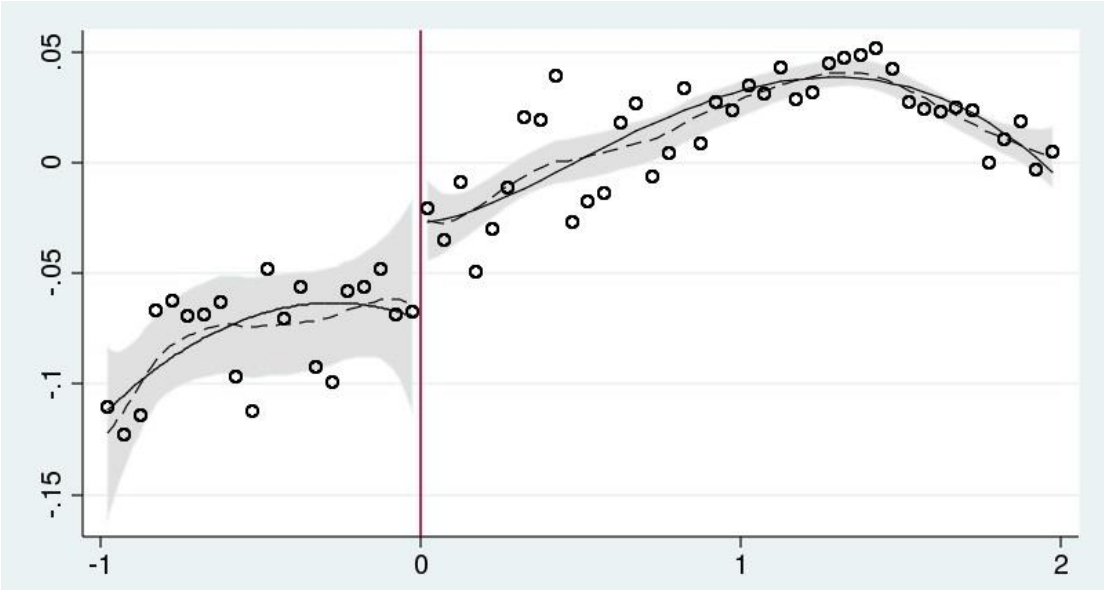
Notes: number of applications received by the FG. Source: FG dataset.

Figure 3. Probability of receiving the treatment



Notes: The running (forcing) variable is on the x-axis. The threshold between Type-0 and Type-1 firms is normalized at the value of 0. Source: our own calculations.

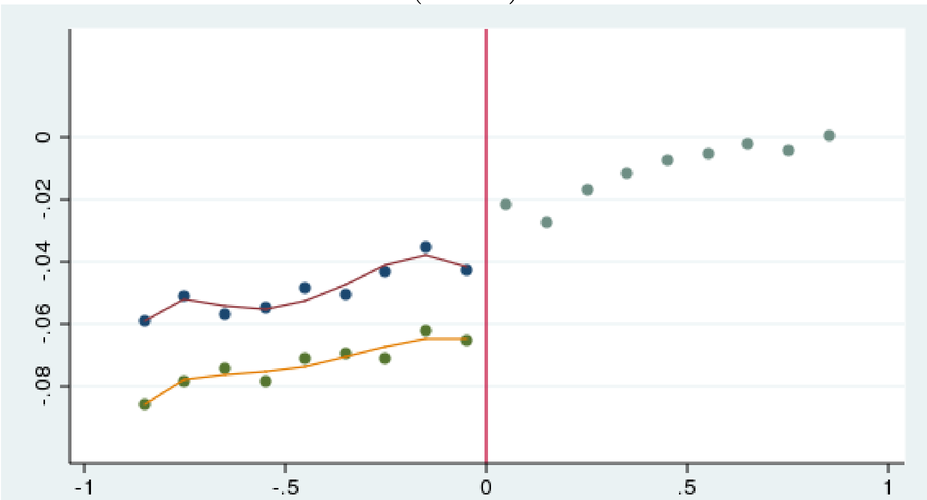
Figure 4. Disbursed loans



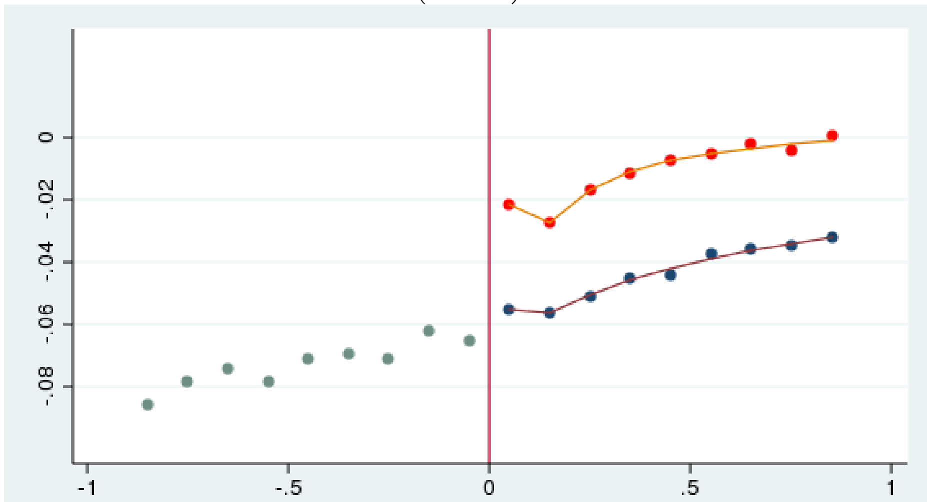
Notes: graphical representation of RDD. Circles stand for averages of the outcome (two-years variation in logarithm of disbursed loans) computed at 0.05 bins (for a total of 60 bins). Solid line (dashed line) stands for polynomial regression (local linear regression). The local linear regression is computed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

Figure 5. CIA-based estimates, Disbursed loans

(Panel A)



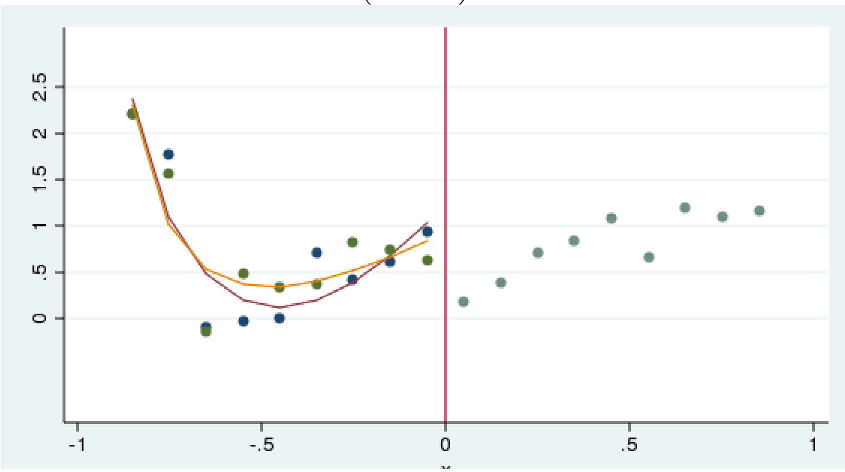
(Panel B)



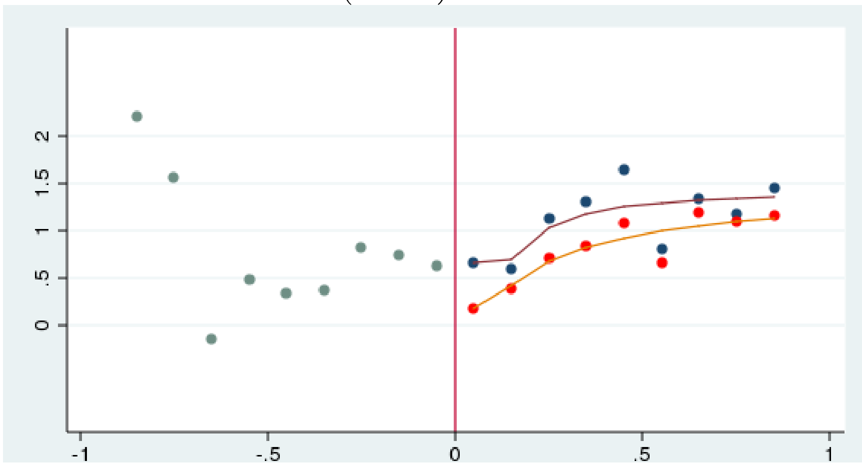
Notes: graphical representation of CIA-based estimates (Angrist and Rokkanen, 2015). The extrapolations are computed through Kline's linear reweighting procedure (Kline, 2011). In Panel A, to the left of the cutoff blue dots represent the CIA-based extrapolations while the green dots represent the fitted 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 6. CIA-based estimates, Interest rate

(Panel A)



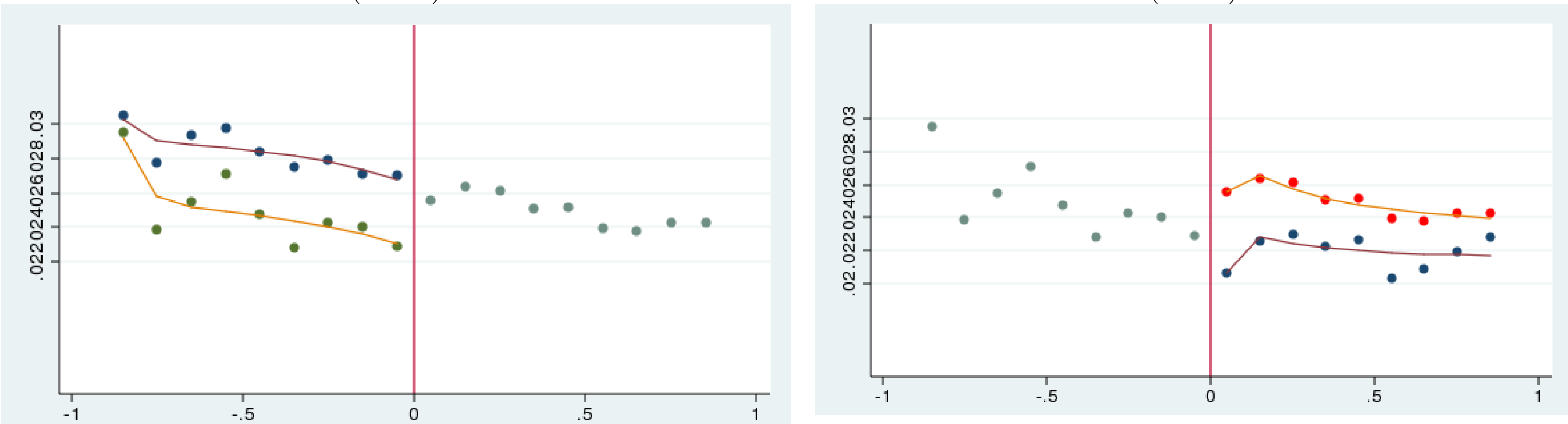
(Panel B)



Notes: graphical representation of CIA-based estimates (Angrist and Rokkanen, 2015). 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 7. CIA-based estimates, Probability of bad loans

(Panel A) (Panel B)



Notes: graphical representation of CIA-based estimates (Angrist and Rokkanen, 2015). 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 FG scoring system: yearly categories and types (1)

Year t-2 Partial score	Year t-1 Partial score	Types	Outcome
A B	A A	Type-2	Eligible Firms. Banks send the application to FG. Approval based on additional assessment
C A B C A	A B B B C	Type-1	Eligible Firms. Banks send the application to FG. Approval based on in-depth additional assessment
B C	C C	Type-0	Not eligible firms

Source: FG official guidelines.

(1) A=good; B=intermediate, C=bad.

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	-0.0201	-0.319
	(0.0164)	(0.292)
Δ Investments	-0.0923***	-1.360**
	(0.0287)	(0.637)
Δ Disbursed loans	-0.0429	-0.619
	(0.0291)	(0.483)
Δ Granted loans	0.0381*	-0.585
	(0.0217)	(0.407)
Δ Probability of bad loan	0.00232	0.0464
	(0.00154)	(0.0336)
Herfindahl index	-0.124	-2.288
	(0.949)	(17.34)
Bank share	-1.214	-18.28
	(0.954)	(14.45)
Sales	0.0727	1.403
	(0.0444)	(0.869)
	B. Non-parametric analysis	
Δ Sales	-0.00766	-0.196
	(0.0106)	(0.272)
Δ Investments	-0.0566***	-1.199***
	(0.0163)	(0.360)
Δ Disbursed loans	-0.0303	-0.51
	(0.0196)	(0.334)
Δ Granted loans	-0.0365*	-0.606*
	(0.0213)	(0.364)
Δ Probability of bad loan	0.000985	0.0154
	(0.00260)	(0.0407)
Herfindahl index	-0.118	-1.83
	(0.894)	(13.85)
Bank share	-1.021	-16.09
	(0.998)	(15.67)
Sales	-0.0349	-1.028
	(0.0433)	(1.299)

Notes: Optimal bandwidth for non-parametric estimates: Imbens and Kalyanaraman (2012) procedure, triangular Kernel. Δ = 2 years variation computed in the pre-treatment period. Standard errors in brackets.

Table 3. Balancing properties (cont.)

Baseline covariate:	ITT	LATE
	A. Parametric analysis	
Probability of bad loan	0.00251	0.0508
	(0.00186)	(0.0403)
Working capital	0.118	2.986
	(0.0768)	(2.207)
Commercial debts	0.0457	0.899
	(0.0581)	(1.129)
	B. Non-parametric analysis	
Probability of bad loan	0.00274	0.0447
	(0.00259)	(0.0426)
Working capital	0.0965	2.908
	(0.100)	(3.036)
Commercial debts	-0.0378	-1.111
	(0.0620)	(1.845)

Notes: Optimal bandwidth for non-parametric estimates: Imbens and Kalyanaraman (2012) procedure, triangular Kernel. Δ = 2 years variation computed in the pre-treatment period. Standard errors in brackets.

Table 4. The impact of FG on the main outcomes

Parametric analysis			Non-parametric analysis	
ITT (1)	LATE (2)	F-test (3)	ITT (4)	LATE (5)
		A. Disbursed loans		
0.0495**	0.563*	9.746	0.0303*	0.457*
(0.0247)	(0.300)		(0.0180)	(0.275)
N=57632				
polynomial degree: 3				
		B. Granted loans		
0.0381*	0.551*	9.632	0.0377**	0.560**
(0.0190)	(0.314)		(0.0149)	(0.229)
N=57912				
polynomial degree: 3				
		C. Interest rate		
0.0621	0.855	8.029	0.0335	0.478826
(0.132)	(1.840)		(0.115)	(1.643)
N=51001				
polynomial degree: 3				
		D. Probability of bad loans		
0.0325***	0.503**	9.527	0.0302**	0.479**
(0.0125)	(0.254)		(0.0125)	(0.205)
N=57502				
polynomial degree: 3				

Notes: columns (1) to (3) report parametric estimates. Columns (4) and (5) report non-parametric estimates. The optimal bandwidth for non-parametric estimates has been retrieved by Imbens and Kalyanaraman (2012) procedure with triangular Kernel. Outliers below 5 or above 95 percentile were dropped. Standard errors in brackets.

Table 5. The impact of FG on additional outcomes

Parametric analysis			Non-parametric analysis	
ITT (1)	LATE (2)	F-test (3)	ITT (4)	LATE (5)
		A. Investments		
0.0001	0.002	10.03	-0.0114	-0.175
(0.0263)	(0.389)		(0.0156)	(0.240)
N= 59930				
polynomial degree: 3				
		B. Sales		
0.0192	0.304	7.949	0.02	0.306
(0.0179)	(0.301)		(0.0126)	(0.194)
N= 60470				
polynomial degree: 3				
		C. Inventories and accounts receivable		
0.0404***	0.599**	19.32	0.0389**	0.598**
(0.0161)	(0.272)		(0.0182)	(0.286)
N=60129				
polynomial degree: 3				
		D. Cash and marketable securities		
-0.0697	-0.995	18.28	-0.0945*	-1.552*
(0.0496)	(0.748)		(0.0523)	(0.877)
N=53193				
polynomial degree: 3				
		E. Commercial debts		
-0.0223	-0.349	16.45	-0.0349	-0.573
(0.0216)	(0.351)		(0.0259)	(0.431)
N=57058				
degree: 3				

Notes: columns (1) to (3) report parametric estimates. Columns (4) and (5) report non-parametric estimates. The optimal bandwidth for non-parametric estimates has been retrieved by Imbens and Kalyanaraman (2012) procedure with triangular Kernel. Outliers below 5 or above 95 percentile were dropped. Standard errors in brackets.

Table 6. Conditional independence test

Window	Loans disbursed		Probability of bad loans		Interest rate	
	below the threshold	*above the threshold*	*below the threshold*	*above the threshold*	*below the threshold*	*above the threshold*
1.2	0.0082	0.0187*	-0.2074*	-0.0063**	-10.9680	2.2551
	(0.0239)	(0.0098)	(0.0125)	(0.0028)	(39.4724)	(4.306)
Obs	3,498	22,035	2,559	18,028	2,334	14,256
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	934	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.

Table 7. CIA Estimates of the effect of the guarantee

	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	0.0307***	0.0429***	0.0117***	0.0120**	-0.0734	-0.0106
	(0.0062)	(0.0091)	(0.0032)	(0.0038)	(0.0637)	(0.0626)
0.6	0.0284***	0.0429***	0.0128***	0.0106**	-0.0743	-0.0248
	(0.0055)	(0.0091)	(0.0027)	(0.0036)	(0.0620)	(0.0710)
0.3	0.0306***	0.0411***	0.0120***	0.0088**	-0.0438	-0.0161
	(0.0047)	(0.0091)	(0.0025)	(0.0035)	(0.0546)	(0.0693)

Notes: Bootstrapped standard errors with 200 replications.

Table A1. The impact of FG on the main outcomes, manufacturing firms and extended sample

manufacturing firms			extended sample		
ITT	LATE	F-test	ITT	LATE	F-test
(1)	(2)	(3)	(4)	(5)	(6)
A. Disbursed loans					
0.0710*	0.586*	9.882	0.0397*	0.471*	21.1
(0.038)	(0.346)		(0.022)	(0.274)	
N=20359			*N=72300*		
B. Granted loans					
0.0520*	0.436*	9.365	0.0280**	0.411**	25.23
(0.0275)	(0.260)		(0.012)	(0.189)	
N=20739			*N=71802*		
C. Interest rate					
0.281	2.366	7.829	0.12	1.657	10.08
(0.194)	(1.826)		(0.118)	(1.704)	
N=18898			*N=61752*		
D. Probability of bad loans					
0.0359**	0.329*	10.858	0.0437***	0.677***	13.4
(0.0169)	(0.190)		(0.012)	(0.259)	
N=21986			*N=82680*		
E. Investments					
0.0602	0.473	10.75	0.00496	0.0747	12.11
(0.0381)	(0.324)		(0.024)	(0.359)	
N= 20692			*N= 71616*		
F. Sales					
-0.00345	-0.0328	6.884	0.0174	0.28	9.637
(0.0282)	(0.269)		(0.016)	(0.276)	
N= 20531			*N= 71817*		

Notes: parametric estimates, using a 3rd degree polynomial. Outliers below 5 or above 95 percentile were dropped. Standard errors in brackets.

Table A2. The impact of FG on the main outcomes, extended sample

firms guaranteed by mutual guarantees associations (1)		parametric estimates controlling for the years of crisis (2)			parametric estimates controlling for the lagged amount of fixed assets (3)		
ITT	LATE	ITT	LATE	Crisis	ITT	LATE	Lagged assets
(a)	(b)	(c)	(d)	(e)	(f)	(g)	(h)
			A. Disbursed loans				
0.0458*	0.522*	0.0450*	0.528*	-0.0546***	0.0500**	0.465*	-0.0168*
(0.025)	(0.295)	(0.025)	(0.306)	(0.015)	(0.025)	(0.239)	(0.009)
N=53288			*N=57632*			*N=57582*	
			A. Granted loans				
0.0376**	0.646*	0.0359*	0.527*	-0.0688***	0.0363*	0.364*	-0.0157**
(0.019)	(0.375)	(0.019)	(0.314)	(0.014)	(0.019)	(0.200)	(0.008)
N=53616			*N=57912*			*N=57874*	
			C. Interest rate				
0.072	1.144	0.0564	0.778	-2.208***	0.0401	0.394	-0.0398
(0.132)	(2.121)	(0.112)	(1.569)	(0.072)	(0.132)	(1.303)	(0.052)
N=46689			*N=51001*			*N=50968*	
			D. Probability of bad loans				
0.0334***	0.601**	0.0329***	0.525**	0.0366***	0.0246**	0.261*	-0.0197***
(0.013)	(0.290)	(0.013)	(0.267)	(0.014)	(0.013)	(0.143)	(0.005)
N=53228			*N=57502*			*N=57502*	
			E. Investments				
-0.00184	-0.0324	-0.00259	-0.039	-0.0783***	0.0273	0.274	0.0226**
(0.026)	(0.465)	(0.026)	(0.396)	(0.015)	(0.026)	(0.266)	(0.011)
N= 55677			*N= 59930*			*N= 59930*	
			F. Sales				
0.0192	0.359	0.0156	0.252	-0.105***	0.0282	0.292	0.0273
(0.018)	(0.351)	(0.018)	(0.297)	(0.012)	(0.018)	(0.196)	(0.026)
N= 56156			*N= 60470*			*N= 59930*	

Notes: (1) treated firms are those that received the public guarantee by means of mutual guarantee associations; firms that received the guarantee by banks were dropped; (2) parametric estimates using as additional control the dummy crisis, equals to 1 for firms (controls and treated) whose outcomes were observed after 2008; (3) parametric estimates using as additional control the lagged values of assets; polynomial degree: 3

Table A3. Non-parametric estimates of the impact of FG using an alternative bandwidth

	all firms		manufacturing firms	
	ITT	LATE	ITT	LATE
	(1)	(2)	(3)	(4)
	0.0284*	0.406*	0.0435	0.461
disbursed loans	(0.0160)	(0.231)	(0.0282)	(0.305)
	0.0359***	0.503***	0.0416*	0.432*
granted loans	(0.0133)	(0.191)	(0.0230)	(0.248)
	0.0128	0.175	-0.0312	-0.331
interest rate	(0.101)	(1.386)	(0.160)	(1.699)
	0.0246**	0.395**	0.0401***	0.401***
prob of bad loans	(0.0113)	(0.184)	(0.0145)	(0.150)
	-0.0124	-0.182	0.0395	0.364
investments	(0.0151)	(0.222)	(0.0321)	(0.298)
	0.020	0.295	-0.00317	-0.0287
sales	(0.0123)	(0.181)	(0.0202)	(0.183)

Notes: non parametric estimates. The bandwidth is +/- 25% larger with respect to the optimal bandwidth retrieved by Imbens and Kalyanaraman (2012), characterizing our baseline estimates.

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). The control sample recovered from CERVED includes about 71,000 firms.

Since the time dimension of the sample of applying firms has been collapsed, the years considered in the post treatment period will differ accordingly to when each applying firm reached the Fund. As regards the firms in the control group there is no application date. Nonetheless, we need to select a post treatment period for each of them as well. To avoid that our analysis is biased by different trends in the outcome variables over the sampled years, we make sure that the distribution of the treatment periods for the control firms over the years 2005 onwards mirrors that of the treated firms. To do so we randomly associate all control firms to a given treatment period.

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)
- 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)

Appendix 3 – Scoring mechanism

For each of the two years preceding that of the application, the software calculates a continuous "partial score" for each of the following four indicators: asset coverage, leverage, incidence of financing costs, EBITDA. Afterwards, these four scores are converted into a discrete score, according to whether the continuous score falls within four intervals with given thresholds: namely, the values are 0 (very poor), 1, 2, or 3 (outstanding). Then, the discrete scores obtained over the four indicators are summed up, resulting in a total score ranging from 0 to 12. From the latter score, firms are finally given a partial score, which is discretized in three categories (A=good; B=intermediate, C=bad). The combination of these two partial scores, one for each year (with higher weights envisaged for more recent scores) allows the assignment of the final score as described in Table 1. According to the final score, 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). The additional assessment concludes with the ultimate approval or rejection. Rejection, however, has been a rare event. Figure 2 shows, for the period 2005-12, the numbers of requests received by the FG by year and type of final decision.

Starting from the discrete final score (types 0, 1, and 2) we devise a continuous forcing variable, ranging from -1 to 2. Types 0 firms will fall in the interval (-1,0); types 1 firms in the interval (0,1);

types 2 firms in the (1,2) interval. The continuous variable is computed following an algorithm such that, within their interval, firms having better partial scores are characterized by a greater value of the continuous forcing variable.

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